A long walk to freedom?
Nonviolent civil resistance and campaign duration

Mark Williamson

Master of Arts

Political Science

McGill University

Montreal, Quebec

October 15, 2018

A thesis submitted to McGill University in partial fulfillment of
the requirements for the degree of Master of Arts

Acknowledgements

Abstract

An emerging empirical literature on civil resistance has demonstrated that nonviolent campaigns have a higher probability of achieving their political objectives than groups that opt for armed violence against the government. However, existing research has not considered the extent to which the effectiveness of nonviolent versus violent strategies is related to the duration of each type of campaign. This thesis expands upon current understandings of civil resistance to explicitly consider the relevance of campaign duration to the outcome of an uprising. The findings suggest that the strategic advantage of nonviolent resistance over armed rebellion is time-dependent rather than absolute. Nonviolent campaigns, while more likely to succeed overall, become increasingly less likely to realize their objectives as time goes on. The main mechanisms that explain the waning effectiveness of nonviolent campaigns are a gradual demobilization of participants and a decreasing likelihood of defections from the security forces. An analysis of the tactics and outcomes of 250 violent and nonviolent insurrections between 1945 and 2006 provides support for these claims. Concerns about selection bias are addressed using an instrumental variable approach and illustrative case studies provide evidence of the hypothesized causal mechanisms.

Résumé

Une littérature empirique émergente sur la résistance civile a démontré que les campagnes non-violentes ont une probabilité plus élevée d'atteindre leurs objectifs politiques que les groupes qui optent pour la violence armée contre le gouvernement. Cependant, la recherche existante n'a pas examiné dans quelle mesure l'efficacité des stratégies non violentes versus violentes est liée à la durée de chaque type de campagne. Cette thèse développe les compréhensions actuelles de la résistance civile pour considérer explicitement la pertinence de la durée de la campagne par rapport au résultat d'un soulèvement. Les résultats suggèrent que l'avantage stratégique de la résistance non-violente sur la rébellion armée dépend du temps plutôt que de l'absolu. Les campagnes non-violentes, bien que plus susceptibles de réussir globalement, deviennent de moins en moins susceptibles de réaliser leurs objectifs avec le temps. Les principaux mécanismes qui expliquent l'affaiblissement de l'efficacité des campagnes non-violentes sont la démobilisation progressive des participants et la diminution des risques de défection des forces de sécurité. Une analyse des tactiques et des résultats de 250 insurrections violentes et non-violentes entre 1945 et 2006 étaye ces affirmations. Les préoccupations concernant le biais de sélection sont traitées à l'aide d'une approche de variable instrumentale et des études de cas illustratives fournissent des preuves des mécanismes causaux hypothétiques.

Table of Contents

List of Tables

List of Figures

Introduction

Between 2000 and 2005, a series of 'Colour Revolutions' swept authoritarian leaders from power in Serbia, Georgia, Kyrgyzstan and Ukraine. In each country, the speed by which regime change was achieved was remarkable: none of the popular protests that overthrew the government lasted for more than a few months (Finkel and Brudny 2012). Similarly, all of the successful civil resistance campaigns that came about during the Arab Spring lasted less than a year.

Meanwhile, numerous armed movements have had to endure decades of war against their governments to achieve their political objectives. In Chad, for example, the Front de liberation nationale du Tchad (FROLINAT) struggled for 24 years before finally defeating the government in 1990 (Buijtenhuijs 2001). The Free Aceh Movement in Indonesia sustained an insurgency from 1976 to 2005 until it forced the central government to grant autonomy and end its occupation of the Aceh region (Stange and Patock 2010).

In terms of the speed by which their objectives are achieved, anecdotal evidence suggests that nonviolent campaigns possess a comparative advantage over movements that opt for armed conflict against the government. Yet thus far there has been no systematic comparison of the durations of movements that adhere to these two different tactical approaches. In this thesis, I seek to extend current understandings of civil resistance by explicitly considering how campaign duration affects the outcome of an uprising.

I argue that nonviolent campaigns do not possess an absolute advantage over armed rebellions, but rather that their relative effectiveness is time-dependent. Nonviolent campaigns, while more likely to succeed overall, become increasingly less likely to realize their objectives as time goes on. To a large degree, this waning effectiveness

of nonviolent resistance is driven by a gradual demobilization of participants and a decreasing likelihood of provoking defections from the security forces.

To assess my theoretical claims, I employ a series of event history models using data on the tactics and outcomes of violent and nonviolent campaigns between 1945 and 2006 (Chenoweth and Stephan 2011; Chenoweth and Lewis 2013b). I demonstrate empirical support for the argument that nonviolent campaigns become less effective over time and validate the causal effect through an instrumental variables design that uses a country's fertility rate as an instrument for campaign type. I also provide indirect support for my theory by demonstrating that the mechanisms that predict nonviolent success − large campaign sizes and security force defections − similarly tend to dissipate over time.

My analysis is structured as follows. First, I review the literature on campaign strategy and success. Second, I outline a theory of how nonviolent and violent strategies relate to campaign duration and make an argument about the relative effectiveness of the two approaches over time. Third, I describe the data and empirical strategy that is used to test my hypotheses and identify the causal effect of nonviolence. Fourth, I present the results of my cross-campaign analyses. Fifth, I describe two case studies that help to illustrate the causal mechanisms that are identified in my quantitative models. Finally, I conclude with a discussion of the implications of my findings for the future study of nonviolent resistance.

The success of nonviolent resistance

Over the last several decades, nonviolent resistance has emerged as an important topic for students of contentious politics. This increased attention is partly motivated by a growing prevalence of nonviolent protest as a resistance strategy. As Figure 1
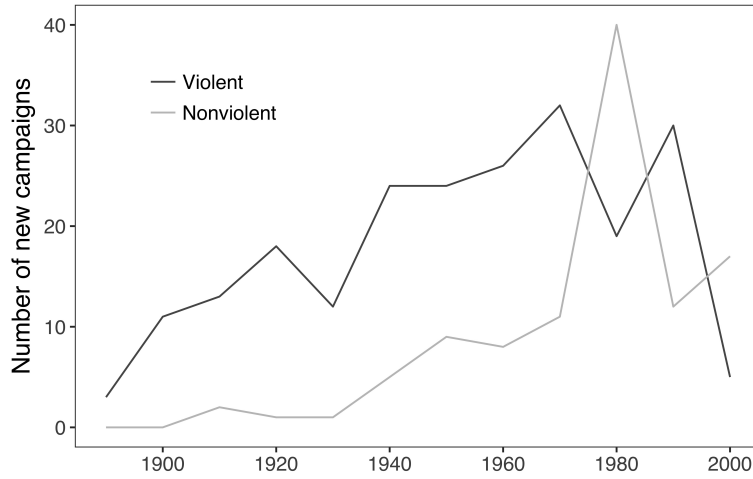
Figure 1: Number of new resistance campaigns by decade, 1899 to 2006

shows, the number of new civil resistance campaigns has risen steadily throughout the twentieth century. Nonviolent resistance even surpassed armed rebellion as the strategy of choice among campaigns during the third-wave transitions of the 1980s and more recently in the 2000s.

Conceptually, violence and nonviolence can be seen as opposing ends on a continuum of noninstitutional, confrontational strategies for achieving political objectives. Violent resistance relies on the use of physical force to harm one's opponent and coerce them into concessions. By contrast, nonviolent resistance encompasses a range of tactics, including protests, boycotts, strikes, sit-ins and other acts of civil disobedience that use mass mobilization to oppose policies or remove opponents from power (Sharp 1973). The use of nonviolence as a strategic tool is distinct from, but may overlap with, individuals' moral positions on the use of violence (Schock 2013).

Most studies of nonviolent resistance identify campaigns as a relevant unit of analysis. The dominant approach defines a campaign as 'a series of observable, continuous, purposive mass tactics or events in pursuit of a political objective' (Chenoweth and Lewis 2013b, 416). The events that make up a campaign can span anywhere from

3
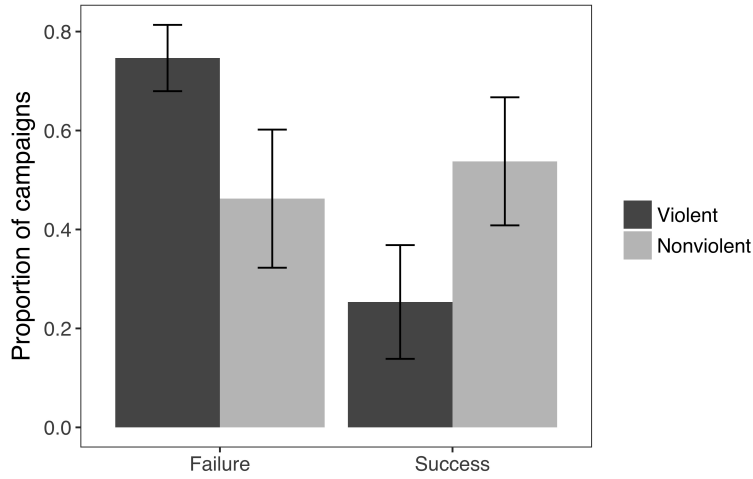
Figure 2: Outcomes of resistance campaigns by strategy

days to years, but campaigns always constitute more than spontaneous, one-off acts. Studies of armed conflict have relied on a similar approach, where scholars have focused on campaigns, namely wars and insurgencies, as aggregations of coordinated battle events.

Based on these definitions, an emerging empirical literature on civil resistance has demonstrated that nonviolent campaigns tend to be more successful in achieving their objectives than campaigns that opt for armed violence against the government (Bayer, Bethke, and Lambach 2016; Celestino and Gleditsch 2013; Chenoweth and Stephan 2011; Gleditsch, Olar, and Radean 2016). This literature has grown considerably with the availability of new data, which has helped to bring greater analytic rigour to the study of nonviolent tactics (Lehoucq 2016). Prior to this, scholars of civil resistance largely relied on qualitative case study approaches and were less concerned with the direct comparison of nonviolent versus violent tactics (Chenoweth, Perkoski, and Kang 2017; Schock 2013; e.g. Ackerman and Kruegler 1994; Bob and Nepstad 2007; Schock 2005).

Among recent empirical studies, the most influential work demonstrating a com-
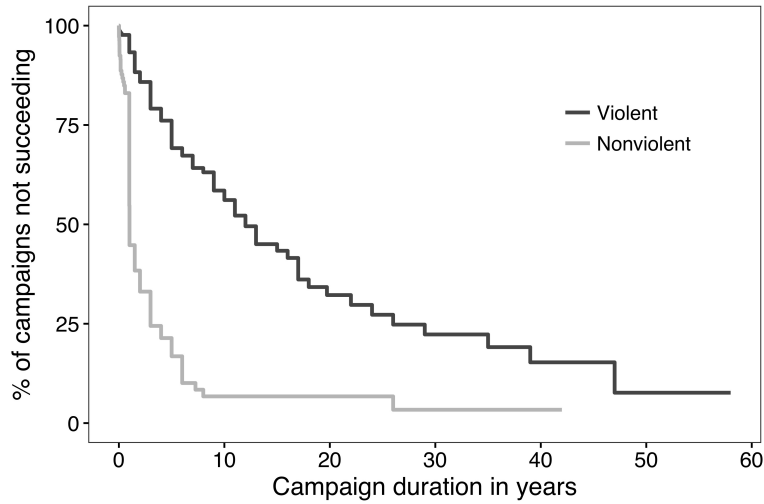
Figure 3: Kaplan-Meier estimates of survivorship by campaign strategy

parative advantage of nonviolence is Chenoweth and Stephan's (2011) *Why Civil Resistance Works*. In this book, the authors introduced a dataset of resistance campaigns and found that those using nonviolence were approximately twice as likely to achieve their objectives as those that employed violence. Figure 2 summarizes the outcomes of the campaigns included in the dataset. Chenoweth and Stephan's analysis focused on large campaigns with ambitious political goals, such as regime change, secession or expulsion of a foreign occupier. Success was coded qualitatively, based on whether a campaign achieved its stated goals and had a discernible effect on the outcome. Additional analyses showed that the principle mechanisms driving the success of nonviolent campaigns were their ability to mobilize a large number of participants and provoke military defections.

Up until now, the existing literature has not explictly considered whether the effectiveness of nonviolent versus violent strategies is related to the duration of each type of campaign. Yet in Chenoweth and Stephan's data, there is a large degree of variation in the length of time that violent and nonviolent movements must struggle to achieve their goals. As Figure 3 shows, violent movements generally appear to

succeed at a slow but constant rate, while successful nonviolent campaigns are most often short-lived.

As I argue in the following sections, this pattern is explained by a time-dependent relationship between campaign strategy and the likelihood of success. Nonviolent campaigns, while overall more likely to achieve their objectives, become increasingly less likely to succeed as time goes on. It is only in the first several years of a campaign that nonviolent strategies possess a strategic advantage over armed insurrection. I offer a theoretical explanation for these claims in the following section.

Campaign strategy and duration

Nonviolent and violent resistance campaigns follow distinct pathways to success that differ to a large degree in how they relate to campaign duration. In this section, I describe the typical trajectories of violent and nonviolent campaigns, with a focus on how the effectiveness of their respective strategies depends on the length of their struggle.

Among violent campaigns, quick victories are rare. For an armed rebellion, defeating the government typically requires a protracted conflict because (a) establishing an organization capable of fighting the state requires a substantial investment of time and (b) the success of an insurgency depends on wearing down the opponent over time. The nature of these two strategic necessities means that campaign duration does not exert a predictable effect on the likely outcome across the full range of violent insurgencies.

First, building a successful rebel army is a long and involved process (Weinstein 2007). Unlike with popular protests, insurgents are selective about who may join the movement. In particular, rebel leaders try to seek out high-commitment individuals,

6

which requires an extensive process of gathering information and vetting potential recruits. Then, once soldiers are inducted, they must be trained in the tactics of rebel warfare before being deployed. All of this comes in addition to the important but time-consuming work of building relationships with noncombatant populations, which rebels often depend upon for support (Kalyvas 2006). For these reasons, few rebellions are prepared to defeat the government when they first emerge. Rather, they must build their organizational strength over time before presenting a credible challenge.

Second, in most cases rebel groups are engaged in asymmetric conflicts, or wars where they are materially weaker than the state. Lacking the outright strength to militarily overpower the state, insurgents must instead avoid direct confrontation while trying to destroy the government's will to fight (Arreguin-Toft 2001; Mack 1975). Rebels seek to eventually frustrate their stronger opponent into conceding by imposing sustained costs on the government, often through years of targeted assasinations, hit-and-run attacks and clandestine sabotage. Since the costs of guerilla warfare are cumulative over time, the likelihood of rebel victory generally hinges on the insurgents' ability to sustain the conflict over a long period (Buhaug, Gates, and Lujala 2009; Cunningham, Gleditsch, and Salehyan 2009).

Overall, a slow startup period, combined with a strategic imperative to engage in a drawn out conflict, means that most armed insurgencies involve lengthy campaigns. Over the duration of these campaigns, the baseline probability of victory fluctates very little because the exact point a which the government's will to continue fighting is broken will vary from conflict to conflict based on military and political contingencies. For example, while the theoretical likelihood of success remains fairly constant over time for all rebellions, the actual probability of victory for a given rebel group can change idiosyncratically with the intervention of third-parties or access to new

resources (Balch-Lindsay and Enterline 2000; Regan 2002; Ross 2004). Since these kinds of battlefield contingencies do not follow any discernible temporal pattern across cases, the expected probability of violent campaign success will remain unaffected by time.

By contrast, nonviolent resistance relies on an altogether different strategy for success. For nonviolent campaigns, the key to government defeat lies in removing the target regime's political, economic and military pillars of support (Sharp 1973). Unlike in violent insurgencies, this does not necessarily require a protracted struggle. Swift, creative strategies can be highly effective in creating divisions between the regime and its supporters within the military, business elite and political class. To create these divisions and force the regime to concede, Chenoweth and Stephan argue that nonviolent campaigns generally realize two intermediary mechanisms: a large campaign size and security force defections.

First, mass participation is of utmost importance. While armed groups can generally make up for a smaller number of participants because of the disruptive power of violence, the logic of nonviolent resistance depends on a large membership base to disrupt the government's economic and political sources of power (Chenoweth and Stephan 2011; DeNardo 1985; Sharp 1973). Without a critical mass of participants, strikes cannot stall the economy and street protests cannot draw the attention of potential allies in the international community. By attracting large numbers of people from diverse sectors of the population, nonviolent campaigns also have a greater ability to innovate in the tactics they use, further bolstering their ability to target the regime's weak points (Chenoweth and Stephan 2011, 114).

Second, defections from the state security apparatus are crucial to nonviolent campaign success. By imposing costs on protestors and disincentivizing participation, repression is one of the most effective strategies the state can use to neutralize a

resistance movement (Moore 2000; Tilly 1978). However, applying repression requires the regime to maintain the loyalty of the security apparatus, because a divided military is less capable of implementing orders from leadership (Hendrix and Salehyan 2016; Lee 2009). If even a small subset of the security forces refuse to obey orders, the regime becomes vulnerable to coups and rioting that can drive it from power. Overall, since nonviolent movements are materially defenseless against the state, their success rests to a large extent on their ability to remove the regime's capacity for violence by provoking security force defections (Binnendijk and Marovic 2006; Nepstad 2011b, 2013).

Nonviolent campaigns generally possess characteristics that allow them to realize these two intermediate objectives. On the one hand, the barriers to participation in nonviolent demonstrations are typically very low, which naturally reduces the burden of recruiting new members (Chenoweth and Stephan 2011; Schock 2015 164-5). On the other hand, military defections are often possible because nonviolent campaigns are by definition not physically threatening to the safety of would-be deserters. The large size of civil resistance campaigns also means that there are often informal ties linking members of the security forces with friends and family in the movement, which can reduce the security forces' willingness to apply repression (Binnendijk and Marovic 2006; Thurber 2015).

However, as I will argue in the following sections, the ability for nonviolent campaigns to realize these mechanisms is closely tied to their duration. The longer a campaign survives, the more difficult it becomes to attract large participation numbers and to induce loyalty shifts in the security forces. The logical corollary to this point is that if these two mechanisms of success gradually become less likely over time, the likelihood of succeeding will similarly decline over time.

Campaign size

If campaign size is a meaningful predictor of success, then demobilization, or the process by which individuals stop participating in a campaign, represents a major threat to long-term campaign prospects (Davenport 2014; Schock 2005). However, nonviolent campaigns generally lack effective means to counter demobilization. In contrast to armed groups, nonviolent movements almost never provide material incentives to encourage continued participation and they rarely use violence to punish and discourage defection (Schock 2015, 28). Since nonviolent campaigns are nearly powerless to stop their members from abandoning the cause at any time, their membership numbers − and by extension their likelihood of success − are highly dependent on the motivations of individual participants.

Among other factors, instrumentality has long been identified as a key motivation for participation in collective action like nonviolent protest (Van Stekelenburg and Klandermans 2013, 3). This perspective views grievances alone as an insufficient precondition for mobilization. Rather, aggrieved individuals will only engage in collective action if they believe that their involvement will be a viable means to effect the change they desire. The benefits to protest participation therefore depend to a large degree on expectations about the likelihood that the collective goal will be realized (Klandermans 1984, 1997; Klosko, Muller, and Opp 1987; Kurzman 1996). When individuals perceive that success is unlikely, they are substantially less willing to endure the costs of continued participation, including violent repression, imprisonment, and foregone income.

Of course, an individual's assessment of a campaign's prospects is highly subjective. Participants tend to calculate the likelihood of success based more so on their perceptions of the strength of the opposition than on the perceived vulnerability of

the state (Kurzman 1996). Often this means that participants estimate the number of other protestors that they expect to turn out as a proxy for the likelihood of success (Klandermans 1984; Oberschall 1980). Most theoretical models therefore suggest that individuals will choose to participate when they expect protest actions to reach a sufficiently large threshold (e.g. Granovetter 1978; Kuran 1989).

Yet individual dissidents also look to the length of a campaign as an indicator of its strength. Longer campaigns are, by definition, characterized by a litany of past failures. The longer a nonviolent movement continues its struggle, the more likely that individuals will begin to question whether the regime is defeatable or whether nonviolence is the appropriate strategy for achieving the campaign's objectives. Over time, participants gradually become disillusioned and increasingly likely to end their involvement in the resistance. Even if it is only the lowest commitment individuals that initially choose to stay home, their demobilization can have knock-on effects for others in the movement who are looking to membership size as a barometer for the likelihood of campaign success.

Consider, for example, the most recent wave of unrest in Venezuela. After months of massive demonstrations against the Maduro government, protests began to die down in late 2017 without any indication of impending regime change. At the time, one onlooker spoke to the BBC and echoed a common sentiment among the population about the efficacy of nonviolent action, which had been taking place on and off for over three years: 'What they did ... no it was not the way. With these protests, they did not get anywhere. Now there is calm. But the problems are still there' (quoted in Hernández 2017). Thus while the grievances that initially incited the protests remained, it was the gradual loss in perceived efficacy that ultimately led to widespread discontent with the resistance efforts. A student protestor further described the failure of the campaign's leadership in maintaining participation:

'I just think that it was a big task and they did not know how to encourage the protests. They did not manage to keep motivating people and that is why the thing declined' (quoted in Hernández 2017). While Venezuela's resistance campaign has been relatively short-lived, it exemplifies a pattern that is common in longer nonviolent campaigns. Over time, individual participants gradually become disillusioned with the likelihood that collective action will produce the changes they desire, while at the same time campaign leadership is powerless to prevent those participants from leaving the movement.

This explanation is not incongruent with previous arguments that hold state repression to be the main cause of individual disengagement (e.g. Boykoff 2007; Davenport 2014; Tilly 1978; Tilly and Tarrow 2007). Indeed, just as the expected benefits to collective action decrease over time, the tangible costs of participating increase. Harassment, imprisonment, foregone income, and fear of repression impose cumulative costs on campaign participants that can eventually become intolerable. Moreover, the trauma of repression can have long-lasting effects on how individuals frame their decisions about future participation. As Davenport (2014, 303) found with one U.S. social movement, 'dissidents talked or thought about ... previous repressive behavior for months, and occasionally for years, after relevant events occurred.' Ultimately, I expect that participants disengage from a nonviolent campaign over time due to a combination of rising costs, declining expected returns and an inability on the part of leadership to discourage individuals from abandoning the movement.

Security force defections

Campaign duration also has a negative effect on the likelihood of realizing security force defections. To understand why, it is necessary to outline what motivates defection in general terms. First and foremost, members of the security forces are

self-interested actors. Confronted by an unarmed insurrection, military and police personnel will decide whether to defect based on the course of action that minimizes harm to themselves. In particular, defection is likely to result in personal losses if the movement fails, because a defector cannot return to his job and may become a target of regime violence. However, defection will be the safer option if the resistance movement succeeds and subsequently attempts to either hold the security forces criminally responsible for repression or purge the military of sympathizers from the old regime (Dragu and Lupu 2017; e.g. Binnendijk and Marovic 2006, 425; Hale 2005, 156). For each individual, the costs of defection are therefore highly contigent on the eventual success or failure of the nonviolent uprising.

To avoid these risks, an individual member of the security forces must therefore attempt to align her decision to defect with the anticipated outcome of the resistance campaign. If the security forces believe the regime will survive, they will support it, but they will defect if the regime's collapse seems imminent (Binnendijk and Marovic 2006; Hale 2005; Kou 2000; McLauchlin 2010; Nepstad 2013).

Soldiers tend to estimate a regime's prospects for survival based on expectations about the behaviour of their fellow service members (Dragu and Lupu 2017; McLauchlin 2010). Since individual soldiers know that defeating the resistance depends on the security forces remaining unified in obeying orders, they will drastically lower their estimates of the probability of regime survival when they believe that enough other troops are planning to defect. Because of these interdependent expectations, even a small number of initial deserters can produce a cascade of additional defections (Fearon 2004, 289). In this way, soldiers that expect others to defect have a rational incentive to defect themselves.

However, expectations about whether others will defect are in turn influenced by campaign duration. As the resistance campaign progresses, members of the security

forces will tend to downgrade the likelihood that other troops will defect because they gradually gain information on the strength of the resistance movement and the past behaviour of fellow troops.

First, soldiers are likely to predict their peers' behaviour at least in part based on the strength of the resistance movement. Chenoweth and Stephan (2011) showed that the largest nonviolent campaigns have more than a 50% higher probability of inducing defections than the smallest campaigns. As discussed in the previous section, there are good reasons to expect that membership − and therefore soldiers' perceptions of campaign strength − will decline over time. In addition to raw membership numbers, soldiers are just as likely as members of the resistance campaign to view the history of a lengthy campaign as a sustained record of failures to outmaneuver the government. Given such evidence, the security forces will have significantly less confidence in regime collapse and it will become less probable that any cadre of first-movers will trigger a wave of defections.

Second, over longer campaigns, soldiers iteratively gather information about the behaviour of their fellow troops. In particular, members of the security forces look at the history of others' actions throughout the resistance campaign to determine their propensity to defect (McLauchlin 2010, 345-6). A failure to defect in the past is often taken as strong evidence that defection is unlikely to occur in the future. However, in the early stages of an uprising, the security forces lack this type of concrete evidence on how others will react to popular demonstrations. During longer campaigns, soldiers have repeated interactions with demonstrators. Each time that members of the security forces remain loyal during these interactions with demonstrators, their fellow soldiers gain additional assurances about their trustworthiness.

Therefore, if nonviolent campaigns are to provoke defections, they must do so early on, when members of the security forces are most uncertain about which course

of action is likely to protect their own interests. According to the data I use in my analysis, 47% of all nonviolent campaigns are able to cause security force defections, but among the longer nonviolent campaigns that fail to induce defections in their first year, only 22% are able to provoke those loyalty shifts at a later point. Weathering an initial challenge increases the security forces' expectations that others will remain unified, which in turn bolsters their confidence in regime survival and reduces their incentive to defect.

Of course, there are myriad other explanations for military defection. The scholarly literature has highlighted the importance of the ethnic composition of the forces and the resistance movement (Enloe 1980; McLauchlin 2010; Nepstad 2011a, 2011b), the nature of the regime's political institutions (Lee 2014), the level of economic and political benefits that the military receives from the regime (Nepstad 2011a, 2013; Pion-Berlin, Esparza, and Grisham 2014) and pre-existing factionalism within the military (Lee 2005, 2009). However, these variables are less instructive about the effects of campaign duration because they are typically slow-moving and often causally prior to the onset of a resistance campaign. Nor do these explanations contradict the core logic of how the security forces progressively gaining information influences their likelihood of defecting. Rather, each of these variables simply adjusts the underlying propensity for defection from one context to another.

Defection is a complex process shaped by diverse and often countervailing causes. To a large extent though, it is shaped by individuals seeking to minimize harm to themselves. Security forces members are most likely to defect when they believe it will shield them from reprisals by a victorious resistance movement. They will quietly acquiesce when they believe the current regime will survive. Their beliefs about the probability of a nonviolent victory are determined by the available information, which increases the longer a campaign survives. In this way, campaign duration

should negatively impact how the security forces perceive a nonviolent movement's likelihood of victory, which in turn will make them less likely to defect as time goes on.

In summary, I expect to verify several hypotheses in my empirical analyses. My main contention is that nonviolent campaigns will have a discernible advantage over violent campaigns only in the first several years of their existence. As time goes on, the likelihood of a nonviolent victory will gradually decline and eventually become statistically indistinguishable from the success rate of violent movements. Violent campaigns will be much less affected by duration and will instead show a more constant probability of success over their lifecycle. In terms of the mechanisms that lead nonviolent campaigns to achieve their objectives, large campaign sizes and security force defections should be strong, positive predictors of success. Moreover, the likelihood of realizing these two preconditions for success will be negatively affected by campaign duration.

Data

To test my hypotheses, I use data from the Nonviolent and Violent Campaigns and Outcomes (NAVCO) data project, which records information on nonviolent and violent insurrections with maximalist political objectives, such as regime change, territorial secession, or the exit of a foreign occupier (Chenoweth and Lewis 2013b). This maximalist objectives criteria, along with certain standards for the minimum level of participation, means that the findings in my study are concerned with a particular subset of contentious politics involving "major" resistance campaigns.

While the original *Why Civil Resistance Works* analysis was based on a sample of 323 campaigns from 1900 to 2006, I use an updated version of the dataset, NAVCO 2.0,

which focuses on 250 campaigns from 1945 onwards and includes additional variables measured at the campaign-year. I also replicate my core findings in the appendix using Chenoweth and Stephan's original data (see Section A.1).

Using the designations made in the Chenoweth and Stephan study, each campaign is coded as either primarily violent or primarily nonviolent and remains so throughout its entire duration.[1] The coding in the NAVCO data categorizes cases based on the tactical approach that is most prevalent. While a binary categorization is imperfect, this approach is a reasonable compromise that permits a first step towards understanding the trajectory of campaigns that rely on broadly distinct tactics.

For the dependent variable, campaign success, I rely on a dichotomous measure of whether a campaign achieved its stated objectives or obtained significant concessions within a year of the peak of campaign activities.[2] This judgement is made by the dataset coders based on a qualitative case assessment. A campaign can only achieve success once and success always occurs in the final year of the campaign.

Defining the duration of a campaign is also essential to my analysis. In the NAVCO data, a campaign is determined to have begun when a series of observable, coordinated, contentious collective actions with 1,000 or more participants take place within one year of each other (Chenoweth and Lewis 2013b, 417). A campaign ends when its objectives are realized or participation during peak events no longer reaches 1,000. Campaigns that were ongoing in the last year of the dataset (2006) and campaigns that end without achieving their objectives are treated as right-censored.

I also rely on a number of control variables derived from the existing literature,

---

1. In practice, some campaigns switch tactics throughout their lifecycles, but the number that do so is very small. In the NAVCO data, 6% of campaign-years see tactical switching. I investigate the impact of these observations on my findings in the appendix (Section A.5).

2. As the dataset's creators note, coders had difficulty distinguishing between campaigns that achieved *all* objectives versus only attaining significant concessions (Chenoweth and Lewis 2013a, 18). I therefore collapse both outcomes into an indicator of 'strategic' success to account for this ambiguity.

which are both static and time-varying. Variables that are constant for each campaign include the target regime's level of democracy, measured on the Polity scale (Marshall, Jaggers, and Gurr 2014), the target country's military capacity, based on the Correlates of War project's composite index (Sarkees and Schafer 2000), and the logged population and GDP per capita in the country in which the campaign takes place, using data from Haber and Menaldo (2011) included in the Varieties of Democracy dataset (Coppedge et al. 2017). Each of these structural variables are measured in the year prior to campaign onset and are held constant over campaign duration to avoid issues of simultaneity. Other covariates that vary at the campaign-year come from the NAVCO 2.0 dataset, including the type of campaign goal, an ordinal measure of campaign size, the level of government repression, and binary variables indicating foreign state support to the campaign and security force defections. A dummy for the Cold War period is also included to account for conditions in the international environment.

In my analysis of the mechanisms of nonviolent campaign success, I set the dependent variable as a binary indicator for each of the two mechanisms that are thought to produce success. For campaign size, this outcome variables scores '1' if a campaign had a peak membership greater than 100,000 in a given year, and '0' otherwise.[3] The security force defection variable similarly scores '1' to indicate major loyalty shifts in the military or police. Both variables come from the NAVCO 2.0 dataset.

Finally, I use an instrumental variable, a country's fertility rate, to identify the causal effect of nonviolent strategy. The logic of this variable is described below. The source is Schaternaar's (2017) dataset based on World Bank indicators (World Bank 2013).

---

3. A complete explanation for the choice of this threshold is provided in the appendix (Section C.1).

Empirical strategy

My empirical strategy relies on event history analysis techniques that allow for an explicit modelling of campaign duration and time dependence. Event history analysis has played an important role in the study of contentious politics, although its application has mostly been focused on the determinants of civil war duration (e.g. Balch-Lindsay and Enterline 2000; Cunningham, Gleditsch, and Salehyan 2009; Fearon 2004; Regan 2002). My methodological approach closely parallels these earlier studies, except I extend the substantive scope to include nonviolent as well as violent uprisings.

In these studies, and in the social sciences more generally, Cox models have been a popular method for studying the duration of political events. In my analysis, it is technically possible to use Cox models to model the hazard of campaign success, but there is little substantive difference between the Cox model and more widely understood generalized linear models when the data are structured into uniform discrete time intervals, like campaign-years (Beck, Katz, and Tucker 1998). I therefore model the hazard of campaign success using logistic regression where the dependent variable is a dichotomous indicator of success. I unpack the time-dependent effects of a nonviolent strategy by adding interactions between campaign type and time, conditional on controls related to the target regime and country context. Of course, specifying duration using cubic polynomials is often recommended when modelling binary dependent variables (Carter and Signorino 2010). For ease of interpretation, I instead opt to use an untransformed time variable, but I repeat my analyses in the appendix using a cubic polynomial specification and find no meaningful difference from the results presented here (see Section A.2).

After having shown the hypothesized relationship using a simple logistic model, I

address the problem of endogeneity using an instrumental variables design. I discuss my identification strategy in more detail in the next section.

In the final part of my analysis, I investigate the mechanisms − campaign size and security force defections − that explain nonviolent success, again using logistic regression models. To do so, I first demonstrate that these two mechanisms are strong predictors of success in a sample only including nonviolent campaigns. Then I define the dependent variable as each of the two intervening variables and assess whether these core mechanisms weaken over time by including a campaign duration term as a predictor, which I expect to produce a negative coefficient estimate.

Instrumental variable

There is an underlying problem of selection bias that has been left unaddressed in my methodology thus far. In particular, a campaign's choice to use nonviolent versus violent tactics may be driven by some unobservable factors that are correlated with its likelihood of success. My inference becomes doubtful if nonviolent campaigns typically emerge where victory is already imminent − akin to what Levitsky and Way (2010) call 'rotten-door transitions' − and violent campaigns only arise when a longer struggle is necessary from the outset.

Chenoweth and Stephan (2011) addressed this problem of selection bias through an instrumental variables approach. The authors created an instrument based on several known predictors of violent conflict that have been identified in the civil war literature. I also employ an instrumental variables design to accurately identify the causal effect of nonviolence, but use a measure of gender equality as a new instrument for campaign type.[4] Building on a previous literature linking lower gender equality

---

4. In the appendix, I test my results against the original *Why Civil Resistance Works* instruments as a robustness check on my own design (see Section B.2).

with a higher risk of armed conflict (Bjarnegård et al. 2015; Caprioli 2000, 2005; Melander 2005), Schaftenaar (2017) showed that a country's fertility rate is a strong predictor of nonviolent campaign onset. I will use this same measure as an instrument for nonviolent campaign occurrence.

For fertility rates to serve as a good instrument, this variable must meet two criteria. First, it must be a strong predictor of the endogenous regressor. Theoretically, Schaftenaar identifies two key reasons why the fertility rate should have a strong effect on the probability of nonviolent conflict. First, gender equality, proxied by the fertility rate, shapes the cultural framework within which a population selects its resistance strategy. Specifically, there are 'cultural norms that dictate whether conflicts should be resolved with violence or more peaceful means' (Schaftenaar 2017, 765). These conflict norms are often gendered and when they become culturally dominant, they can strongly inform the strategies that resistance movements choose. Second, gender norms can determine the pool of potential participants in a resistance movement. In gender-equal societies, men and women feel equally empowered to engage in activism and those within resistance movements are welcoming of participants from either sex. By contrast, in societies where women are discouraged from participation in political activism due to gender discrimination, women may be less willing to engage politically and movements may be less accepting of female participants. Since nonviolent campaigns require large numbers of participants more than violent groups, campaign leaders debating whether to pursue a violent or nonviolent insurrection must carefully estimate the potential mobilization pool available to them. Where the number of potential recruits is large because gender norms do not restrict movement participation, the choice to use nonviolent resistance should become more likely.

None of this is to say that nonviolent strategies are more common in gender-equal societies simply because women are inherently more peaceful than men (Schaftenaar

21

2017, 766). Rather, gender norms are closely tied to (a) conflict norms and (b) the available mobilization pool, which suggests a theoretically justifiable rationale for why higher fertility rates, as a proxy for weaker gender equality, should be a strong and negative predictor of nonviolent campaign type. The actual strength of this instrument is tested statistically in my results section.

The second criteria of a good instrument is that fertility rates must only affect the likelihood of campaign success through the campaign type variable. Of course, fertility rates are not randomly assigned to countries and therefore are likely correlated with a number of structural factors that have an indirect effect on the likelihood of a campaign suceeding. This represents a potentially serious violation of the second criteria, also known as the exclusion restriction.

Importantly, however, the exclusion restriction is conditional on the controls that are included in the first and second stages of the regression. Including covariates that represent alternative causal pathways between the instrument and outcome can address what is essentially an omitted variable problem in the first stage of two-stage least squares estimation (e.g. Acemoglu, Johnson, and Robinson 2001; Kern and Hainmueller 2009). The idea behind this approach is to obtain a more accurate estimate of the instrument's true effect on the outcome via the endogenous variable.

To implement this approach in my identification strategy, I control for several alternative causal pathways. First, since I have argued that gender norms influence a campaign's capacity to mobilize participants, there is a risk that nonviolent movements are more successful only because they occur in contexts where they are *a priori* more likely to attract the large number of participants they need to succeed. To disentangle the normative contribution of gender equality from its effects on mobilization, all models include a measure of campaign membership size. Second, according to the classic modernization argument, gender equality may simply be a reflection of the

level of socioeconomic development a country has attained, which is also a possible predictor of campaign success. To address this possibility, I include a control for the country's log GDP per capita in the year before the campaign began.[5] Third, gender equality could be linked to the level of democracy, which may influence campaign outcomes. I therefore control for a country's Polity score in the year before the campaign began. Finally, fertility rates are correlated with demographic factors that may shape the outcome of the campaign. A country's log population and its population density in the year before a campaign began are both included in the models.

Of course, whether my modelling choices fully satisfy the exclusion restriction is difficult to test when only using one instrument. However, there are some diagnostics that allow the exclusion restriction to be evaluated when additional instruments are available. In the appendix, I introduce two further instruments, the female-to-male education enrollment ratio and the presence of nonviolent campaigns in neighbouring countries, as a way to implement tests of overidentifying restrictions. When I include these additional instruments, the models are consistent with the results presented in the following section and a test based on the Hansen J statistic indicates that fertility rates do not violate the exclusion restriction (see Section B.1).

Results

In this first section of my results, I demonstrate that nonviolent campaigns, while overall more successful than their violent counterparts, become less likely to succeed over time. Table 1 presents the results from a series of logistic regressions with

5. Fertility rates may also be capturing the state of a country's public health infrastructure, which GDP per capita only measures indirectly. In supplementary analyses (not shown here), I also control for a country's log infant mortality rate, which others have shown to be a strong predictor of large-scale political instability (e.g. Goldstone et al. 2010). The results are nearly identical to the estimates presented below.

campaign success as the dependent variable. Model 1 confirms that Chenoweth and Stephan's finding that nonviolent campaigns are more successful translates to the NAVCO 2.0 sample of campaigns. Here, the effect of campaign duration is assumed to be the same for both violent and nonviolent campaigns. After calculating the average marginal effects (using software from Leeper 2017), this model indicates that nonviolent campaigns have over a 10% greater chance of succeeding than violent ones and that both types of campaigns become 0.3% less likely to succeed with each passing year.

The remainder of the models in Table 1 interact nonviolence with campaign duration in order to evaluate whether the effect of duration varies depending on the strategy of the campaign. Model 2 is the simplest test of the theory and includes no additional covariates, whereas Models 3 to 5 attempt to determine whether the time-dependent relationship between nonviolence and success is being driven by other covariates specific to the target regime, the country context or the campaign itself. The main takeaway from each of these models is that the progressive introduction of control variables does not substantially alter the statistical significance of the co-efficient estimates on the nonviolent, nonviolent × duration, and duration variables. Only in Model 5 does the statistical significance of the main interaction term fail to reach the standard 95% confidence level; here the estimate is of a similar magnitude to the other models, but is only statistically distinguishable from zero at $\alpha = 0.08$.

However, the coefficients and statistical significance of the variables involved in the interaction do not explain how the uncertainty around each estimate varies across all values of the two constitutive variables (Berry, Golder, and Milton 2012). It is therefore impossible to tell from the results table whether the positive effect of a nonviolent strategy is statistically significant across all values of campaign duration. To get a more comprehensive picture of the likelihood of success for each campaign

24

Table 1: Results from logistic models of campaign success

	Campaign success				
	(1)	(2)	(3)	(4)	(5)
Nonviolent	1.443***	1.874***	1.352***	2.416***	2.366***
	(0.180)	(0.246)	(0.327)	(0.468)	(0.464)
Nonviolent × Duration		−0.093**	−0.104**	−0.131***	−0.099*
		(0.036)	(0.048)	(0.051)	(0.056)
Duration	−0.045***	−0.025	−0.017	−0.001	0.006
	(0.013)	(0.014)	(0.015)	(0.022)	(0.022)
Cold War			−0.630**	−0.896***	−0.804***
			(0.246)	(0.293)	(0.300)
Repression			−0.272**	−0.382***	−0.382**
			(0.118)	(0.145)	(0.148)
Foreign state support			0.454**	0.344	0.346
			(0.227)	(0.305)	(0.297)
Campaign size			0.485***	0.433**	0.381**
			(0.127)	(0.168)	(0.165)
Goal: Regime change			0.687**		0.659*
			(0.311)		(0.373)
Goal: Secession			0.015		0.520
			(0.383)		(0.602)
Goal: Anti-occupation			0.207		−0.615
			(0.441)		(0.663)
Population (log)				−0.410***	−0.410***
				(0.113)	(0.119)
GDP per capita (log)				−0.433***	−0.360**
				(0.148)	(0.157)
Target Polity				0.003	−0.003
				(0.022)	(0.023)
Target military capacity				−0.140	4.128
				(4.238)	(4.731)
Observations	1726	1726	1501	1097	1097
Log likelihood	-446.21	-442.89	-354.33	-232.45	-228.09

*p<0.1; **p<0.05; ***p<0.01. Estimates presented as log odds with standard errors clustered by country-year in parentheses.

type over time, Figure 4 plots the predicted probabilities for nonviolent and violent campaigns using the estimates from Model 3.[6] For comparison, the panel on the left shows the predicted values from a model from a model that is analogous to Chenoweth and Stephan's, where the likelihood of success is assumed to be constant over the full duration of a campaign. Under this specification, nonviolence is always expected to have a higher chance of success than armed conflict.

However, the right-hand panel demonstrates that nonviolent campaigns' advantage over violent campaigns is in fact time-dependent. While a quarter of nonviolent campaigns are predicted to succeed in their first year, the likelihood that nonviolence leads to success declines to around 10% by the tenth campaign-year. Meanwhile, the likelihood of violent campaign success is relatively constant over time, declining by less than a single percentage point over the first ten years.[7] Most importantly, the confidence intervals around these estimates reveal that the advantage of nonviolence is only statistically significant until the seventh or eight campaign-year.[8]

Having found evidence that nonviolent campaigns become less successful over time, I will now attempt to show that this relationship holds even when instrumenting for the type of campaign strategy. In the next set of models, the outcome variable is still a binary indicator of success, but I proceed by using a two-stage least squares

6.   While the longest observed campaign duration is 59 years, the graphs focus on the first 20 campaign years since the two sets of predicted probabilities remain statistically indistinguishable in later years. After year 20, there is a lack of common support across the two campaign types and interpretation beyond this point involves a degree of interpolation (Hainmueller, Mummolo, and Xu 2018). In the appendix, I test whether my results are biased by observations beyond 20 years by dropping two outlier campaigns and arbitrarily right-censoring campaigns at 20 years; the results are consistent with the models presented here (see Section A.4).

7. Of course, these probabilities should not be taken as universally applicable because they are calculated based on average marginal effects and the effects of coefficients in logistic models are dependent on the values of the model's other covariates.

8. The exact point at which the confidence intervals for each level of campaign type overlap is sensitive to model specification. Plots in the appendix show expected values for each level of campaign type using a simulation-based approach (see King, Tomz, and Wittenberg 2000). Overall, the results point to a similar conclusion: the relative effectiveness of nonviolent campaign type is not constant over time and atrophies faster for nonviolent than violent campaigns (see Section A.3).
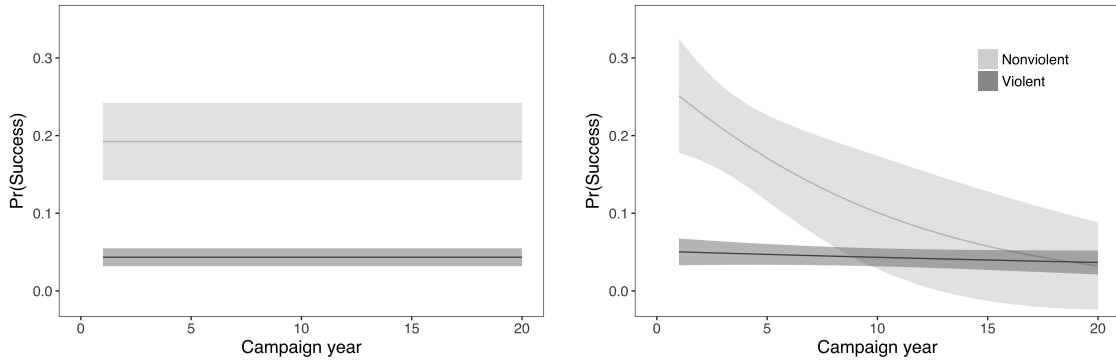
Figure 4: Predicted probability of success by campaign type when not accounting for duration (left) and allowing for time-dependent effects (right).

Predictive margins and 95% confidence intervals are calculated from a model including the following covariates: Cold War, repression, foreign state support, campaign size and campaign goals.

(2SLS) model (Wooldridge 2002, 472).[9] Under this specification, I treat success as if it were continuous, with the outcome variable now approximating a latent propensity to succeed (Bollen 2012, 47-8).

In estimating the 2SLS model, there is one minor technical challenge that must be addressed. Because nonviolence is endogenous to success, the interaction term between nonviolence and campaign duration will also be endogenous to success (Achen 1986). To purge the endogeneity from both terms, I need to instrument for both the nonviolent variable on its own as well as the nonviolent $\times$ duration term in the first stage. Similarly, when predicting these two endogenous variables, fertility rates must be interacted with the campaign duration variable. The non-interacted duration term is not instrumented and instead acts as a regular covariate in both the first and second stage.

Table 2 presents the results from a 2SLS model using fertility rates to predict campaign type. Beginning with the bottom panel, Models 8 and 9 use the fertility rate and its interaction with campaign duration to predict nonviolent campaign type

---

9. When using a probit model in the second stage, the algorithm could not converge. See Angrist and Pischke (2008, 197-8) for a discussion on why OLS is a satisfactory, and in some cases preferable, specification for the second stage.

and nonviolent campaign type $\times$ duration. Note that in these models, the relevant coefficients depend on the dependent variable. In Model 8, the non-interacted fertility rate is the key determinant of instrument strength. The coefficient on this variable is statistically significant and in the expected direction: higher fertility rates, as a proxy for weaker gender equality, are negatively correlated with the likelihood of a nonviolent campaign strategy. In Model 9, where the dependent variable is an interaction between nonviolence and duration, the relevant parameter is the coefficient on the fertility rate $\times$ duration interaction. Again, the estimate here is negative and statistically significant.

Beyond the statistical significance in the first stage, fertility rates meet the standard thresholds for a strong instrument. In both Models 8 and 9, the F statistic exceeds the rule-of-thumb value of 10 (Staiger and Stock 1997). A second diagnostic, presented in the upper panel of Table 2, shows that for Model 7 as a whole, the Cragg-Donald F exceeds the Stock-Yogo critical value at 10%, which provides good evidence that the instrument is not overly weak (Stock and Yogo 2005). Finally, an underidentification test (Anderson cannon. corr. LM statistic) indicates that the instrument is indeed relevant.

These results suggest that it is safe to interpret the second stage of the 2SLS model under the assumption that fertility rates are a strong instrument. Here, the fitted values for nonviolent campaign type and its interaction with duration from the lower panel of Table 2 are substituted for the endogenous variables in the biased model.

For comparison, Model 6 presents the biased OLS. The coefficients on the nonviolent campaign and interaction term indicate similar conclusions as the earlier logistic models: nonviolence has a large advantage over violence initially, but nonviolent campaigns become around 1% less likely to succeed with each passing year. The 2SLS model should approximate these estimates if the earlier findings were based on a truly

Table 2: Results from 2SLS models of campaign success

	Second stage: Dependent variable is campaign success	
	OLS	2SLS
	(6)	(7)
Nonviolent	0.227***	0.330***
	(0.040)	(0.106)
Nonviolent × Duration	−0.009***	−0.026
	(0.002)	(0.016)
Duration	−0.0001	0.004
	(0.001)	(0.004)
Observations	1,015	1,015
Unreported covariates	Yes	Yes
Cragg-Donald F Statistic	−	9.078
Stock-Yogo 10% critical value	−	7.03
Anderson cannon. corr. LR	−	18.227***

	First stage: Dependent variable is endogenous regressor	
	Nonviolent	Nonviolent × Duration
	(8)	(9)
Fertility	−0.097***	−0.155
	(0.010)	(0.118)
Fertility × Duration	0.002***	−0.032**
	(0.001)	(0.013)
Duration	−0.017***	0.289***
	(0.003)	(0.072)
Observations	1,015	1,015
Unreported covariates	Yes	Yes
Adjusted R^2	0.403	0.230
F Statistic on instruments	48.54***	13.90***

*p<0.1; **p<0.05; ***p<0.01. Standard errors clustered by country-year in parentheses. Unreported covariates include: Cold War, repression, foreign state support, campaign size, target Polity, population (log), GDP per capita (log), target military capacity and population density.

causal relationship.

Model 7 shows that the 2SLS estimates are broadly similar to the biased model. The most notable discrepancies are the larger coefficients on the nonviolent campaign type term and its interaction with duration, as well as the loss of statistical significance in the nonviolent × duration interaction. The 2SLS estimates suggest an even greater initial advantage to nonviolence, but a much more rapid decline in efficacy over time. While the loss of statistical significance is worrying, this is largely a product of the larger standard errors that are common in IV estimation.

Figure 5 presents predicted probabilities of success for each type of campaign based on the methods developed by Gold, Olin and Wang (2018). The plot indicates that nonviolent campaigns are expected to succeed around a third of the time in their first year, but their chances of victory decline by roughly 2.5% per year. The likelihood of violent success is again fairly constant over time, increasing only marginally over the course of the campaign. As in earlier models, the likelihood of a nonviolent campaign succeeding is only statistically distinguishable from that of a violent movement during the first several years.

Overall, the fact that comparable results were found using both the endogenous and instrumental variables models gives us confidence that the time-dependent relationship between nonviolent resistance and success is in fact driven by a causal process, despite the potential for selection bias in campaign strategy.

Mechanisms of nonviolent campaign success

In this final section of my results, I investigate the mechanisms that drive the negative relationship between campaign duration and nonviolent campaigns' likelihood of succeeding.
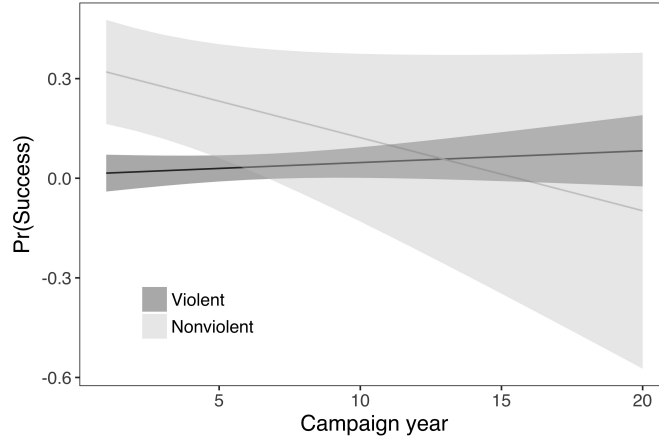
Figure 5: Predicted probability of success by campaign type, 2SLS model

Predictive margins and 95% confidence intervals are calculated from model including the following covariates: Cold War, repression, foreign state support, campaign size, target Polity, population (log), GDP per capita (log), target military capacity and population density.

First, I will attempt to confirm the specific mechanisms that explain success among nonviolent campaigns. In Figure 6, I present the average marginal effects (AME) derived from a series of logistic regression models where the dependent variable is nonviolent campaign success and each of the three model specifications progressively add covariates. I operationalize the two principle mechanisms as binary indicators for a campaign size greater than 100,000 and the presence of security force defections. A full rationale for the campaign size threshold is provided in the appendix, though I will note here that this is the point at which mass participation should have a discernible effect on the likelihood of success (see Section C.1).

In all models, a large campaign size and security force defections are positively correlated with success among nonviolent campaigns. Model 12 offers the most conservative estimates, which indicate that the likelihood of success is nearly 6% higher when a campaign's membership exceeds 100,000 and almost 14% greater when members of the security forces defect.[10] The magnitude of these effects is substantial,

---

10. Given that Figure 6 is based on logistic models, we should be cautious in interpreting the estimates across models with different samples and covariates (Mood 2010). It is sufficient here to note that all three models reveal similar patterns.
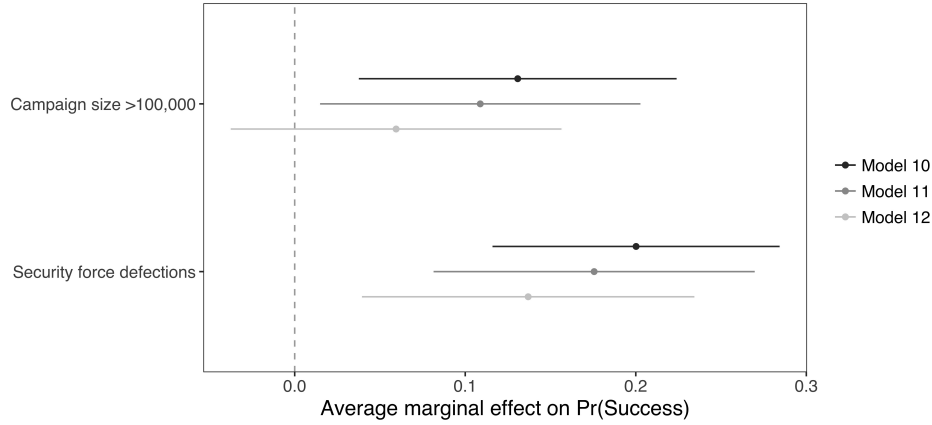
Figure 6: Determinants of nonviolent campaign success

Results are from three logistic regression models of campaign success among nonviolent campaigns ($n = 258$, 258, 192). Estimates presented as average marginal effects with 95% confidence intervals. Unreported control variables: campaign duration, international sanctions, foreign state support (Models 10, 11 and 12), Cold War, repression, campaign goal (Models 11 and 12), population (log) and GDP per capita (log) (Model 12).

considering the predicted probabilities presented earlier. Moreover, almost all of the AMEs in Figure 6 are statistically significant. Only in Model 12, with the most co-variates and smallest number of observations, does the effect of a large campaign size become indistinguishable from zero.

Having identified two of the main mechanisms of nonviolent campaign success, I now estimate the likelihood of each being realized as time goes on. In Table 3, I set the dependent variable as a binary indicator of each mechanism and include a measure of campaign duration as an explanatory variable to test the effect of time.[11]

Each of control variables included in Models 13 and 14 are motivated by a possible effect that they might exert on the likelihood that citizens will participate in the uprising and that security forces personnel will defect to it. Repression is expected to reduce participation by imposing costs on protestors; it is also likely to increase

---

11. Of course, initial defections from the security forces may influence the subsequent likelihood of further defection through a bandwagon effect. As Lutscher (2016, 358-9) notes, this creates a risk that by modelling all campaign-years, including those that come after initial defections, my results may be biased. I address this possibility in the appendix by dropping all campaign years that come after the first wave of security force defections. The results are similar, but slightly less robust compared with what is presented here in the main text (see Section C.3).

Table 3: Results from logistic models of mechanisms of success, nonviolent campaigns only

	Campaign size >100,000	Security force defections
	(13)	(14)
Duration	−0.163***	−0.224**
	(0.036)	(0.108)
Repression	−0.289*	0.268
	(0.163)	(0.197)
Foreign state support	1.580***	0.818*
	(0.350)	(0.418)
Population (log)	−0.010	−0.455***
	(0.131)	(0.163)
GDP per capita (log)	0.064	−0.629**
	(0.221)	(0.251)
Observations	227	220
Log likelihood	-115.13	-96.46

*p<0.1; **p<0.05; ***p<0.01. Estimates presented as log odds with standard errors clustered by country-year in parentheses.

defections by creating moral costs to security forces personnel tasked with carrying out violence against unarmed demonstrators. By providing a source of rents to the campaign, foreign state support should increase both the number of participants as well as the probability of defections. GDP per capita acts as a proxy for citizens' opportunity costs of protesting. Per capita wealth may also capture state capacity, in which case security defections should be less common due to presumably higher levels of professionalization and better material compensation in the military and police. Lastly, population size is included to control for the fact that larger countries are simply more capable of mobilizing movements of over 100,000 than smaller ones.[12]

After controlling for these alternative causes of campaign participation and security force defection, the coefficient estimate for the campaign duration variable is

---

12. The robustness of my findings are tested further in the appendix using additional control variables (see Section C.2).

negative and statistically significant in both models. The AMEs from these models indicate that each passing year reduces the likelihood of realizing participation of over 100,000 by 2.8%. Security force defections become even less likely over time, with each year reducing their probability by just over 3.2%.

Interpreted together, Models 13 and 14 provide indirect support for my main theoretical argument. If indeed nonviolent movements become increasingly less likely to succeed over their lifecycle, we should see concurrent declines in the probability of the mechanisms that predict success. In this section, I have confirmed the key mechanisms of nonviolent success and demonstrated that they too are likely to decay over time. The longer a nonviolent campaign continues its struggle against the government, the less likely it will be to maintain a large campaign size and induce defections from the state security forces.

Illustrative examples

Before concluding this study, I will present two brief case study campaigns to illustrate the causal mechanisms identified above. The purpose of these 'pathway' cases is to elucidate the finer points of my theory, rather than to confirm or disconfirm its central hypotheses (see Gerring 2007). As such, I select two nonviolent campaigns which conform to the outcomes that would be expected by my theory: one which succeeded quickly and another which struggled for several years before failing. The first case looks at the several months of organized protest which led to the resignation of Alberto Fujimori in Peru. The second case looks at South Africa's first anti-Apartheid campaign and contrasts its failure with the later success of the more well-known second campaign that began in the late 1980s.

The two cases, along with their outcomes and start and end dates, come from the

NAVCO 2.0 dataset. As noted earlier, campaign onset occurs 'when there is a series of coordinated, contentious collective actions with at least 1,000 observed participants' and a campaign ends when participation falls below this threshold (Chenoweth and Lewis 2013b, 417). Therefore, the anti-Fujimori campaign is recorded as only occurring in 2000, though Peru did experience some minor, uncoordinated pro-democracy protest before this point. To ensure consistency with my earlier analyses, I continue to rely on the NAVCO 2.0 criteria for campaign onset and success when discussing the trajectories of the two case studies below.

The selection of these particular cases is further motivated by a desire to demonstrate the applicability of my theory across diverse contexts. The two campaigns took place in separate regions of the world and during very different historical periods, including one during the early years of the Cold War and one nearly a decade after. While neither target regime was fully democratic, the first Apartheid government held a slightly higher Polity score than Fujimori's administration (4 versus 0 on the 21-point scale). Moreover, the two campaigns adopted unique approaches to defining their objectives. In Peru, regime change was always the end game, while in South Africa, dissidents began first by demanding more limited policy change and only later elevated their claims into demands for full-scale regime change. This would seem to suggest that black South Africans, compared with their Peruvian counterparts, should have had an easier time in achieving their initially less ambitious goals during the early period of the conflict.

Yet, as I show below, these different structural and campaign-specfic conditions exerted much less of an impact on campaign outcomes than the temporal dynamics inherent in the movements themselves. The discussion that follows helps to elaborate on why the cases saw different trajectories by focusing on the causal mechanisms identified by my theory.

Anti-Fujimori campaign in Peru

The campaign to remove President Alberto Fujimori from power was unexpected both in terms of its success and its short duration. Fujimori had held Peru's presidency since being elected in 1990 and had relied on authoritarian tactics to maintain rule after carrying out an *autogolpe* − or self-coup − with the support of the military in 1992 (Carrión 2006; Crabtree 2001). During his first term in office, the state's victory over the Sendero Luminoso insurgency and his government's success in controlling inflation bolstered Fujimori's popularity and helped him secure a convincing win in the 1995 election. State intimidation and weak political party organizations further quelled civil society resistance during his first decade in power (Burt 2006; Levitsky and Cameron 2003).

Therefore when Fujimori announced that he would run in the 2000 elections, despite a term limit that he had introduced eight years prior, few expected the domestic opposition to prevent his continuation in power. While there was some brief mobilization to hold a referendum on his eligibility for re-election in 1997, the opposition was fragmented and could not sustain a concerted pro-democracy movement (Berntzen and Skinlo 2010, 203; Levitsky and Cameron 2003, 15; Roberts 2006, 98). The success of the nonviolent campaign in 2000 then cannot be attributed to a weak opponent. Indeed, Fujimori had further proven his resolve against domestic challengers by brutally repressing the Sendero Luminoso and Túpac Amaru insurgencies just a few years prior. The failure of these guerilla movements, despite their having different political objectives, suggests that nonviolent resistance was not chosen simply as an easier alternative to armed struggle.

The first round of the 2000 elections on April 9 saw widespread fraud and harassment of the opposition (Berntzen and Skinlo 2010). It is widely believed that

Fujimori would have been declared winner immediately after the first round of voting if not for large street protests led by the second-place candidate, Alejandro Toledo, calling for a run-off ballot (Conaghan 2001, 14; L. Taylor 2005). Instead, electoral officials announced a second round, which Fujimori then won after Toledo withdrew when it became clear that the government would continue to manipulate the election to guarantee a Fujimori victory (Berntzen and Skinlo 2010).

Having thus failed to oust Fujimori through electoral channels, the opposition turned to nonviolent resistance. A massive demonstration, La March de los Quatro Suyos, brought protesters from across the country to Lima for a three-day period that coincided with Fujimori's inauguration on July 29 (Brett 2000; Conaghan 2001; Muñoz 2009; L. Taylor 2005). The demonstration was brutally repressed and state intelligence agents infiltrated the protests to incite violence as a way to justify a government crackdown (Brett 2000; Levitsky and Cameron 2003, 19). This action began a several month period of protests calling on Fujimori to resign (L. Taylor 2005). One resistance organization, *Colectivo Sociedad Civil*, staged weekly performances in public spaces calling attention to the transgressions of the regime. These acts included a symbolic funeral for the electoral authority, a ceremonial washing of the Peruvian flag and the positioning of Fujimori effigies made of trash at several government buildings (Buendía 2014). Another organization, *Resistencia*, organized a 'Minute of Resistance' every Friday evening where the group called on citizens to chant, bang on pots and march in a public square in Lima's Miraflores district. (Conaghan 2001, 21). Compared to the spontaneous, one-off resistance efforts in the past, the campaign in 2000 was much more highly coordinated.

The campaign came to a head when, on September 14, a video was aired on national television showing Vladimiro Montesinos, the head of the national intelligence service and Fujimori's closest ally, paying a bribe to buy the support of a congress-

man (Berntzen and Skinlo 2010, 198; Cameron 2006). The release of the videotape sparked further protests and swiftly delegitimized the governing coalition (BBC News 2000). After a few days, Fujimori announced he would call new elections and finish his term early. Two months later, on November 20, Fujimori fled to Japan and faxed his resignation to Congress (McDermott 2000). New elections were held in April 2001, which Toledo won, and a return to democracy began.

While the Montesinos videotape appears to be the contigent moment that broke the regime (Cameron 2006), there would have been no transition of power if not for the consistent pressure that was applied by the civil resistance campaign. Indeed, the protest movement kept 'the debate about democratization alive ... [so that] when the Montesinos scandals hit the front pages, Fujimori was forced to deal with the problem in a context in which the regime's margin to maneuver had been effectively reduced' (Conaghan 2001, 2). The scandal is best understood as a necessary but not sufficient condition for the regime's fall; without the concurrent resistance campaign, it is not certain that the videotape alone could have ousted Fujimori.

The two key mechanisms of nonviolent campaign success were present in the anti-Fujimori movement. First, the size of the campaign was sustained throughout the several months of protest in 2000, with nonviolent demonstrations regularly seeing tens of thousands of participants. Despite a weak civil society and a scarce history of nonviolent action, the post-election demonstrations were much larger than any mobilization in the previous two decades (Crabtree 2001, 302). Unlike in the previous ten years of Fujimori's presidency, citizens were genuinely convinced of the potential for nonviolent resistance to oust him from power, which in turn encouraged them to participate.

Second, defections were closely linked to the expected success of the resistance campaign. Once it appeared that the campaign could overthrow Fujimori, defectors

from within the state security sector and political elite attempted to realign. Just a few weeks after the Montesinos videotape was released, army captain Ollanta Humala led a rebellion in Arequipa that, while insignificant in military terms, created the spectre of looming regime collapse (Berntzen and Skinlo 2010, 199). Nearly at the same time, Vice President Francisco Tuledo resigned and other congressmen and ministers defected from the governing party (Pérez-Liñan 2007, 184; Weyland 2006, 34). It was this realignment in the legislature that allowed the opposition to unseat the pro-government congressional leadership just days before Fujimori resigned (Berntzen and Skinlo 2010). These defections certaily would not have taken place if the prospects for removing Fujimori appeared weaker and the street mobilizations did not demand action from the military and political elite (Hochstetler 2006, 410).

Overall, the anti-Fujimori campaign illustrates that rapid mobilization is crucial to nonviolent resistance. Several months of large-scale protest managed to dislodge an authoritarian regime that seemed firmly entrenched after a decade in power. It is impossible to evaluate the counterfactual of what would have occured if Fujimori weathered the intial mobilization and forced the campaign to continue its struggle later into his new term in office. Yet part of the success of the campaign in attracting participants appears to be the novelty of nonviolent action after a long period of silent acquiesence (Buendía 2014). It likely would have been difficult to sustain the momentum of the movement if Fujimori re-entrenched himself in 2000.[13] If the anti-Fujimori campaign was to succeed at all, it had to do so quickly within the brief window provided by the fraudulent election and corruption scandal.

---

13. Even in recent history, there is no shortage of competitive authoritarian regimes that have consolidated power after a post-election civil resistance campaign failed to quickly channel anger over a fradulent vote into regime change. See, for example, elections in Zimbabwe (2008), Honduras (2017) and Kenya (2017).

First anti-Apartheid campaign in South Africa

The first attempt at nonviolence in the fight against Apartheid is much less frequently discussed in the scholarly literature than the successful nonviolent campaign of the 1980s. Yet this initial mobilization, which lasted from 1952 to 1961, according to the NAVCO data, offers important insights into why campaigns fail and how their failure relates to the duration of the struggle. While the first anti-Apartheid campaign was intially quite successful in mobilizing black South Africans and drawing attention to their cause, state repression caused momentum to gradually wane until the nonviolent strategy was dropped altogether in favour of armed insurrection.

The nonviolent campaign of the 1950s was foremost a reaction to the 1948 election of the National Party, which, upon coming to power, introduced a series of wide-ranging laws that institutionalized racial discrimination against non-whites through segregation and political disenfrachisement. The African National Congress (ANC), the principle national organization representing black interests, turned to mass mobilization as a way to pressure the government into reforming these Apartheid laws and providing for the black majority's participation in the political system. While the ANC was formed in 1912, its resistance to white domination up to this point had largely taken place through institutional channels and lobbying, to little avail (Schock 2005).

The nonviolent resistance began officially in 1952, when the ANC, along with other racial organizations, called for a 'Defiance of Unjust Laws Campaign' (Ackerman and DuVall 2000; Kuper 1957; Lodge 2009; Seidman 2000; Zunes 1999). Organizers encouraged participants to purposefully break Apartheid laws by violating curfews, defying segregation ordinances and burning the passbooks that black citizens were required to carry at all times. Participants were expected to refuse to pay fines for

their transregressions and instead accept jail time, with movement leaders expecting mass incarceration to overwhelm the system and force change from the government (Lodge 2009; Schock 2005). These tactics were accompanied by more traditional forms of nonviolent action, including protests, strikes and boycotts (Ackerman and DuVall 2000; Kuper 1957).

The state, supported by a well-trained and well-equipped security apparatus, responded with severe repression and mass arrests (Kuper 1957; Seidman 2000). Similar to the Peruvian case, the state's *agents provocateurs* often incited rioting during protests in order to justify repression (Zunes 1999, 150-1). Ultimately, state violence led the ANC to call off all defiance actions in 1953. However, the campaign continued throughout the decade in the form of boycotts and stay-at-home strikes, as well as increased ANC involvement in protests over local issues like bus fare hikes (Ackerman and DuVall 2000; Feit 1967).

The failure of protests and strikes to produce tangible results caused more and more activists within the movement to question the efficacy of nonviolent action (Feit 1967). From 1959 onwards, a rival campaign organization, the Pan-Africanist Congress (PAC), presented a more militant alternative to the moderate approach of the ANC (Ackerman and DuVall 2000). In one action in 1960, the PAC led demonstrators in burning their passbooks and antagonizing a police station in Sharpville, which resulted in nearly 70 deaths when police opened fire on protestors (Lodge 2009). The Sharpville massacre sparked weeks of mass protests across the country, which the state responded to with widespread arrests and the banning of both the PAC and ANC (Ackerman and DuVall 2000; Seidman 2000). Recognizing the failure of their nonviolent strategy, and now forced to operate underground, the ANC formed an armed wing, *Umkhonto we Sizwe*, which effectively ended the nonviolent campaign in 1961 in favour of a guerilla insurgency (Ackerman and DuVall 2000; Lodge 2009).

Umkhonto leaders, including Nelson Mandela and Walter Sisulu, were captured soon after and sentenced to life imprisonment (Schock 2005).

The failure of South Africa's first anti-Apartheid campaign can be understood with reference to the two key mechanisms of nonviolent campaign success. In terms of campaign size, the first campaign was consistently in the range of 10,000 to 100,000 participants, but this support was concentrated only in particular regions (Kuper 1957, 123). While this range is the same as the Peruvian campaign, and perhaps even larger in some years, it is still relatively small given the eighty percent non-white population in South Africa that could have potentially been mobilized. More importantly, the movement failed to grow its membership over its duration. The mass incarceration and repression during the defiance actions in the first year of the campaign discouraged new recruits and substantially reduced the number of leadership personnel and rank-and-file participants available in later actions (Kuper 1957, 145; Schock 2005). Ultimately, the intransigence of the Apartheid government sapped momentum from the movement over time. As the prospects for change gradually became weaker throughout the 1950s, would-be dissidents opted to stay on the sidelines rather than participate in demonstrations.

The campaign also never succeeded in eliciting security sector defections and there are no accounts that suggest it tried to do so. Of course, the struggle was framed in explicitly racial terms that may have alienated potential white supporters. This likely was the only option and the second anti-Apartheid campaign still succeeded despite never inducing defections either. Nonetheless, if defections were ever possible, they certainly became less likely after the government weathered the initial defiance campaign in 1952, when the intensity of the resistance was greatest. From this point onwards, the nonviolent movement appeared increasingly unlikely to succeed and few within the security sector were willing to risk their careers to support the Apartheid
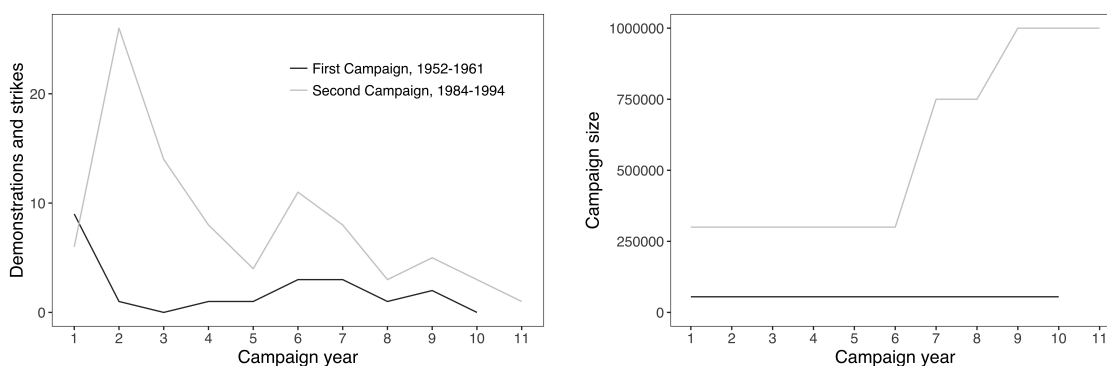
Figure 7: Number of protest events and campaign size in South Africa anti-Apartheid campaigns

Campaign membership sizes are based on the mid-point values for the categorical campaign size measure in the NAVCO 2.0 dataset. Protest event data is from Banks (2001).

regime's challengers.

Before concluding on the South African case, it is instructive to contrast the earlier campaign with the second nonviolent uprising, which succeeded after an even longer campaign from 1984 to 1994. My hypothesized relationship between campaign duration and nonviolent success suggests this second campaign should not have succeeded and, indeed, it is only one of four campaigns in the dataset that achieved victory after more than ten years of struggle. On the whole, the success of the second campaign is largely unaccounted for by the theoretical explanation offered above. A number of other variables were at play, including international sanctions (Zunes 1999), a larger, wiser ANC (Mandela 1994; Seidman 2000) and a stronger bargaining position following the guerilla resistance of previous decades (Chenoweth and Schock 2015; Seidman 2000).

Yet even more important may have been the unique mobilizational achievements of the second campaign. As Figure 7 shows, the second campaign organized more protest events over its duration and increased membership figures drastically as time went on. This steady growth in participation is highly idiosyncratic when contrasted with the

majority of nonviolent campaigns, which tend to flatline or lose members over time, as was the case with the first campaign. Indeed, many within the second campaign actually perceived the likelihood of victory as increasing as time went on (Zunes 1999, 161-2). The regime appeared increasingly vulnerable and these perceptions fuelled participants' willingness to join the resistance campaign. These same motivational dynamics were absent in the first abortive attempt at overthrowing Apartheid, where the perceived likelihood of success was negatively correlated with the campaign's duration.

Overall, the failure of South Africa's first nonviolent campaign against Apartheid serves as an illustrative example of how longer campaigns tend to lose momentum over time. Despite an initially impressive mobilization against Apartheid laws, the ANC failed to continue building its participant base in the face of heavy state repression. The campaign's perceived lack of efficacy grew over time and eventually produced stagnation in protest participation, which in turn led to an unsuccessful end to the first campaign and a switch to violent tactics.

Conclusion

The empirical study of nonviolent civil resistance remains an ongoing project. Chenoweth and Stephan (2011) offered the first concrete evidence that nonviolent campaigns are more successful in achieving their goals than armed rebellion. In this study, I set out to extend this line of inquiry by exploring how the effectiveness of nonviolent and violent strategies is related to campaign duration. I have argued that temporality presents an important caveat to our current understanding of the strategic advantage of nonviolence. Nonviolent campaigns, while overall more likely to succeed than armed resistance, become increasingly less likely to achieve their objectives over time.

After the first several years, their comparative advantage over violent campaigns vanishes. The waning effectiveness of nonviolent resistance appears to be driven to a large degree by a declining ability to continue mobilizing participants and provoking defections from the security forces.

These findings suggest important practical and policy considerations. For activists leading a maximalist resistance movement, the key lesson from my analysis is that building a nonviolent campaign should be done as quickly as possible. The momentum that drives participation and security force defection is often fleeting and so there is little rationale for conserving resources in the early period of an uprising. In general, whatever resources, funding or manpower a campaign possesses should be expended early on, so as to capitalize on the novelty and uncertainty that accompany a new resistance campaign.

Similarly, foreign governments that are evaluating whether to provide assistance to a nonviolent campaign in another country should be careful to consider the timing of their interventions. Foreign state support encompasses a range of activities, such as the provision of material or logistical resources, diplomatic interventions or economic sanctions. If we assume that these activities will most effectively bolster resistance efforts when a campaign is already at its peak strength, then external states should attempt to provide support as early on as possible. Delaying assistance to a resistance movement may mean that activists receive the backing they need only after the possibility for mass mobilization and security force defection has already passed.

That being said, there are limitations to my study. First, as the case studies reveal, there is an inherent difficulty in measuring and attributing success in resistance campaigns. In Peru, the campaign's regime change goals were clearly achieved when Fujimori left power. Yet the importance of the Montesinos scandal makes it hard to determine the extent to which civil resistance or this contingent event tipped the

outcome. I would surmise that these kinds of ambiguities underline the outcomes of most other resistance campaigns, which makes the project of understanding nonviolent successes neither less important nor less challenging. In South Africa, the first defiance campaign's stated objectives were clearly not met, as Apartheid went on to survive another three decades. However, activists within the resistance, while obviously disappointed in their strategic failure, nonetheless considered the campaign successful for mobilizing the previously acquiescent black population and helping restore their sense of dignity. These types of symbolic or psychological victories are infinitely more difficult, if not impossible, to quantify. So while my analysis has revealed important patterns in the strategic use of nonviolence, it has less to say about what success should mean in the context of civil resistance.

Second, my findings are only applicable to a subset of all resistance campaigns. In particular, the NAVCO data does not record information on campaigns that are supressed in their infancy before they can meet the criteria for inclusion in the dataset (Chenoweth and Lewis 2013b, 420). For this reason, I cannot make broader claims about the efficacy of these 'would-be' campaigns, or about campaigns with less ambitious political goals or smaller membership figures.

Finally, there are inherent limitations to a dichotomous classification of resistance strategies (Lehoucq 2016, 273). In this study, campaigns are categorized as violent or nonviolent based on their primary resistance method. The NAVCO 2.0 dataset does include variables on tactical switching and radical flanks, but there remains a larger question of whether diverse events and resistance organizations can be accurately summarized into a single campaign-year measurement.

Therefore future research on campaign trajectories could benefit from the growing number of high-quality event-level datasets on nonviolent action. The Social Conflict Analysis Database (Salehyan et al. 2012) and the recently-released NAVCO

3.0 dataset (Chenoweth, Pinckney, and Lewis 2018) record detailed information on individual demonstrations, including the number of participants, tactics used, level of state repression and location. Investigating temporal patterns and resistance methods at the event-level could offer important new insights into how nonviolent campaigns evolve and transform over time. That being said, one challenge with using this data to explore trajectories will be to find a way to identify particular events as parts of broader campaigns.

Beyond these methodological concerns, there are also substantive questions that remain to be explored in the study of civil resistance. A common theme in the study of social movements and contentious politics has been the lack of investigation into the processes that lead to movement decline and death (Davenport 2014; V. Taylor 1989). This has been equally true in the literature on nonviolent civil resistance, where a majority of studies have either focused on cases of success or else have not provided more general insights into how some campaigns fail or stagnate. My argument has pointed to how expectations shape collective action and the behaviour of security forces, but there is room for future research to investigate other reasons for movement decline. For example, switching to violent tactics, the emergence of radical flanks, media coverage and the willingness of foreign states to intervene or impose sanctions are all likely to play a role in the viability of nonviolent campaigns over time.

On the other side of the problem, more empirical work is also needed to identify the factors that make nonviolent campaigns resilient against demobilization or faltering expectations over time. As a starting point, Shock (2005, 142-4) highlighted how tactical innovation and a decentralized organizational structure can bolster the resiliency of nonviolent resistance. Other analyses have looked into when and why increased mobilization occurs after repression (e.g. Anisin 2016; Sutton, Butcher, and Svensson 2014) and this research could be extended by investigating how these 'back-

fire' dynamics contribute to overall success or are themselves influenced by campaign duration.

Finally, my findings suggest an interesting puzzle that could be explored in greater depth in future work. Namely, how and why do nonviolent campaigns continue to challenge the government even after the likelihood of success has begun to diminish quickly? In the NAVCO data, some campaigns end after a few years of fruitless struggle, while others manage to remain in the dataset by maintaining a degree of mass mobilization in spite of weakening odds. In terms of the mechanisms of sustaining participant engagement, a campaign's recruitment infrastructure and its social networks would seem a likely starting point for an explanation. But the harder question is why campaigns would choose to pursue a strategy of continued resistance. My findings shows that it is irrational to prolong a nonviolent campaign after a certain point, but it is unclear whether campaign leaders are aware of their declining likelihood of success or if they are simply blind to such strategic realities. As the anti-Apartheid case study shows, there may in fact be some advantage to ending a faltering mass resistance campaign early, with the hope that re-engaging at a latter time will produce success.

—

In this thesis, I have highlighted an important qualification to current understandings of the strategic advantage of nonviolence over violence. For resistance campaigns seeking to effect ambitious political objectives, nonviolence is not the panacea previous research has suggested. Rather, a nonviolent strategy only offers a meaningful advantage over violence when the resistance movement can quickly translate its mass mobilization into political pressure. Nonviolent campaigns that are forced to prolong their struggle over several years become increasingly unlikely to achieve victory. This

is in stark contrast to armed rebellions, where the very logic of victory depends upon surviving over a long period in order to frustrate the government into concessions.

Up until now, most academic research has focused on the unprecedented mass movements that can effect political change seemingly out of thin air. However, I have demonstrated that many nonviolent movements do not follow this model. Therefore, as the empirical literature on civil resistance continues to expand, researchers would be remiss not to incorporate time into their theories and analyses. From both a scholarly and practical perspective, this would greatly improve our ability to understand and assist the nonviolent campaigns that have become increasingly common over the past several decades.

Appendix

The appendix is organized into three sections according to the analysis presented in the main text: (A) duration models, (B) instrumental variables models and (C) the mechanisms of campaign success.

Before proceeding to these additional analyses, Table A-1 provides the descriptive statistics for the variables used in the analysis in the main text and here in the appendix.

Table A-1: Descriptive statistics

Variable	N	Mean	St. Dev.	Min	Max
Strategic success	1,726	0.082	0.275	0	1
Nonviolent campaign	1,726	0.198	0.399	0	1
Campaign year (duration)	1,726	9.632	9.946	1	59
Campaign size (0-5)	1,558	0.682	0.771	0	3
Repression (0-3)	1,701	2.661	0.834	0	3
Security sector defections	1,145	0.229	0.420	0	1
Regime defections	1,116	0.160	0.367	0	1
Foreign state regime support	1,503	0.593	0.491	0	1
Foreign state campaign support	1,634	0.559	0.497	0	1
International sanctions	1,690	0.153	0.360	0	1
Cold War	1,726	0.728	0.445	0	1
Goal: Regime change	1,710	0.418	0.493	0	1
Goal: Secession	1,710	0.204	0.403	0	1
Goal: Anti-occupation	1,710	0.209	0.407	0	1
Target Polity (initial)	1,550	−0.688	7.056	−10	10
Log GDP per capita (initial)	1,400	7.718	0.925	5.772	9.591
Log population (initial)	1,407	16.842	1.488	13.237	20.819
Target military capacity	1,668	0.016	0.032	0.00001	0.210
Fertility rate	1,386	4.774	1.873	1.085	8.198
Mountainous terrain (log)	1,700	2.558	1.174	0.000	4.407
Ethnic fractionalization	1,700	0.521	0.275	0.004	0.925
Violent campaign ongoing	1,726	0.843	0.364	0	1
Interstate war	1,504	0.230	0.421	0	1
Female-male education ratio	960	85.838	17.353	0.000	114.935
Population density	1,369	0.0001	0.0002	0.00000	0.001
Ongoing NV in neighbours (%)	1,521	0.087	0.148	0.000	1.000
Mean neighbour polity	1,516	−0.881	4.253	−9.000	9.875
NV success in neighbour	1,521	0.076	0.266	0	1

A   Duration models

A.1   Cox models using *WCRW* data

The analysis presented in-text relies only on NAVCO 2.0 data, which records observations at the campaign-year for 250 campaigns between 1945 and 2006. The original analysis in Chenoweth and Stephan (2011) is based on a larger sample of 323 campaigns between 1900 and 2006. This NAVCO 1.0 dataset only includes one row observation for each campaign and there are no time-varying covariates. In the authors' original analysis, campaign success was modelled using logistic regression. To test whether my proposed theory holds when using the original data, I re-run these analyses using Cox proportional hazard models with time-dependent coefficients for campaign type. Like the event-history models used in the main text, this approach allows for the baseline hazard rate to vary between violent and nonviolent campaigns.

In Table A-2, I present a series of analyses using the original data from *Why Civil Resistance Works*. Following the original analysis, I model the outcome of interest as campaign success. Duration is measured in campaign-days. Models 1 and 2 are essentially replications of Chenoweth and Stephan's logistic regression models, except the analysis here uses Cox models. These two models confirm that the core findings of Chenoweth and Stephan are robust when using survival analysis. In both models, nonviolent campaign type drastically increases the likelihood of campaign success, even when controlling for important covariates in Models 2. The hazard ratios on the nonviolent campaign variable indicate that the "risk" of success in these two models is 4.9 and 5.8 times greater for nonviolent versus violent campaigns (when assuming proportional hazards).

I test my own theory in Model 3. In this specification, the hazard ratio for nonviolent campaign type is allowed to vary over time. Specifying nonviolent campaign

type with a time-varying coefficient (TVC) permits the baseline hazard to differ between violent and nonviolent campaigns. This approach is analogous to interacting duration and campaign type in the logistic regression models presented in the main text. In Model 3, two coefficients are presented: one which represents the static effect of campaign type, and the other that alters this effect as a function of campaign duration. The first coefficient, the overall effect of nonviolent campaign type, is positive and statistically significant in this model, indicating that nonviolent campaigns overall are more successful when campaign duration is zero. The second coefficient indicates how the initial hazard ratio changes with each passing unit of campaign duration. The coefficient of 0.999 indicates that with each subsequent day that a nonviolent campaign survives, the likelihood of its success is 99.9% of what it was the day before. This effect may seem small in the abstract, but because the effect decays by a fixed percentage, the relative advantage of a nonviolent strategy quickly deteriorates. For example, after one year, nonviolent campaigns become over 12% less likely to succeed than when they first begin; after five years, their relative advantage over violent campaigns is nearly halved. In sum, these results present a similar story to the analysis presented in the main text using the campaign-year NAVCO 2.0 data: nonviolent campaigns, while more successful overall, tends to decline rapidly relative to violent campaigns as campaign duration increases.

A.2   Alternative time specification

In the main text, I allow the hazard function to vary by campaign type by including a simple interaction between campaign-year and the binary indicator for nonviolent campaign type. Another specification of time, recommended by Carter and Signorino (2010), involves including linear time, as well as its squared and cubic terms in logistic

Table A-2: Results from Cox models using Chenoweth and Stephan data

| | Success | | |
	(1)	(2)	(3)
NV campaign (static)	4.936***	5.788***	10.462***
	(1.059)	(1.747)	(4.084)
NV campaign (TVC)			0.999**
			(0.000)
Log membership		1.091	1.089
		(0.073)	(0.070)
Repression		0.385***	0.435***
		(0.121)	(0.137)
Security defections		1.815***	1.622**
		(0.391)	(0.365)
Foreign state support		1.322	1.261
		(0.377)	(0.368)
Economic sanctions		0.843	0.808
		(0.255)	(0.233)
Observations	323	258	258
Log likelihood	-524.98	-350.79	-347.61

*p<0.1; **p<0.05; ***p<0.01. Estimates presented as hazard ratios. Standard errors clustered by country in parentheses.

models, and then interacting these three time measures with the relevant treatment (i.e. nonviolent campaign type) to allow the hazard to vary by treatment group. This approach allows the underlying hazard function to vary more freely across values of campaign-year, potentially picking up any non-linearity in the relationship with campaign success.

In Table A-3, I repeat my analyses from the main text using a single duration variable alongside equivalent models when the cubic polynomials for duration are also included. It is difficult to compare these models directly without calculating predictive margins, but it is encouraging to see that the coefficient on the non-interacted nonviolent campaign variable is fairly similar regardless of whether cubic polynomials are included or not. Moreover, the log likelihood for each pair of models do not indicate that the inclusion of the polynomials meaningfully improves model fit.

In the right panel of Figure A-1, I show the predicted probabilities of campaign success when modeling time under Carter and Signorino's t, t^2, t^3 specification. The results are substantively similar to when duration is included in its untransformed form (left panel of Figure A-1). The plots are based on Models 1 and 2 in Table A-3, which rely on the largest sample.

Table A-3: Results from logistic models with cubic polynomial and untransformed time specifications

	Success					
	(1)	(2)	(3)	(4)	(5)	(6)
Nonviolent	1.352***	1.271**	2.416***	2.693***	2.366***	2.563***
	(0.327)	(0.601)	(0.468)	(0.771)	(0.464)	(0.782)
Nonviolent × Duration	−0.104**	−0.049	−0.131***	−0.542	−0.099*	−0.458
	(0.048)	(0.339)	(0.051)	(0.449)	(0.056)	(0.459)
Nonviolent × Duration^2		−0.001		0.086		0.075
		(0.049)		(0.068)		(0.069)
Nonviolent × Duration^3		−0.0003		−0.004		−0.003
		(0.002)		(0.003)		(0.003)
Duration	−0.017	−0.030	−0.001	−0.102	0.006	−0.121
	(0.015)	(0.098)	(0.022)	(0.128)	(0.022)	(0.130)
Duration^2		0.003		0.007		0.009
		(0.006)		(0.009)		(0.009)
Duration^3		−0.0001		−0.0001		−0.0002
		(0.0001)		(0.0002)		(0.0002)
Observations	1501	1501	1097	1097	1097	1097
Log likelihood	-354.49	-353.61	-232.29	-230.84	-228.40	-226.96

*p<0.1; **p<0.05; ***p<0.01. Standard errors clustered by country-year in parentheses. Unreported covariates include Cold War, repression, foreign state support, campaign size, campaign goals (Models 1, 2, 5 and 6), Polity score, population size (log), GDP per capita (log) and target military capacity (Models 3, 4, 5 and 6).
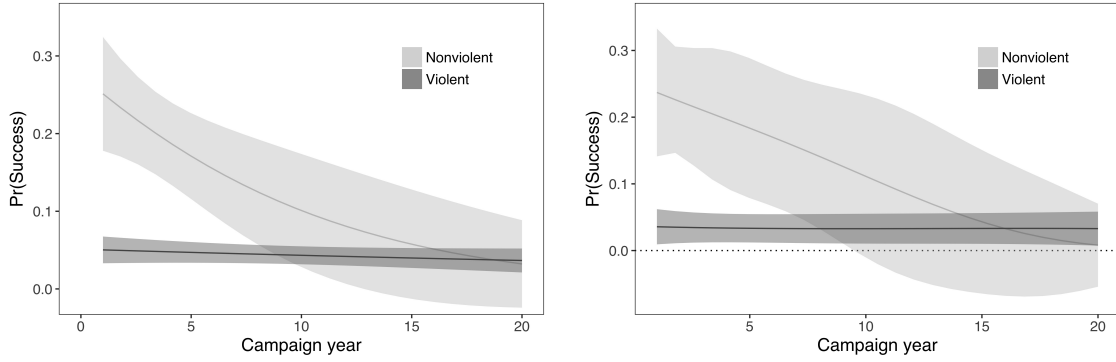
Figure A-1: Predicted probability of success by campaign type specified using un-tranformed time (left) and cubic polynomial time (right).

Predictive margins and 95% confidence intervals are calculated from a model including the following covariates: Cold War, repression, foreign state support, campaign size and campaign goals.

A.3 Visualizing model output using simulation

The predicted probability plots presented in the main text are based on predictive margins on the probability scale. An alternative approach to visualizing estimates involves statistical simulation (King, Tomz, and Wittenberg 2000). Under this approach, expected values are calculated from parameters sampled N times from the variance-covariance matrix of the model estimates. The resulting simulated values can then be summarized into quantities of interest by taking the median and relevant quantiles for the desired confidence level.

In Figure A-2, I present the results from such a simulation using 5,000 draws from the same model used to calcluate predicted probabilities in the main text. In panel (a), the likelihood of nonviolent campaign success is clearly defined by a similar decay function as was evident in the predicted probabilities graphs. However, the confidence intervals around these estimates are much larger and show an overlap between nonviolent and violent campaign types across all values of campaign duration. In order to determine whether the relative advantage of nonviolent campaign type is statistically significant at some values of campaign duration, I take the first differences

between the two sets of simulated values. In panel (b), the first differences and their confidence intervals show that the advantage of nonviolence remains statistically distinguishable from zero only for the first several years of the campaign. Thus the simulation-based approach to generating expected values reveals similar findings as the predictive margins that are presented in the main text.
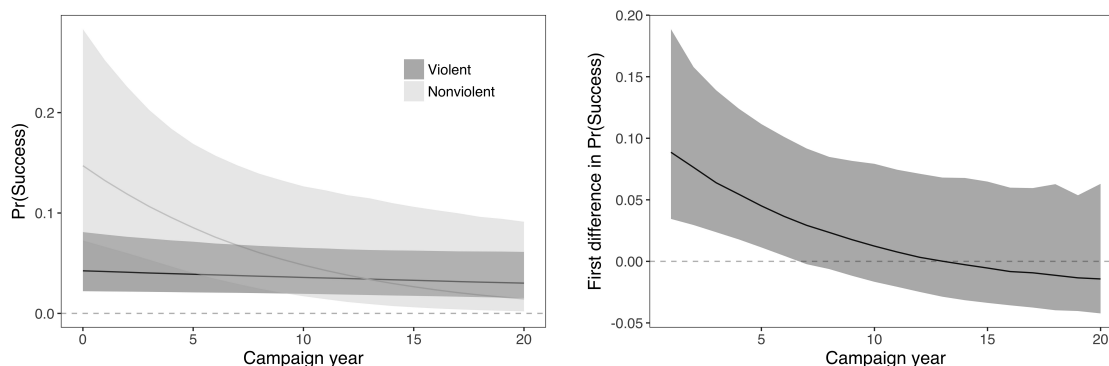
Figure A-2: Simulated probability of success by campaign type (left) and first difference between campaign types over time (right).

Simulated probabilities and 95% confidence intervals are calculated from a model including the following covariates: Cold War, repression, foreign state support, campaign size and campaign goals.

A.4 Sensitivity to outliers

One possible issue with the analysis presented in the main text is that the distribution of campaign duration is highly skewed and very few nonviolent campaigns last longer than twenty years. Only 19 campaigns in the dataset – or 7.6% of all campaigns – survive past 20 years. Just two of these were nonviolent and both failed. The inclusion of these campaigns could bias the results by increasing the number of nonviolent observations with high campaign duration values and no successes. To test this possibility, I removed these campaigns – the Palestinian Liberation (1973-2006) and the West Papua Anti-Occupation (1964-2006) – and re-ran the main model. As a second check against bias resulting from large campaign duration values, I arbitrarily right-censored all observations of either campaign type at their twentieth year.

Table A-4 compares the coefficient estimates on the main explanatory variables under the three specifications. The first column presents the original analysis that is included in the main text. The second columns shows the results when the two long nonviolent campaigns are removed and the last column presents the results when observations are right-censored at 20 years. All models include the control variables used in the main analysis in-text.

Broadly speaking, the results are not overly sensitive to the presence of outliers. The coefficient estimates on each of the three key covariates are fairly constant across each model. The uninteracted nonviolent campaign variable coefficients varies at most by 0.1. The interaction between nonviolent campaign type and duration seems most affected by the removal of the two outliers (notably in the outliers-removed model). In this sample, the coefficient size is nearly halved, suggesting a weaker time decay effect among the non-outlier nonviolent campaigns. The uncertainty around this estimate is also much larger relative to the magnitude of the effect, but, as I show below through predictive margins, the main finding that nonviolent campaigns only possess a statistically significant advantage in their first few years is still supported when using this sample. Lastly, the non-interacted duration term − which corresponds to the time trend for violent campaigns − has a nearly identical coefficient in the first two samples. While the non-interacted variable does change sign when observations are right-censored at 20 years, the magnitude of the effect is negligible (see Figure A-3, described further below).

To confirm that the results are consistent between the three samples, I calculate the three sets of predicted probabilities in Figure A-3. As the plots show, the uncertainty around the predictions naturally increases when observations are removed. Yet a constant in each plot is that the separation between the two campaign types is only significant in the first several years. Even for the sample with the nonviolent

57

Table A-4: Results from logistic models of success testing sensitivity to outliers

	Full sample	NV outliers removed	Duration ≤ 20
Nonviolent	1.352***	1.305***	1.426***
	(0.327)	(0.356)	(0.376)
Nonviolent × Duration	−0.104**	−0.065	−0.121*
	(0.048)	(0.073)	(0.067)
Duration	−0.017	−0.013	0.005
	(0.015)	(0.015)	(0.032)
Observations	1,501	1,425	1,307
Log Likelihood	−354.494	−351.122	−330.009
Controls	Yes	Yes	Yes

*p<0.1; **p<0.05; ***p<0.01. Standard errors clustered by country-year in parentheses. Unreported covariates include Cold War, repression, foreign state support, campaign size and campaign goals.

outliers removed (middle), which shows a higher baseline likelihood of nonviolent success, the relative advantage of nonviolent strategy is clearly time-dependent. These results suggest that while there is some evidence of model dependence in the analysis presented in the main text, the presence of outliers does not substantively alter the core findings.
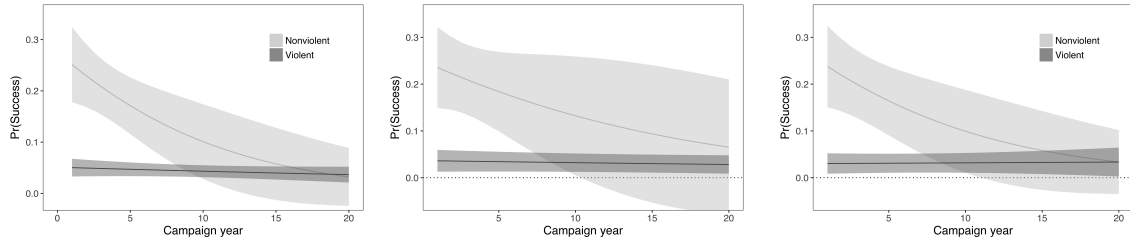
Figure A-3: Predicted probability of success by campaign type using the full sample (left), NV outliers removed sample (middle) and sample right-censored at 20 years (right).

Predictive margins and 95% confidence intervals are calculated from a model including the following covariates: Cold War, repression, foreign state support, campaign size and campaign goals.

A.5   Tactical switching

The main independent variable in my analysis is a dummy variable indicating whether a campaign adheres to a nonviolent strategy. This variable does not vary over the duration of a campaign and is based on the original designations made in Chenoweth and Stephan's (2011) *Why Civil Resistance Works*. However, a small number of campaign-years − around 6% in the NAVCO 2.0 data − see campaigns switch tactics away from their overall strategy.

In Table A-5, I show that my results are not particularly affected by the presence of tactical switching. Model 1 in this table is the same specification from the main-text, presented here for comparison. Model 2 adds in a dummy variable that scores '1' for campaign-years where the movement is using tactics that differ from its overall static designation. This tactical mismatch variable is insignificant and the estimates on the nonviolent and nonviolent × duration terms are nearly identical to Model 1.

In Model 3, I investigate whether the effect of tactical switching varies by campaign type. I disaggregate the tactical mismatch variable from Model 2 into indicators for nonviolent campaigns using violent tactics in a given year and violent campaigns using nonviolence. As the estimates on these two variables show, the insignificance of the tactical mismatch variable in Model 2 was likely a result of countervailing effects between the two campaign types. As theory might suggest, nonviolent campaigns tend to be less likely to succeed when they switch to violence and violent groups perform better when converting to nonviolence. The main estimates on the nonviolent and nonviolent × duration variables are only marginally changed in this third specification. The nonviolent term alone remains large and statistically significant; the coefficient for the interaction with duration is slightly attenuated and is no longer statistically significant. This change is notable, but it is nonetheless encouraging to

find that the coefficient estimate is still negative, indicating the expected decline in nonviolent campaign efficacy over time.

Ultimately, tactical switching is endogenous to success. Campaigns that believe their current strategy is unlikely to produce success are more likely to switch tactics. For this reason, the time-invariant measure of campaign strategy is preferable because it avoids the bias that is likely to result from campaigns that switch tactics when victory is already unlikely. While this approach introduces some measurement error, it facilitates a comparison across strategies that gets closer to estimating a causal effect.

Table A-5: Results from logistic models testing effects of tactical switching

	Campaign success		
	(1)	(2)	(3)
Nonviolent	1.352***	1.352***	1.513***
	(0.327)	(0.328)	(0.347)
Nonviolent × Duration	−0.104**	−0.105**	−0.077
	(0.048)	(0.053)	(0.050)
Duration	−0.017	−0.017	−0.014
	(0.015)	(0.015)	(0.015)
Tactical mismatch		0.032	
		(0.626)	
Nonviolent using violent			−15.193***
			(0.377)
Violent using nonviolent			1.389**
			(0.590)
Observations	1501	1501	1501
Log likelihood	-354.49	-354.49	-347.91

*p<0.1; **p<0.05; ***p<0.01. Standard errors clustered by country-year in parentheses. Unreported covariates include Cold War, repression, foreign state support, campaign size and campaign goals.

B Instrumental variables

B.1 Overidentification tests

In addition to fertility rates, I intend to use additional instruments in order to conduct overidentification tests that help to assess the validity of my main instrument. These two instruments are (a) the ratio of female to male enrollment in primary education and (b) the diffusion of nonviolent campaigns. The logic of the female education variable closely parallels that of fertility rates: increased gender equality, measured as greater female participation in education, increases the likelihood that a campaign will opt for a nonviolent strategy. The idea for this variable is also borrowed from Shafternaar (2017).

The second instrument takes advantage of the tendency for nonviolent campaigns to occur simultaneously within regions. The Colour Revolutions and Arab Spring are two prominent examples of spatiotemporal clustering of nonviolent conflict. To operationalize these regional diffusion dynamics as an instrument, I measure the proportion of neighbouring countries with an ongoing nonviolent campaign in the previous year. Following Gleditsch and Rivera (2017), I constructed a dataset of neighbouring countries using the Cshapes historical maps (Weidmann, Kuse, and Gleditsch 2010) and defined neighbours as states who are located within 500 km of each others' outer boundaries.[14]

I expect that as the proportion of neigbouring countries experiencing a nonviolent campaign at $t-1$ increases, the likelihood of a campaign to choose a nonviolent strategy increases. For this instrument to satisfy the exclusion restriction, and thus to be useful in the overidentification tests, it must only influence the outcome through

---

14. As the authors note, diffusion effects were not particularly sensitive to the specific distance threshold chosen, nor to the use of temporal lags (Gleditsch and Rivera 2017, 1141).

the endogenous variable. In this case, regional diffusion should have no effect on the likelihood of campaign success except through the choice of nonviolent versus violent tactics.

However, transnational diffusion is a multi-faceted process and some of the mechanisms by which it takes place may indirectly influence the likelihood of campaign success. I adapt the work of Elkins and Simmons (2005) to argue that diffusion occurs through two main channels: demonstration and learning.

The demonstration process is similar to an "availability heuristic," or the tendency for people to pay disproportionate attention to recent or dramatic events regardless of the logical relevance of those events to one's own situation (see Weyland 2005). Events that are uniquely "available," such as a nonviolent campaign in a neighbouring country, can cause individuals to modify their own behaviour, regardless of whether those available events are applicable or beneficial to themselves. In this sense, a neighbouring nonviolent campaign may encourage individuals to protest nonviolently in their own country, but it will not necessarily increase their likelihood of succeeding. I argue that this process represents a plausibly exogenous pathway for nonviolent diffusion to affect the choice of campaign type.

The second channel, learning, presents a more serious challenge to the exclusion restriction. If a nonviolent campaign emerges after observing a nonviolent campaign in a neighbour, there is a strong possibility that it gains information about tactics that can be emulated to increase the likelihood one's own campaign succeeds. To control for the possibility that diffusion as learning influences the outcome independently of the instrument, I add a binary control variable to the first and second stages of the instrumental variables regression that indicates whether or not a nonviolent campaign succeeded in a neighbouring country during the previous year. The goal with this control variable is to close off the possibility that diffusion dynamics increase

the likelihood of success not only by encouraging nonviolent movements to initially emerge, but also by demonstrating effective tactics that can improve their chances of succeeding.[15]

Finally, there are two more indirect causal pathways that must be addressed with the nonviolent diffusion instrument. First, foreign state support to a regime can often act as a bulwark against domestic challengers. Moreover, within regions, it is common for autocratic leaders to offer support to neighbouring regimes as a means to prevent the diffusion of resistance campaigns into their own countries. However, it may be the case that domestic challenges can disrupt an authoritarian regime's ability to provide support to its neighbours, since resources and attention must firstly be directed to the immediate threat at home. If this intuition is correct, the regional diffusion instrument may incidentally be capturing the loss of regime support that comes with an outbreak of nonviolent resistance within the region. To obviate this possibility, I add a control variable indicating whether or not the target regime for a given campaign possesses external state support in a campaign-year. This variable should cancel out any correlation between nonviolent conflict in neighbouring states and the availability of external support to a regime.

Second, regional levels of democracy are likely to be correlated with both the success of a movement and the number of ongoing nonviolent movements in the region. For example, the degree to which neighbouring countries are democratic

---

15. Even if we do not accept that diffusion by learning can be fully accounted for by this control variable, there are no guarantees about the efficacy of emulation. As Elkins and Simmons (2005, 43) note, "what is interesting about learning, from a diffusion perspective, are the numerous ways that it can go wrong." In many cases, the fact that the cause of diffusion is a desire to emulate effective tactics does not necessarily mean that emulation will lead to campaign success. Gleditsch and Rivera (2017, 1127) stress that "whether the information transmitted is "correct" with regard to regime strength, opposition mobilization capacity, or the effectiveness of specific tactics is less important than the impact on potential dissidents and mobilization." Simply put, we should not overstate the degree to which diffusion influences the likelihood campaign success beyond the strategic approach a movement chooses to adopt.

can make it more or less likely that a nonviolent movement will succeed because of both direct (material) and indirect (diplomatic) assistance to the resistance campaign (Brinks and Coppedge 2006; Gleditsch and Ward 2006). Thus the number of ongoing nonviolent campaigns within the region is likely to be correlated with the outcome as well as an omitted intervening variable of the level of democracy in the region. To address this possible violation of the exclusion restriction, I include a measure of the mean Polity score among neighbouring states at $t-1$. Some might further argue that neighbouring democratic governments are more likely to provide material support to a nonviolent, rather than violent, resistance movement and that these resources can improve its chances of success. To account for the availability of external resources from neighbouring states, I include a control variable for foreign state support to the campaign in all models.

—

Having laid out the logic of the nonviolent diffusion instrument, I now proceed to conduct overidentification tests.

I rely on two specifications to introduce the two additional instruments. In the first, I simply add the female education and nonviolent diffusion variables, plus their interactions with campaign duration, to the instrumental variable model presented in-text that only uses fertility rates as the instrument. In the second specification, I use the same approach, but also add in the three additional controls that are necessary to satisfy the exclusion restriction on the nonviolent diffusion instrument. These variables are foreign support to the regime, mean neighbour Polity, and an indicator of nonviolent campaign success in a neighbour.

The bottom panel of Table B-1 presents the first stage models for these two specifications. In all instances, the three instruments are statistically significant predictors

of nonviolent campaign type and its interaction with campaign duration. All coefficient estimates have the anticipated sign in the models predicting nonviolent campaign type alone. Most importantly, in nearly all models, the F statistic is large and statistically significant, indicating that together the instruments satisfy the strong instrument requirement. Only in Model 2.2 is the F statistic particularly small; yet in this case, the Cragg-Donald F statistic still exceeds the Stock-Yogo critical value for maximal IV bias at 10%, suggesting there is no evidence of a weak instrument problem (see upper panel of Table B-1). The Cragg-Donald value is also strong for all of the other first-stage models.

The upper panel of Table B-1 shows the second stage results. Model 1.0 includes all instruments but does not add the additional covariates required to satisfy the exclusion restriction on the diffusion instrument; Model 2.0 does include these covariates. Overall, it is encouraging to find that the coefficient estimates on the instrumented regressors are not substantially different from the model in the main text when the two additional instrumental variables are introduced.

The Hansen J statistic for these over-identified models helps to assess the exogeneity of the instruments. In both Models 1.0 and 2.0, the null hypothesis that the instruments are valid (i.e. uncorrelated with error term) cannot be rejected at the 5% confidence level. In Model 2.0, when the control variables needed to satisfy the exclusion restriction are added, the test statistic is even larger (with an associated smaller p-value), which suggests that these variables may have been successful in closing off alternative causal pathways by which nonviolent diffusion could be indirectly affecting the likelihood of campaign success. Ultimately, the failure to reject the null hypothesis in both of these models gives us confidence that the none of the three instruments are correlated with the error and that there are no omitted variable problems (conditional on the covariates).

Overall, the consistency of the results when introducing additional instruments, plus the evidence from the Hansen J tests on the over-identified models, is encouraging. The main instrument, fertility rates, appears to meet the exclusion restriction and passes standard tests for a strong instrument, even when included in models with additional instruments.

Table B-1: Results from overidentified 2SLS models

	Second stage: Dependent variable is campaign success	
	(1.0)	(2.0)
Nonviolent	0.415***	0.435***
	(0.094)	(0.101)
Nonviolent × Duration	−0.017***	−0.016***
	(0.005)	(0.005)
Duration	0.004*	0.005**
	(0.002)	(0.002)
Observations	709	629
Controls for diffusion added	No	Yes
Unreported covariates	Yes	Yes
Cragg-Donald F Statistic	14.620	11.751
Stock-Yogo 10% critical value	9.48	9.48
Anderson cannon. corr. LR	84.618***	68.906***
Hansen J p-value	0.117	0.171

	First stage: Dependent variable is endogenous regressors			
	Nonviolent	Nonviolent × Duration	Nonviolent	Nonviolent × Duration
	(1.1)	(1.2)	(2.1)	(2.2)
Fertility	−0.087***	0.059	−0.096***	0.244
	(0.014)	(0.172)	(0.016)	(0.221)
Fertility × Duration	0.001	−0.063***	0.001	−0.078***
	(0.001)	(0.018)	(0.001)	(0.022)
Female education	0.004***	0.057***	0.004**	0.079***
	(0.001)	(0.017)	(0.001)	(0.022)
Female education × Duration	−0.0002**	−0.004**	−0.0002*	−0.006***
	(0.0001)	(0.002)	(0.0001)	(0.002)
Neighbouring nonviolent	0.375**	−10.875***	0.459***	−10.810***
	(0.160)	(2.263)	(0.168)	(2.563)
Neighbouring nonviolent × Duration	0.011	1.335***	0.008	1.364***
	(0.010)	(0.275)	(0.010)	(0.306)
Duration	0.007	0.645***	0.003	0.873***
	(0.012)	(0.212)	(0.013)	(0.253)
Observations	709	709	629	629
Controls for diffusion included	No	No	Yes	Yes
Unreported covariates	Yes	Yes	Yes	Yes
Adjusted R^2	0.422	0.330	0.461	0.388
F Statistic on instruments	23.51***	10.41***	15.33***	5.94***

*p<0.1; **p<0.05; ***p<0.01. Standard errors clustered by country-year in parentheses. Unreported covariates include: Cold War, repression, foreign state support, campaign size, target Polity, population (log), GDP per capita (log), target military capacity and population density. Controls for the diffusion instrument include foreign support to the regime, mean neighbouring Polity, and an indicator of nonviolent campaign success in a neighbour and are only included in Model 3.

B.2 Testing original *WCRW* instruments

In Chenoweth and Stephan's (2011) original analysis, selection bias was also addressed through an instrumental variable design. Their setup included several instruments used to predict campaign type, including secessionist goals, logged percentage of a country's terrain that is mountainous, GDP per capita, target Polity, target military capacity, the number of ongoing civil conflicts, ethnic fractionalization, and dummy variables for whether there was an ongoing violent campaign and whether the target was engaged in an interstate war.

One of the contributions of my study is to test a new variable that can be used to instrument for nonviolent campaign type. That being said, it is still useful to implement the Chenoweth and Stephan instrumental variable design for two reasons. First, the original instrumental variable models were run on the aggregate NAVCO 1.0 data and it would be valuable to know whether the same instruments can be used when using the campaign-year NAVCO 2.0 data. Second, comparing my models based on fertility rates to models with instruments used by Chenoweth and Stephan can serve as a robustness check on my core findings.

To replicate the Chenoweth and Stephan analysis, I rely on a number of variables already described in the main text. In addition to these variables, I add mountainous terrain and ethnic fractionalization from Fearon and Laitin (2003) and a binary indicator of whether the target country was engaged in an international conflict based on data compiled by the Varieties of Democracy project (Coppedge et al. 2017).[16]

I run two specifications. The first treats the various instruments proposed by Chenoweth and Stephan as normal covariates included in the first and second stages,

---

16. The only instrument from the original Chenoweth and Stephan analysis that is not included here is the number of ongoing civil conflicts. The source of this variable is unclear. This variable was not a significant predictor of campaign type in the authors' original analysis, so its exclusion here should not pose an issue.

with fertility rates as the only assumed-exogenous instrument. The second approach drops fertility rates as an instrument and treats all of the Chenoweth and Stephan instruments as exogenous.

Table B-3 presents the first stage results for these two specifications. Models 1 and 2 show that including the Chenoweth and Stephan instruments as covariates alongside fertility rates does not drastically affect the ability of fertility to predict nonviolent campaign type. With the exception of military capacity and interstate war, all of the Chenoweth and Stephan instruments are statistically significant predictors of nonviolence. While the coefficient estimates for the fertilty rate instrument are slightly different from those presented in the main text, the key variables − fertility in Model 1 and fertility × duration in Model 2 − remain statistically significant and with the correct sign. Moreover, the Cragg-Donald F statistic for the model (see Table B-2) still exceeds the Stock-Yogo critical value that would indicate a weak instrument.

Table B-2: Results from 2SLS models with Chenoweth and Stephan instruments (Second stage estimates)

	Second stage: Dependent variable is campaign success	
	CS instruments as confounders	CS instruments as exogenous
	(1)	(2)
Nonviolent	0.500**	0.124**
	(0.240)	(0.048)
Nonviolent × Duration	−0.020	−0.002
	(0.014)	(0.002)
Duration	0.001	−0.003**
	(0.004)	(0.001)
Observations	922	979
Unreported covariates	Yes	Yes
Cragg-Donald F Statistic	11.157	66.369
Stock-Yogo 10% critical value	7.03	10.96
Anderson cannon . corr. LR	22.48***	731.91***
Hansen J p-value	−	0.008

*p<0.1; **p<0.05; ***p<0.01. Standard errors clustered by country-year in parentheses. Unreported covariates include: Cold War, repression, foreign state support, campaign size, population (log), and population density.

Table B-3: Results from 2SLS models with Chenoweth and Stephan instruments (First stage estimates)

	First stage: Dependent variable is endogenous regressor			
	CS instruments as confounders		CS instruments as exogenous	
	Nonviolent	Nonviolent × Duration	Nonviolent	Nonviolent × Duration
	(1)	(2)	(3)	(4)
Fertility	−0.043***	0.040		
	(0.008)	(0.148)		
Target Polity	−0.006***	−0.088***	−0.004***	0.064***
	(0.001)	(0.020)	(0.001)	(0.018)
GDP per capita (log)	−0.053***	−0.874***	0.054***	1.065***
	(0.009)	(0.203)	(0.012)	(0.186)
Target military capacity	−0.543	6.235	−0.988**	−25.443***
	(0.383)	(6.282)	(0.444)	(8.025)
Mountainous terrain (log)	−0.015***	−0.588***	0.013	0.521***
	(0.006)	(0.101)	(0.008)	(0.125)
Ethnic fractionalization	0.192***	4.288***	0.121***	−0.252
	(0.027)	(0.665)	(0.039)	(0.528)
Secession	−0.181***	−3.236***	−0.100***	1.114***
	(0.026)	(0.566)	(0.025)	(0.426)
Violent ongoing	−0.768***	−2.987***	−0.806***	0.081
	(0.031)	(0.736)	(0.036)	(0.561)
Interstate war	0.017	0.162	−0.102***	−1.117**
	(0.026)	(0.436)	(0.031)	(0.465)
Fertility × Duration	0.0001	−0.046***		
	(0.0004)	(0.014)		

Table continued on following page.

Table B-3: Results from 2SLS models with Chenoweth and Stephan instruments (First stage estimates) (Cont'd)

	CS instruments as confounders		CS instruments as exogenous	
	Nonviolent	Nonviolent $\times$ Duration	Nonviolent	Nonviolent $\times$ Duration
	(1)	(2)	(3)	(4)
Mountainous $\times$ Duration			-0.004^{***}	-0.136^{***}
			(0.001)	(0.025)
Ethnic fractionalization $\times$ Duration			-0.006^{*}	0.0004
			(0.004)	(0.089)
Secession $\times$ Duration			-0.006^{***}	-0.369^{***}
			(0.002)	(0.060)
Violent ongoing $\times$ Duration			0.002	-0.715^{***}
			(0.005)	(0.140)
Interstate war $\times$ Duration			0.012^{***}	0.176^{**}
			(0.003)	(0.082)
GDP per capita $\times$ Duration			-0.007^{***}	-0.205^{***}
			(0.001)	(0.033)
Military capacity $\times$ Duration			0.140^{***}	6.575^{***}
			(0.050)	(1.595)
Target Polity $\times$ Duration			-0.0003^{**}	-0.017^{***}
			(0.0001)	(0.003)
Duration	-0.0005	0.389^{***}	0.071^{***}	2.860^{***}
	(0.002)	(0.077)	(0.013)	(0.367)
Observations	922	922	979	979
Unreported covariates	Yes	Yes	Yes	Yes
Adjusted R^2	0.748	0.349	0.767	0.627
F Statistic on instrument(s)	19.23^{***}	8.85^{***}	180.45^{***}	24.87^{***}

$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$. Standard errors clustered by country-year. Unreported covariates include: Cold War, repression, foreign state support, campaign size, population (log) and population density.

Models 3 and 4 in Table B-3 exclude fertility as a predictor of campaign type in the first stage. In this specification, nearly all of the Chenoweth and Stephan instruments are statistically significant predictors of campaign type. In both models, the F statistic is substantially larger than when fertility is the only instrument interacted with campaign duration. Further diagnostics based on the Cragg-Donald F statistic and the Anderson cannon. cor. LR test are provided in Table B-2, all of which indicate no problems with weak instruments. Overall, the Chenoweth and Stephan variables clearly meet the strong instruments requirement and do not weaken the strength of fertility rates as an instrument.

Table B-2 summarizes the results from the second stage for these two sets of models; Figure B-1 translated these results into simulated probabilities of success for the two campaign types. As Model 1 shows, when Chenoweth and Stephan's instruments are treated as confounders to be included in both the first and second stages, the results are highly similar to the models presented in the main text. The predicted probabilities plot shows that the strategic advantage of nonviolence is only barely apparent during the first several years (left hand panel). The wider confidence intervals here are likely a product of the imprecision that comes with 2SLS estimation, but it is encouraging to find broadly similar results to the main model presented in text, particularly since many of the Chenoweth and Stephan instruments proved to be strong predictors of nonviolent campaign type in the first stage. The fact that the results in the second stage are overly not affected by the inclusion of these variables suggests that the original results using the fertility instrument were not simply a result of omitted variable bias.

Model 2, where the Chenoweth and Stephan instruments are only included in the first stage and fertility rates are dropped, shows slightly different results. The coefficient estimates are attentuated for both the nonviolent campaign type variable
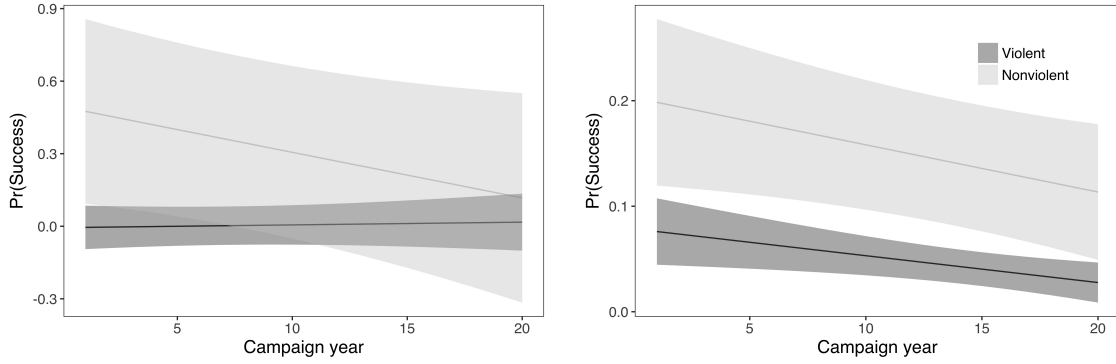
Figure B-1: Predicted probability of success when treating Chenoweth and Stephan instruments as confounders (left) and excluded instruments (right)

and its interaction with campaign duration. The results are similar in that they reveal a declining efficacy over time, but, as the predicted probabilities plot in Figure B-1 shows (right hand panel), there is separation in the likelihood of success between the two campaign types over their first 20 years in this model. Nonetheless, we should be cautious in interpreting this model. The Hansen J test statistic for this model shows that we must reject the null hypothesis that the instruments are valid (i.e. uncorrelated with error term). In this case, at least some of the Chenoweth and Stephan instruments appear not to be truly exogenous to success.[17] This raises some doubt about whether the original instruments from *Why Civil Resistance Works* can accurately identify the causal effect when those instruments are applied to the NAVCO 2.0 data.

Overall, the analysis in this section reveals two key points. First, the results using the fertility rate instrument in the main text are robust to the inclusion of Chenoweth and Stephan's instrumental variables as covariates in the first and second stages of the instrumental variable model. Second, when the Chenoweth and Stephan instruments are used in place of the fertility rate instrument, the results are less pronounced in

---

17. Note that this overidentification test is only available when the model is over-identified, or where there are more instruments than endogenous regressors. This is not the case in the model where Chenoweth and Stephan's instruments are treated as confounders.

terms of the time-dependence of the nonviolent campaign type. However, overidentification tests suggest that the exclusion restriction may not hold for at least some of the Chenoweth and Stephan instruments.

C   Mechanisms of nonviolent success

C.1   Explanation of campaign size threshold

To assess the effect of duration on campaign size, I recategorize an ordinal measure of the number of participants from the original NAVCO 2.0 data into a dichotomous indicator of whether a campaign surpassed 100,000 participants in a given year. This threshold was chosen for two reasons. First, it offers a reasonable approximation for the level of campaign participation required to observe an effect on the likelihood of nonviolent success. Second, 100,000 participants is a sufficiently attainable threshold that a large number of campaigns are able to realize. Given that the sample of nonviolent campaigns is relatively small to begin with, I want to select a threshold for which enough cases score '1' on the dependent variable so that there are no concerns about potential bias caused by a small number of influential observations driving my results.

First, in terms of the correlation between size and success, Table C-1 summarizes the results from three logistic regression models with campaign success as the dependent variable for a sample of only nonviolent campaigns. In Model 1, I predict success using the campaign size variable on its original ordinal scale, which shows that only a campaign size of greater than one million has a statistically significant effect on success. A campaign size less than one million, but still greater than 100,000, still has a relatively large effect but is only significant at a 90% confidence level. In Models 2

Table C-1: Results from logistic models estimating threshold for campaign size effect on success

	Success		
	(1)	(2)	(3)
Campaign size $10,000 - 99,999$	0.685		
	(0.653)		
Campaign size $100,000 - 999,999$	1.198*		
	(0.645)		
Campaign size >1 million	1.721**		
	(0.709)		
Campaign size >100,000		0.910**	
		(0.402)	
Campaign size >1 million			0.912*
			(0.524)
Observations	258	258	258
Log likelihood	−96.56	−97.68	−98.58

*p<0.1; **p<0.05; ***p<0.01. Standard errors clustered by country-year in parentheses. Unreported covariates include duration, security force defections, international sanctions, foreign state support, Cold War, repression and campaign goals.

and 3, I convert the ordinal variable to a simple dummy variable indicating whether a campaign realized a size above 100,000 or 1 million in a given year. Under these specifications, the 100,000 threshold produces a significant estimate at $\alpha = 0.05$, while the 1 million mark only barely fails to reach this level of significance. Interpreted together, the results from Table C-1 suggest that either 100,000 or 1 million would be a reasonable point at which to expect campaign participation to affect a nonviolent campaign's probability of success.

However, there is a need to strike a balance between the predictive power of a given threshold and the number of observations that meet that threshold. A campaign size of greater than a million, while a indeed a meaningful predictor of success, is fairly uncommon. In the NAVCO 2.0 data, only 13.7% of nonviolent campaign-years see participation larger than one million and only 10.1% of campaigns experience membership greater than one million at any point. Given that I am interested in

modelling how the broader universe of nonviolent campaigns are affected by duration, predicting an outcome that only 10% of campaigns ever realize means that I would not be able to reach very generalizeable findings.

By comparison, the number of campaigns that realize participation of 100,000 is much greater and there is more balance relative to the number of campaign-years that fail to reach the threshold. Indeed, 41.8% of nonviolent campaign-years realized the 100,000 participant threshold and 36.8% of nonviolent campaigns saw this campaign size at least once over their duration.

Given the results in Table C-1 and the distribution of observations, I opt for 100,000 participants as the crucial point that I use when modelling campaign success. While a dichtomous indicator for campaign size is a blunt instrument for assessing the relationship between duration and participation, the threshold of 100,000 participants offers a reasonable approximation that facilitates a simple estimation of campaign size as the outcome variable in the second stage of mechanisms models that are included in the main text.

C.2 Robustness check on time-dependence of mechanisms

In the main text, I demonstrated that the likelihood of a nonviolent campaign realizing either a campaign size greater than 100,000 or security defections decreases with campaign duration. In the logistic models presented in the main text, the control variables were repression, foreign state support, GDP per capita and population size. These variables were expected to influence the motivation for citizens to participate in the movement of security sector members to defect to it.

In Table C-3, I show that the main finding − that these two mechanisms become less likely over time − is robust to the inclusion of the following additional control

76

variables: Cold War, target Polity score and target military capacity. The results are less robust when the type of campaign goal is included in the model.

Models 1 and 5 present the specifications provided in-text for reference. Models 2 and 6 show that the estimate for campaign duration remains statistically significant when Cold War, target Polity score and target military capacity are included in the model. None of these control variables were significant predictors of campaign size or defections.

Models 3 and 7 introduce controls for campaign goal. Under this specification, both models' duration coefficients are no longer statistically significant and the estimates are reduced in size, particularly in the campaign size model. Yet the campaign goal covariates are not fully estimated; in both models, the dummy variable for the secession goal cannot be estimated because of perfect prediction.

Inspecting the tabulation of the two outcome variables against the various types of campaign goals (Table C-2) reveals that very few security force defections or campaign sizes greater than 100,000 occured among campaigns of secession or anti-occupation. This is especially true when looking at security defections, where less than 15% of defections occured in non-regime change campaigns. The small number of positive outcomes in these categories likely affected the estimation of Models 3 and 7.

To determine whether campaign goals have an independent effect on the likelihood of the two mechanisms, and whether their inclusion addresses an omitted variable problem with campaign duration, I run two final specifications. Model 4 includes only campaign duration and the indicators for each campaign goal. None of the campaign goals are statistically significant in this specification. The campaign duration coefficient is roughly half the size of the estimates in Models 1-3, but is again statistically significant at $\alpha = 0.05$. Model 8 runs a similar specification for security sector defections, but, given the small number of secession and anti-occupation ob-

servations, only includes a dummy for regime change, with all the other campaign goals serving as a reference category. Here, the regime change variable is statistically significant and has a substantial effect. The coefficient for the duration variable has a similar magnitude to Models 5 and 6, but again is only significant at $\alpha = 0.1$.

Overall, these results suggest that campaign size and security sector defections have a negative relationship with campaign duration, but that this relationship may be conditional on the objectives of the campaign. The small number of defections and large membership sizes among secessionist and anti-occupation campaigns make it difficult to properly determine the effect of duration when accounting for objectives. This represents an important caveat to the findings presented in the main text.

Table C-2: Distribution of mechanisms by campaign goal

	Regime change	Secession	Anti-Occupation	Other
Security defections = 0	73	18	71	61
Security defections = 1	52	3	2	4
Campaign size >100,000 = 0	53	9	66	37
Campaign size >100,000 = 1	76	13	7	22

Table C-3: Results from logistic models implementing robustness checks on mechanisms over time

	Campaign size >100,000				Security sector defections			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Duration	−0.163***	−0.196***	−0.047	−0.095***	−0.224**	−0.210*	−0.147	−0.195*
	(0.036)	(0.043)	(0.057)	(0.033)	(0.108)	(0.123)	(0.129)	(0.116)
Repression	−0.289*	−0.292*	−0.265		0.268	0.384*	0.158	
	(0.163)	(0.174)	(0.197)		(0.197)	(0.231)	(0.237)	
Foreign state support	1.580***	1.890***	1.823***		0.818*	0.653	0.470	
	(0.350)	(0.400)	(0.425)		(0.418)	(0.418)	(0.455)	
Population (log)	−0.010	−0.063	−0.040		−0.455***	−0.658***	−0.540***	
	(0.131)	(0.167)	(0.189)		(0.163)	(0.208)	(0.193)	
GDp per capita (log)	0.064	0.104	−0.031		−0.629**	−0.351	−0.474*	
	(0.221)	(0.249)	(0.261)		(0.251)	(0.265)	(0.283)	
Cold War		0.144	0.381			−1.118**	−0.830	
		(0.428)	(0.500)			(0.470)	(0.515)	
Target Polity		0.019	0.011			−0.002	−0.016	
		(0.035)	(0.036)			(0.044)	(0.049)	
Target military capacity		5.201	17.039*			13.060	18.979***	
		(7.466)	(9.998)			(9.567)	(7.344)	
Goal: Regime change			1.211***	0.878***			2.043***	2.008***
			(0.435)	(0.336)			(0.664)	(0.372)
Goal: Secession			N.A.	1.033**			N.A.	
				(0.494)				
Goal: Anti-occupation			−2.819**	−0.905			−0.322	
			(1.165)	(0.567)			(0.818)	
Observations	227	213	213	285	220	206	206	283

*p<0.1; **p<0.05; ***p<0.01. Standard errors clustered by country-year in parentheses.

79

C.3   Modelling first defections

In the main models in text, I show that security force defections both predict campaign success and are themselves negatively predicted by campaign duration. My analysis relies on the full sample on nonviolent campaign-years. But by modelling all campaign-years, including those that come after initial defections, there is a risk that my results will be biased because initial defections from the security sector tend to influence the subsequent likelihood through a bandwagon effect.

Following Lutscher (2016, 358-9), I address this possibility by dropping all campaign years that come after the first year of security sector defections. This results in the loss of 45 of the 283 campaign-year observations for which defection information is available. Tables C-4 and C-5 show that the findings are similar to what is presented in the main text when using this restricted sample.

In Table C-4, I show that security sector defections are a strong, positive predictor of campaign success, regardless of whether I include observations that occur after the first set of defections. The coefficient estimates on the main explanatory variable are similar in size, though the standard errors on the estimates are naturally larger in the reduced sample in Model 2.

In Table C-5, I repeat the same exercise for the models where security defections are the dependent variable. Model 1 shows the results from the main text and Model 2 displays the change in estimates when using the sample with post-defection observations dropped. In both models, campaign duration is negatively correlated with the likelihood of observing defections and the coefficient estimates on the campaign duration variable are similar in size. The one notable difference between the two models is that duration is no longer statistically significant at any conventional confidence level in the reduced sample. It is close to significance ($p = 0.12$) and the larger standard

errors in Model 2 are likely a combination of the smaller sample size. Ultimately, the effect of campaign duration on the likelihood of defections is notably less robust when only considering first instances of security force disloyalty.

Table C-4: Results from logistic models of campaign success with and without post-defection observations

	Campaign success	
	All defections	First defections only
	(1)	(2)
Security sector defections	1.468***	1.353***
	(0.442)	(0.513)
Observations	258	213
Log Likelihood	−97.683	−81.838

*p<0.1; **p<0.05; ***p<0.01. Standard errors clustered by country-year in parentheses. Unreported control variables: campaign duration, international sanctions, foreign state support, Cold War, repression, campaign goal.

Table C-5: Results from logistic models of security sector defection models with and without post-defection observations

	Security sector defections	
	All defections	First defections only
	(1)	(2)
Duration	−0.224**	−0.302
	(0.108)	(0.228)
Observations	220	175
Log Likelihood	−96.458	−72.687

*p<0.1; **p<0.05; ***p<0.01. Standard errors clustered by country-year in parentheses. Unreported control variables: population size (log), repression, foreign state support and GDP per capita (log).

References

Acemoglu, Daron, Simon Johnson, and James A Robinson. 2001. The colonial origins of comparative development: An empirical investigation. *The American Economic Review* 91 (5): 1369–1401.

Achen, Christopher H. 1986. *The Statistical Analysis of Quasi-experiments.* Berkeley/Los Angeles, CA: University of California Press.

Ackerman, Peter, and Jack DuVall. 2000. *A Force More Powerful: A Century of Nonviolent Conflict.* New York, NY: St. Martin's Press.

Ackerman, Peter, and Christopher Kruegler. 1994. Strategic Nonviolent Conflict, The Dynamics of People Power in the Twentieth Century. *Survival* 36 (2): 172–174.

Angrist, Joshua D, and Jörn-Steffen Pischke. 2008. *Mostly Harmless Econometrics: An Empiricist's Companion.* Princeton, NJ: Princeton University Press.

Anisin, Alexei. 2016. Violence begets violence: Why states should not lethally repress popular protest. *The International Journal of Human Rights* 20 (7): 893–913.

Arreguin-Toft, Ivan. 2001. How the weak win wars: A theory of asymmetric conflict. *International Security* 26 (1): 93–128.

Balch-Lindsay, Dylan, and Andrew J Enterline. 2000. Killing time: The world politics of civil war duration, 1820–1992. *International Studies Quarterly* 44 (4): 615–642.

Banks, Arthur. 2001. *Cross-National Time Series, 1815-1999.* Binghampton, NY: Center for Comparative Political Research, State University of New York.

Bayer, Markus, Felix S Bethke, and Daniel Lambach. 2016. The democratic dividend of nonviolent resistance. *Journal of Peace Research* 53 (6): 758–771.

BBC News. 2000. Peruvians protest against scandal-hit spy chief. *BBC News* (September).

Beck, Nathaniel, Jonathan N Katz, and Richard Tucker. 1998. Taking time seriously: Time-series-cross-section analysis with a binary dependent variable. *American Journal of Political Science* 42 (4): 1260–1288.

Berntzen, Einar, and Tor-Einar Holvik Skinlo. 2010. Peru and the Fujimori Presidential Breakdown in 2000: Continuismo Gone Bad. In *Presidential Breakdowns in Latin America,* edited by Mariana Llanos and Leiv Marsteintredet, 197–211. New York, NY: Palgrave Macmillan.

Berry, William D, Matt Golder, and Daniel Milton. 2012. Improving tests of theories positing interaction. *The Journal of Politics* 74 (3): 653–671.

Binnendijk, Anika Locke, and Ivan Marovic. 2006. Power and persuasion: Nonviolent strategies to influence state security forces in Serbia (2000) and Ukraine (2004). *Communist and Post-Communist Studies* 39 (3): 411–429.

Bjarnegård, Elin, Erik Melander, Gabrielle Bardall, Karen Brounéus, Erika Forsberg, Karin Johansson, Angela Muvumba Sellström, and Louise Olsson. 2015. Gender, Peace and armed conflict. *SIPRI Yearbook 2015. Oxford: Oxford University Press,* 101–109.

Bob, Clifford, and Sharon Erickson Nepstad. 2007. Kill a leader, murder a movement? Leadership and assassination in social movements. *American Behavioral Scientist* 50 (10): 1370–1394.

Bollen, Kenneth A. 2012. Instrumental variables in sociology and the social sciences. *Annual Review of Sociology* 38:37–72.

Boykoff, Jules. 2007. Limiting dissent: The mechanisms of state repression in the USA. *Social Movement Studies* 6 (3): 281–310.

Brett, Sebastian. 2000. *Perú: La Marcha de los Cuatro Suyos.* Technical report. Human Rights Watch, July.

Brinks, Daniel, and Michael Coppedge. 2006. Diffusion is no illusion: Neighbor emulation in the third wave of democracy. *Comparative Political Studies* 39 (4): 463–489.

Buendía, Felipe Cala. 2014. The Cultural Resistance of Colectivo Sociedad Civil in Peru: Performing Citizenship in the Time of Fear. In *Cultural Producers and Social Change in Latin America,* edited by Felipe Cala Buendía, 53–77. New York, NY: Palgrave Macmillan.

Buhaug, Halvard, Scott Gates, and Päivi Lujala. 2009. Geography, rebel capability, and the duration of civil conflict. *Journal of Conflict Resolution* 53 (4): 544–569.

Buijtenhuijs, Robert. 2001. The Chadian Tubu: Contemporary nomads who conquered a state. *Africa* 71 (1): 149–161.

Burt, Jo-Marie. 2006. "Quien habla es terrorista": The Political Use of Fear in Fujimori's Peru. *Latin American Research Review* 41, no. 3 (November): 32–62.

Cameron, Maxwell. 2006. Endogenous Regime Breakdown: The Vladivideo and the Fall of Peru's Fujimori. In *The Fujimori Legacy: The Rise of Electoral Authoritarianism in Peru,* edited by Julio F. Carrión, 268–293. University Park, PA: Penn State University Press.

Caprioli, Mary. 2000. Gendered conflict. *Journal of Peace Research* 37 (1): 51–68.

———. 2005. Primed for violence: The role of gender inequality in predicting internal conflict. *International Studies Quarterly* 49 (2): 161–178.

Carrión, Julio. 2006. *The Fujimori legacy: The rise of electoral authoritarianism in Peru.* University Park, PA: Penn State University Press.

Carter, David B, and Curtis S Signorino. 2010. Back to the future: Modeling time dependence in binary data. *Political Analysis* 18 (3): 271–292.

Celestino, Mauricio Rivera, and Kristian Skrede Gleditsch. 2013. Fresh carnations or all thorn, no rose? Nonviolent campaigns and transitions in autocracies. *Journal of Peace Research* 50 (3): 385–400.

Chenoweth, Erica, and Orion A. Lewis. 2013a. Nonviolent and violent campaigns and outcomes (NAVCO) data project, version 2.0, Campaign Year Data, Codebook. Josef Korbel School of International Studies, University of Denver.

———. 2013b. Unpacking nonviolent campaigns: Introducing the NAVCO 2.0 dataset. *Journal of Peace Research* 50 (3): 415–423.

Chenoweth, Erica, Evan Perkoski, and Sooyeon Kang. 2017. State Repression and Nonviolent Resistance. *Journal of Conflict Resolution* 61 (9): 1950–1969.

Chenoweth, Erica, Jonathan Pinckney, and Orion Lewis. 2018. Days of rage: Introducing the NAVCO 3.0 dataset. *Journal of Peace Research* 55 (4): 524–534.

Chenoweth, Erica, and Kurt Schock. 2015. Do contemporaneous armed challenges affect the outcomes of mass nonviolent campaigns? *Mobilization: An International Quarterly* 20 (4): 427–451.

Chenoweth, Erica, and Maria J Stephan. 2011. *Why Civil Resistance Works: The Strategic Logic of Nonviolent Conflict.* New York, NY: Columbia University Press.

Conaghan, Catherine M. 2001. *Making and unmaking authoritarian Peru: Re-election, resistance, and regime transition.* Coral Gables, FL: Dante B. Fascell North-South Center, University of Miami.

Coppedge, Michael, John Gerring, Staffan I. Lindberg, Svend-Erik Skaaning, Jan Teorell, David Altman, Frida Andersson, Michael Bernhard, M Steven Fish, and Adam Glynn. 2017. V-Dem Codebook V7. *Varieties of Democracy (V-Dem) Project.*

Crabtree, John. 2001. The collapse of Fujimorismo: Authoritarianism and its limits. *Bulletin of Latin American Research* 20 (3): 287–303.

Cunningham, David E, Kristian Skrede Gleditsch, and Idean Salehyan. 2009. It takes two: A dyadic analysis of civil war duration and outcome. *Journal of Conflict Resolution* 53 (4): 570–597.

Davenport, Christian. 2014. *How Social Movements Die: Repression and Demobilization of the Republic of New Africa.* New York, NY: Cambridge University Press.

DeNardo, James. 1985. *Power in Numbers: The Political Strategy of Protest and Rebellion.* Princeton, NJ: Princeton University Press.

Dragu, Tiberiu, and Yonatan Lupu. 2017. Collective action and constraints on repression at the endgame. *Comparative Political Studies* 51 (8): 1042–1073.

Elkins, Zachary, and Beth Simmons. 2005. On waves, clusters, and diffusion: A conceptual framework. *The Annals of the American Academy of Political and Social Science* 598 (1): 33–51.

Enloe, Cynthia H. 1980. *Ethnic Soldiers: State Security in Divided Societies.* Athens, GA: University of Georgia Press.

Fearon, James D. 2004. Why do some civil wars last so much longer than others? *Journal of Peace Research* 41 (3): 275–301.

Fearon, James D., and David D. Laitin. 2003. Ethnicity, insurgency, and civil war. *American Political Science Review* 97 (1): 75–90.

Feit, Edward. 1967. *African Opposition in South Africa: The Failure of Passive Resistance.* Stanford, CA: Hoover Institution on War, Revolution and Peace, Stanford University.

Finkel, Evgeny, and Yitzhak M Brudny. 2012. No more colour! Authoritarian regimes and colour revolutions in Eurasia. *Democratization* 19 (1): 1–14.

Gerring, John. 2007. Is there a (viable) crucial-case method? *Comparative Political Studies* 40 (3): 231–253.

Gleditsch, Kristian Skrede, Roman Olar, and Marius Radean. 2016. Going, going, gone: Varieties of dissent and leader exit. Typescript, Peace Research Institute Oslo.

Gleditsch, Kristian Skrede, and Mauricio Rivera. 2017. The diffusion of nonviolent campaigns. *Journal of Conflict Resolution* 61 (5): 1120–1145.

Gleditsch, Kristian Skrede, and Michael D Ward. 2006. Diffusion and the international context of democratization. *International organization* 60 (4): 911–933.

Gold, Alex, Nat Olin, and Annie Wang. 2018. *Modmarg: Calculating Marginal Effects and Levels with Errors.* R package version 0.9.2.

Goldstone, Jack A., Robert H. Bates, David L. Epstein, Ted Robert Gurr, Michael B. Lustik, Monty G. Marshall, Jay Ulfelder, and Mark Woodward. 2010. A global model for forecasting political instability. *American Journal of Political Science* 54 (1): 190–208.

Granovetter, Mark. 1978. Threshold models of collective behavior. *American Journal of Sociology* 83 (6): 1420–1443.

Haber, Stephen, and Victor Menaldo. 2011. Do natural resources fuel authoritarianism? A reappraisal of the resource curse. *American Political Science Review* 105 (1): 1–26.

Hainmueller, Jens, Jonathan Mummolo, and Yiqing Xu. 2018. How much should we trust estimates from multiplicative interaction models? Simple tools to improve empirical practice. *Political Analysis,* no. Forthcoming.

Hale, Henry E. 2005. Regime cycles: Democracy, autocracy, and revolution in post-Soviet Eurasia. *World Politics* 58 (1): 133–165.

Hendrix, Cullen S, and Idean Salehyan. 2016. A house divided: Threat perception, military factionalism, and repression in Africa. *Journal of Conflict Resolution* 61 (8): 1653–1681.

Hernández, Alicia. 2017. Cómo Venezuela pasó de 4 meses de choques y protestas diarias a una aparente calma [How Venezuela went from 4 months of daily clashes and protests to an apparent calm]. *BBC News Mundo* (October).

Hochstetler, Kathryn. 2006. Rethinking presidentialism: Challenges and presidential falls in South America. *Comparative Politics* 38 (4): 401–418.

Kalyvas, Stathis N. 2006. *The Logic of Violence in Civil War.* New York, NY: Cambridge University Press.

Kern, Holger, and Jens Hainmueller. 2009. Opium for the masses: How foreign free media can stabilize authoritarian regimes. *Political Analysis* 17 (4): 377–399.

King, Gary, Michael Tomz, and Jason Wittenberg. 2000. Making the Most of Statistical Analyses: Improving Interpretation and Presentation. *American Journal of Political Science* 44 (2): 347–361.

Klandermans, Bert. 1984. Mobilization and participation: Social-psychological expansions of resource mobilization theory. *American Sociological Review* 49:583–600.

———. 1997. *The Social Psychology of Protest.* Oxford, UK; Cambridge, MA: Blackwell Publishers.

Klosko, George, Edward N Muller, and Karl Dieter Opp. 1987. Rebellious collective action revisited. *American Political Science Review* 81 (2): 557–564.

Kou, Chien-Wen. 2000. Why the military obeys the party's orders to repress popular uprisings: The Chinese military crackdown of 1989. *Issues and Studies* 36 (6): 27–51.

Kuper, Leo. 1957. *Passive Resistance in South Africa.* New Haven, CT: Yale University Press.

Kuran, Timur. 1989. Sparks and prairie fires: A theory of unanticipated political revolution. *Public choice* 61 (1): 41–74.

Kurzman, Charles. 1996. Structural opportunity and perceived opportunity in social-movement theory: The Iranian revolution of 1979. *American Sociological Review* 61 (1): 153–170.

Lee, Terence. 2005. Military cohesion and regime maintenance: Explaining the role of the military in 1989 China and 1998 Indonesia. *Armed Forces & Society* 32 (1): 80–104.

———. 2009. The armed forces and transitions from authoritarian rule: Explaining the role of the military in 1986 Philippines and 1998 Indonesia. *Comparative Political Studies* 42 (5): 640–669.

———. 2014. *Defect or Defend: Military Responses to Popular Protests in Authoritarian Asia.* Baltimore, MD: Johns Hopkins University Press.

Leeper, Thomas J. 2017. *Margins: Marginal Effects for Model Objects.* R package version 0.3.0.

Lehoucq, Fabrice. 2016. Does nonviolence work? *Comparative Politics* 48 (2): 269–287.

Levitsky, Steven, and Maxwell A Cameron. 2003. Democracy without parties? Political parties and regime change in Fujimori's Peru. *Latin American Politics and Society* 45 (3): 1–33.

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

Lodge, Tom. 2009. The Interplay of Non-violent and Violent Action in the Movement against Apartheid in South Africa, 1983-94. In *Civil Resistance and Power Politics: The Experience of Non-Violent Action from Gandhi to the Present,* edited by Adam Roberts and Timothy Garton Ash, 213–230. New York, NY: Oxford University Press.

Lutscher, Philipp M. 2016. The more fragmented the better? The impact of armed forces structure on defection during nonviolent popular uprisings. *International Interactions* 42 (2): 350–375.

Mack, Andrew. 1975. Why big nations lose small wars: The politics of asymmetric conflict. *World Politics* 27 (2): 175–200.

Mandela, Nelson. 1994. *Long Walk to Freedom*. Randburg, South Africa: Macdonald Purnell.

Marshall, Monty, Keith Jaggers, and Ted Robert Gurr. 2014. Polity IV project: Political regime characteristics and transitions, 1800–2013. Center for Systemic Peace.

McDermott, Jeremy. 2000. Peruvian president resigns from 'exile' in a Japanese hotel. *The Telegraph* (November).

McLauchlin, Theodore. 2010. Loyalty strategies and military defection in rebellion. *Comparative Politics* 42 (3): 333–350.

Melander, Erik. 2005. Gender equality and intrastate armed conflict. *International Studies Quarterly* 49 (4): 695–714.

Mood, Carina. 2010. Logistic regression: Why we cannot do what we think we can do, and what we can do about it. *European Sociological Review* 26 (1): 67–82.

Moore, Will H. 2000. The repression of dissent: A substitution model of government coercion. *Journal of Conflict Resolution* 44 (1): 107–127.

Muñoz, Aurora. 2009. *Peruvians campaign to overthrow Dictator Alberto Fujimori (The March of the Four Directions), 2000*. https://nvdatabase.swarthmore.edu/, September.

Nepstad, Sharon Erickson. 2011a. Nonviolent resistance in the Arab Spring: The critical role of military-opposition alliances. *Swiss Political Science Review* 17 (4): 485–491.

———. 2011b. *Nonviolent Revolutions: Civil Resistance in the Late 20th Century*. New York, NY: Oxford University Press.

Nepstad, Sharon Erickson. 2013. Mutiny and nonviolence in the Arab Spring: Exploring military defections and loyalty in Egypt, Bahrain, and Syria. *Journal of Peace Research* 50 (3): 337–349.

Oberschall, Anthony. 1980. Loosely structured collective conflict: A theory and an application. *Research in Social Movements, Conflicts, and Change* 3:45–68.

Pérez-Liñan, Aníbal S. 2007. *Presidential impeachment and the new political instability in Latin America.* New York, NY: Cambridge University Press.

Pion-Berlin, David, Diego Esparza, and Kevin Grisham. 2014. Staying quartered: Civilian uprisings and military disobedience in the twenty-first century. *Comparative Political Studies* 47 (2): 230–259.

Regan, Patrick M. 2002. Third-party interventions and the duration of intrastate conflicts. *Journal of Conflict Resolution* 46 (1): 55–73.

Roberts, Kenneth. 2006. Do Parties Matter? Lessons from the Fujimori Experience. In *The Fujimori Legacy: The Rise of Electoral Authoritarianism in Peru,* edited by Julio Carrión, 81–101. University Park, PA: Pennsylvania State University Press.

Ross, Michael L. 2004. What do we know about natural resources and civil war? *Journal of Peace Research* 41 (3): 337–356.

Salehyan, Idean, Cullen S Hendrix, Jesse Hamner, Christina Case, Christopher Linebarger, Emily Stull, and Jennifer Williams. 2012. Social conflict in Africa: A new database. *International Interactions* 38 (4): 503–511.

Sarkees, Meredith Reid, and Phil Schafer. 2000. The Correlates of War data on war: An update to 1997. *Conflict Management and Peace Science* 18 (1): 123–144.

Schaftenaar, Susanne. 2017. How (wo)men rebel: Exploring the effect of gender equality on nonviolent and armed conflict onset. *Journal of Peace Research* 54 (6): 762–776.

Schock, Kurt. 2005. *Unarmed Insurrections: People Power Movements Nondemocracies.* Minneapolis, MN: University of Minnesota Press.

———. 2013. The practice and study of civil resistance. *Journal of Peace Research* 50 (3): 277–290.

Schock, Kurt. 2015. *Civil Resistance Today.* Cambridge, MA: Polity Press.

Seidman, Gay W. 2000. Blurred Lines: Nonviolence in South Africa. *Political Science and Politics* 33 (2): 161–167.

Sharp, Gene. 1973. *The Politics of Nonviolent Action.* Vol. 3. Boston, MA: Porter Sargent.

Staiger, Douglas, and James H Stock. 1997. Instrumental variables regression with weak instruments. *Econometrica* 65 (3): 557–586.

Stange, Gunnar, and Roman Patock. 2010. From rebels to rulers and legislators: The political transformation of the Free Aceh Movement (GAM) in Indonesia. *Journal of Current Southeast Asian Affairs* 29 (1): 95–120.

Stock, James H, and Motohiro Yogo. 2005. Testing for weak instruments in linear IV regression. In *Identification and Inference for Econometric Models: Essays in Honor of Thomas Rothenberg,* edited by Donald W K Andrews and James H Stock, 80–108. New York, NY: Cambridge University Press.

Sutton, Jonathan, Charles R Butcher, and Isak Svensson. 2014. Explaining political jiu-jitsu: Institution-building and the outcomes of regime violence against unarmed protests. *Journal of Peace Research* 51 (5): 559–573.

Taylor, Lewis. 2005. From Fujimori to Toledo: The 2001 elections and the vicissitudes of democratic government in Peru. *Government and Opposition* 40 (4): 565–596.

Taylor, Verta. 1989. Social movement continuity: The women's movement in abeyance. *American Sociological Review* 54 (5): 761–775.

Thurber, Ches. 2015. *Social structures of revolution: Networks, overlap, and the choice of violent versus nonviolent strategies in conflict.* Working Paper.

Tilly, Charles. 1978. *From Mobilization to Revolution.* Reading, MA: Addison-Wesley Publishing Company.

Tilly, Charles, and Sidney G Tarrow. 2007. *Contentious Politics.* Boulder, CO: Paradigm Publishers.

Van Stekelenburg, Jacquelien, and Bert Klandermans. 2013. The social psychology of protest. *Current Sociology* 61 (5-6): 886–905.

Weidmann, Nils B, Doreen Kuse, and Kristian Skrede Gleditsch. 2010. The geography of the international system: The CShapes dataset. *International Interactions* 36 (1): 86–106.

Weinstein, Jeremy M. 2007. *Inside Rebellion: The Politics of Insurgent Violence.* New York, NY: Cambridge University Press.

Weyland, Kurt. 2005. Theories of policy diffusion lessons from Latin American pension reform. *World Politics* 57 (2): 262–295.

———. 2006. The Rise and Decline of Fujimori's Neopopulist Leadership. In *The Fujimori Legacy: The Rise of Electoral Authoritarianism in Peru,* edited by Julio Carrión, 13–38. University Park, PA: Pennsylvania State University Press.

Wooldridge, Jeffrey M. 2002. *Econometric Analysis of Cross-section and Panel Data.* Cambridge, MA: MIT Press.

World Bank. 2013. *World Bank Development Indicators.* http://data.worldbank.org/data-catalog/world-development-indicators.

Zunes, Stephen. 1999. The role of non-violent action in the downfall of apartheid. *The Journal of Modern African Studies* 37 (1): 137–169.