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Three Essays on Non-State Actors and Repression in Dictatorships

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By Pearce Edwards

How do authoritarian regimes address challenges to their rule which emerge from the civilian population, and how do civilians resist or contribute to regimes' efforts to address these challenges, particularly repression? In literature on repression and resistance in dictatorships, civilians typically enter as victims of state violence or participants in mass protest events which seek to challenge the regime. The papers in this dissertation each complicate this perspective on civilians' role in authoritarian repression. The first paper focuses on leaders of religious institutions who are embedded among and carry influence with civilians, arguing that when these leaders oppose repression, they are able to reduce its frequency in areas under their authority. Evidence from Catholic bishops in Argentina during its 1976-1983 military dictatorship supports the argument. The second paper examines how civilians may facilitate authoritarian repression, motivated by partisan rivalries to seek harm on their political opponents in areas of close electoral competition. Examination of repression in the period following Chile's 1973 military coup establishes a link between areas with close competition under democratic elections and repression in the early months following the coup. The third paper analyzes the effects of repression and of dissidents who seek to use revolutionary violence against dictatorships on the decision of dissidents to participate in nonviolent protests. Using a game-theoretic model, the paper demonstrates that these two shadows of violence may either increase dissidents' willingness to protest or decrease it. The strategic anticipation of future violence accounts for how violence can increase protest. Each of the three papers offers a novel contribution to literature on repression and resistance. The first and third paper study a particular type of civilian—religious leaders and violent revolutionaries, respectively—which receive comparatively little attention in research on repression and resistance in dictatorships. The second paper suggests a particular kind of civilian behavior—rivalry-driven support for authoritarian repression—which receives less focus than opposition to state violence.

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Chapter 1

Introduction

1.1 Non-State Actors and Repression in Dictatorships

Dictatorships face two principal problems in securing their rule: power-sharing, in which a ruling coalition of elites must be assembled to support the incumbent regime, and control, in which resistance to the regime by citizens must be contained to prevent a revolution (Svolik 2012). Regimes control citizen resistance—mass threat—with strategies such as empowering security forces to put down collective action with repression and co-opting potential opposition with institutions such as mass-based political parties (Gandhi 2008). Because dictatorships retain power without the mandate of democratic legislative or executive selection, it is logical that the "median voter" in these regimes has preferences which diverge from the regime (Cheibub, Gandhi and Vreeland 2010, Gandhi and Przeworski 2006). The regime sets policies which a majority of citizens oppose and would vote against if given the opportunity in a free and fair election. Focusing on the difference in the ideal points between a regime and its citizens is important for making predictions about how these regimes respond on the aggregate to political opposition. Yet there are other attributes of the distribution of

citizen preferences besides the median: citizens in each tail of the distribution. Some citizens need no inducements such as repression or co-optation to support the regime, while for others no inducement could overcome their opposition.

In this dissertation, I study the relationship between regime efforts to control mass threat and citizens in the tails of this distribution of preferences. The three articles in the dissertation illustrate the effects of these citizens' attributes on the strategy and outcome of regime repression, and make an original contribution to literature on authoritarian politics. Particularly, the projects disaggregate the preferences of citizens, and non-state actors more generally, in authoritarian regimes. The research distinguishes between citizens who are unswerving ideological loyalists to the regime, citizens who would not resist the regime under the right inducements, and citizens who would resist the regime regardless of attempts to induce them otherwise. This distribution of types of citizens affects a regime's ability to address the problem of authoritarian control. Loyalists voluntarily carry out the regime's work, absorbing some costs of implementation. Radical revolutionaries draw in some moderate citizens to their cause, rendering regime control less effective. Regimes may also unintentionally empower radical revolutionaries when controlling a moderate opposition.

There is considerable evidence of loyalist citizens in dictatorships. Przeworski (1986) describes a principle motivating authoritarian regimes as "control over outcomes" and "reduction of uncertainty." There are several social groups which share these same principles, and may privilege them at the expense of democracy. Przeworski identifies bureaucrats and government functionaries as one such group. Members of the economic elite who rely on returns to capital, especially those with ties to foreign investment, may also have such priorities to reduce uncertainty (Kaufman 1986). Finally, citizens and civic leaders who value tradition, social order, and hierarchy may be sympathetic to such principles of a right-wing dictatorship (Kocher, Lawrence and Monteiro 2018). In the specific context of repression, these non-state

actors may act consistently with the aims of the regime, helping eliminate threats to regime control within their midst. Civilian informers (Bergemann 2017, Gellately 1996), owners of capital (Klor, Saiegh and Satyanath 2020), and civic leaders (Kopstein and Wittenberg 2011, Ruderer 2015) can all aid regime repression.

Radical revolutionaries, on the other hand, complicate a regime's ability to repress dissent. Bueno de Mesquita (2010) describes a revolutionary vanguard which attempts to incite mass protest: "the tool that the vanguard has at its disposal is insurgent violence, such as guerrilla or terrorist attacks" (446). Revolutionary violence is qualitatively different from the conception of protests or riots typically considered in the empirical study of mass threats to authoritarian rule (Woo and Conrad 2019). In particular, some citizens are more capable of carrying out revolutionary violence, with military training, access to weapons, or demographic characteristics which facilitate their use of insurgent violence. These revolutionaries attract other citizens to join their violent uprisings, because citizens believe revolutionaries make violence more likely to succeed. Revolutionaries may also repel citizens away from violence, encouraging them to peacefully protest in order to prevent a future violent uprising. Greater repression of protest could drive citizens to join revolutionaries' insurgent violence, while greater repression of violence could drive citizens to decide to protest. Considering revolutionary violence demonstrates that a regime faces a tradeoff when repressing mass threat beyond the risk of delegating too much power to security forces (Tyson 2018).

The dissertation considers in turn each tail in this distribution of preferences. The first paper considers the role of religious institutions in helping or hindering regime repression, with an empirical application in the "Dirty War" under the 1976-1983 Argentine military dictatorship. The second paper builds on a logic of civilian complicity with dictatorships, showing how political competition shaped the frequency and targeting of repression in the months following Chile's 1973 coup which brought to power

a military dictatorship. The third paper builds on a model of protest coordination in the shadow of revolutionary violence, considering the effects of regime repression and the threat of revolutionary violence. The model makes predictions about when repression increases or decreases protest, as well as whether revolutionary violence is a substitute or complement to protest.

1.2 Overview of the Dissertation

1.2.1 Paper 1: Religious Institutions and Resistance to Repression

Nondemocratic regimes require the support of at least some portion of the societies they govern. Yet these regimes lack mechanisms for popular selection of their leaders, such as competitive elections (Cheibub, Gandhi and Vreeland 2010). Instead, nondemocratic regimes build support and reduce opposition using fiscal, institutional, and military power (Gandhi 2008). A common assumption in theories of nondemocratic regime survival is that regimes face a society opposed to the regime and that under the right conditions citizens resist the regime. The assumption of an opposition which contests regime authority leaves out an important intermediary between regime and society: social institutions. These institutions—primarily devised for structuring social interactions, such as civic groups, churches, and ethnic organizations—contain regime supporters or opponents which affect the regime's political strategy independently of the distribution of supporters and opponents among ordinary citizens.

The regime has many tools to address threats "from below." Repression, raising the cost of citizen collective action, is a common approach (Tilly 1978). Regimes may preventively repress by making credible threats against citizens who consider collective action against the regime (Ritter and Conrad 2016) or repress in reaction to collective action in what Davenport (2007a) calls the "Law of Coercive Respon-

siveness." Regimes repress not just to deter collective action but to undermine the organizations responsible for mobilizing citizens (Sullivan 2016a). As a less coercive measure, regimes address threats from citizens by manipulating or controlling the flow of information (King, Pan and Roberts 2017, Lorentzen 2014), propagandizing (Wedeen 1998), or by promising economic growth (Desai, Olofsgård and Yousef 2009) or services such as education (Manzano 2017).

This paper takes a different vantage point by considering how religious institutions affect a regime's preferences for and ability to carry out repression. Religious institutions can shape the regime's repression because they are both *locally embedded* and have *public influence*. In areas in which religious institutions oppose state violence, the regime's repressive operations are obstructed. Using local embeddedness to gain information about repressive operations, sympathetic leaders then use their public influence to pressure the regime and turn public attitudes against it. This paper's argument predicts the regime will reduce repression in areas in which leaders of religious institutions are opposed to state violence, given that repression and co-optation of these leaders may be counterproductive.

To test the hypotheses derived from the argument, I use original archival data from the Argentine military dictatorship of 1976-1983. The military regime killed or "disappeared" tens of thousands of Argentines in a campaign against leftism and Communism in the name of national security (Pion-Berlin 1988). Of particular importance to the regime was the Argentine Catholic Church, the bishops of which ranged from public supporters to public opponents of the regime. Public opponents were particularly involved in human rights advocacy. Using among other empirical strategies the plausible exogeneity of the timing of certain appointments to the episcopacy, I test the relationship between opposed bishops and the frequency of repression. Tests of the local embeddedness and public influence mechanisms reveal that opposed bishops were more effective when they oversaw more likeminded local agents—priests—and

also witnessed a greater retention of Catholic adherents in their jurisdictions during the dictatorship.

This paper contributes to the literature in three ways. First, it adds to research on how regimes manage mass threats by considering the role of religious institutions in repression. Employer associations and organized labor are other social institutions which have previously been considered in this regard (Kim and Gandhi 2010, Klor, Saiegh and Satyanath 2020). Second, it adds to work on the role of the institutional church in the modern state (Gill 1998, Kalyvas 1998, Spenkuch and Tillmann 2018, Woodberry 2012) by returning to the prior attention in this literature to authoritarian repression (Mainwaring and Wilde 1989). Third, the paper sheds light on the processes by which right-wing Latin American dictatorships repressed leftist activists and militants with the aid or resistance of domestic actors and institutions (Bautista et al. 2018, Mallimaci 2012, Obregón 2005, Pereira 2008, Scharpf 2018).

1.2.2 Paper 2: Political Competition and Authoritarian Repression

Repression in authoritarian regimes comes in a number of forms—targeted arrests, killings, and censorship—that require information. Authoritarian governments need to know the identity of political activists and the location of opposition supporters to be able to repress dissent. The regime must be able to neutralize potential mass threats in order to survive. The problem, however, is that while it must maximize the probability it represses enough dissidents to survive, the political loyalty of individuals are not immediately observable and are therefore imperfectly known. To identify dissidents, the regime requires a supply of accurate information.

Institutions such as elections and regime parties can uncover this information (Brownlee 2009, Reuter 2017), yet not all authoritarian regimes have elections and parties that have the capacity to elicit truthful information. More commonly, security

forces are responsible for gathering information on citizens by infiltrating organizations, entrapping civilians, and monitoring communications (Blaydes 2018). Yet in many regimes, the security services may have low capacity to collect information due to fragmentation (Greitens 2016, Stepan 1988). Fragmented security forces may be the result of the regime's coup-proofing strategies (Tyson 2018), but fragmentation is also common soon after the regime comes to power (Policzer 2009). Nascent regimes rarely have unified, efficient security institutions that can address both latent and mobilized mass threats. How then can authoritarian regimes repress dissent when they lack institutions capable of information-gathering?

This paper describes the conditions under which civilian informants offer a solution to the regime's information problem (Bergemann 2017, Fitzpatrick 2005, Gellately 1996). Informants embedded in the private sphere—neighborhoods, schools, and workplaces—learn about the sincere political loyalty of those around them through conversation networks (Frye, Reuter and Szakonyi 2019). The challenge for the regime is how to recruit civilian informants. Material incentives can attract informants, but the incentives often lead to false reporting. As a result, regimes may take advantage of citizens' ideological motivations to offer information on the activities and sympathies of civilians in their area. In particular, areas of close political competition generate heightened partisan rivalries and motivate politically-engaged civilians to seek harm upon their opponents and contribute to repression (Balcells 2010). Yet these civilians tend to inform on any political opponent, rather than the active, resistance-mobilizing dissidents which regimes are most inclined to repress.

To study how partisan rivalries and civilian information provision shape repression, this paper examines the relationship between areas of close political competition and the frequency and targeting of repression in the months following the 1973 military coup in Chile. The country's nascent military regime struggled with coordinating its operations and gathering information on dissidents, and historical evidence shows

a pattern of civilians providing information to the military and assisting in repressive operations. Over the three months following the 1973 coup, the regime killed more than 1,000 civilians, many of whom were not politically-active dissidents. The results suggest that more civilians—including ones not politically active—were killed in Chilean areas with close political competition in the democratic elections immediately preceding the coup than in other areas.

This paper makes two main contributions to literature on repression and resistance in authoritarian regimes. First, scholars often emphasize regime-based explanations for variation in repression, expecting state violence to vary with regime characteristics (Bautista et al. 2018, Thomson 2017), and the targeting of repression to correspond to the efficiency or compliance of regime agents (Gregory 2009, Greitens 2016). By considering political rivalry, I advance a population-based explanation how the use of coercion varies even when regime attributes are held constant (Klor, Saiegh and Satyanath 2020, Kopstein and Wittenberg 2011). Second, civilians tend to receive attention only to the extent that they participate in anti-regime collective action, though recent work has turned its focus to the role of ordinary civilian supporters (Esberg 2018) and regime allies among the civilian economic elite (Klor, Saiegh and Satyanath 2020). By considering how regime supporters encourage repression, I highlight a distinct and understudied aspect of civilian agency in dictatorships (Bergemann 2017, Fitzpatrick 2005, Gellatelly 1996).

1.2.3 Paper 3: Protest in the Shadow of Repression and Extremist Violence

When does repression of protest by authoritarian regimes lead to backlash protests and when does it deter protest? What effect, if any, does violent anti-regime resistance have on protests? Consider dissent in authoritarian regimes during the Arab Spring uprisings. In Mubarak's Egypt and Ben Ali's Tunisia, repression failed to deter

protest and the regimes fell. In Syria, the Assad regime's use of lethal force against protesters led to the collapse of nonviolent resistance and initiation of violent civil conflict. Dissidents abandoned protests and militant groups became the primary regime opposition. In Bahrain, harsh repression deterred protests and the Al Khalifa regime survived.

Mass nonviolent protests are considered an effective method to overthrow authoritarian regimes from below. In particular, there is evidence that when a regime represses peaceful protest, the repression leads to *backlash* protests. Stephan and Chenoweth (2008) argue backlash "leads to power shifts by increasing the internal solidarity of the resistance campaign, creating...conflicts among the [regime]'s supporters, increasing external support for the resistance campaign, and decreasing external support for the [regime]" (11). Protests are also effective because they cross boundaries of formal organizations and develop through networks (Steinert-Threlkeld 2017, Thomson 2018), challenging the regime's ability to contain them. Pearlman (2018) explains how protests affect onlookers' moral identity and increase their desire to express distaste for a regime through protest, while Aytaç, Schiumerini and Stokes (2018) describe emotional reactions by bystanders who witness regime repression, leading to protest cascades.

This paper considers two shadows of violence which affect dissidents' protest decision-making: repression and the presence of violent revolutionary movements. Repression is state coercion which increases the cost of collective action (Tilly 1978), and decreases dissidents' beliefs that other dissidents will protest (Young 2019). Repression of observed peaceful protests makes dissidents self-censor: meaning nonviolent dissent we do observe has *selected into* protest despite repression and dissidents are more likely to believe they will succeed (Ritter and Conrad 2016).[1] This means observing peaceful protest is contingent on the beliefs of dissidents that the regime

[1]For analysis of repression across political regimes, see Davenport (2007a).

can be overthrown. In addition, repression can induce protesters to instead use violent tactics as they attempt to minimize the effects of state violence (Lichbach 1987, Moore 1998).

The effects of these shadows of violence are studied in a model of coordination and regime change in which dissidents choose whether to nonviolently protest when faced with regime repression and the option to support a violent revolutionary movement. Violent revolutionary movements emerge after failed protests in which a proportion of protesters have been arrested or detained. Analysis of the model reveals non-monotonic effects of repression and dissidents' capabilities for violence on the size and success of nonviolent protests. Repression can either generate a *backlash effect*—leading to increased protest when dissidents' support for violence is low—or a *deterrence effect* in which state violence decreases protest. Dissidents' capabilities for carrying out revolutionary violence may also increase protest in an *incitement effect* or decrease it in a *spoiler effect*. The model elucidates a mechanism of the strategic anticipation of violence: dissidents make protest decisions when considering the costs they would face for experiencing revolutionary violence in the future and may change their behavior to avoid such an outcome.

This paper builds on arguments in similar settings which focus on dissidents choosing whether to protest, and the regime changing if a sufficient proportion protest. dissidents receive signals about the strength of the regime which inform their beliefs about the likelihood of regime change. The information can come from other dissidents' actions (Lohmann 1993), from communication technology (Little 2015), from revolutionary vanguards (Bueno de Mesquita 2010), or from regime agents (Dragu and Lupu 2018). Unlike arguments which depict only protest or include a limited role for a violent revolutionary organization, this paper explicitly incorporates support for violence as an option which follows a failed anti-regime protest. Introducing support for violence reveals effects described in empirical literature on repression

and dissent, providing analytical leverage which models studying only the decision to protest lack. The logic of the model also more closely fits the empirical setting of anti-regime protests in dictatorships in which peaceful protest and violent revolutionary movements exist simultaneously. It also builds a theoretical link between nonviolent protest movements and the onset of civil conflict.

Chapter 2

Religious Institutions and Resistance to Repression: The Bishops Opposed to Argentina's Dirty War

2.1 Introduction

Civil society organizations such as religious institutions shape politics, even under dictatorships which restrict or co-opt their activity. While scholars have shown religious institutions' ability to sustain political identity (Wittenberg 2006) and attitudes (Nalepa and Pop-Eleches 2021), motivate participation (McClendon and Riedl 2019), nurture activism (Amat 2019, Hale 2015), encourage public goods provision (Tsai 2007), and stimulate regime change (Woodberry 2012), little recent quantitative political science scholarship—with notable exceptions such as Braun (2016)—examines how religious institutions affect a key aspect of politics in dictatorships: repression. Yet these institutions and their leaders are among the "communal elite" on whom

regimes depend to justify their use of coercive power (Slater 2010). If religious leaders oppose the use of coercive power, they may be able to undercut it.

This paper argues that religious leaders' opposition to repression decreases its use in dictatorships. Opposed religious leaders draw on their institutions' regular interaction with adherents in a given area (Carter and Hassan 2021, McClendon and Riedl 2019) to gather information about repression with the assistance of local agents. This local embeddedness allows leaders to encourage subordinate agents who assist targets of repression. Opposed leaders also draw on their institutions' traditional power based on knowledge and sacred order handed down from the past (Condra, Isaqzadeh and Linardi 2019, Loveman 2005, Nelson 1993, Pattin 2019). Such public influence gives weight to leaders' opposition to state violence among religious adherents, thereby pressuring the regime. The argument implies repression is reduced in the areas under the jurisdiction of opposed leaders.

I examine the argument and its mechanisms in the Argentine military dictatorship of 1976-1983, in which the right-wing regime disappeared and killed thousands in a campaign known as the Dirty War. I use original archival data on the country's Catholic Church to find those few among its leaders—bishops—who opposed repression in a religious institution otherwise aligned with the military (Gill 1998, Morello 2015, Obregón 2005). Bishops could use the resources and influence of the Catholic Church, being the institution's primary executive authority in areas under their jurisdiction. Through the Church, bishops were locally embedded: overseeing local religious life and service delivery. Likewise, bishops had public influence derived from the Church: Argentines—more than 90% of whom were Catholic adherents in the 1970s—agreed with the Catholic Church more than the military.

Because the Catholic pope exerted little control over the characteristics of jurisdictions to which he appointed bishops, I can describe the relationship between opposed bishops and repression free of many potential confounding variables. I find

a significant, robust negative relationship: an average opposed bishop prevented 128 disappearances and 32 killings in his jurisdiction during the dictatorship. However, identifying the relationship between opposed bishops and repression in Argentina presents a challenge: opposition and repression are often endogenous (Ritter and Conrad 2016).[1] Demonstrating a causal effect requires opposition neither caused by repression nor correlated with characteristics on which the regime targets repression. I address endogeneity, and also measurement error, using an instrumental variable drawing on the exogenous timing of some bishops' appointments (Gill 1998, Hale 2015). I also rule out the possibility that bishops who supported the Dirty War account for the results.

I also present evidence that opposed bishops resisted state violence through two mechanisms, drawing on the local embeddedness and public influence of the Catholic Church. First, opposed bishops encouraged likeminded subordinate agents—priests—who assisted victims of repression. These bishops are associated with a greater reduction in repression when more likeminded priests served in their jurisdictions. A follow-on test using novel micro-level data from the Argentine capital, Buenos Aires, underscores opposed priests' complementary efforts: priests in opposed Catholic religious orders—organizations with some independence from bishops—are associated with reduced repression in nearby areas even under an unopposed bishop. Second, opposed bishops pressured the regime with public human rights appeals. Consistent with this mechanism, evidence suggests opposed bishops shaped public attitudes: areas with opposed bishops retained more Catholic adherents from 1970 to 1988.

Existing repression research emphasizes states' use of coercive power against domestic threats (Blaydes 2018, Davenport 2015, Ritter and Conrad 2016). I consider a different type of power—traditional or symbolic power (Condra, Isaqzadeh and Linardi 2019, Loveman 2005, Nelson 1993)—and describe how religious leaders use

[1]That is, repression affects opposition or repression is targeted on the basis of characteristics correlated with opposition.

this power to command local agents and exercise public influence (Kubik 1994, Nalepa and Pop-Eleches 2021). The findings connect literature on repression with research on religious leaders' influence in dictatorships (Koesel 2014, Wittenberg 2006) and politics (Grzymala-Busse 2016, Spenkuch and Tillmann 2018). Incorporating religious leaders and other sources of traditional power into repression research can also extend theories about how subnational variation in regime opposition shapes regimes' strategies of repression (Blaydes 2018, Carter and Hassan 2021).

More generally, I show that civil society organizations such as religious institutions can check the arbitrary use of state power under dictatorship (Schmitter 1993). This finding adds to research on the Catholic Church's contributions to popular mobilization and regime change in Latin America (Gill 1998, Hale 2015, Mainwaring and Wilde 1989, Trejo 2012). Furthermore, I show that even a religious institution aligned with a dictatorship and a national monopoly on religious adherence—such as the Catholic Church in Argentina—may be divided: a minority of leaders can break with the institution's prevailing views to challenge the state. This finding suggests that looking within civil society organizations otherwise reputed to be monolithic or co-opted by regimes reveals rich subnational variation. Analysis within such institutions also reveals patterns, such as complementarities between leaders and agents, which explain the institutions' durable political influence.

2.2 Religious Leaders and State Violence

Social institutions, civil society organizations which structure citizens' social interactions, have "repeated, strong interactions" with political regimes (Tilly 2006). I examine the interaction between one regime type—dictatorship—and leaders of one such institution—religious institutions. Religious institutions engage their adherents with "ideational content, rituals and practices, organizational resources, hierarchies,

social networks, social identity categories, social insurance, [and] service delivery" (McClendon and Riedl 2019, 26).

Religious institutions have acquiesced to dictatorships, standing by or lending influence to repressive regimes (Slater 2010). In the Nazi regime, for example, Germany's "rich associational life provided a critical training ground for eventual Nazi cadres and a base from which the National Socialist German Workers' Party (NSDAP) could launch its seizure of power" (Berman 1997, 402). Spenkuch and Tillmann (2018) find that Catholic local religious agents—priests—with connections to the NSDAP influenced adherents' views, increasing voting for the fascist party in interwar German elections even as the Catholic Church initially opposed the NSDAP. In Cold War-era Latin America (Gill 1998) and early 21st-century China and Russia (Koesel 2014), religious institutions often struck cooperative bargains with dictatorships, seeking the access to power and resources these regimes offered.

On the other hand, some religious institutions spurn cooperation and oppose dictatorships. Through education and social movements, religious opposition to colonial regimes made transitions to democracy more likely (Woodberry 2012). In the Nazi-occupied Netherlands, local religious communities helped Jews evade capture (Braun 2016). In Communist-era Hungary, Catholic priests preserved citizens' religious and political identities (Wittenberg 2006). In Mexico under the PRI, religious networks in indigenous communities provided necessary resources for mobilization against economic reforms (Trejo 2012).

This paper focuses on the set of religious institutions' leaders who oppose repression —the use of state coercive power which violates basic rights (Davenport 2015)— in dictatorships. Opposed religious leaders are those who make statements and take actions which indicate disapproval of state violence.[2] In so doing, their institutions become "reservoirs of....resistance to arbitrary or tyrannical action" (Schmitter 1993,

[2] "Repression" and "state violence" are used equivalently.

15). Religious leaders who oppose repression amplify their resistance with two characteristics derived from their institutions: local embeddedness and public influence.

First, religious leaders draw on institutions' local embeddedness, aggregating local information through interactions with subordinate agents who work among adherents (Carter and Hassan 2021, Wittenberg 2006). Leaders build "knowledge of the jurisdiction" and "social and professional bonds" from these interactions (Carter and Hassan 2021, 6). These bonds facilitate the exchange of news, allowing leaders to gather information about local events. Because news about repression spreads by word of mouth (Blaydes 2018), leaders of locally embedded institutions acquire the knowledge about repression necessary to oppose it. In particular, leaders may learn about repression from subordinate agents who are most embedded with adherents (Braun 2016).

Religious leaders who oppose repression draw on local embeddedness—particularly subordinate agents—to take two actions which resist state violence:

- **Local assistance:** In this mechanism, opposed leaders encourage subordinate agents who also oppose repression. Among other actions, these agents help targets of repression evade state violence (Braun 2016) and may also deter repression at the local level (Brooke et al. 2020). Networks of local agents thus create a "free space" for these targets (Gautier 1998). Thus, opposed religious leaders should be better able to resist repression when they have likeminded agents who provide local assistance.

- **Backlash mobilization:** Religious leaders provide resources such as educational infrastructure, backing for activist networks, and protection of human rights organizations to enable local collective action against repression (Amat 2019, Trejo 2012, Woodberry 2012). Repression in areas with opposed religious leaders then triggers backlash, enabling collective action against the regime

in response to state violence. Opposed religious leaders thereby increase the regime's costs of repression.

Religious leaders' second key characteristic is public influence. The religious institution's symbols and trappings give leaders traditional power, "the sanctity of the order and the attendant powers of control as they have been handed down from the past" (Nelson 1993, 656). Leaders' traditional power lends influence distinct from the state's legal power (Loveman 2005, Slater 2010): religious adherents, members of the public, and the regime and its agents give religious leaders' statements additional weight when forming attitudes and taking actions (Condra, Isaqzadeh and Linardi 2019, McClendon and Riedl 2019). Thus, by acting on information about state violence in their jurisdiction and opposing its use, religious leaders become opinion leaders (Smith 2019). Leaders' traditional power, used to oppose repression, erodes the regime's legal power and ability to justify coercion (Slater 2010).

Drawing on public influence—particularly traditional power and influence with adherents—religious leaders who oppose repression can take two types of actions:

- **Public appeals:** In this mechanism, leaders draw on the institution's public influence to criticize repression in front of their adherents: their political statements draw on religion's traditional power and influence on public opinion (Nalepa and Pop-Eleches 2021, Spenkuch and Tillmann 2018), thereby decreasing support for repression.

- **Private appeals:** Under this mechanism, religious leaders appeal personally to regime agents to reduce repression. Regime agents respond to these appeals, especially if the leaders uplift the moral standing of agents who are themselves religious adherents (Tsai 2007). Agents sympathetic with religious leaders who oppose state violence may defy orders to repress or even defect, thus reducing repression.

Opposed religious leaders' actions threaten the regime, as local embeddedness and public influence make them a "creator, repository, and propagator...of values" parallel to the state (Kubik 1994, 119). The regime may control opposed religious leaders with co-optation, further repression, or accommodation (Gandhi 2008). Co-opting opposed leaders, attempting to purchase their (or their agents') loyalty, is one option (Nalepa and Pop-Eleches 2021). Yet leaders' opposition is often rooted in moral conviction, making co-optation ineffective (Wittenberg 2006). Repression is another option. While repression may control opposition from local agents or adherents, leaders' position in their institutions bestows prominence and influence with the public such that repression is counterproductive. Repressing leaders angers adherents, leading to a net increase in opposition (Esberg 2020*a*, Gautier 1998). This risk of increased opposition limits repression: in Argentina during the Dirty War, for example, the regime repressed less than two percent of opposed religious officials.[3]

The regime's remaining option is accommodation, permitting reduced repression in areas under opposed religious leaders' jurisdiction. This produces an observable implication: *in areas of a regime's territory where religious leaders oppose repression, repression is reduced compared with areas where religious leaders do not oppose repression.*

2.3 The Argentine Dirty War and the Catholic Church

To test the argument's main implication, I examine leaders of the Catholic Church—a large, worldwide religious institution—in Argentina during the country's 1976-1983 right-wing military dictatorship. The military regime came to power via coup on March 24, 1976, implementing a "systematic utilization of terror" against the popu-

[3]This figure is based on data from Pattin (2019) and CONADEP (1984).

lation which extended far beyond its stated leftist targets and became known as the Dirty War (Obregón 2005, Sikkink 2004). The regime decentralized repression, empowering specialized repressive battalions and military commanders of the geographic zones which partitioned the country to kidnap, disappear, torture, and execute thousands of persons, taking them from homes and workplaces to nearby clandestine detention centers (Scharpf and Gläßel 2020). Dismantling organized labor, the regime repressed unions, especially those in firms with regime connections (Klor, Saiegh and Satyanath 2020). To prevent news of repression from spreading, the regime blocked publication of victims' obituaries in newspapers, forbade public announcements of deaths, and obstructed human rights investigations (Morello 2015). The regime thus kept the Argentine public uninformed about the scale of state violence.[4]

The Argentine Catholic Church was known for its ideological alignment with the military during the Dirty War (Gill 1998, Scharpf 2018). Many Catholic religious leaders—bishops—"condoned the association that the military made in its public statements between state terrorism and Christian virtues" (Romero and Brennan 2013, 238), and the military sought the Church's aid in justifying repression (Finchelstein 2014).[5] To this end, "the very day of the coup, March 24, 1976, members of the military junta met for a long time with Archbishop Adolfo Tortolo of Paraná...president of the Bishops' Conference of Argentina" (Mignone 1986, 19). Tortolo was also Bishop of Argentina's Military Vicariate, a Church institution staffed with hundreds of priests that lent spiritual support to the armed forces.

Because the Argentine Church's top leadership supported the military, Argentina is a hard case for evaluating religious opposition to repression. The military's high-ranking allies in the Church promoted "unity and homogeneity," suppressing intra-Church disagreement about the "alliance of cross and sword" (Pattin 2019, 21-22).

[4]Morello (2015) notes: "No previous military regime had resulted in such a bloodbath. Because of this, in the first months, no one believed the rumors" (76).

[5]The regime styled itself as defending "Western, Christian civilization" (Romero and Brennan 2013).

Military allies in the Church, moreover, labeled human rights principles "atheistic" and linked their advocates with violent leftist groups (Feitlowitz 2011, 41). The Vatican's representative in Argentina until 1980, Papal Nuncio Pio Laghi, equivocated on the Dirty War, declaring the military's actions taken in self-defense (Mignone 1986). Opposition to repression, therefore, offered little benefit to bishops' Church careers.

A minority of bishops nonetheless opposed the military's campaign of disappearances and killings. Admiral Emilio Massera, a leader of the coup, remarked that "...bishops would influence military officials such that the military officials would carry out less bloody repression" (Diario de Juicio 1985). For example, the "families of political prisoners and disappeared persons in [the province of] Misiones....made public their appreciation...especially to the bishop of Posadas, Monsignor Jorge Kemerer...for his interventions in favor of the freedom of the political prisoners of the province" (La Razón 1983). A prisoner recounted that "Monsignor Marengo, Bishop of Azul, was worried about my fate and spoke with General Alfredo Saint Jean, asking that they not kill me, which worked" (CONADEP 1984). Juan Tomé, Bishop of Mercedes, "intervened before the authorities to liberate" six persons (Excelsior 1979). Two human rights organizations resisting the Dirty War relied on the "personal relations of religious leaders" in the Catholic Church (Loveman 1998, 509).

Furthermore, the Argentine Church was locally embedded and possessed public influence. Bishops lead a Catholic jurisdiction known as a diocese, of which Argentina had 59 during the Dirty War. Within each diocese, bishops control the employment of agents—priests—who are embedded in local Catholic parishes. Bishops also supervise a bureaucracy which administers religious life: in 1971 in the Argentine capital of Buenos Aires, for example, the bishop oversaw 155 parish churches, 116 community organizations, and 262 schools (Archdiocese of Buenos Aires 1971). For influence, the Argentine public—up to 90% of whom were Catholic adherents in the 1970s—

identified with the Catholic Church more than the military. In a 1971 public opinion survey, 78% of Argentine respondents expressed as much or more agreement with the Church as they did with the military (Turner 1971).

2.4 Research Design

2.4.1 Measuring Bishop Opposition

To test the relationship between bishop opposition and repression, I first determine which Argentine bishops opposed the Dirty War. I create a biographical sketch for each of the 82 bishops during the 1976-1983 period with sources drawn from three Argentine human rights archives, three contemporaneous Catholic periodicals, and declassified United States intelligence. From this sketch, I code a bishop as opposed to repression if he made pre-regime statements and actions opposing state violence and advocating for human rights while not reducing such opposition during the Dirty War. Perhaps due to pressure from the military or concerns for their Church career prospects, four bishops who advocated for human rights before the 1976 coup pointedly declined to criticize repression, or the military generally, after it. These bishops are not coded as opposed.[6]

An example of an opposed bishop is Alberto Devoto, of the diocese of Goya. From 1968 to 1970 Devoto participated in meetings of the progressive Movement of Priests for the Third World (MSTM)—a left-leaning clerical movement which organized local political action against perceived socioeconomic injustice, was labeled a "red bishop" in 1974 for his progressive tendencies, and during the regime offered assistance to political prisoners. An example of an unopposed bishop is Desiderio Collino, of the diocese of Lomas de Zamora. In 1974 he denounced the MSTM, and during the Dirty

[6]No bishops advocated for human rights after 1976 without a previous record of advocacy. The Supplementary Information (SI) Appendix A reports the bishop opposition coding procedure.

War he opposed the investigations of human rights organizations into repression. In all, twenty-five bishops made statements or took actions indicating opposition to repression. Fifty-five bishops ranged from statements and actions indicating no position on repression to statements and actions indicating support for repression. Two bishops lacked sufficient information to code.

Bishops are next linked to the departments, Argentina's second-level administrative division, which compose dioceses.[7] The explanatory variable $Bishop\ Opposed_{idt}$ takes the value of 1 if department i contained in diocese d in year t had an opposed bishop, and 0 otherwise. If an opposed bishop replaced an unopposed bishop or vice versa—as occurred nine times during the dictatorship—the variable is coded based on the bishop who served for the majority of the given year. Opposition is measured geographically because information about repression in a department is most likely to come to the attention of the bishop whose diocese contains that department: the Church obligates bishops to acquire local knowledge.[8]

Figure 1.1 depicts a cross-section of the bishop opposition variable from 1976, the first year of the military regime. Spatial clustering presents a potential concern, particularly if opposed bishops sought appointments near likeminded colleagues. To address this concern, I partition Argentine departments into geographic regions using k-means clustering. I then generate 500 random assignments of dioceses to bishop opposition, and conduct a χ-square test of each assignment's independence from the clusters. Finally, I compare the χ-square value of true bishop opposition with the resulting distribution of test statistics. The results suggest the true geographic pattern of bishop opposition is not significantly correlated with geographic clusters.[9]

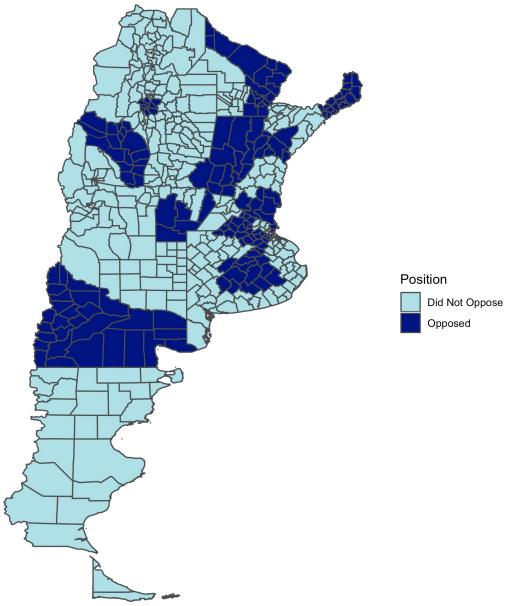
An advantage of this measurement is that it addresses the concern that, while orig-

[7] In the Buenos Aires province, this division is a *partido*. For simplicity, all units are labeled departments.

[8] Per Catholic Canon 396,"the Bishop is bound to visit his diocese in whole or in part each year" and Canon 383, "the diocesan Bishop is to be solicitous for all Christ's faithful entrusted to his care."

[9] Figure 1.B.1 in Appendix A reports full results.

Figure 1.1: Bishop Opposition to the Dirty War, 1976



Note: Map depicts Argentine bishop attitudes towards the military regime's campaign of repression in 1976. Each geographic division is a department, the units of which dioceses are composed.

inal archival data provide "more comprehensive and less biased information on political conflict than any other systematic collection of data" (Balcells and Sullivan 2018, 139), the archival sample could be susceptible to "politically motivated...manipulations of the types of documents that get released" (140). Manipulation of released documents would introduce measurement error, thereby biasing estimation with the bishop opposition variable. My sample addresses this concern in two principal ways. First, I draw on sources with cross-cutting motivations: left-leaning Argentine human rights activists have shaped the contents of its domestic archives, while Sikkink (2004) suggests the U.S. government attempted to suppress information critical of right-wing regimes during the Ford and Reagan administrations (167). Second, for the Catholic periodicals, I accessed the full population of issues. To further address measurement error, I later conduct an instrumental variables analysis.

2.4.2 Measuring Repression

For the outcome variables of interest, I create $Disappearances_{idt}$, the count of disappearances in department i contained in diocese d in year t, and $Killings_{idt}$, a similar count of killings.[10] Data are from the 1984 Argentine National Commission on the Disappearances of Persons (CONADEP) report, which records 8,961 victims of repression during the Dirty War and specifies the department and date of each disappearance and killing. These data are "highly reliable" (Sikkink 2004, 96): the department and year of repression are known for 99.4% of the victims. The specific sites of repression were typically in or near the victim's place of residence or employment: victims' homes account for 57% of sites (CONADEP 1984).

Nonrandom measurement error in the outcome variables could, if present, bias an estimate of the relationship between bishop opposition and repression. Bishops unopposed to repression could have obstructed CONADEP as it gathered data on disappearances and killings. This would lead to fewer reports of disappearances and killings in these bishops' dioceses.[11] Conversely, bishops opposed to repression helped CONADEP gathered data on disappearances and killings (Brysk 1994). However, such systematic measurement error would bias estimates against the expectations of the argument, meaning I likely underestimate opposed bishops' relationship with repression.

It is possible, however, that measurement error in the outcome variables works in the opposite direction: CONADEP could have uncovered more repression in the dioceses of unopposed bishops if the commission suspected these bishops were uncooperative with investigators. There are two reasons to rule out this concern. First, CONADEP relied on voluntary testimony and "did not have the power of subpoena" (Osiel 1986, 141). The commission lacked legal authority to compel uncooperative of-

[10] These were the modal types of repression the military used during the dictatorship.

[11] The Argentine human rights community maintains CONADEP's tally underestimates the count of disappearances and killings by a factor of three or more.

ficials to reveal information. Second, unopposed bishops advocated for forgetting past repression even as the military undertook "destruction of records preceding the 1983 transition to democracy" (Brysk 1994, 682). It is unlikely that unopposed bishops intervened to preserve records.

2.4.3 Other Covariates

I include department-level covariates which measure socioeconomic characteristics before the dictatorship. From the 1970 Argentine census, I include literacy rates, the log of total population, and the proportion of foreign-born residents. Each covariate corresponds with potential predictors of repression: it often occurred in urban departments, targeted organized labor, and—through collusion with other regimes in what was known as Plan Condor—foreigners suspected of leftist activism. Furthermore, opposed bishops may have sought out departments with characteristics reflecting their social or political preferences.

I account for department-level political characteristics with two covariates. First, I include a covariate for the vote share of the Peronist Justicialist party—a party aligned with ex-president Juan Perón—in September 1973 presidential elections (Ministry of the Interior 1973).[12] Second, I include a pre-dictatorship measure of political grievances with the share of spoiled ballots drawn from the same source.

Finally, I account for diocese-level characteristics which could relate to both bishop opposition and repression: whether the bishop served an archdiocese, the bishop's tenure in office, and the count of priests affiliated with the MSTM in 1967.[13] The archdiocese indicator in particular is important because bishops who aspired to this higher rank may have had career incentives to conform to the Argentine Church's ideological alignment with the military. Appendix A Tables 1.A.2 and 1.A.3 present

[12]These were the last democratic elections held before the dictatorship.

[13]Bishop tenure accounts for a potential SUTVA violation in which a unit's exposure to bishop opposition depends on the bishop's time in office.

descriptive statistics.

2.4.4 How Are Bishops Appointed?

Bishops' appointments were, by law, made independently of the Argentine state.[14] Appointments of new bishops were consistent with the 1966 Concordat between the Vatican and the Argentine government, which stipulated the "Argentine state recognizes and guarantees the Roman Catholic apostolic Church...jurisdiction in the sphere of its own competence" (Mignone 1986, 76) and in particular granted the "independence of the church in its spiritual domain, the naming of bishops, and the creation of ecclesiastical boundaries" (Villa 2000, 253). The Concordat ended a system of state-controlled appointments, and Catholic popes thereafter appointed bishops. Accordingly, Argentine regimes treated appointments with "kid gloves" for fear of angering the pope (Gill 1998, 160).

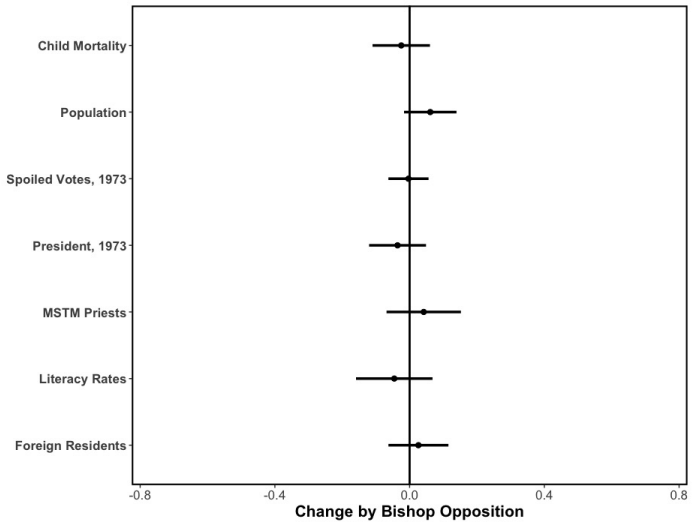
Even with evidence of the Church's independence, prospective bishops nevertheless could have sought appointments to dioceses with their preferred socioeconomic political, or religious characteristics. For example, bishops who opposed repression could have requested appointments in dioceses with MSTM presence. To rule out this selection process, I compare pre-dictatorship characteristics of department-years with bishops who opposed repression with characteristics of department-years with bishops unopposed to repression. I conduct bivariate regressions of the bishop opposition variable on different political and demographic characteristics and the count of priests who in 1967 affiliated with MSTM. Department-years are balanced on pre-dictatorship characteristics between bishops who opposed and bishops who did not oppose repression. Figure 1.2 depicts results from the test.[15]

Furthermore, bishop appointments could have become endogenous during the dic-

[14] Trejo (2012) and Osorio, Schubiger and Weintraub (2020) make similar arguments for Mexico.

[15] Full results are in Appendix A Tables 1.B.1, 1.B.2, 1.B.3, and include a diocese-level balance test and evidence that appointments made during the 1966-1973 dictatorship, after the Concordat, were likewise balanced.

Figure 1.2: Balance of Department Characteristics by Bishop Opposition



Note: Figure depicts estimated differences in department-year characteristics between opposed and unopposed bishops. Variables are standardized through z-transformation, with 95% confidence intervals displayed.

tatorship. That is, once repression began, the Vatican and its representatives in Argentina, such as the Papal Nuncio, could have conditioned opposed bishops' appointments on the frequency of repression under their potential diocese. For example, opposed bishops may have replaced unopposed bishops where repression was lesser if the Vatican sought to prevent opposed bishops from disrupting state violence. Alternatively, opposed bishops may have, to obstruct the military, replaced unopposed bishops where repression was greater.

To address the possibility of strategic appointments, I conduct a test with an explanatory variable for a future change to an opposed bishop, $\Delta Bishop\ Opposed_{id,t+1}$. The variable measures whether department i contained in diocese d changed from an unopposed bishop to an opposed bishop between year t and year $t+1$. If opposed bishops were appointed based on current frequency of repression, then this variable would have a significant relationship with the outcome variable of disappearances and killings in year t. Results from this test indicate no consistent relationship between

future appointment of opposed bishops and current repression.[16]

Finally, though bishop opposition is unrelated to diocese characteristics or prior repression, the military could have nonetheless interfered with opposed bishops' appointments. In particular, in 1976, Admiral Massera sought the resignation of three opposed bishops: Enrique Angelelli, Raúl Scozzina, and Jaime de Nevares. He pressed the issue only lightly with top Church leadership (Morello 2015). Instead, the military staged a covert assassination of Angelelli in August 1976 which angered adherents and made Angelelli a martyr. Resorting to less violent tactics thereafter, the military pressured Scozzina to resign in March 1978. The last targeted bishop, de Nevares, stood firm and continued his opposition to the Dirty War. Archival evidence suggests none of the other twenty-two opposed bishops resigned or died under military pressure.[17] However, because military pressure introduces endogeneity in bishop opposition, I later employ an instrumental variables strategy which leverages exogenous variation in bishop appointments (Gill 1998, Hale 2015, Tuñón 2018).

2.4.5 Statistical Estimation

I use linear regression to estimate opposed bishops' relationship with repression. The right-hand side of the estimating equation for repression in department i in diocese d and year t contains bishop opposition in diocese d in year t, department-level covariates X_i, diocese-level covariates Z_d, military zone fixed effects γ_z, year fixed effects μ_t, department-specific quadratic time trends $\gamma_{1,i}t + \gamma_{2,i}t^2$ and standard errors clustered at the diocese level ϵ_{id}:

$$Repression_{idt} = \alpha + \beta Bishop\ Opposed_{dt} + \delta_1 X_i + \delta_2 Z_d + \gamma_z + \mu_t + \gamma_{1,i}t + \gamma_{2,i}t^2 + \epsilon_{id}$$

[16] Appendix A Table 1.B.4 reports full results from this test.

[17] While some suspect Carlos Ponce de León was one such victim, no conclusive evidence has been found.

Military zone fixed effects adjust for unobserved, time-invariant geographic characteristics, such as commanding officer attributes, which could relate to both bishop opposition and repression. The military was not a unitary actor, granting autonomy to these zones' commanding officers (Scharpf 2018). Other specifications use two smaller geographic fixed effects: military subzone and department. Across these specifications, β estimates the within-unit change in repression associated with changes in bishop opposition. I also report a specification with no unit fixed effects, given two-way fixed effects models' limitations in adjusting for both temporal and unit confounding (Imai and Kim 2020).

To adjust for temporal patterns, I first include year fixed effects to account for events affecting repression across Argentina in particular years, such as the military scaling back repression during a 1979 visit from the Interamerican Commission on Human Rights. Second, unit-specific quadratic time trends disentangle the relationship between bishop opposition and repression from underlying patterns of state violence: after 1977, disappearances and killings decreased rapidly and remained low through 1983.

2.5 Analysis

2.5.1 Opposed Bishops and Repression

Opposed bishops are associated with reduced repression in departments under their jurisdiction: the estimated change in disappearances and killings corresponding to an opposed bishop in a given department-year is consistently negative and statistically significant. There were between one and three fewer disappearances and between one-third and three-quarters fewer killings per year, on average, in department-years with an opposed bishop than those with an unopposed bishop. Given the average diocese has eight departments and the dictatorship lasted almost eight years, an opposed

bishop could have reduced disappearances by around 128 and killings by around 32 had he been a bishop throughout the dictatorship. This reduction is underestimated if opposed bishops assisted in reporting repression.

Table 1.1 reports coefficient estimates for opposed bishops in greater detail. The first three columns report estimates from specifications with the first outcome variable, *Disappearances*, while the second three columns report estimates from specifications with the second outcome variable, *Killings*. I next subject these results to a series of robustness tests.

Table 1.1: Opposed Bishops and Repression in the Argentine Dirty War

	Disappearances				Killings			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bishop Opposed	$-3.84^{\dagger}$	-1.43^{**}	$-1.19^{\dagger}$	-3.63	-0.80^{**}	-0.53^{**}	-0.34	-0.70^{*}
	(2.26)	(0.52)	(0.71)	(2.25)	(0.29)	(0.19)	(0.28)	(0.30)
Adj. R^2	0.12	0.75	0.89	0.09	0.13	0.41	0.82	0.10
Observations	3731	3731	3763	3763	3731	3731	3763	3763
Covariates	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Unit Trends	✓	✓	✓		✓	✓	✓	
Geographic FE	Zone	Subzone	Dept.		Zone	Subzone	Dept.	

$^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$

[1] *All standard errors clustered at the diocese level.*

2.5.2 Robustness Tests

I first address the possibility of measurement error in the explanatory variable first by using stricter coding criteria: any bishop who took repeated public-facing actions against repression is coded as opposed. The estimates, in Appendix A Table 1.C.1, are generally larger in magnitude and retain significance. Second, I use looser coding criteria: any bishop who did not openly support repression is coded as opposed. The estimates, in Appendix A Table 1.C.2, are attenuated as expected from a noisier variable. I next recode each bishop's opposition one at a time and re-estimate the relationship between bishop opposition and repression. The procedure determines whether the relationship is sensitive to marginal changes in measuring bishop oppo-

sition. The results, reported in Appendix A Figure 1.C.1, suggest almost complete robustness. The bishop whose recoding would most change the relationship, Bernardo Witte, was publicly unopposed to repression: he labeled criticism of the military a campaign of defamation.

The distribution of the outcome variables—repression concentrated in cities and early in the dictatorship—presents several possible concerns. First, outliers could drive the results: repression in the capital city's diocese, Buenos Aires, was about sixteen standard deviations higher than any other department. Its bishop was also unopposed to repression. The relationship between bishop opposition and repression is re-estimated with data dropping Buenos Aires. I continue this procedure, dropping each diocese in turn. Appendix A Tables 1.C.3 and 1.C.4 and Figure 1.C.2 report results, which remain consistent. Second, excess zeros in the outcome variables could lead to a violation of the normality assumption for the regression models' error terms. To address this problem, I re-estimate the relationship between opposition and repression with data only from 1976 to 1978, during which more than nine-tenths of disappearances and killings occurred. I also aggregate the data to the diocese-year level and re-estimate this relationship. Finally, negative binomial regression models are specified. Results, reported in Appendix A Tables 1.C.5, 1.C.6, and 1.C.7, remain consistent.

Spillovers, if present, could violate the assumption of non-interference between units: opposed bishops could have affected repression in nearby departments without an opposed bishop. This would be the case if, for example, a military officer redirected repression to departments in his zone without an opposed bishop, or if an opposed bishop intervened to reduce repression in departments under another bishop's diocese. To test for spillovers, I create two new variables, *Bishop Opposed, 50km*$_{idt}$ and *Bishop Opposed, 100km*$_{idt}$, which count the departments in the diocese of an opposed bishop within 50 and 100-kilometer radii of department i in diocese d and year t.

These variables are then interacted with bishop opposition (Ichino and Schundeln 2012). If there are spillovers, the baseline terms for *Bishop Opposed, 50km*$_{idt}$ and *Bishop Opposed, 100km*$_{idt}$ in the interaction would be positive and significant. That is, repression in department-years with an unopposed bishop would increase in the count of nearby departments with an opposed bishop. Results, in Appendix A Tables 1.C.8 and 1.C.9, show baseline terms near zero and insignificant.

It could also be the case that the consistently negative relationship between opposed bishops and repression is the product of coincidence, or spurious correlation. To address this possibility, I conduct a placebo test modeled after a randomization inference procedure. Twenty-five of the 82 bishops are randomly assigned as opposed, and a model identical to the main specification estimates the relationship between bishop opposition and repression under that placebo assignment. I repeat this procedure 1000 times, creating a distribution of coefficient estimates against which to compare the estimate from the original specifications. The results, reported in Appendix A Figure 1.C.3, show a distribution centered around zero and with the original estimate significant at the $p = 0.01$ level.

Finally, I conduct a sensitivity analysis. The procedure derives a robustness value—the strength of a potential unobserved confounding variable's correlation with bishop opposition and repression needed to render their estimated relationship insignificant—measured by partial R^2 (Cinelli and Hazlett 2020). This procedure is preferable to other sensitivity analyses because it makes no assumptions about the functional form of bishop opposition assignment or the unobserved variable's distribution. The results, reported in Appendix A Figure 1.C.4, indicate such a variable must explain as much or more variation in repression than population to render the coefficient estimates for bishop opposition insignificant in models with both the disappearances and killings outcome variables.

2.5.3 An Instrumental Variables Strategy

There are two main challenges to interpreting the estimated relationship between bishop opposition and repression as a causal effect. First, bishops' opposition could be the result of less repression, rather than opposition reducing repression. In particular, only bishops in dioceses with less repression could have felt secure enough to oppose state violence. Second, there could still be measurement error in the explanatory variable. Bishops who did not oppose repression either in their statements or actions may have feigned a lack of opposition to gain more influence with the military in reducing repression (Gautier 1998).

I address these challenges with an instrumental variables strategy from Tuñón (2018), who leverages the exogenous vacancies created by bishops' natural death in office or their reaching the Church-mandated retirement age of 75. The instrument draws on the observation that Pope John Paul II, who acceded in October 1978, appointed bishops who were less progressive than bishops appointed by his predecessors Paul VI and John XXIII (Gill 1998, Hale 2015). Thus, vacancies created by bishops who died in office or turned 75 after October 1978 create exogenous variation in bishop opposition: John Paul II's conservative tendencies means his appointees were less likely to oppose repression by the right-wing dictatorship.

For consistency with the previous analyses, the instrument, $Vacancy_{idt}$ inverts the instrument in Tuñón (2018): it takes the value of 0 if the bishop whose diocese d included department i in October 1978 died or turned 75 before year t and 1 otherwise. The first stage regresses bishop opposition on $Vacancy_{idt}$. This is a strong instrument, with a first stage F-statistic of 73. The second stage uses predicted values from the first stage regression to form the instrumented variable $\widehat{Bishop\ Opposition}_{idt}$ and makes it the right-hand-side predictor for the outcome variable $Disappearances_{idt}$ and $Killings_{idt}$. Because John Paul II became pope after the military seized power and the frequency of repression decreased over time, I include a linear time trend.

Standard errors are clustered by diocese.

The estimated local average treatment effect from this test—the effect of bishop opposition among those bishops whose opposition was a function of being appointed by a different pope than John Paul II—is 4.06 fewer disappearances and 1.16 fewer killings in a given department-year. Both estimates are significant at the $p = 0.1$ level.[18] These results address concerns about reverse causality. Furthermore, given the two-stage estimates are larger in magnitude than coefficient estimates in the original specifications, the instrumental variables results are consistent with some measurement error attenuating the original estimates.

2.5.4 Alternative Explanation: Supporting Bishops

An alternative explanation is that the results reflect supporting bishops increasing repression rather than opposed bishops reducing it. While both processes could occur, opposed bishops' relationship with repression may not be robust to accounting for supporting bishops. To address this possibility, I first code unopposed bishops as either supporting—making statements or taking actions consistent with support for repression—or neutral.[19] I include this bishop support variable in model specifications alongside bishop opposition, with neutral bishops as the reference category. If the alternative explanation is correct, the coefficient estimate for opposed bishops would be zero, while the coefficient estimate for supporting bishops would be positive. Results, reported in Appendix A Table 1.E.1, suggest the opposite: less repression occurred under opposed bishops than under neutral bishops. Supporting bishops, meanwhile, have no less repression in departments in their dioceses than neutral bishops.[20]

[18] Results, including tests from Tuñón (2018), are in Appendix A Tables 1.D.1, 1.D.2, and 1.D.3 and Figure 1.D.1: bishops complied with the retirement age and popes did not appoint younger bishops later in their tenure.

[19] This procedure is similar to coding bishop opposition, which Appendix A reports in detail.

[20] I also show in Appendix A Table 1.E.2 and Figure 1.E.1 that supporting bishops did not increase repression, and show in Appendix A Table 1.E.3 that opposed bishops still reduce repression when compared only with supporting bishops.

2.6 Testing Mechanisms of Bishop Influence

How did opposed bishops reduce repression during Argentina's Dirty War? In this section, I focus on evidence that bishops' influence was consistent with two types of actions. First, opposed bishops used their local embeddedness to encourage likeminded local agents—priests—who sympathized with victims of repression. Opposed bishops could work with these agents to shelter potential targets of repression. Indeed, these targets disclosed the existence of "a network of safe houses" with Church assistance which concealed "those who would have disappeared" (Duzdevich 2019). Bishops and priests also visited prisoners to offer the Church's aegis (Mignone 1986). Second, a greater retention of Catholic adherents under opposed bishops suggests their influential public appeals shaped attitudes.[21] To this end, Morello (2015), evaluating Catholic resistance in Argentina, argues the military "responded better to the pressure of civil society than to actions taken in private" (186).

2.6.1 Local Assistance: Movement of Priests for the Third World

If opposed bishops encouraged likeminded priests' actions, these bishops should be associated with a greater reduction in repression when more such priests serve in their diocese. Priests opposed to repression, particularly those in the Movement of Priests for the Third World (MSTM), were activists in Argentina's poor communities (Adair 2017) who helped targets of repression "seek refuge and a safe exit from the country" during the Dirty War (Scocco 2020, 9). Opposed bishops, in turn, could create conditions favorable for MSTM priests to act with their authority to appoint priests and to make rules governing the diocese. To that end, opposed bishops gave MSTM priests a "wide range within which to operate" and "protected them" (Dodson

[21] Results in Appendix A Tables 1.F.7, 1.F.8, and 1.F.9 suggest that backlash mobilization and private appeals may also have been effective bishop actions.

1974, 66). For example, Bishop Jaime de Nevares of Neuquén defended MSTM actions before the military in 1977, and at least seven opposed bishops affirmed the MSTM in meetings as early as 1969 (Pattin 2019).

Unopposed bishops, rather than encouraging opposed priests' actions, used their authority to constrain the MSTM. Indeed, some of these bishops imposed constraints upon the MSTM's formation. In 1969, Archbishop Guillermo Bolatti of Rosario forbade priests from activism in the poor communities in his diocese. In the early 1970s, Bishop Juan Laise of San Luis expelled a priest who sympathized with leftist movements from his diocese. Bishop Leon Kruk of San Rafael pushed for priests' education to become more conservative, emphasizing obedience to political authorities, and also banned leftist Catholic publications in his diocese. In 1974, a group of unopposed bishops denounced the MSTM in a right-wing Catholic periodical.[22]

To test the implication that opposed bishops are associated with a greater reduction in repression with more opposed priests, I interact a measure of priest opposition to repression—the count of priests in a diocese affiliated with the MSTM in 1967—with the explanatory variable and estimate models otherwise identical to the main specifications. If opposed bishops reduced repression by encouraging likeminded priests' actions, the interaction term should be negative. The results, in Appendix A Table 1.F.1, are consistent with this expectation—at least for killings. Furthermore, the baseline term for opposed bishops—these bishops' relationship with repression when no MSTM priests served in their diocese—is attenuated. These findings suggest priests' resistance to repression complements bishops' efforts.

2.6.2 Confirming Local Assistance: Micro-Level Evidence

I next validate the finding above that repression is reduced when a bishop imposes fewer constraints on opposed priests' actions. Under an opposed bishop, these priests

[22]These examples draw on various primary sources which are detailed in full in Appendix A.

are less at risk of being forbidden from activism, expelled from their diocese, or mandated to preach a message unopposed to repression. Opposed priests thus reduce repression when their bishop, at minimum, permits rather than constrains their actions, or blocks attempts originating outside the Church to impose constraints. There are also situations in which bishops are less able—even if they are willing—to constrain opposed priests' actions. In these situations, opposed priests should likewise reduce repression.

Priests in Catholic religious orders—international organizations whose priests were given charge of some local Catholic jurisdictions known as parishes—are one such situation. Priests of an order are under the dual authority of their order's superior and the area's bishop, making their parishes less constrained by the bishop than "secular" parishes under the bishop's sole authority (McDermott 2004).[23] Thus a bishop unopposed to repression can impose fewer constraints on the parishes of religious orders than he can on secular ones. As a result, priests in opposed religious orders should reduce repression similarly to opposed priests in the employ of an opposed bishop.

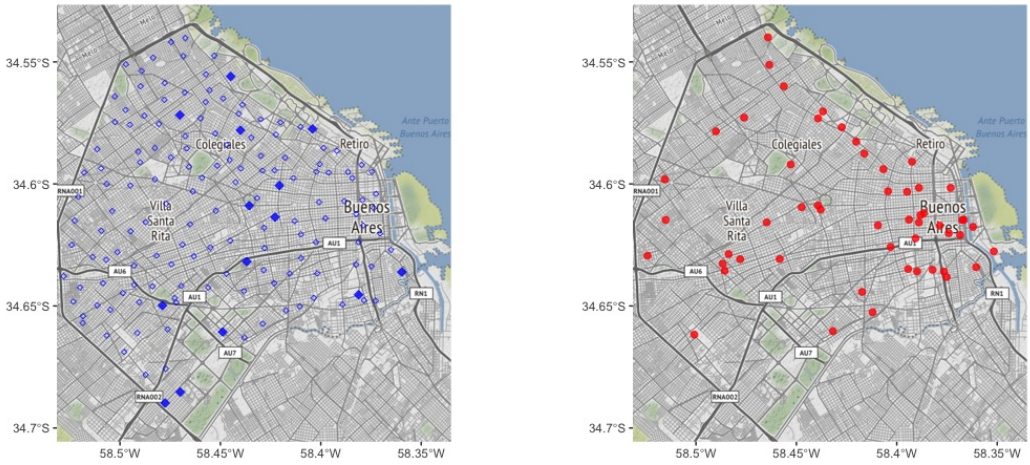
To test whether opposed orders reduced repression, I document the religious orders which spoke out against the Dirty War (Catoggio 2010). Based on these accounts, I determine which orders had a record of opposition to repression and match those orders to parishes in their charge in the Archdiocese of Buenos Aires, whose bishop was unopposed to repression.[24] The Ecclesiastical Guide of the Archdiocese of Buenos Aires (1971) geolocates each Catholic parish in the diocese which existed in 1971 and indicates which order, if any, administered that parish. For disappearances and killings, measurements are imprecise below the department level. As a proxy measure, I geolocate the physical sites of repression—clandestine detention centers (CCDs)—in

[23]In Argentina, orders received parishes before, and thus free from the interference of, the military regime.

[24]The archbishop concealed information about repression from the public (Finchelstein 2014). An enumeration of these orders and corresponding parishes is in Appendix A Table 1.F.2.

Buenos Aires (CONADEP 1984).[25] The military selected CCD locations endogenously, converting 34 of 53 Buenos Aires police stations into CCDs. Figure 1.3 maps Catholic parishes and CCDs in Buenos Aires.

Figure 1.3: Catholic Parishes and Detention Centers in Buenos Aires, 1970s



Note: The left panel depicts the geographic location of parish churches in Diocese of Buenos Aires in 1971, with parishes in the charge of opposed religious orders shaded. The right panel depicts the geographic location of clandestine detention centers in the same area during the dictatorship.

If priests in opposed religious orders reduced repression, CCDs—as a proxy for repression—should be more distant from parishes with opposed religious orders than other parishes. To test the implication, I regress the variable *Distance to Nearest CCD (km)*$_p$, which measures the distance in kilometers from a parish church p to the nearest detention center, on the explanatory variable, *Opposed Order*$_p$, which indicates whether parish p was in the charge of an opposing order. I also include zone and neighborhood fixed effects, accounting for geographic characteristics which could explain both the presence of opposed religious orders and the availability or selection of CCD locations.

Table 1.2 reports the results: parishes in the charge of opposed religious orders were, on average, about 0.4 kilometers farther from the nearest CCD than other parishes. These estimates are substantively large—about one standard deviation

[25] Appendix A Table 1.F.3 shows CCDs strongly predict disappearances and killings at the national level.

of the outcome variable among parishes without opposed orders.[26] Given parishes' compact jurisdiction in densely-populated Buenos Aires, a 0.4-kilometer distance can spell the difference between a CCD being within or outside of a particular parish's jurisdiction.

Table 1.2: Religious Orders and Repression in Buenos Aires

	Parish Distance to Nearest CCD (km)		
	(1)	(2)	(3)
Opposed Order	0.38	0.46*	0.33*
	(0.24)	(0.22)	(0.16)
Adj. R^2	0.04	0.20	0.37
Observations	154	154	154
Fixed Effects		Zone	Neighborhood

$^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$

2.6.3 Public Appeals: Catholic Adherents

Next, given their public opposition to the Dirty War, opposed bishops should shape the attitudes of adherents in their dioceses. There is qualitative evidence that opposed bishops used their public-facing position to oppose the Dirty War and pressure the regime. Archbishop Vicente Zazpe of Santa Fe—leader of about four million Catholics—preached a message pressing the "urgency of justice" with respect to human rights (U.S. State Department 1979). Other times bishops endorsed human rights organizations such as the Mothers of the Plaza de Mayo. Bishop Justo Laguna of Morón, a friend of Argentine human rights activist and Nobel Peace Prize winner Adolfo Pérez Esquivel, publicly denounced repression (U.S. State Department 1979). Bishop Jorge Novak of Quilmes linked advocacy for human rights with Catholic adherence, organizing a combined human rights march and large Mass in 1981 (Adair 2017). By this time, public support for the regime was eroding.

[26] A test validating the opposed order measurement is included in Appendix A Table 1.F.4. Opposed orders are also generally closer to the 19 police stations which were not converted into CCDs, as shown in Appendix A Table 1.F.5.

Opposed bishops' human rights advocacy in a Catholic context gained favor as the regime lost public support in the early 1980s: repression became common knowledge and pressure built for accountability. Simultaneously, unopposed bishops' "alliance of cross and sword" fell from favor. As Gill (1998) argues: "if government officials pursue policies contrary to the interests of a certain social group...and religious leaders avoid criticizing such policies...the credibility of the church will suffer" and adherents will leave (68). Opposed bishops should thus have retained more Catholic adherents during the regime than unopposed bishops (Wittenberg 2006). To test this implication, I aggregate the data to a department-level cross section and estimate the relationship between bishop opposition—the proportion of the dictatorship a department had an opposed bishop—and the change in Catholic adherence between 1970 and 1988. This measure accounts for the possibility that opposed bishops had more adherents before the Dirty War. Having an opposed bishop throughout the dictatorship corresponds to a five-percent greater retention of Catholic adherents.[27]

2.7 Discussion

Religious leaders can reduce repression in dictatorships. This study of Catholic bishops in Argentina during the 1976-1983 military regime shows that bishops opposed to repression reduced disappearances and killings in areas under their jurisdiction. An average bishop who opposed repression could have prevented 128 disappearances and 32 killings over the course of the dictatorship. I also leverage exogenous variation in bishop vacancies to suggest a causal effect of opposed bishops. These findings affirm the influence of communal elites such as religious leaders in dictatorships. The findings also show religious institutions' stances toward regimes are not monolithic, documenting the few opposed leaders in an Argentine Catholic Church which otherwise ideologically aligned with the military regime.

[27] Appendix A Table 1.F.6 reports full results.

Religious leaders' influence generalizes beyond Argentina. First, the country's regime-aligned Church makes this a hard case in which to estimate the effects of opposed leaders. When the Catholic Church is less regime-aligned, such as in Chile under Pinochet, religious resistance is even more likely to be effective (Amat 2019, Smith 1982). Second, religious institutions are ubiquitous in dictatorships: in 2000, 87% of citizens in dictatorships were adherents of a major world religion—an increase of 8% over the prior three decades.[28] Drawing on their embeddedness and influence, religious leaders take center stage in opposing (and supporting) these regimes. Considerable evidence shows religious leaders' influence in Southeast Asian (Slater 2010), Eastern European (Wittenberg 2006), Sub-Saharan African (McClendon and Riedl 2019), and Latin American (Gill 1998, Smith 2019) regimes. Third, religious leaders' influence on state violence extends to eroding democracies, such as how Catholic parishes curtailed killings in the Philippine drug war (Brooke et al. 2020).

Despite the breadth of their influence in dictatorships, religious leaders' effects on repression could break down in several ways. First, in religiously fragmented states—where public adherence is divided among several religions or no religious affiliation at all—leaders will be less embedded and less influential with regimes (Koesel 2014, 25). A smaller share of adherents limits information gathering and leaders' traditional power. This may constrain leaders' effectiveness in Sub-Saharan Africa, for example. Second, if a regime can prevent backlash when targeting leaders—because state violence is pervasive enough to deter expression of opposition or because foreign powers offer support for repression—then repression of opposed leaders, rather than accommodation, would be a best response. Third, the regime may co-opt entire religious institutions, integrating them with the state and precluding appointment of opposed leaders (Wainscott 2017). Fourth, religious leaders may oppose a regime without also opposing repression. In Argentina, for example, it was common to criticize both the

[28] Appendix Acontains more detailed information about scope conditions.

regime and the armed militants among its victims (Feitlowitz 2011). Religious leaders may not oppose repression when it targets such militants.

This paper's focus on religious leaders who command local subordinate agents and influence with adherents comports with a larger literature which shows these institutions can check the arbitrary use of state power (Braun 2016, Brooke et al. 2020, Hale 2015). By showing opposed religious leaders reduce state violence, I advance this scholarship. Studying leaders' use of local embeddedness and public influence also follows the call of Davenport (2015) for a "rigorous attempt at documenting the internal workings of those who challenge political authorities" (308). Furthermore, showing the interaction between religious leaders and their subordinate agents suggests their actions are complementary. Future research should similarly explore the internal workings of other challenger groups, studying how such interactions shape leaders' effectiveness in resisting state violence.

In this paper, I have also considered the effects of a different type of power than the coercion typically studied in repression research. The symbolic, or traditional, power of religious leaders differs from state coercive power, shaping public attitudes through appeal to morality and sacredness (Condra, Isaqzadeh and Linardi 2019, Kubik 1994). While literature has considered the role of religion in politics and qualitative accounts document the interaction of dictatorships and religious institutions, traditional power has made few inroads into the quantitative study of dictatorships. The use of traditional power could explain outcomes such as regime durability, military loyalty, and public support for regimes, reinforcing or substituting for co-optation or repression (Slater 2010).

Finally, the paper has revealed an Argentine Catholic Church more divided than in past accounts of the institution during the Dirty War. While the Church's top leadership aligned with the right-wing regime, a minority of bishops broke from the institution's prevailing views. This finding suggests two paths for future research.

First, scholars may discover subnational variation in other religious institutions and explore how such variation explains these institutions' influence. Second, the origins of opposed bishops' preferences present a puzzle. Whereas many accounts predict Protestant competition prompts Catholic leaders' human rights advocacy (Smith 2019, Trejo 2012), the lack of competition in 1970s Argentina (Gill 1998) indicates scholars should consider other explanations for these bishops' opposition.

Chapter 3

Political Competition and Authoritarian Repression: Evidence from Pinochet's Chile

3.1 Introduction

Authoritarian regimes use their security forces to prevent or respond to the mobilization of mass resistance (Ritter and Conrad 2016, Svolik 2012, Thomson 2017).[1] Yet security forces find it difficult to repress the dissidents who mobilize resistance, as dissidents organize covertly (Sullivan 2016a). Covertness presents an information problem: security forces must first identify and locate dissidents who mobilize popular threat before repressing them (Blaydes 2018, Gregory 2009, Greitens 2016). As a result, regimes allocate repression across their territory consistent with available information on dissidents. One way regimes can address the information problem is civilian collaboration. For example, Argentina's 1976-1983 military regime told the public, "your weapons are your eyes, your ears, and your intuition...Your information

[1]Repression is physical sanctions imposed by a regime to eliminate potential and active mass threats (Davenport 2007b).

is always useful. Bring it to us" (Feitlowitz 2011, 26). Why do civilians share such information about dissidents, and how does it shape repression?

This paper considers the role of information provision by civilians in nascent authoritarian regimes. It argues that civilians share more information with regimes in areas which had intense political rivalries under democracy, altering the frequency and targeting of repression (Balcells 2017). Regimes seek out dissidents to prevent future mobilization (Ritter and Conrad 2016, Sullivan 2016b). Identifying dissidents is difficult, however, because their covert action and shared loyalties with civilians reduce regimes' ability to gather information from the population (Blaydes 2018, Greitens 2016). At the same time, greater divisions among civilians in areas with close political competition give regimes an opportunity: heightened political rivalries motivate civilians who support the regime to offer political opponents as targets for repression (Bergemann 2017). Inviting repression on political opponents reduces opponents' electoral influence while satisfying a desire to inflict harm. However, supporters invite repression against any civilian regime opponents rather than the smaller subset of dissidents regimes prefer to target (Gregory 2009, Sullivan 2016b). The argument implies that, in areas of close political competition, civilians' political rivalries lead regimes to repress more frequently and target more non-dissidents than in areas without close competition.[2]

There are two main empirical challenges in testing the argument's implications. First, the argument applies to preventive repression, in which regimes repress dissidents to stop the mobilization of latent opposition (Sullivan 2016b). Yet repression can be also be responsive, in which regimes use coercion to curtail overt resistance such as protest. A research design which does not separate these two types of repression will produce biased estimates: preventive and responsive repression relate to

[2]Consistent with the conceptualization of Gutierrez-Sanin and Wood (2017, 26), frequency of repression is the rate of political killings, and targeting is the social groups against which repression is carried out.

opposition differently (Ritter and Conrad 2016). Despite this, research designs which distinguish types of repression are rare (Sullivan 2016b). Second, the relationship between political competition and repression often runs in the opposite direction: repression affects preferences toward the regime (Bautista et al. 2018, Rozenas, Schutte and Zhukov 2017), attitudes toward other civilians (Nugent 2020), and induces civilians to conceal their true preferences (Ritter and Conrad 2016), which thereby affects competition. Addressing reverse causality requires measuring political competition in a manner free of the effects of anticipated or actual repression.

To overcome these challenges, I examine repression in the first months after the September 1973 military coup in Chile—a period with intense partisan rivalries and qualitative evidence of civilian information provision. The coup deposed democratically-elected president Salvador Allende and brought to power a military dictatorship. This setting also addresses the two empirical challenges. To address types of repression I leverage that, after the coup, the regime repressed to prevent rather than respond to the mobilization of popular threats (Remmer 1980). The post-coup period isolates preventive repression, providing a clean test of the argument. To address reverse causality, I leverage Chile's status as a liberal democracy before the coup.[3] In the democratic period, Chile's armed forces did not credibly threaten repression, nor did civilians expect the military to retain power and repress with such frequency after a coup (Navia and Osorio 2019, Policzer 2009). Before September 1973, Chilean civilians therefore expressed political views free from the effects of regime violence.

I use original archival data on political killings of 1,120 people in the period after the coup to estimate the relationship between political competition and the frequency and targeting of repression across Chile's municipalities. The regime killed significantly more victims in municipalities with close political competition between Sal-

[3]Chile's democratic system was uninterrupted for 48 years before 1973 (Valenzuela 1978).

vador Allende's legislative coalition and its political opponents than in municipalities without close competition. The regime also killed more non-dissidents—individuals not active in labor and political organizations—in municipalities with close electoral competition than in those without close competition. These relationships are robust to different measurements of close elections and dissident classifications, while a sensitivity analysis addresses some omitted variable bias concerns.

Following the main results, I test implications of the partisan rivalry mechanism. First, the regime should rely more on civilian information in municipalities without army bases in which it has greater capacity to collect intelligence. Therefore, areas of close competition without army bases should have greater repression than those with bases. Second, research on social networks in conflict suggests that more weak ties between rival civilian groups increase violence after the start of intergroup conflict (Larson 2016). Consequently, I expect that municipalities with close competition that also have more weak ties between social groups provide the regime with more information and experience greater repression. Third, partisan rivalries should generate civilians' active participation in repression in addition to their information provision. I find support for each of these implications.

I also address alternative explanations. First, I consider that the regime may not have faced an information problem, meaning that repression in areas of close political competition resulted only from its strategic purposes. That is, the Chilean military dictatorship could have repressed more frequently in these municipalities as part of a plan to reinforce political control for its civilian supporters upon an eventual return to democracy. Indeed, there is compelling evidence that the Chilean regime repressed with the continued backing of its civilian supporters in mind (Esberg 2018). I evaluate this possibility by examining the repression in terms of the electoral rules and districts the regime designed in the 1980s, and find no evidence that repression

occurred in areas which could have affected national-level electoral outcomes.[4]

I evaluate a comparable alternative explanation for the targeting of non-dissidents: the regime could have intended to target these victims based on a plan to repress civilians. Inferring the regime's objectives for targeting requires studying incidents of repression in which its top leadership authorized violence with no information problem. I use historical data on a military unit which the regime sent to kill prisoners—the Caravan of Death—and find that the Caravan killed dissident prisoners but not non-dissident prisoners in the municipalities it visited. I also consider that some dissident victims turned themselves in (Constable and Valenzuela 1993) and evaluate differing processes of urban and rural repression.

This paper makes four main contributions. First, scholars often emphasize regime-based explanations for variation in repression, expecting state violence to vary with regime characteristics such as the ability of its security forces to project force and carry out operations (Bautista et al. 2018, Thomson 2017). Furthermore, existing arguments emphasize that the targeting of repression corresponds to the efficiency or compliance of regime agents, as agents who lack incentives or institutional tools are less likely to align their targets with the intentions of their regime principals (Gregory 2009, Greitens 2016). By considering political rivalry, I advance a population-based explanation of how coercion varies even when regime attributes are held constant. Such a focus on variation in population characteristics is consistent with work on local political milieus (Kopstein and Wittenberg 2011) or relational ties between the regime and the population (Carter and Hassan 2021, Klor, Saiegh and Satyanath 2020) that condition the activities of security forces.

Second, the paper specifies the population-based explanation for repression through the pathway of civilians' enablement of state violence. Civilians in the study of au-

[4]While premeditated strategies did shape repression in areas of Chile with the most regime supporters (Esberg 2018), such a strategy does not explain the increase in repression in areas of close competition.

thoritarian regimes tend to receive attention to the extent that they participate in anti-regime collective action, though recent work has turned its focus to the role of ordinary civilian regime supporters (Esberg 2018) and regime allies among the civilian economic elite (Klor, Saiegh and Satyanath 2020). By considering how regime supporters encourage repression, I highlight a distinct and understudied aspect of civilian agency in dictatorships (Fitzpatrick 2005, Gellatelly 1996). Furthermore, scholars who have studied civilian complicity in state violence detail both material and ideological incentives for the complicit (Bergemann 2017). This paper suggests that, in the absence of material inducements for civilian informants, ideological or other personal motivations likely drive their actions.

Third, considering civilian agency in regimes' use of coercion contributes to literature on preventive repression. Sullivan (2016a) observes that "governments may prefer to repress mobilization prior to the onset of overt, collective challenges, they often have trouble identifying the initial instances of mobilization" (651). Yet the sources of regimes' ability to identify dissidents, while amply documented in intelligence studies (Lowenthal 2009), remain under-theorized and tested in empirical political science. Recent literature, however, observes that increasingly sophisticated technology aids regimes in identifying potential mobilization and preventing its growth (Gohdes 2020, Xu 2020) in the rise of "digital authoritarianism" (Dragu and Lupu 2020). This paper considers an analog source of information for preventive repression—civilians—while also explaining variation in the quantity and quality of this information.

Fourth, the paper suggests a mutually reinforcing relationship between repression and polarization. Recent scholarship has shown that experiences of repression affect the salience and nature of group identification (Blaydes 2018, Nugent 2020), with the particular consequence of increasing social and political polarization between the repressed group and other, less-repressed social groups. This paper, at least in the case of Chile, shows a reverse process: highly-polarized environments in which groups

are in close competition for political power create conditions favorable for repression. The resulting cycles of polarization and repression suggest deep, intractable roots of political cleavages in authoritarian regimes, imperiling the prospects of transitions to democracy, and make democracy more fragile if transition occurs.

3.2 Popular Threats, Repression, and Political Competition

A restive population, which may overthrow a dictatorship in a mass uprising, threatens a regime's survival. Among the people who mobilize a restive population are dissidents, or those who "plan, publicize and initiate anti-regime protest" (Lawrence 2017, 701). Dissidents carry out mobilization while embedded in a population: they have shared loyalties with local civilians who possess anti-regime attitudes. These shared loyalties are characteristics or organizational affiliations between dissidents and ordinary civilians, such as membership in the same ethnic group, social class, workplace, or religious identification. Unlike dissidents, however, civilian regime opponents generally do not exhibit anti-regime behavior absent large-scale popular mobilization such as protests. Dissidents mobilize civilian regime opponents by leveraging social ties, organizations, and communication technology (Clarke 2014). In turn, dissidents rely on civilian regime opponents to conceal mobilization activities, which are most effective when covert (Amat 2018, Sullivan 2016b).

By mobilizing groups of civilians for overt collective action, dissidents threaten a dictatorship's control over the population. Dictatorships turn to repression to maintain or reestablish control when other strategies to counter dissidents, such as cooptation, are ineffective (Thomson 2017).[5] Repression accomplishes this objective when it levies physical sanctions against dissidents embedded in the population, undercutting

[5]Repression imposes control by raising the costs of collective action against the regime.

their covert efforts to organize overt, public resistance to a regime (Sullivan 2016b). This repression occurs preventively, before dissidents can mobilize such public resistance (Ritter and Conrad 2016). At the same time, regimes prefer not to repress the civilians whom dissidents organize to avoid backlash; mobilization grows when civilians learn of the regime's harshness and react angrily to violence against the population (Pearlman 2018).

When carrying out preventive repression, regimes must choose how to allocate repression across their territory. Given the risk of popular threats, the regime should condition the frequency of repression on the geographic location of dissidents who compose the threat. In targeting dissidents for repression, the dictatorship deters their mobilization efforts and thereby prevents dissidents' followers from protesting in the future (Sullivan 2016b). Yet a regime faces constraints on time and resources: it must set priorities, increasing the frequency of repression in certain locations of popular threat over others (Blaydes 2018, Thomson 2017).

Opposition strongholds are areas of popular threat in which a regime prefers to repress more frequently. Here dissidents have the highest concentration of civilian regime opponents to mobilize for protest. However, these strongholds are difficult areas for the regime to exert control (Carter and Hassan 2021, Letsa 2019). A regime has limited ability to repress in opposition strongholds because information about local dissidents is scarce. Opposition areas provide less information, as shared loyalties between dissidents and civilian regime opponents make these civilians unwilling to reveal dissidents' identities, locations, or movements to the repressive apparatus (Greitens 2016, Hassan 2017). As a result, dissidents become indistinguishable from the civilian population.

Dictatorships are particularly concerned about distinguishing dissidents from civilians such that dissidents can be repressed. Blaydes (2018) calls this the "legibility" problem (49). French counterinsurgency theory, which influenced repression in many

20th-century dictatorships (Policzer 2009, 51), elaborates: "in modern warfare, the enemy is far more difficult to identify. No physical frontier separates the two camps...it is a non-physical, often ideological boundary, which must however be expressly delineated if we want to reach the adversary and defeat him" (Trinquier 1985, 26). If the regime fails to delineate this boundary, instead repressing civilians, it leaves dissidents untouched while inflaming popular threat (Blaydes 2018, Rozenas, Schutte and Zhukov 2017, Sullivan 2016b).

Regimes often have institutional tools to solve the legibility problem, such as intelligence agencies (Greitens 2016). However, areas of close political competition between regime supporters and opponents can also solve the legibility problem. In these areas, civilians provide more information to the regime on potential targets of repression: close political competition between civilian groups heightens political rivalries, which in turn motivate regime supporters to provide information about opponents who security forces should target for repression. Studies of historical dictatorships suggest regime supporters provide information to encourage repression of those in the immediate area, including dissidents (Klor, Saiegh and Satyanath 2020) and civilian political rivals (Bergemann 2017, Gellatelly 1996). A similar process occurs in civil conflict: heightened political rivalries in areas of close political competition motivate civilians to share information with security forces and participate in local acts of violence (Balcells 2010, 2017, Kalyvas 2006).

Close political competition generates information-sharing through both rational and emotional incentives. When rational incentives motivate regime supporters, they "take into consideration the effects of violence for the future of their locality" and provide information to security forces to reduce regime opponents' influence and secure future electoral gains (Balcells 2010, 298). The use of force against regime opponents in competitive areas alters the balance of electoral power in favor of regime supporters (Collier and Vicente 2013). Emotional incentives also motivate regime supporters

to share information. Close electoral competition sharpens identification with civilians' political in-group, and decreases their identification with political opponents (Eifert, Miguel and Posner 2010). Strengthening in-group identification raises "status passions"—emotions of pride, envy, or spite—which create desires to protect the in-group and harm opponents (McClendon 2018, 12). Regime supporters convert these emotions into political action (Valentino et al. 2011) by providing information the regime needs to repress more possible targets. The first hypothesis follows:

Frequency Hypothesis: *A dictatorship facing popular threat represses more frequently in areas with close political competition between regime supporters and opponents, compared to areas without close political competition.*

Yet regime supporters do not share the regime's objectives for whom repression should target, creating a principal-agent problem between the regime and civilian informers (Bergemann 2017). Civilians are unreliable repressive agents, providing information which satisfies their grievances rather than the regime's objectives (Fitzpatrick 2005, Gellatelly 1996). In an environment of close competition, regime supporters have grievances with *any* regime opponent in the population. Targeting these opponents would change the balance of future electoral power and inflict harm on a desired target. On the other hand, the regime prefers to target dissidents. Repressing dissidents prevents them from mobilizing opposed civilians, whereas repressing civilians may inflame popular threat. Information from regime supporters leads the security forces to target more non-dissidents. The second hypothesis follows:

Targeting Hypothesis: *A dictatorship facing popular threat targets more non-dissidents in areas with close political competition between regime supporters and opponents, compared to areas without close political competition.*

Here it is important to consider the scope conditions of the argument. The link between political competition and repression first depends on rivalries being sufficiently intense that civilians are willing to inflict harm, and potentially death, on political opponents (Bergemann 2017). Rivalries' intensity varies over time and space. In electoral autocracies, for example, intensity may increase leading up to and surrounding the vote, as opposition parties mobilize and the incumbent represses the opposition or lays the groundwork for electoral manipulation (Bunce and Wolchik 2010, Rauschenbach and Paula 2019). In non-electoral autocracies, rivalries intensify around periods of opposition mobilization which follow a "calendar" of coordinated dissent and preventive repression (Truex 2019). More generally, rivalries increase when political power is contested: the prospect of regime transition heightens the salience of ideology for civilians, offering the regime's civilian supporters the opportunity to protect or solidify the regime's hold on power by providing information on regime opponents.

Not only must rivalries be intense, civilians seeking to provide information on regime opponents must also encounter security forces willing to repress dissidents and prevent mobilization. More liberalized regimes which "have loosened restrictions on speech and association, creating the conditions for a larger and more diverse organizational ecology" (Berman 2020, 4) are less likely to repress preventively or give security forces license to kill suspected dissidents. Regimes may also pursue other policy responses to potential mobilization, such as co-opting dissidents or otherwise using material inducements to prevent future unrest (Thomson 2017). Even if regimes seek to repress dissidents, they may substitute kinetic force with "less visible, low-intensity repression" (Xu 2020, 5) such as censorship. Whether partisan rivalries generate demands from regime supporters for low-intensity repression is beyond this paper's scope, though it is plausible that censoring opposing views is among supporters' preferences (Esberg 2020*b*).

Finally, regimes with limited information about dissidents are more likely to rely on civilians' information provision to select targets of repression. When regimes' security forces can coordinate operations, they are better equipped to independently collect and analyze intelligence and target dissidents (Greitens 2016). Furthermore, well-coordinated security forces tend to recruit and incentivize civilian informers with "monetary rewards and career advancement" which do not depend on local characteristics such as political competition (Bergemann 2017, 5). Therefore, it is uncoordinated security forces lacking incentive-based information provision which rely most on information from civilians self-motivated by partisan rivalries relative to other sources. In these cases, political competition will most relate to the frequency and targeting of repression.

The following section introduces an empirical case which fits these scope conditions. Chile in 1973 experienced intense partisan rivalries, a new military regime prioritizing preventive repression, and security forces beset with coordination and information problems.

3.3 Research Design

3.3.1 The Military Coup and Civilian Collaboration in Chile, 1973

The hypotheses are tested in Chile during the turbulent first months of its military dictatorship which ruled from 1973 to 1990. The prelude to the dictatorship began when Salvador Allende—a socialist running under a left-wing coalition called Popular Unity (UP)—won a plurality in 1970 presidential elections. Allende's redistributive economic policies, including land reform and nationalization of industry, generated support from organized labor and the working class, while drawing opposition from the middle and upper classes (Esberg 2018). His election particularly unnerved the

Chilean military. With its links to the United States' Cold War national security program, the military had a strong anticommunist bent (Policzer 2009, 49). Ardent anticommunism led top ranks of the armed forces to believe that intervention in politics was necessary to contain leftist movements.

Chile's political situation destabilized by 1972. Despite economic growth in the first year of the Allende government, copper prices collapsed—limiting Chile's primary commodity export—and strikes occurred against land redistribution programs. Allende's legislative coalition partner, the centrist Christian Democrats, defected and joined the right-wing National Party in a new coalition named the Confederation of Democracy (CODE). Allende became politically isolated, and CODE defeated the UP in March 1973 legislative elections (San Francisco 2018). By the middle of the year, CODE called for a military intervention to transfer power from Allende to themselves. In August, military hardliners forced commander-in-chief Carlos Prats, who opposed intervention, to resign. Political elites and the public thus divided on the question of whether Allende should stay in power.

From these divisions, political rivalries between Allende's civilian supporters and opponents intensified throughout 1973. Valdés (2012) describes how the months leading up to September contained "confrontations among the people" and revealed "a society clearly polarized in which no middle existed" (168-169). Furthermore, there were "conflicts and fights, including within families: parents and children" (169). One civilian noted the collective "psychosis and panic" of 1973, and that "in our hearts we were all at war" (Constable and Valenzuela 1993, 28). Civilians, such as those in far-right paramilitary groups, began to consider the possibility of armed conflict between supporters and opponents of the Allende government (Bautista 2015). One group, Fatherland and Liberty, stockpiled weapons in anticipation of targeting Allende supporters (Grugel 1985, Salazar 2011).[6]

[6]Fatherland and Liberty was a fascist political organization which agitated against foreigners and Marxists while attempting to undermine democracy (Grugel 1985).

The climactic moment came when the military staged a coup on September 11, deposing Allende with the backing of UP's partisan rivals. The new regime, led by General Augusto Pinochet, declared a countrywide state of siege with its Decree Law No. 3. On September 12, the regime issued Decree Law No. 5, which declared "the need to prevent and severely punish, with the greatest speed, crimes committed against internal security" (OAS 1974). The regime imposed a strict curfew, restricted public gatherings, and targeted dissidents—officials and supporters of the Allende government—for repression. Amat (2018, 31) observes that the regime based its repression on "lists of known individuals" composed of "leaders of left-wing political parties, trade unions, and political activists of the left" (Bautista et al. 2018, 5) and "people linked to...revolution" (Valdés 2012, 179). Members of the Chilean security forces sought out suspected dissidents with the objective of "preventing a resurgence of [dissident] group political activity" (Remmer 1980, 285).

Evading the security forces, many dissidents went underground after the coup: "those who supported the [Allende] government [did not go home]. They did not want to accept the new situation, and began to defend the government" (Valdés 2012, 177). Information on their locations, activities, and movements was therefore difficult to gather. Seguel (2020) notes that "repression in the first weeks [after the coup]...was not conducted under conditions of strategic intelligence" (777). Furthermore, security forces struggled to coordinate operations and share information (Policzer 2009, Remmer 1980). To fill the resulting intelligence gaps, civilian regime supporters aided in the identification and location of dissidents.

Ample qualitative evidence supports that civilians provided information to the regime. Chile's Truth and Reconciliation Commission notes "civilians were involved in pointing out who these 'conflictive' people were...and a smaller number provided help for the arrests in the form of vehicles or interrogation sites or were even involved in executions" (162). In the Mulchén municipality, where UP received 49% of the March

1973 vote, "military and civilians of Mulchén went to...farms, carrying a previously prepared list of people who had to be detained and who were subsequently killed" (Museum of Memory and Human Rights 2018). Other evidence from the commission describes civilians providing information:

> "In late 1973 some people who had power settled accounts with others who did not. Those who did so were not only, nor indeed primarily, the military, but also and perhaps mostly civilians who at that moment through their authority, friendship, false accusations, or political intrigue were in a position to utilize the power of weapons for their own purposes" (164-165).

Smith (1982) makes a similar observation:

> "After the events of 11 September 1973 many people took advantage of the situation to denounce their own neighbors who had lived in the area for years, and who had expressed sympathies or identification with the Left. Others toasted the fall of the former government" (337).

Regime supporters also erased evidence of the Allende government, joining a campaign of "disinfection" in which ideological representations associated with leftist political movements were removed from public spaces (Errázuriz 2009, 141). The regime institutionalized civilian assistance by the end of 1973, forming a brigade of civilian informants inside the newly-created National Intelligence Directorate (Kornbluh 2003).

3.3.2 Addressing Two Threats to Inference

Studying Chile after its 1973 military coup also addresses two main threats to inference: reverse causality and separating types of repression. Reverse causality would be a threat if political competition in Chile was a result of repression. A measurement of political competition should therefore meet two criteria: it should be taken before the onset of repression, and the threat of future repression should not be credible when the measurement is taken. Otherwise, ongoing or anticipated repression would affect civilians' expressed preferences. Yet devising a measurement free of such effects is challenging. The threat of repression induces civilian regime opponents to

self-censor, creating incongruence between their public expression and their private preferences (Ritter and Conrad 2016). Additionally, repression affects civilians' attitudes towards other political factions (Nugent 2020), and regimes often repress before the public expression of opposition.

Chile in 1973 provides a measurement of political competition which meets the criteria: voting in the March 1973 legislative elections. If UP supporters—who were thus regime opponents—feared repression after the coup and self-censored before September 11, then this measure of political competition would not be free of repression's influence. However, the military made no credible threats of repression before the March 1973 elections.[7] Democratic institutions in Chile went uninterrupted between 1925 and 1973, and Chileans saw the armed forces as a "guarantee for the survival of democracy" rather than an institution that would undermine it (Navia and Osorio 2019, 197). An army chief stated the Chilean military's historical role: "the armed forces have a clear doctrine: military power is consciously subordinated to the political power" (Valenzuela 1978). Furthermore, Allende's legislative opposition supported the coup under the belief the military would defer to civilian rule: leaders of the Christian Democrats met with regime officers on October 10 to request a "prompt transfer of power" back to civilian government. (Policzer 2009, 55). Civilians, even coup supporters, did not anticipate the military's retention of power.

The case also separates preventive and responsive repression, allowing a test of hypotheses pertaining to preventive repression. It could be the case that the regime both repressed to prevent popular mobilization and in response to overt collective action such as protests. Using aggregate repression to measure preventive repression would therefore introduce systematic measurement error. An ideal measurement strategy would separate the two purposes of repression by identifying events in which repression intended to prevent future mobilization and those in which repression responded to

[7]The first credible sign the military posture would change was after the March 1973 elections. In the June 1973 Tanquetazo, an attempted coup by Chilean armored divisions.

overt collective action. While recent research has used this strategy (Sullivan 2016b), it requires a level of detail on individual repressive operations which repressive events data generally lack.

An alternative strategy is to leverage a case in which repression was only preventive. The Chilean case fits this strategy because there was not overt collective action after the coup to which the regime could have responded. The Truth and Reconciliation report observes that "within forty-eight hours [of the coup]...armed activity in Santiago and its region had come to a halt" (153), and Allende-sympathizing militants fled the country. The regime also did not seek to avenge past violence against the military, as militants did not target the armed forces before the coup (Barros 2002). The coup also undercut nonviolent mobilization, preventing overt collective action for most of the 1970s. Because the regime targeted dissidents in leftist parties and trade unions, these groups were unable to mobilize for years after the onset of repression (Remmer 1980, 290).

3.3.3 Data

The primary data source is an original list of 1,120 political killing victims between September and December 1973.[8] I collected these data through archival sources which documented political killings after the end of the dictatorship, drawing on the testimony of contemporaneous witnesses. The first archive is from Chile's Center for Research and Information and the University of Chile (Archivos-Chile 2012). The second archive is the database of the country's Museum of Memory and Human Rights (2018). Using these sources, I am able to identify the municipalities in which 1,091 of the 1,120 killings occurred. I then aggregate the data to the municipality level, of which there were 461 in 1973.[9]

[8] I select this period because it is a distinct phase in which the regime faced information constraints (Greitens 2016).

[9] Municipalities, called comunas, are Chile's lowest-level political division. As part of 1974 administrative reform under the dictatorship, the number of municipalities fell to 346. The data here

In many of these killings, the regime targeted dissidents (Barros 2002). For example, on October 11, members of the military kidnapped and executed union leader Máximo Neira Salas in the southern city of Talcahuano. Non-dissidents fell victim to repression, however, due to false accusations: one target reported "[she] was arrested in September 1973 and accused of having acted as a spy...[she] said that she had not taken part in any political activity" (OAS 1974, 29). Though arrest and torture were also widespread, data on precise locations and dates of their use are not publicly available.

3.3.4 Variables

The dependent variable of interest for the frequency hypotheses is the number of political killing victims, *Total Victims*, measured at the municipality level. To ascertain the municipality of each victim, I use the following rule: I code the killing as occurring in the municipality in which the repressive operation that resulted in the killing was carried out. For example, if the regime abducted a victim in municipality x and killed them in municipality y, the killing is coded as occurring in municipality x.[10] The archival sources often code the location of repression as the municipality of death, requiring verifying the location of the repressive operation itself from each incident description. Based on these descriptions, I create a count of the total number of political killing victims in each municipality.

The dependent variable of interest for the targeting hypothesis is the number of non-dissident victims, also measured at the municipality level. For this measurement, I simply identify the subset of victims in each municipality who were not dissidents. Using the classification scheme for victims' dissident status reported in Figure 2.1, I create a count variable labeled *Non-Dissident Victims*. The variable counts the

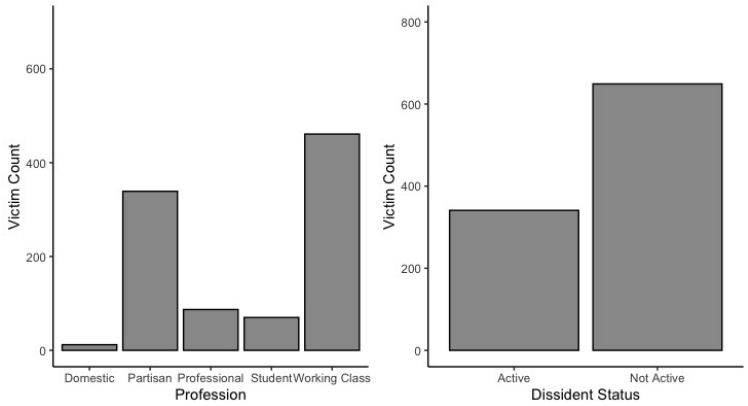
correspond to territorial divisions before the reform (FLACSO 1989).

[10]The information which led to the repression is more likely to have originated in municipality x than any other. Below, I account for violations of this assumption by restricting the sample to rural areas in which it is most likely to hold.

number of political killing victims in each municipality who were not active in a leftist party, militant group, or labor union—categories of dissidents described in Remmer (1980). Non-dissidents, therefore, include victims who were members of the working class, students, members of the professional class, and domestic workers. Because of concerns about measurement error, the robustness tests show results are consistent with both more and less restrictive classifications of non-dissidents.

Figure 2.1 shows descriptive statistics for victims' dissident status. Thirty-five percent of victims whose background is known were dissidents: active leftist party leaders, union leaders, or militants. Sixty-five percent of victims were non-dissidents: neither active members of left-wing party leadership nor of organized labor. These victims included workers, farmers, students, and members of white collar professions.

Figure 2.1: Political Killing Victim Characteristics



Note: Figure shows descriptive statistics from individual-level archival data on repression following the 1973 military coup. Victim category is on the x-axis and count of killing victims is on the y-axis.

The independent variable is an indicator of whether a municipality had close political competition between regime supporters and opponents. I operationalize whether a municipality has a close election through comparing electoral support for Salvador Allende's UP coalition with that of its political competitors. To do this, I create the *Close Election, 1973* indicator variable, which takes a value of 1 if a municipality had a close UP electoral margin, either winning or losing the election,

relative to non-UP vote share. For the main results, this margin is six percent (53%-47%) or less. All other electoral margins outside this range take a value of 0.[11] After the main results, I use as independent variables a series of similarly close electoral margins in which the results are consistent. Furthermore, Appendix B Tables 2.B.1, 2.B.2, and 2.B.3 show results using another close election variable measured similarly to Balcells (2010), an ordinal scale, and a continuous vote margin.

Figure 2.2 depicts patterns of repression and close elections in 1973. Each division in the map represents a municipality. The left panel displays variation in a dichotomized coding of the dependent variable: shaded municipalities experienced at least one political killing between September and December 1973. The right panel displays variation in the independent variable: shaded municipalities had close elections in March 1973. Overall, 30% of municipalities experienced at least one political killing, and 16% had a close election.

Despite addressing the threat of reverse causality, potential endogeneity concerns remain. That is, there are likely to be omitted variables which correlate with both political competition and the frequency or targeting of repression. To this end, I include several control variables based on plausible sources of omitted variable bias. First, it could be that political killings occur more frequently in municipalities with greater population which are also municipalities with close elections. To account for this, I include the logarithm of total votes in each municipality from 1970 elections and label the variable *Log (Total Votes, 1970)*.[12]

Political killings could also result from the regime repressing based on social identity group rather than political loyalty. Given socioeconomic class was a salient social cleavage in Chile in the 1970s (Valenzuela 1978), the regime may have repressed more frequently or targeted more non-dissidents in municipalities with a higher working class vote share and closer political competition. I measure a municipality's working

[11] Data are from the Facultad Latinoamerica de Ciencias Sociales (FLACSO).

[12] Total votes from a prior election are used given unavailable census data at the municipality level.

Figure 2.2: Repression and Close Elections in Chile, 1973

Note: Figure depicts geographic variation in the dependent and independent variables of interest. The left panel shows variation in 1973 political killings across Chile's municipalities. Highlighted municipalities experienced at least one political killing. The right panel shows variation in close elections. Highlighted municipalities had between 47% and 53% vote share for the UP in March 1973 legislative elections.

class composition through its 1972 infant mortality rate: a higher infant mortality rate correlates with lower average household income (Sims, Sims and Bruce 2007). I label the variable *Infant Mortality, 1972*.[13] If the regime targeted repression based on class there should be a positive relationship between infant mortality and frequency of political killing.

It could also be the case that repression varies based on the regime targeting

[13]Data are from Chile's Instituto Nacional de Estadísticas (1973).

areas of past social conflict and contention, and that these areas are associated with 1973 political competition. Chile's capital region, Santiago, was a center of social conflict in the years before the 1973 military coup. Because Santiago is the most densely urbanized, industrialized region in Chile, municipalities around the capital were centers of working-class mobilization (Valenzuela 1978). The capital was also one of the most politically divided and electorally contested regions of the country (FLACSO 1989). I create the variable *Santiago*, and code it 1 for municipalities in the Santiago region and as 0 for municipalities not in the region.[14]

Finally, municipalities with army bases could introduce omitted variable bias. These bases increased the regime's capacity to carry out repressive operations and also affected local political preferences (Bautista et al. 2018). I create the variable *Army Base* and code it 1 for municipalities with an army base and as 0 for municipalities without an army base.

3.3.5 Estimation Strategy

To test the frequency hypothesis, I estimate the relationship between close electoral competition and the number of political killing victims in municipality i using linear regression with robust standard errors. A count model specification is described in the robustness tests. The cross-sectional estimating equation takes the following form, where X_i is a vector of covariates for a given municipality i.

$$\textit{Political Killing Victims}_i = \alpha + \beta \cdot \textit{Close Election, 1973}_i + \delta \cdot X_i + \epsilon_i$$

The coefficient β in the regression estimates the expected difference in the number of political killing victims between a municipality with close political competition and a municipality without close political competition. I specify a baseline model with

[14] Appendix B Table 2.B.13 also reports results from specifications with region dummies.

the independent variable of interest only, *Close Election, 1973*, as well as a model with covariates *Log Total Votes 1970*, *Infant Mortality, 1972*, *Santiago*, and *Army Base*.

For the targeting hypothesis, I estimate the relationship between close elections and non-dissident victims in municipality i using linear regression with robust standard errors.

$$\textit{Non-Dissident Victims}_i = \alpha + \beta \cdot \textit{Close Election, 1973}_i + \delta \cdot X_i + \epsilon_i$$

The coefficient β estimates the expected difference in the number of non-dissident victims between a municipality with close political competition and a municipality without close political competition. Model specifications are otherwise identical to those for the test of the frequency hypothesis.

3.4 Analysis

3.4.1 Main Results

Table 2.1 displays results for tests of the two hypotheses. The table's first and second columns report coefficient estimates for tests of the frequency hypothesis. The third and fourth columns report estimates for tests of the targeting hypothesis. The results show a clear pattern: municipalities with close elections in 1973 witnessed an increase in political killings and killings of non-dissidents compared to municipalities without close elections. Coefficient estimates for the *Close Election, 1973* variable are significant at the $p = 0.05$ level in both specifications for total victims. Estimates are also substantively significant: a municipality with a close election corresponds to an increase in political killing victims of between two and three compared to a municipality without a close election. The magnitude of the estimates is more than

one-fourth of a standard deviation of the total victims variable. For non-dissident victims, a municipality with a close election corresponds to an increase of between one and two compared to municipalities without close elections. The magnitude of the estimates is about one-fifth of a standard deviation of the non-dissident victims variable. Figure 2.3 visually depicts the estimates in a coefficient plot.

Table 2.1: Political Killing Victims by Municipality Electoral Characteristics

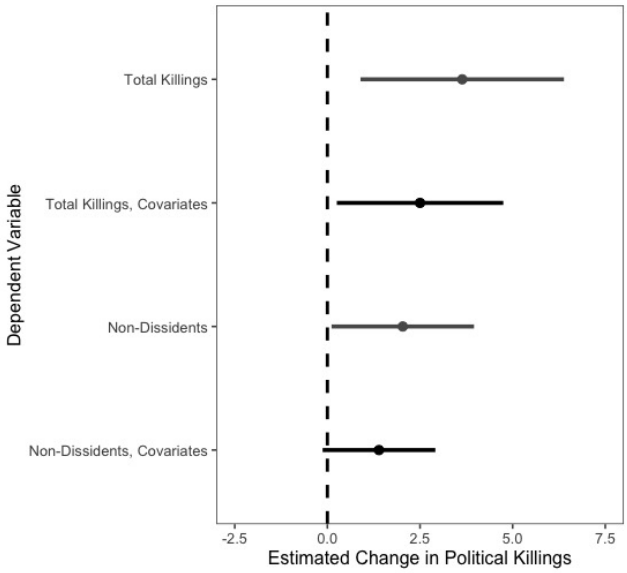
	Dependent variable:			
	Total Victims		Non-Dissident Victims	
	(1)	(2)	(3)	(4)
Close Election, 1973	3.64^{**}	2.49^{*}	2.03^{*}	$1.39^{\dagger}$
	(1.40)	(1.14)	(0.98)	(0.78)
Infant Mortality, 1972		0.03		0.02
		(0.02)		(0.01)
Log(Total Votes, 1970)		1.37^{**}		0.78^{**}
		(0.46)		(0.27)
Santiago		11.15^{***}		8.21^{***}
		(2.25)		(1.57)
Army Base		5.84^{*}		1.01
		(2.29)		(1.18)
(Intercept)	1.82^{***}	-10.13^{**}	1.08^{***}	-5.88^{**}
	(0.39)	(3.39)	(0.25)	(2.02)
Adj. R^2	0.02	0.34	0.02	0.34
Observations	453	453	453	453

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$,$^{\dagger}p < 0.1$

3.4.2 Robustness Tests

To increase confidence in the results, I conduct several robustness tests. First, since the dependent variables of interest are count variables, I estimate a series of negative binomial regression models. I estimate two model specifications with each dependent

Figure 2.3: Political Competition and the Frequency and Targeting of Repression

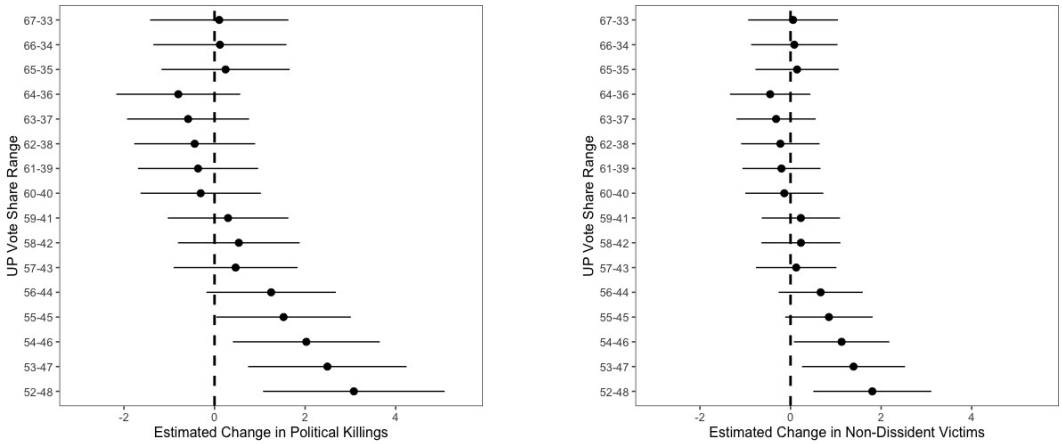


Note: Figure depicts the expected change in political killing victims between close election and non-close election municipalities and the expected change in non-dissident victims between close election and non-close election municipalities. 95% confidence intervals are shown. Model specifications are on the y-axis.

variable—total killings and non-dissident victims—as in the main analysis, one with the independent variable alone and one including covariates *Log Total Votes, 1970*, *Infant Mortality Rate, 1972*, *Santiago*, and *Army Base*. The coefficient estimates for *Close Election, 1973* are increased in statistical significance. Full results are in Appendix B Table 2.B.4.

Next, I consider measurement error. The results for the targeting hypothesis—non-dissident victims—could be sensitive to the classification of dissidents and non-dissidents. Determining who constitutes a dissident is fraught empirically (Esberg 2018). *Non-Dissident Victims* includes all victims who were not members of leftist parties, militant groups, and labor unions. To test the sensitivity of this operationalization, I estimate model specifications first with a more restrictive non-dissident classification and then with a loosened one. In the more restrictive classification, I reclassify students from non-dissidents to dissidents. Many student victims, particularly those in universities, may have been activists. In the loosened classification,

Figure 2.4: Varying Close Election Thresholds



Note: Figure depicts the expected change in political killing victims between close election and non-close election municipalities across widening UP support thresholds defining a close election. 95% confidence intervals are formed from robust standard errors. UP vote ranges defining close elections are on the y-axis.

I reclassify rank-and-file leftist party members from dissidents to non-dissidents: all victims except union and party leadership and members of violent militant groups are classified as non-dissidents. Under both classification schemes, results—reported in Appendix B Table 2.B.5—remain consistent.

It is also important to test whether the main results are sensitive to the threshold for measuring a close election. The concept underlying this variable is political competition close enough that repression of regime opposition could affect the future electoral victor or close enough to trigger more intense negative emotions. Therefore, if the measurement of close elections widens to include larger margins, the main results should weaken. Model specifications, reported in Appendix B Tables 2.B.6, 2.B.7, 2.B.8, and 2.B.9, vary the close election variable by incrementally widening the threshold separating close election municipalities and non-close election municipalities. Figure 2.4 reveals a pattern: the estimated increase in political killings in close-election municipalities approaches zero as the close election threshold widens.[15]

[15]In Appendix B Tables 2.B.1, 2.B.2, and 2.B.3, I present additional tests which use a measure based on Balcells (2010), an ordinal scale, and a continuous Popular Unity electoral margin as the main explanatory variable.

Another concern is that the sample of political killings could include responsive, rather than preventive, repression. In particular, the military did not gain "complete control" of the country until late afternoon on September 11, and met isolated armed resistance for several days (Constable and Valenzuela 1993, 19). Since the data include killings beginning on the day of the coup, September 11, it could be that killings which occurred as the military fought for initial control explain the results if they occurred in close-election municipalities. To address this possibility, I take a subset of the data excluding killings on September 11 and 12. The models are re-estimated with no change in the significance of the close election coefficient estimates. Full results are in Appendix B Table 2.B.10.[16]

Finally, because there remain unobserved variables which could confound the relationship between political competition and repression, I perform a sensitivity analysis. The analysis specifies the amount of correlation between an omitted variable and both the explanatory variable and outcome of interest needed to invalidate significant coefficient estimates for the explanatory variable. To invalidate the main results, an omitted variable would need to have an impact more than three times greater than the relationship between the logarithm of 1970 votes and number of political killings. Appendix B Tables 2.B.11 and 2.B.12 report results.

3.5 Testing the Partisan Rivalry Mechanism

There is robust evidence that more political killings and killings of non-dissidents occurred in municipalities of close political competition between Popular Unity and its opponents. The argument posits that partisan rivalry explains these findings, driving civilians to provide more information on potential targets of repression in areas of close political competition. If the partisan rivalry mechanism is correct,

[16]This test also rules out dissidents who may have turned themselves in on the day of or after the coup, a possibility tested directly under Alternative Explanations.

three additional implications should follow.

The first implication pertains to the areas in which information from civilians is most useful to the regime. The theory suggests that, in Chile, civilian supporters of the regime provided more information to the military in municipalities of close political competition, who used this information to select targets of repression. However, authoritarian regimes, in general, can have the military itself "uncover and pre-empt" its targets (Greitens 2016, 201). In municipalities with army bases, the Chilean regime had such capacity to uncover targets: Bautista et al. (2018) argue that Chilean military bases increased local civilians' "interaction with... military personnel" and that the cost of military action "increase[d] with geographical distance" from a military base (14). If the regime relied more on supporters' information away from bases, then the relationship between close elections and repression should remain significant there but attenuate in municipalities with bases.

To test this implication, I interact the *Army Base* variable with the *Close Election, 1973* variable. Close elections should retain a positive and significant relationship with political killings in municipalities without army bases, but this relationship should attenuate in municipalities with army bases. That is, the total estimate for close elections in municipalities with bases should be indistinguishable from zero. Results for the test, reported in Appendix B Table 2.C.1, are consistent with expectations: close elections correlate more strongly with killings in municipalities without army bases than municipalities with bases.

Another implication of the partisan rivalry mechanism pertains to conditions under which civilians are most likely to provide information the regime needs for repression. Larson (2016) shows that the interaction between rival groups—particularly short interactions at gatherings before the onset of intergroup conflict—can increase future violence. This is because members of one group who are poorly connected to their fellow group members present a "tempting target" for rivals who interact with

them at such gatherings (Larson 2016, 469). A type of gathering which generates short interactions and "weak ties" between members of different groups is weddings (Srinivasan, Guo and Devaraj 2017, 1070). More weddings thus create more opportunities and temptations for information provision about rival group members. The relationship between close elections on political killings, therefore, should be stronger where more weddings occurred soon before the Chilean military coup.

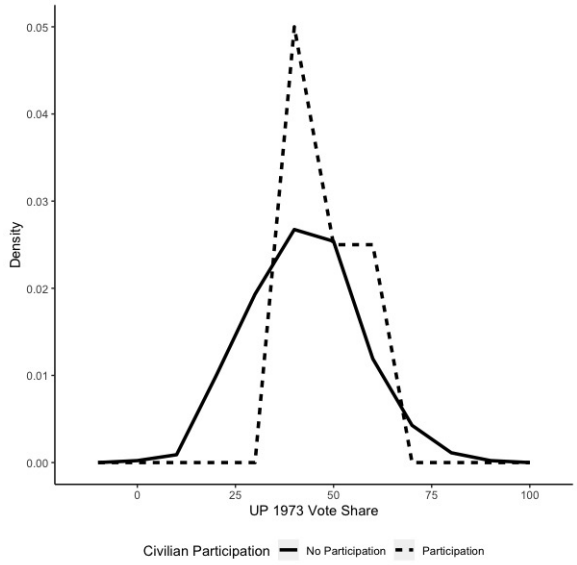
To test this implication, I create a new variable, *Log(Marriages, 1972)*, which takes the logarithm of the frequency of marriages performed in each municipality in 1972 and interact it with the close elections indicator.[17] Religiosity is a potential confounding variable in an analysis with marriages and repression, so I also include as a covariate the proportion of Catholics measured at the diocese level. Results for the test of this implication are consistent with the expectation: more marriages in a municipality strengthens the relationship of close elections and political killings. Full results are in Appendix B Table 2.C.2.[18]

Finally, another implication for civilian behavior which follows from partisan rivalries is participation in repression. That is, civilians' desire to affect the balance of electoral competition or to inflict harm on opponents in areas of close political competition should also incentivize participation in repression. Chile's Truth and Reconciliation report identifies eight municipalities in which civilians aided killings. The mean vote share for Popular Unity in these eight municipalities was 48.15%, while its mean vote share in other municipalities was 42.63%. A difference in means test—reported in Appendix B Table 2.C.4—returns a p-value of 0.058, close to rejecting the null that elections are not closer in municipalities with civilian participation in repression. Another way to test this claim is to compare the distribution of Popular

[17]Marriage data are from the Instituto Nacional de Estadísticas (1973).

[18]In Appendix B, I also test an implication from literature on the long-run effects of Chilean repression: violence increased voting against the regime in the country's 1988 plebiscite (Bautista et al. 2018). Appendix B Table 2.C.3 shows municipalities with close elections in 1973 had more anti-regime vote share in the plebiscite.

Figure 2.5: Civilian Participation in Repression and UP Vote



Note: Figure depicts density plots for Popular Unity 1973 vote share for municipalities mentioned in the Truth and Reconciliation report as having civilian participation in repression and municipalities which were not mentioned. x-axis depicts vote share and y-axis depicts density.

Unity vote share in municipalities with and without civilian participation in repression. Figure 2.5 shows density plots for these two groups. A Komolgorov-Smirnov test comparing the distributions returns a p-value of 0.120, likewise close to rejecting the null.

3.6 Alternative Explanations

3.6.1 Regime Strategy

It is possible that the main results are explained not by partisan rivalries but by a regime strategy to increase repression and target more non-dissidents in areas of close political competition. One reason for such a strategy could be the intent to alter future electoral power in favor of regime supporters. In a dictatorship which intends to permit elections, a position held by some members of the Chilean regime after the coup (Barros 2002), repression would reduce the need to commit future fraud or

violence to win the election.

To evaluate this possibility, I first aggregate the municipality-level data to the electoral district level and examine district vote margins. The military created 60 such districts, each of which began electing two deputies to Chile's National Congress during the transition to democracy. If the regime repressed to shore up its performance in future elections, there would be more frequent political killings in *electoral districts* in which the regime could prevent the left from electing congressional deputies. Under the unique D'Hondt method the regime later designed, the top two vote-getters in each district receive one seat, and the top finisher must win twice as many votes as the second-place finisher to win both seats. However, given two-party competition, no electoral districts had UP vote margins within ten percent of the threshold to lose or gain a seat under the method.[19] Appendix B Tables 2.D.1 and 2.D.2 report results from a test examining electoral districts with close competition in 1973 under the same operationalization as at the municipality level. There is no significant relationship between close competition and repression at the district level.

Ruling out regime strategy as an explanation for the targeting results requires showing that the regime preferred to target dissidents, not non-dissidents. This requires inferring the regime's preferences by identifying who it repressed when not facing an information problem about its targets. To make this inference, I examine the killings of specific prisoners that the regime directly ordered after the coup. To carry out these orders, General Sergio Arellano Stark created the Caravan of Death, a military unit tasked with "accelerating and making more severe the process for prisoners" (Escalante 2000, 26). If the theory is correct and dissidents were the regime's preferred targets, the municipalities the Caravan of Death visited should have a greater number of dissident victims than municipalities it did not visit. Furthermore, these same municipalities should not have more non-dissident victims.

[19] Appendix B Figure 2.D.1 shows the distribution of district-level vote margins.

To test the implication, I create the indicator variable *Caravan of Death*, and code a municipality as 1 if the Caravan of Death visited.[20] The first dependent variable of interest for this test is *Dissident Victims*, which counts the total number of political killing victims identified as a member of a leftist political party, militant group, or labor union. The second dependent variable of interest is *Non-Dissident Victims*, from the main test of the targeting hypothesis. This includes working class, student, professional class, and domestic worker victims. I estimate linear and negative binomial regression models, including specifications with and without covariates. In specifications with dissident victims as the outcome, the coefficient estimates are positive and significant. In specifications with non-dissident victims as the outcome, the coefficient estimates are not significant, consistent with rejecting the regime strategy explanation. Results are in Appendix B Tables 2.D.3 and 2.D.4.

3.6.2 Different Victim Behaviors

Another alternative explanation is that partisan rivalries and information provision did not contribute to repression, which was instead determined by other victim—whether civilian or dissident—behaviors. The first such process is dissidents potentially becoming "legible" to the regime through their own actions. In the Chilean context, high-profile dissidents—such as UP officials and leaders—may have turned themselves in soon after the military coup, responding to broadcasts of "lists of political figures who should report for questioning" (Constable and Valenzuela 1993, 30) and believing themselves safe from physical harm (Esberg 2020*a*). Some centrist Christian Democratic politicians were also summoned. To account for this alternative process of repression, which would place such dissident leaders outside the scope of the analysis, I estimate the relationship between political competition and repression on a sample excluding 167 leftist and centrist party officials and leaders—only 15%

[20] Data are from Escalante (2000).

of the victims—from the sample. Results are in Appendix B Table 2.E.1.

Second, it could be the case that repression in urban municipalities occurred through a different process than in rural ones. In particular, city-dwelling victims could have been repressed in different municipalities than those in which they lived or worked. Especially in the capital of Santiago, municipalities are tightly clustered and travel between them for routine activities is common. Furthermore, Seguel (2020) notes civilian assistance in repression occurred mainly in rural areas (777). To account for the potentially different process of repression in urban municipalities due to different travel patterns, I estimate the relationship between political competition on two reduced samples: one excluding municipalities in the metropolitan region of Santiago, and another excluding urban municipalities generally. Results, in Appendix B Tables 2.E.2 and 2.E.3, are robust across three-fourths of these specifications.

3.7 Discussion

To study the relationship between political competition and repression, I have examined the state violence which followed Chile's 1973 military coup. Historical accounts and quantitative tests suggest that close political competition—heightening partisan rivalries among civilians—corresponded with a greater frequency of repression. Repression also targeted more non-dissidents in areas with close elections compared to areas without close elections. Tests showing army bases and marriage rates condition the relationship close elections with repression strengthen support for the partisan rivalry mechanism. I find little support for the alternative explanations that repression in those areas was a result of regime strategy or different victim behavior. The link between political competition and repression demonstrates that population-based characteristics shape the frequency and targeting of preventive repression, particularly when dissidents are concealed among civilians.

The evidence comes from a Cold War-era military dictatorship in Latin America, but the argument that civilians provide information to the regime—and thereby affect repression—likely applies to other regimes and time periods. Scholars have documented the role of civilians in stimulating violence in party-based dictatorships (Fitzpatrick 2005, Gellatelly 1996), hereditary monarchies (Bergemann 2017), and in civil conflict (Balcells 2017, Kalyvas 2006). The breadth of these findings suggests that civilian agency in violence is a widespread and enduring feature of politics. When presented with an opportunity, such as a military coup, political rivalries turn emotion and dislike (Valentino et al. 2011) into behavior which inflicts violence on opponents. While political competition is one source of rivalries, an emerging research agenda suggests repression itself inflames intergroup rivalries (Blaydes 2018, Nugent 2020). Taken together, these findings point to rivalries and repression as mutually reinforcing.

The findings also correspond to a growing focus on non-state actors in the study of authoritarian repression. While a preponderance of scholarship considers regime type, institutions, and principal-agent relationships to explain variation in repression (Davenport 2007b, Gregory 2009, Greitens 2016, Svolik 2012), recent work suggests that looking beyond the regime at non-state actors, such as civilians, and their preferences reveals new explanations for state violence (Carter and Hassan 2021, Esberg 2018). Integrating civilian agency—outside of protest—into the study of repression aligns this literature with others that consider non-protest forms of civilian behavior in dictatorships, such as voting and propagandizing.

Finally, future research should expand on the links this paper suggests between the microdynamics of civil conflict and the of turbulent periods around military coups in dictatorships. Civilian agency is a well-documented process in conflict settings (Balcells 2017, Kalyvas 2006), and civilian support is critical to the survival and success of armed groups. This paper has shown one aspect of theories of civilian

agency in conflict that have analogs in authoritarian repression. Chile's military regime took power at a time of domestic political, social, and economic turmoil. Rivalries among the public were intense, repression was the military's top priority, and intelligence was limited. For these reasons, the regime is a most likely case for studying civilian agency in repression. Identifying other dictatorships in which civilian agency in state violence might also be operative, and in those cases examining both civilian supporters' information provision and regime-opposing civilians' efforts to shelter dissidents, are fruitful subjects for future scholarship.

Chapter 4

Protest in the Shadow of Repression and Extremist Violence: Backlash, Incitement, and the Anticipation of Conflict

4.1 Introduction

Nonviolent anti-regime protest is considered an effective method to achieve political objectives, up to and including overthrowing authoritarian regimes. In particular, there is evidence that when a regime represses peaceful protest, the repression leads to *backlash* protests. Stephan and Chenoweth (2008) argue backlash "leads to power shifts by increasing the internal solidarity of the resistance campaign, creating...conflicts among the [regime]'s supporters, increasing external support for the resistance campaign, and decreasing external support for the [regime]" (11). Protests are also effective because they cross boundaries of formal organizations and develop through networks (Steinert-Threlkeld 2017, Thomson 2018), challenging the regime's

ability to contain them. Pearlman (2018) explains how protests affect onlookers' moral identity and increase their desire to express distaste for a regime through protest. Despite these many advantages to nonviolent protest, there remain few explanations for the conditions under which it grows even in the shadow of violence.

This paper examines dissidents' choice to participate in nonviolent protest under two shadows of violence common in authoritarian regimes: repression and the presence of violent revolutionary movements. In particular, it investigates how the presence of violent revolutionary movements interacts with the threat of repression to either constrain or increase participation in nonviolent resistance. The first shadow of violence, repression, is state coercion which increases dissidents' costs for protesting (Tilly 1978) and decreases dissidents' beliefs that other dissidents will protest alongside them (Young 2019). Repression of observed peaceful protests makes dissidents self-censor, meaning observed nonviolent dissent has selected into protest despite repression and dissidents are more likely to believe they will succeed (Ritter and Conrad 2016). This means observing peaceful protest is contingent on the beliefs of dissidents that their political objectives can be achieved. In addition, repression can induce protesters to instead use violent tactics as they attempt to minimize the effects of state violence (Lichbach 1987, Moore 1998).

Dissidents may also support violent revolutionary movements, contributing to the use of force with the objective of changing the regime status quo. Dissidents forego peaceful anti-regime protest to volunteer for terrorist organizations or violent radical flanks (Bueno de Mesquita 2005, Ryckman 2020), materially support an insurgency (Bueno de Mesquita 2013), or themselves create a violent revolutionary movement (Wickham-Crowley 1992). While violent revolutionary movements may increase protest by persuading dissidents the regime is vulnerable through vanguard actions (Bueno de Mesquita 2010, Leventoglu and Metternich 2018), violent militancy may reduce protest participation in a spoiler effect (Kydd and Walter 2002),

decreasing beliefs of protesters that regime change is possible (Canetti et al. 2017). Protesters may then either support militants or decide to abandon resistance to the regime.

To explore the effects of these shadows of violence, this paper develops a coordination model in which dissidents choose whether to protest when faced with regime repression and the option to support a violent revolutionary movement. Dissidents seek to overthrow the regime, and a protest may achieve this objective. If protests are successful, dissidents benefit in proportion to the infrastructural power of the state: receiving spoils and rewards stemming from their participation.[1] Yet the regime only changes if a sufficient number of dissidents protest. In deciding whether to protest, dissidents also account for the possibility of violent revolutionary movements which could emerge after failed protest. In this case, dissidents benefit if a violent revolutionary movement wins spoils from conflict with the regime. Violence incurs costs, however, as conflict destroys a share of regime spoils. Because benefits to protest and supporting violence grow in the number of dissidents choosing the same actions, dissidents must estimate participation in each of these resistance strategies.

Dissidents are uncertain about how many others will choose to protest or use violence. This is because, while they know how much the regime represses dissent and the costs associated with repression, they do not know the regime's precise infrastructural power. Regime power matters because (1) dissidents gain more material benefits from toppling a regime with greater infrastructural power, and (2) more infrastructurally powerful regimes require a greater number of protesters to overthrow. Dissidents who believe the regime has more of this power are more motivated to protest, but also may believe a protest has a lesser chance of being successful: more protesters must join to overthrow the regime. To understand how dissidents estimate

[1]This is more formally defined in the model as the capacity of the state to extract revenue and implement preferred policy, such as through party, press, and economic institutions (Levi 1988, Slater 2003).

others' behavior, I assess their *beliefs* about how many dissidents will not protest, and how many dissidents will support violence if protest fails. These beliefs shape the benefits to participation in protest and support for violence.

Based on the unique equilibrium of the model, which identifies the proportion of dissidents who choose protest, violence, or do nothing, I derive several empirical implications. First, repression has a non-monotonic effect on protest when few dissidents support violence. Under these conditions, repression creates a *backlash* effect at low levels: increasing state violence makes protest appear more likely to fail, which in turn makes dissidents more likely to join protests as they seek to avoid a violent conflict from developing after a failed protest. Thus, backlash occurs when dissidents' aversion to future conflict outweighs the immediate costs of state violence. At high levels of repression, repression has a *deterrence* effect. Dissidents expect protest to fail and face sufficient risk of being personally repressed such that they forego protest and risk the costs associated with a violent conflict with the state.[2] When many dissidents support violence, however, the backlash effect does not occur and repression always deters protest: citizens are less concerned about a violent outcome and choose not to protest as repression increases.

Second, the model predicts a non-monotonic effect of capabilities for violence on protest. At low to medium levels, capabilities for violence *incite* larger protests. Similar to the logic of the backlash effect, violent capabilities at low levels raise the prospect of increased violent resistance if protest fails. Dissidents who find a violent outcome undesirable therefore become more likely to protest to stave off the possibility of violence in the future. However, as violent capabilities increase, violence becomes both more individually desirable for dissidents and also more likely to win spoils from the state due to the benefits of coordination. Dissidents then opt out of protest, thereby reducing their risk of repression, and wait to use violence if protest fails.

[2]As a result, the level of violent resistance increases at the highest levels of repression.

Protest subsequently decreases. I label this the *spoiler* effect, as an individual-level analog to the effect described by Kydd and Walter (2002).

This paper contributes to related literature in three ways. First, the model offers an explanation for why increasing violence—whether from the state or from dissidents—can motivate increased protest: dissidents rationally anticipate the harms future violence would cause. This new explanation for backlash adds to previous explanations which emphasize the revelation of information or psychological factors (Aytaç, Schiumerini and Stokes 2018, Pearlman 2018). Second, I extend the literature on dissent and mobilization by considering support for violent movements as an outside option for protest. Adding the option of supporting violence integrates literature on protest and repression (Chenoweth and Stephan 2011, Chyzh and Labzina 2018, Dragu and Lupu 2018, Sullivan 2016a) with literature on support for violent revolutionary movements (Leventoglu and Metternich 2018), better matching accounts which suggest violence and nonviolence are part of a strategic interaction (Bueno de Mesquita 2010, Lichbach 1987). Third, the model suggests new direction for empirical research, suggesting that unobserved future violence motivates observed protest decisions in the present and that at least some observed political violence in the present resulted from the failure of past protest. This contribution coincides with the development of data sources which identify the origins of anti-regime violence (Braithwaite and Cunningham 2018).

4.2 Protest and the Anticipation of Violence

Before introducing the model, it is important to ground the analysis in an empirical context: dissidents facing both the prospect of state violence and of violence from within their own ranks if nonviolent resistance fails to achieve its objectives. More importantly, it must be shown that dissidents take into account each of these sources

of violence. While ample past research describes how dissidents weigh the potential for repression before making decisions (Cunningham and Beaulieu 2010, Lichbach 1987, Ritter and Conrad 2016), the notion that dissidents anticipate a violent revolutionary movement's emergence while still considering whether to engage in nonviolent mobilization has received scant attention. The following illustrations present empirical context and specifically highlight how dissidents anticipate violence in making strategic decisions.

4.2.1 Civil Rights in Mississippi, 1963-1964

The struggle for racial justice in the American South—pushing for widespread policy change in social and political rights—took place in *de facto* single-party, subnational authoritarian regimes: particularly Mississippi, Alabama, and Georgia. From the mid-1950s to the early 1960s, the movement consisted of primarily nonviolent resistance such as protests, strikes, and boycotts (Mazumder 2018). Local and state police forces in the American South often brutally repressed nonviolent protesters, imprisoning large numbers of these dissidents. After failed nonviolent resistance in Albany, Georgia and a difficult start to a nonviolent campaign in Birmingham, Alabama, Dr. Martin Luther King Jr. warned about "the threat of violence" developing from groups such as the Nation of Islam "and other potentially violent extremists" (Nimtz 2016, 7). By early 1964, the possibility of an impending, low-grade, violent revolutionary conflict loomed large for dissidents and political elites alike. Democratic Senator Hubert Humphrey believed that extremists could "precipitate small Algerias [i.e. revolutions] all around the country" while Malcolm X himself "clearly had Cuba in mind" for his resistance strategy (Nimtz 2016, 15).

Against this backdrop, dissidents of the civil rights movement planned the well-known campaign in Mississippi known as the Freedom Summer in 1964 (McAdam 1986). Large numbers of activists from across the United States traveled to the

state to engage in nonviolent mobilization activities designed to secure voting rights for Black Mississippians. However, among the activists in Mississippi that summer were members of the Revolutionary Action Movement (RAM) which sought to build a liberation army, wage guerrilla warfare in the Southern countryside, and "create a revolutionary situation" (Umoja 2013, 89). Movement leaders became concerned that their peers were willing to take up arms, and, after the repression (murder) of three Congress of Racial Equality dissident activists by pro-government militias in Neshoba County, dissident Dave Dennis captured the developing mood when he renounced nonviolence: "there was only one solution. If we're gonna have a war, let's have it" (Umoja 2013, 98).

The possibility of future violence became a concern for other Black dissidents across Mississippi. Aaron Henry, in Clarksdale, remarked that "we will not forever be able to channel the activity of Negroes in non-violent channels if this violence against us continues to go on unabated" (Dirks 2007, 84). Bob Moses, another dissident, relayed concerns that if nonviolent resistance in Mississippi ceased, the *apartheid* regimes would "just pile up our bodies, one on top of the other" (Rachal 1999, 322) in the resulting conflict. Moses then redoubled his nonviolent resistance efforts into fall 1964, even after the Freedom Summer officially ended. A comprehensive protection of voting rights, the foremost policy change sought by dissidents, came with the 1965 Voting Rights Act. Along the way, however, dissidents in Mississippi acknowledged they were on the edge of a conflict stemming from the escalation of dissident violence and state repression.

4.2.2 The Political Left in Weimar Germany, 1920-1924

While not occurring in a dictatorship, the mobilization of workers in the early years of Germany's Weimar Republic describes other elements of the empirical context and illustrates dissidents' anticipation of conflict. Following its defeat in World War I,

Germany experienced convulsions of political violence between the ideological left and right, driven in many cases by soldiers returning with disillusionment and weapons (Schumann 2009). Nonetheless, in parts of the new Weimar Republic, a durable coalition of leftist parties—Social Democrats and Communists—formed, securing electoral victories in regions such as Saxony (Mathews 1998). Despite their electoral advances, the parties of the left continued to employ mass nonviolent action such as strikes to advance their political demands.

In 1920, local Communist Party organizations voted to align themselves with the Soviet-sponsored Communist International. A violent fringe among the Communists formed an armed wing, moving to take over mines and factories by force. Composed of primarily younger men and unskilled laborers and led by the charismatic agitator Max Hölz, the fringe prepared for street fighting and "civil war" against the military in 1921 (Price 1999, 96). Yet among even the rank-and-file workers with Communist sympathies, "massive reservations were voiced about these preparations for military clashes" (Schumann 2009, 63) and many workers chose instead to participate in peaceful strikes. There was a general aversion to violence and "putsch-like actions" among labor movement dissidents, and a rejection of Hölz's tactics as extreme and counterproductive—not least because they threatened the Communists' coalition with the avowedly nonviolent tactics of the Social Democrats. Indeed, Communist leaders sough to distance themselves from figures such as Hölz because his actions drained support from the pool of potential dissidents—workers themselves—who redoubled their commitment to political factions not responsible for violence in response to the fringe actions of 1921 (Pryce 1977).

More generally, the fringe of armed workers backed by the Communist International triggered public alarm. Middle-class newspapers predicted "terrible bloodshed" and "a dangerous rush of madness" which would result from the street violence and retaliations from the military (Schumann 2009, 9). Rumors spread of a "Red Army"

emerging among the workers, which, with the recent history of armed Bolshevik forces in Russia, suggested the possibility of bloodshed on the scale of that country's civil war. While Hölz's threat of violence dissipated after March 1921, fear of conflict still loomed over the political left in 1924. That year, the Social Democrats, alarmed by street violence from Communist and far-right paramilitary groups, developed a nonviolent campaign for women with the slogan "women, prevent the civil war," predicated on the belief that the growing violent fringes would "one day escalate into the horrors of a civil war after all" (Schumann 2009, 206).

The model which follows, besides incorporating the elements of the empirical contexts just described, builds on threshold coordination models in which dissidents choose whether to protest, and the regime changes if a sufficient proportion protest. Dissidents receive signals about regime infrastructural power which inform their beliefs about the likelihood of regime change. The information can come from other dissidents' actions (Lohmann 1993), from communication technology (Little 2015), from revolutionary vanguards (Bueno de Mesquita 2010), or from regime agents (Dragu and Lupu 2018). Unlike arguments which depict only protest or include a limited role for a violent revolutionary organization, this paper explicitly incorporates the possibility of violence following failed nonviolent mobilization. Introducing support for violence reveals effects, such as backlash and incitement, described in empirical literature on repression and dissent. This provides insights lacking in models studying only the decision to protest.

4.3 The Model

4.3.1 Players and Actions

Players are a continuum of dissidents, indexed by i, who can mobilize for anti-regime resistance. There are two periods in which actions are chosen simultaneously. In the

first period, dissidents join an anti-regime *protest* or do not protest. After the first period, the regime either falls or not. If the regime falls, the game ends. If the regime does not fall, repression eliminates a proportion of protesters, and the remaining dissidents support *violence* against the regime by a revolutionary movement or do *nothing*. dissidents face a regime with infrastructural power $\theta \in [0, 1]$. Infrastructural power is the ability of the regime to extract revenue and implement its preferred policy through institutions such as state bureaucracy, judiciary, and party (Levi 1988, Slater 2003). Furthermore, infrastructural power does not deterministically set repression: high-power regimes may substitute repression with cooptation (Gandhi 2008) and less powerful regimes may repress maximally. The proportion of dissidents who must protest in order for the regime to fall, $\rho \in [0, 1]$, is equivalent to infrastructural power. If the proportion of dissidents who resist is below the threshold value ρ, then the regime does not change, and dissidents choose in the second round whether to follow the failed protest with violence against the regime. The regime changes only if a sufficient proportion of dissidents protest.

Two other features of the environment are common knowledge. The regime—a non-strategic player—represses with intensity $r \in (0, 1]$. Repression is observable and known perfectly by all dissidents. In the context of mass protest, regime repression involves arrest, detention, or even killings of dissidents.[3] Repression imposes costs on protest and violence, and it also eliminates a proportion r of protesters if protest fails. Second, there is a distribution of dissident capabilities for violence. The distribution of capabilities can be thought of as whether dissidents have training such as military experience or material advantages for the use of violence such as access to weapons.[4] Capabilities are distributed $\psi \sim N(\mu, \alpha^2)$. The capability distribution ψ is common knowledge but each individual dissident's value from this distribution, ψ_i, is not.

[3]These actions are difficult to conceal, especially given modern communication technology (Little 2015).

[4]Capabilities may also be based in moral convictions, emotion, or another source. The psychology of violent actions is outside the scope of the model.

With larger μ, the larger the share of dissidents who have a positive capability for violence.

4.3.2 Information and Strategies

The model is a coordination game with incomplete information: the regime changes and dissidents benefit from regime change if and only if a sufficient proportion of dissidents protest. Because the regime's infrastructural power is not perfectly observed, dissidents must form beliefs about the actions of other dissidents to estimate the probability the regime changes. Beliefs about other dissidents' actions depend on those dissidents' own beliefs about infrastructural power. Each dissident has an improper uniform prior belief with infinite mass.[5] Before choosing an action, each dissident receives a private signal of infrastructural power: $x_i = \theta + \nu_i$. Each ν_i is an independent realization of a random variable distributed $N(0, \sigma^2)$. Thus the private signal is infrastructural power with random, mean-zero noise.

Dissidents' strategies have two components. The second round strategy is a function which maps a signal of infrastructural power, level of repression, destructiveness of violence and distribution of violent capabilities to an action, either violence or doing nothing. The first round strategy is a function which maps these same parameters to an action, either protest or not protesting. Therefore, dissidents have four possible actions. They may either protest and then choose violence if protest fails, protest and then do nothing if protest fails, not protest and then choose violence if protest fails, or not protest and then do nothing if protest fails.

[5]Improper priors are a common assumption in regime change models which allow players' beliefs to be determined by their signal (Morris and Shin 2006).

4.3.3 Payoffs

Dissident payoffs are as follows. If a dissident protests and the regime changes, she receives the benefit θ and pays a cost which is a function of repression $c(r)$. The benefit is the regime's infrastructural power θ. Protesters receive a benefit equal to infrastructural power if the regime changes, consistent with the allocation of spoils and power under a new regime to those who aided the fall of the old regime.[6] However, regime change is uncertain at the time the dissident protests. Regime change occurs with probability p, which is the probability the proportion of dissidents who protest exceeds ρ, a quantity defined below which is a function of other model parameters. With probability $1 - p$ the protest fails. After failure, regime repression eliminates r share of protesters through arrest, imprisonment, or killing, and $1 - r$ protesters remain to support violence or do nothing in the second round. All dissidents who do not protest remain to support violence or do nothing in the second round.

In the second round, a dissident who supports violence faces two possibilities: either the violent conflict with the state is successful—resulting in victory—or it is unsuccessful. If the conflict is successful, the dissident receives the benefit $\theta - \omega$, which is infrastructural power less an amount destroyed in violent conflict $\omega \in [0, \theta]$. If the conflict is unsuccessful, the dissident receives no benefit from the regime. The probability of success is the proportion s of dissidents who choose violence.[7] A dissidents supporting violence also benefit from the capability to support violent revolutionary movements according to her type ψ_i and pay a cost of regime repression $d(r)$. Cost functions are convex and increasing. If a dissident does nothing, she has a payoff of zero. A dissident who protests pays the cost of repression $c(r)$ regardless of whether she chooses violence in the second round, the payoffs of violence for a

[6] Dissidents who do not protest do not benefit from regime change. See the discussion on page 12 for why this is the case.

[7] This is consistent with a coordination benefit to supporting violence (Bueno de Mesquita 2005, 2013).

dissident who protests are also downweighted by $(1 - r)$, accounting for dissidents eliminated by repression after failed protest, reducing participation in violence.

In summary, payoffs for dissident i who protests are:

$$U_i(\theta, r, \omega, \psi) = \begin{cases} p \cdot \theta - c(r) + (1-p)(1-r)(s \cdot (\theta - \omega) + \psi_i - d(r)) & \\ & \text{if Protest, Violence} \\ p \cdot \theta - c(r) & \text{if Protest, Nothing} \end{cases}$$

Additionally, payoffs for dissident i who does not protest are:

$$U_i(\theta, r, \omega, \psi) = \begin{cases} (1-p)[s \cdot (\theta - \omega) + \psi_i - d(r)] & \text{if Not Protest, Violence} \\ 0 & \text{if Not Protest, Nothing} \end{cases}$$

Four attributes of dissident payoffs require explanation. First, a more powerful regime benefits dissidents directly through the parameter θ. This means, all else equal, a more powerful regime makes protest more appealing to dissidents.[8] When dissidents receive a signal the regime has high infrastructural power, this information motivates protest. This is because such regimes offer more spoils, if overthrown, to fulfill the goals of the dissidents. As Tilly (2006) describes, coordination increases with "authorities' control over the stakes of claim-making's outcomes for participants; where authorities lack control over stakes, coordination declines" (127), and "once a new coalition seizes control of a high-capacity government, its actions produce more far-reaching effects than are possible in low-capacity regimes" (163).

In classic treatments of collective action in anti-regime settings (Lichbach 1995), the benefits of the anti-regime action are spread among both participants and non-

[8] In applications around military coups, it is common for the coordinating action to be more appealing when the regime is less powerful. See, for example, Little (2017).

participants in the action. The non-excludability of benefits creates an incentive for free-riding: dissidents have a dominant strategy to not participate, and in equilibrium no one participates in anti-regime action. Yet in coordination games as this one, the benefit to participating increases in the participation of others (Chwe 1998), which implies that there are at least some benefits to anti-regime action which are *conditional on participation*. Thus, θ represents the *net benefit* for participation: the difference between the benefits for participation and the benefits of non-participation given a successful protest (Casper and Tyson 2014). The parameter represents the ability of the new regime to reward protesters for their participation. This could take the form of participation in regime institutions which formed from the dissidents who participated in a revolution, clientelistic benefits from participation in a political party supporting the new regime, public sector jobs, or other material gain which supporters receive. Most basically, this assumes that participating in anti-regime protest or violence makes a difference in the fortunes of the participants.[9]

Second, infrastructural power differentially affects payoffs for dissidents who protest and dissidents who support violence. Protesters receive the benefit of infrastructural power θ if and only if the regime changes. Protest is all-or-nothing: either protesters induce regime change or they do not, relying on political pressure to create a revolutionary situation in which non-state actors challenge state authority and win support from institutions such as the military (Dragu and Lupu 2018, Tilly 1978) or bureaucracy (Gehlbach and Simpser 2015). In contrast, violent supporters receive the payoff of infrastructural power minus losses from conflict conditioned by the proportion who choose violence. Supporting violent movements escalates a conflict in which incremental numerical strength matters. Supporting violence siphons infrastructural power as more dissidents choose it (Bueno de Mesquita 2005). Violence from below also does not overthrow a regime immediately with political pressure. Indeed, au-

[9]If selective incentives are less strong, this will attenuate the effects of θ on the decisions of dissidents. In the analysis, I compare cases with high and low θ which could also have this interpretation.

thoritarian regimes consolidate rather than fracture when facing violent revolutionary movements (Slater 2020).

Third, the cost functions for repression of protest and violence are not identical: dissidents pay different costs depending on whether they protest or support violence. This is a realistic assumption if the regime conditions repression on the type of resistance, or if dissidents bear the costs of repression unequally across types of resistance. For example, violent resistance may be better equipped to evade repression because its actions may be clandestine (Finkel 2015). On the other hand, authoritarian regimes are particularly concerned with repressing challenges to the state's monopoly of violence. Thus, dissident violence incurs higher costs for the same repression r because a regime is more likely to use lethal force against violent dissidents than protesters (Earl, Soule and McCarthy 2003, Hou and Quek 2019). Therefore, $d(r)$ is strictly greater than $c(r)$ for all values of r.[10]

Finally, the distribution of capabilities for violence ψ has an intrinsic effect and coordination effect on the total payoff to violence. The intrinsic effect is through the value of ψ_i to each dissident which depends on the expectation $E[\psi_i]$. When more dissidents are capable of violence, this expectation increases. There are also dissidents with negative capabilities for violence.[11] The coordination effect of ψ comes through the parameter s. This parameter captures the proportion of dissidents who are willing to use violence even when faced with the cost of repression and potentially a negative valuation of violence through their type ψ. Such dissidents are referred to as 'militants' for clarity in the discussion. In particular, $s = Pr(s(\theta - \omega) + \psi - d(r) \geq 0)$. As the expectation of ψ increases, violence becomes more appealing. Even dissidents with negative capabilities for violence receive greater benefit from violence as the proportion of militants increases through a coordination effect.

[10] This assumption can be relaxed without affecting the main results.
[11] This is given the support of the distribution of ψ ($\psi \in \mathbb{R}$).

4.3.4 Equilibrium

I solve the model with a Perfect Bayesian Equilibrium in switching strategies. Specifically, I identify an equilibrium in which each dissident i plays a symmetric switching strategy: using the same switching strategy around a cutpoint k based on her private signal x_i. The Perfect Bayesian Equilibrium requires (1) consistent beliefs, and (2) sequential rationality. An individual dissident updates her beliefs about the regime's infrastructural power using Bayes' Rule and given her posterior beliefs, then chooses to protest if and only if the signal of infrastructural power is sufficiently high.

To determine sequential rationality, the dissident's best response in the second period must first be solved. To do so, I find the expected utility in the second round between the two possible actions in the set $\{Violence, Nothing\}$. Regardless of whether a dissident chooses to protest or not, the payoffs for violence and doing nothing are equivalent across both cases.[12] dissidents choose which action yields the highest payoff. Because the payoff to doing nothing is constant and zero, a dissident prefers violence to doing nothing whenever violence has a positive payoff. Likewise, a dissident prefers doing nothing to violence whenever violence has a negative payoff. A dissident's payoff in the second round may be expressed as the sum of the expected utility of each action within the set multiplied by the probability that action is preferred between the two:

$$EU_i[\text{Second Round}] = U_i[\text{Violence}]Pr(\text{Violence}) + U_i[\text{Nothing}]Pr(\text{Nothing}) \quad (4.1)$$

Note in order to express the proportion of dissidents who choose violence $Pr(Violence)$ and the proportion who choose nothing $1 - Pr(Violence)$ in the second period it is necessary to solve for s, since $s = Pr(Violence)$. In particular, $s = Pr(s(\theta - \omega) + \psi - d(r) \geq 0)$. The following proposition defines the quantity $s^*(\theta)$

[12]Protesters will have paid $c(r)$, but since the cost of repression is paid regardless of their second-round decision it drops from the decision and the decision is identical to those who did not protest.

which is the unique equilibrium proportion of dissidents who choose violence:

Proposition 1: *Under the sufficient condition $\alpha > 1$, meaning the distribution of capabilities for violence among dissidents has large enough variance, for a given signal x_i of infrastructural power θ there is a unique corresponding $s^*(\theta)$.*

With second period rationality defined, I next define dissidents' beliefs about the share of other dissidents who choose to protest. This is necessary to evaluate dissidents' decision to protest in the first period. Upon receiving her private signal x_i, each dissident updates her beliefs in two ways. First, she must have beliefs about what proportion of dissidents received a signal at least as large as k. This is her estimated proportion of protest. The belief of dissident i in the proportion of other dissidents who protest is $Pr(x_j \geq k)$ with $j \neq i$. This can be expressed as $1 - Pr(x_j < k)$. Because of the normal signal distribution and her improper uniform prior, her belief about the proportion of protest is simply:

$$Pr(\text{Protest}) = 1 - Pr(x_j < k) = 1 - \Phi(\frac{k - x_j}{\sqrt{2}\sigma}) \tag{4.2}$$

Where the signal distribution for other dissidents x_j is a mixture distribution of dissident i's and dissidents' $j \neq i$ signal distributions which are each distributed $N(0, \sigma^2)$ for a mixture distribution of $N(0, 2\sigma^2)$. Second, she must estimate the likelihood the protest will succeed based on her private signal. Now the dissident must estimate the probability the regime change threshold is met. In particular, what is the likelihood the regime changes given the proportion who protest?

$$\begin{aligned} p(x_i, k) = Pr(\text{Regime Change}) &= Pr(1 - \Phi(\frac{k - x_j}{\sqrt{2}\sigma}) \geq \rho) \\ &= 1 - \Phi(\frac{k - \sqrt{2}\sigma\Phi^{-1}(1 - \rho) - x_j}{\sigma}) \end{aligned} \tag{4.3}$$

Where ρ is the threshold for regime change and k is the conjectured threshold in the switching strategy used by dissidents when observing a signal x_i. With posterior beliefs defined I now define sequential rationality for the first period.

A dissident protests, rather than not protesting, if the expected utility of protesting exceeds the expected utility of not protesting. The expected utility of protest is expressed similarly to the second period payoffs in which there are two possible cases: successful protest in which the regime changes, and unsuccessful protest in which the regime does not change. A dissident receives the regime change payoff $\theta - c(r)$ with endogenous probability $p(\theta, k)$, and the payoff for no regime change $-c(r)$ with endogenous probability $1 - p(\theta, k)$. This is, formally:

$$\begin{aligned} EU_i[\text{Protest}] = U_i[\text{Regime Change}]Pr(\text{RC}) \\ +U_i[\text{No Regime Change}]Pr(\text{No RC}) \end{aligned} \tag{4.4}$$

Recall since $1 - r$ protesters survive repression of a failed protest and choose whether to use violence in the second round, $EU_i[\text{No Regime Change}] = (1 - r) \cdot \{EU_i[\text{Second Round}] - c(r)\}$. dissidents protest if and only if the expected utility to protest is greater than or equal to the expected utility of not protesting. Setting equal the payoffs in (4.3) and (4.4) and rearranging, each dissident protests if and only if the private signal x_i she receives of infrastructural power θ is sufficiently large, with a cutoff k which for a given private signal satisfies:[13]

$$x_i - \frac{U_i[\text{Violence}] \cdot Pr(\text{Violence}) \cdot [1 - Pr(\text{RC}) \cdot r] + c(r)}{Pr(\text{RC})} = 0 \tag{4.5}$$

The derivation is in Appendix C. Note this expression involves each dissident

[13]That is, each dissident plays a switching strategy defined by:

$$s_i(x_i, r, \omega, \psi) = \begin{cases} \text{Protest} & \text{if } x_i \geq k \\ \text{Not Protest} & \text{if } x_i < k \end{cases}$$

comparing the benefits of protesting (all positive terms in the expression) with the benefits of not protesting (all negative terms in the expression). The following proposition results:

Proposition 2: *Under substantive restrictions in which the utility of using violence is neither too low nor too high, there is a unique equilibrium in switching strategies in which all dissidents play cutoff* θ^**. For every signal* x_i *of infrastructural power* θ *a dissident receives there is a unique corresponding cutoff.*

The proof and technical expression of the two jointly sufficient conditions is in Appendix C. The first condition requires the utility of violence for the mean dissident is negative, which ensures that protest is a more appealing option than violence given a roughly similar proportion of dissidents choose each strategy of resistance. This captures the intuition of the distribution of capabilities, which for many dissidents assigns a negative capability and thus reduces the attractiveness of violence. Dissidents do not, on average, have extensive military training or access to arms, being members of civil society organizations with generally nonviolent purposes (Tilly 2008, Wickham-Crowley 1992). The second condition requires the utility of violence for the mean dissident is not so low that it would not be preferred even when repression of protest was especially costly. In other words, violence must be a plausible outside option to protest for some dissidents. The conditions mark the scope of the model: a setting in which political violence is neither nonexistent nor ubiquitous, but represents a latent violent threat which *could* erupt after failed protest.

Once the equilibrium cutoff θ^* is solved, the true proportion of dissidents who protest may be derived. Because dissidents conjecture after observing the signal the proportion who protest, dissidents do not know this true proportion when making the decision to protest. The proportion who protest is the probability the private

signal each dissident receives is larger than the unique cutoff θ^*, inducing them to protest. The proportion who do not protest is the complement of this probability. Within those who do not protest, it is also possible to determine the proportion who support violence and the proportion who do nothing. Likewise, the dissidents who survive a failed protest also divide into supporting violence and doing nothing. The proportion who support violence are dissidents who did not protest and for whom the payoff to violence exceeds the payoff from doing nothing. The proportion who do nothing are the dissidents who neither protest nor support violence. The following proposition formalizes the discussion, with the proof—including the decomposition of these proportions into those who choose violence or doing nothing after protesting or not protesting—in Appendix C:

Proposition 3: *The endogenous equilibrium proportion of dissidents who protest, support violence, or do nothing may be formally characterized:*

$$\begin{cases} 1 - \Phi(\frac{\theta^* - \theta}{\sigma}) & \text{Protest} \\ (1-r)s^*(\theta) + rs^*(\theta)\Phi(\frac{\theta^* - \theta}{\sigma}) & \text{Violence} \\ (1-r)(1-s^*(\theta)) + r(1-s^*(\theta))\Phi(\frac{\theta^* - \theta}{\sigma}) & \text{Nothing} \end{cases} \tag{4.6}$$

To summarize, a continuum of dissidents observe a private signal of infrastructural power θ and its equivalent regime change threshold ρ, public repression r, distribution of violent capabilities ψ, and destructiveness of violence ω. Each dissident updates beliefs about infrastructural power and forms beliefs about the private signals of others. Dissidents then each choose whether to protest. A proportion of dissidents, determined by the cutoff θ^* and infrastructural power θ, then protest. If the protest succeeds, the regime changes and the game ends. If the protest fails, r share of protesters are removed from the game and all remaining dissidents—divided according to the proportions in Proposition 3—support a violent revolutionary movement or do

nothing.

The outcomes of the model are as follows. In *nonviolent regime change*, the proportion of dissidents who protest meets the regime change threshold. In *failed protest/violence*, the proportion of dissidents who protest does not meet the regime change threshold. Instead, a nonzero degree of political violence breaks out, determined by the quantity for violence in Proposition 2. Importantly, widespread political violence is observed only if protest fails to overthrow the regime.

4.4 Comparative Statics

Comparative statics of two forms are presented. First, I show the effects of repression (r) and the size of violent capabilities (μ) on the endogenous equilibrium cutoff signal θ^*. This analytical exercise demonstrates the effect of the model's exogenous parameters on the proportion of dissidents who protest, and under what conditions repression and violent capabilities increase or decrease protest. Second, I use simulations to identify the equilibrium threshold θ^* under specified parameter values. From the equilibrium threshold are derived the proportions of dissidents who protest, support violence, and do nothing.

The analysis explicates three key contributions of the model. First, repression increases protest in a backlash effect, but only when violent capabilities are low. Second, increasing violent capabilities at low levels can incite additional protest. Third, violent capabilities at high levels decrease protest in a spoiler effect.

4.4.1 Repression

I first consider the effect of repression on protest, as well as the interaction effects of repression and dissidents' violent capabilities on protest. Past empirical literature has been conflicted on these effects: repression is alternatively considered a source of

backlash in which dissent increases under repression (Aytaç, Schiumerini and Stokes 2018, Chenoweth and Stephan 2011, Pearlman 2018), or a deterrent of dissent (Tilly 1978, Young 2019). The model corroborates each of these effects, but under different conditions. First, repression increases protest through a backlash effect which is more likely to occur when the proportion of militants is small, infrastructural power is high, and repression is lower. Second, repression at high levels—and when the proportion of militants is large—deters protest.

Analytically, the expression in equation (4.5) is used to specify the effect of repression on the value of the unique cutoff θ^*. As the unique cutoff decreases, the threshold for dissidents to protest is lower and more dissidents protest. I am interested in the range of values of r in which θ^* is decreasing—meaning repression increases protest—and the range in which θ^* is increasing—meaning repression decreases protest. The following proposition results.

Proposition 4: *The relationship between repression and protest is non-monotone. Given the proportion of militants is neither too large nor too small, implied by Proposition 1, if* $2s^*(\theta)(\theta - \omega) + \mu - d(r)s^*(\theta) < 0$*, repression always increases protest. If* $2s^*(\theta)(\theta - \omega) + \mu - d(r)s^*(\theta) > 0$ *repression increases protest when the effect of repression on the proportion of dissidents who choose violence is sufficiently modest.*[14]

The proof is in Appendix C. Repression affects protest through the value of θ^*, the equilibrium threshold dissidents must observe in order to protest. As θ^* decreases, more dissidents protest. Proposition 4 states a condition for when repression decreases

[14]Formally, this condition is:

$$\frac{\partial}{\partial r} s^*(\theta) \geq \frac{s^*[d'(r)r - (s^*(\theta)(\theta - \omega) + \mu - d(r))] - c'(r) \cdot \frac{1}{1-p}}{r[2s^*(\theta)(\theta - \omega) + \mu - d(r)s^*(\theta)]} \tag{4.7}$$

Where $s^*(\theta)$ *is the proportion of dissidents who choose violence, and* $c'(r)$ *and* $d'(r)$ *are the slope of the cost function for repression of protest and violence, respectively.*

the equilibrium threshold and increases protest in a *backlash* effect. In particular, the condition is more likely to be met when repression is low, infrastructural power is high and the proportion of militants is low. Backlash is also likely to occur when the probability of regime change is high—which occurs at low levels of repression and thus higher levels of expected protest. When the condition is not met, repression deters protest.

What makes the foregoing conditions ripe for producing backlash? First, dissidents are already more inclined to protest: high infrastructural power and large expected protests give large direct and coordination benefits to protest, respectively. Low repression also makes participation in protest less costly and risky. As repression increases, however, a protest could become more likely to fail as some dissidents choose to opt out and avoid bearing repression's costs. As a protest becomes more likely to fail, violent resistance becomes accordingly more likely to be observed in the future. Because violent capabilities are low, dissidents' direct benefits from violence ψ are low—they would find the occurrence of violence personally undesirable—and their coordination benefits from violence $s^*(\theta)$ are likewise low—the knowledge that fewer other dissidents would participate in violence mean little could be gained. To avoid this outcome, some dissidents compensate by joining protests in hopes of preventing their failure. Repression therefore increases protest.

However, as repression reaches higher levels and when the proportion of militants is higher, the deterrence effect replaces the backlash effect. Increasing repression makes protest failure increasingly likely, regardless of whether some dissidents join in backlash protests: the coordination benefits of protest, $p(x_i, k)$, decrease too rapidly in the costs of repression. As the likelihood of protest failure increases, violence is accordingly more likely to emerge. With a higher proportion of militants in particular, dissidents' direct and coordination benefits from violence are higher: dissidents find a violent outcome more desirable, and the violent outcome could secure more gains from

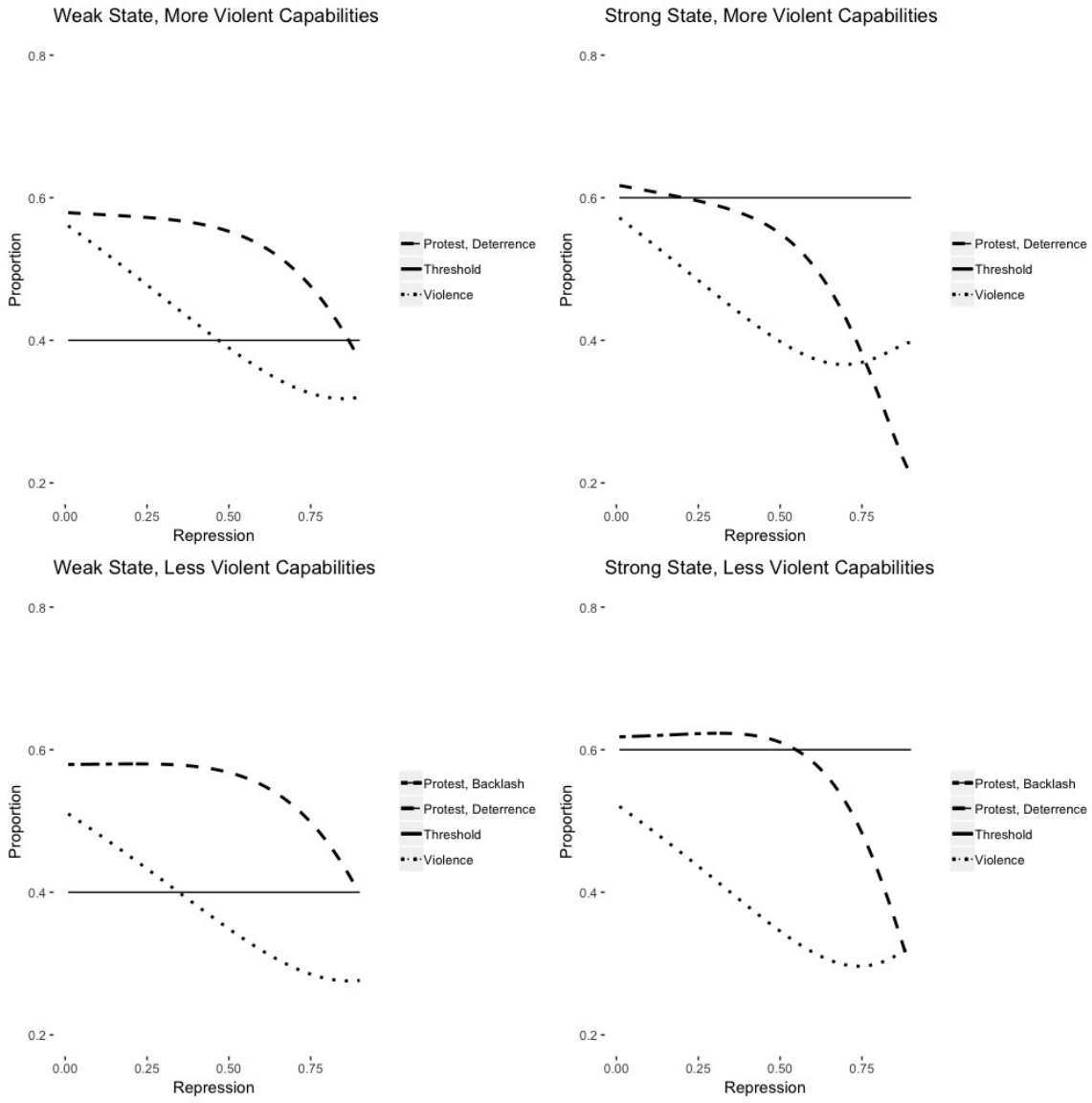
the state as other likeminded dissidents participate. As a result, dissidents anticipate violence will accomplish more than a protest which is likely to fail. Dissidents choose to sit out on a protest, thereby avoiding the risk of repression and elimination—and gamble on a violent outcome.

Figure 3.1 illustrates these differing effects of repression on protest using results from simulations of four different cases of parameters. All panels in the figure depict the proportion of dissidents who protest and support violence, with the horizontal line marking the threshold above which protest induces regime change. Backlash protests—the range of values of r over which protest increases—are indicated separately from deterred protests–the range of values of r over which protest decreases. In the left panels, the regime has low infrastructural power ($\theta = 0.4$).[15] In the top panels, there are high violent capabilities: a large proportion of dissidents in the population are militants ($\mu = 0.5$). In the right panels, the regime has more infrastructural power ($\theta = 0.6$), and in the bottom panels there are low violent capabilities ($\mu = 0$).

The simulations highlight the mechanisms of backlash and deterrence. Increasing repression drives dissidents away from violence as the coordination benefits to violence drop, and repression then leads to *backlash* as the proportion of protesters grows. However, the increase in protest is slight and occurs only when violent capabilities are low. This is because the proportion of militants is too small to attract supporters who would otherwise choose violence as repression increases. As repression increases still further, the figure depicts the *deterrence* effect. Deterrence occurs as the direct and coordination benefits to protest decrease simultaneously, leading to self-reinforcing negative beliefs about the possibility of regime change. As protest collapses the share of dissidents who do nothing increase rapidly. In the right panels where infrastructural power is higher, violence, unlike protest, does not decrease monotonically at high

[15] A regime with low infrastructural power is defined as one in which less than half of dissidents need to protest in order to overthrow the regime..

Figure 3.1: Effects of Repression on Dissident Actions



Note: In each panel, the x-axis depicts the value of repression on the interval (0,1], the y-axis specifies the proportion of dissidents choosing a given action. The horizontal line depicts ρ, the threshold across which the regime changes. Each dashed or dotted line on the graph corresponds to the proportion taking an action (sometimes under a particular effect) across different values of repression.

levels of repression. A small minority of dissidents substitute protest with violence in the second period rather than doing nothing.[16]

[16]Militants with the greatest capabilities for violence support violent revolutionary movements under extreme repression when protest is too costly.

Two implications come from the analysis of repression's effects on protest:

Empirical Implication 1: Repression at low levels increases protest in a small backlash effect, but only when violent capabilities are lower.

Empirical Implication 2: Repression at high levels and when violent capabilities are higher decreases protest in a deterrence effect.

4.4.2 Violent Capabilities

This section considers the effects of changes in dissidents' capabilities for violence—defined by the proportion of militants in the population—on the decision to protest nonviolently. As the proportion of militants increases, dissidents have, on average, higher capabilities for violence as well as increasing coordination benefits to violence. Violent revolutionary movements have been cited as first movers who can motivate protest (Bueno de Mesquita 2010), but also as spoilers who divide dissidents and decrease support for peaceful political change (Canetti et al. 2017, Kydd and Walter 2002). The analysis supports the spoiler effect, though under certain conditions violent capabilities can increase protest.

In this section, I am interested in the range of values of μ in which θ^* is decreasing (increasing violent capabilities increase protest) and in which θ^* is increasing (increasing violent capabilities decrease protest). The following proposition results.

Proposition 5: *There is a non-monotone relationship between the capabilities for violence and nonviolent protest. If* $2s^*(\theta)(\theta - \omega) + \mu - d(r) > 0$ *violent capabilities always increase protest. If* $2s^*(\theta)(\theta - \omega) + \mu - d(r) < 0$ *violent capabilities increase protest when the effects of violent capabilities on the proportion of dissidents who choose violence are sufficiently modest.*[17]

[17]Formally, this is:

$$\frac{\partial}{\partial \mu} s^*(\theta) \leq \frac{-s^*(\theta)}{2s^*(\theta)(\theta - \omega) + \mu - d(r)} \tag{4.8}$$

The proof is in Appendix C. Proposition 5 states the conditions under which violent capabilities decrease the equilibrium threshold and increase protest. Capabilities for violence increase the proportion of dissidents who support violence always, but tend to increase the proportion of dissidents who choose protest in an *incitement* effect when violent capabilities are lower and when infrastructural power is higher.

Similar to the backlash effect, incitement occurs because of dissidents' reaction to the violent resistance which could emerge after a failed protest. Dissidents become increasingly likely to protest when violent capabilities are low and increasing. This occurs because many dissidents have a negative direct benefit ψ to violence which outweighs its coordination benefits $s^*(\theta)$. As violent capabilities grow, an increasing proportion of militant dissidents prefer to sit out during the protest phase and engage in violent resistance later. Dissidents with negative direct benefits to violence, on the other hand, would prefer to avoid future violent resistance and compensate by joining protests to prevent their failure.

By contrast, when violent capabilities are high and increasing, more dissidents have a positive direct benefit from violence. This in turn increases the coordination benefit to violence in a self-reinforcing cycle. As the number of militant dissidents who prefer to sit out protests increases, other dissidents are less motivated to avoid future violence. The utility of not protesting and then choosing violence exceeds the utility of choosing to protest and incurring costs of repression by the regime, and choosing violence later. Dissidents wait out protest and choose violence in the second period. Increasing violent capabilities therefore spoil protests which might otherwise be successful.

Figure 3.2 illustrates these mechanisms with simulations from four different cases of parameters. In the left panels infrastructural power is low ($\theta = 0.4$) and in the

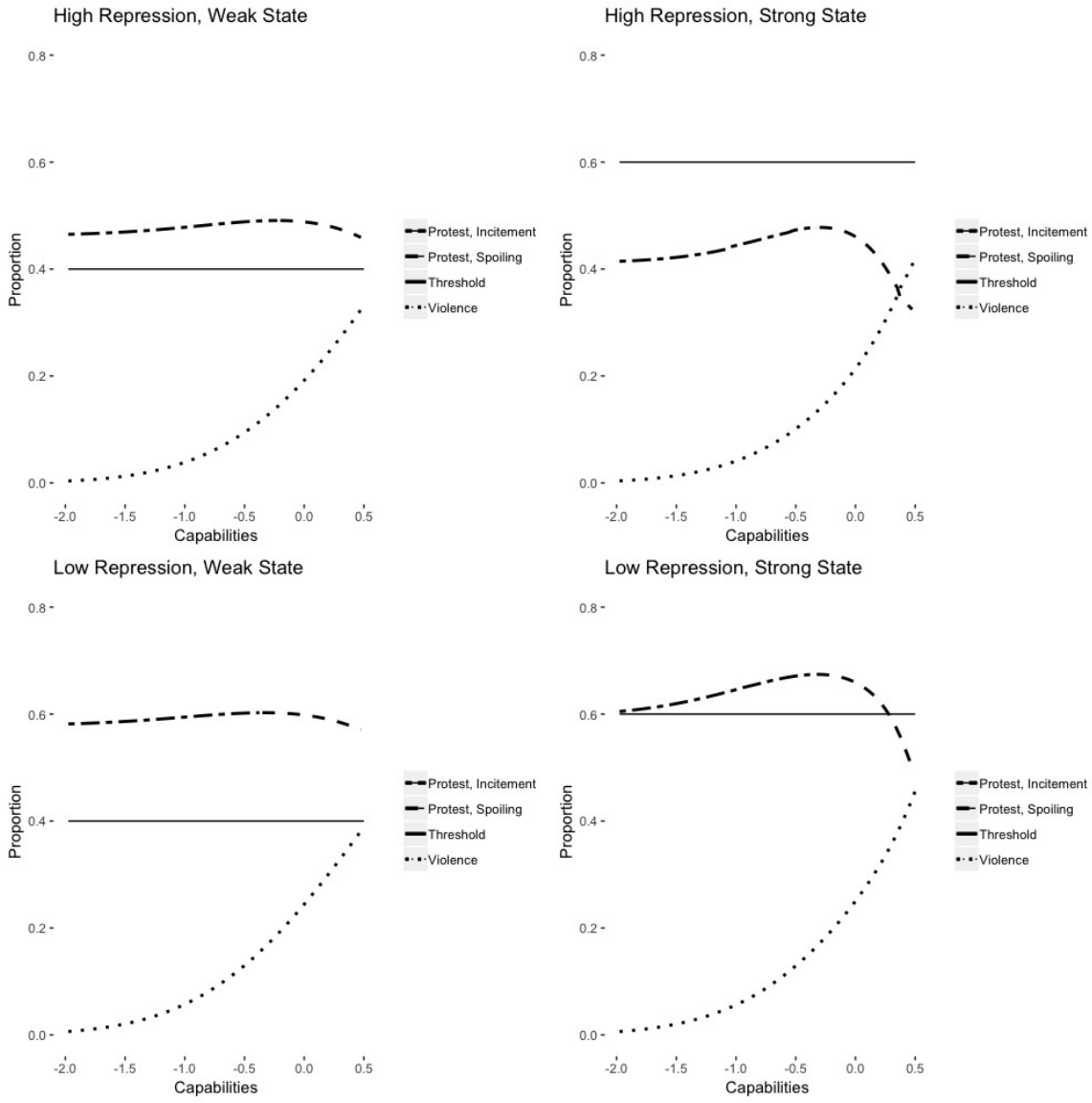
right panels infrastructural power is high ($\theta = 0.6$), but the level of repression varies. In the top panels, repression is high ($r = 0.8$) and in the bottom panels repression is low ($r = 0.2$). The relationship between the magnitude of capabilities for violence and dissident choices is non-monotonic as defined in Proposition 4. The non-monotonicity is more pronounced when infrastructural power is high. At the lowest levels of μ few dissidents support violence and protest is the modal action. At middling levels of μ more dissidents protest, switching away from doing nothing. At high levels of μ protest strictly decreases and the proportion who do nothing strictly decreases.

In all cases the simulations indicate incitement—when violent capabilities increase protest—and spoiler effects—when violent capabilities decrease protest, indicated separately in the figure. In the right panels, when infrastructural power is high, the incitement effect is more pronounced: given there is an incitement effect by the conditions in (8), infrastructural power accentuates the incitement effect by raising the appeal of choosing protest when violence is less preferred. However, once μ increases to high levels and violent capabilities grow, protest decreases and fails to overturn the regime. The high infrastructural power which increased the incitement effect also increases the spoiler effect: once violence is more appealing than doing nothing, higher infrastructural power raises the appeal of choosing violence. In the left panels when infrastructural power is low, protest triggers nonviolent regime change across most levels of μ. The more rapid increases in protest under incitement and decreases in protest under spoiling are tempered.

Two implications come from the analysis:

> **Empirical Implication 3:** Violent capabilities at low-to-medium levels increase protest in an incitement effect, while high levels decrease it in a spoiling effect.
>
> **Empirical Implication 4:** The incitement effect and spoiling effects are more pronounced when infrastructural power is high.

Figure 3.2: Effects of Violent Capabilities on Dissident Actions



Note: In each panel, the x-axis depicts the value of violent capabilities on the interval [-2,0.5], the y-axis specifies the proportion of dissidents choosing a given action. The horizontal line depicts ρ, the threshold across which the regime changes. Each dashed or dotted line on the graph corresponds to the proportion taking an action (sometimes under a particular effect) across different values of violent capabilities.

4.5 Applying the Mechanism

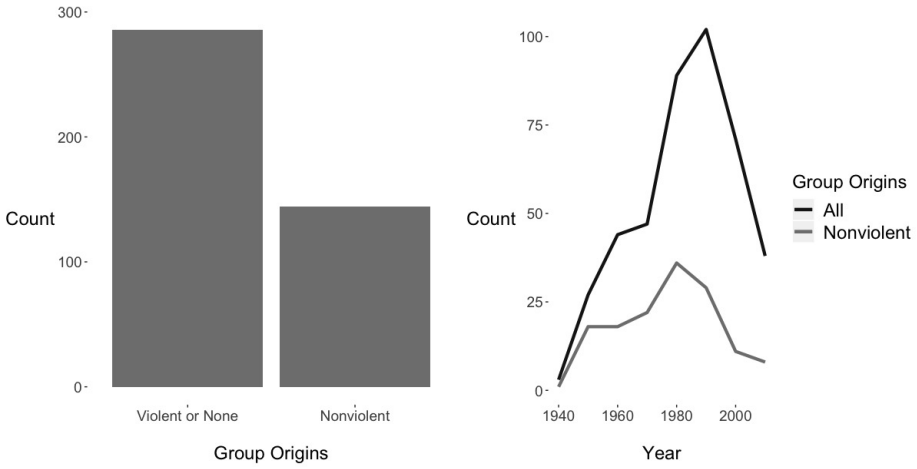
The effects of repression and violent preferences on protest share a common mechanism: dissidents choose whether to protest in part based on their *anticipation of future*

violence. In the backlash effect, protest increases as dissidents react negatively to the costliness of violence under greater repression and gamble on a successful protest in the present to avert costly and destructive violence. In the incitement effect, protest increases as dissidents find violence more appealing yet not worth choosing outright. Rather, dissidents protest in the first round to potentially change the regime nonviolently while keeping the later option of violence if protest fails. The ability to retain violence as an "insurance policy" motivates the decision to protest. In the spoiling effect, dissidents choose not to protest because the anticipation of future gains from violence makes sitting out a protest entirely the best response.

Two general empirical applications proceed from this mechanism. First, an important aspect of the anticipation of violence is that such violence need not be observed to affect dissidents' behavior. That is, if protest succeeds and overturns the regime, we do not witness the violence that would have occurred if the protest failed. Yet violence still motivates dissidents' decisions. Second, the model also suggests that some observed violent conflicts emerge from the ashes of protest movements which failed to overthrow a regime or achieve maximalist objectives (Chenoweth and Lewis 2013, Ryckman 2020). Indeed, many armed non-state organizations originate in civil society organizations which are also likely mobilizers of nonviolent protest. In a welcome opportunity to identify these groups, Braithwaite and Cunningham (2018) offer the new Foundations of Rebel Group Emergence (FORGE) Dataset. Figure 3.3 shows descriptive data from the FORGE dataset: of the 430 rebel group organizations in the data, 144 have their origins in a plausibly nonviolent organization: political parties, political movements, student or youth groups, labor or trade unions, or religious organizations.

Empirical evidence of violent conflicts emerging from failed nonviolent contention presents the opportunity to develop and test new hypotheses relating to this process using data such as FORGE. In particular, the model presented here points to

Figure 3.3: Group Origins from FORGE Data



Note: The left panel shows relative frequencies of groups with violent or no formal origins and groups with nonviolent (party, movement, student, labor, or religious organization) origins. The right panel shows the proportion of the total groups in the FORGE data over time who have these nonviolent origins.

characteristics of armed groups in these conflicts which originated from failed nonviolent contention. These groups likely faced high levels of repression in the past, and were more likely divided between a large share of dissidents who preferred revolutionary violence and some who did not. One research question suggested by this insight from the model is how the actions and effectiveness of armed groups which survived past repression and internal divisions to sustain conflict against the state differ from armed groups without these origins in nonviolent movements. Answering this and other questions would assemble a more complete picture of the processes of contentious politics while also more closely integrating the study of mass protest with civil conflict. Combining data on violent conflict with data on nonviolent mobilization is a helpful starting point (Chenoweth, Hendrix and Hunter 2019).

4.6 Discussion

This paper develops a model of coordination among dissidents in the shadow of violence. Regime repression and dissidents' capabilities for violence constrain nonviolent action. When violent capabilities are lower, repression has a non-monotone effect on the likelihood of nonviolent regime change. Increasing repression leads to a small backlash effect as dissidents switch into protest. At its highest levels, repression decreases violence through deterrence. Violent capabilities also affect the size of protests. As the proportion of dissidents with violent capabilities increases, protest increases in an incitement effect as dissidents anticipate future militant violence and switch into protest to head it off with regime change. At high levels of violent capabilities protest decreases in a spoiler effect.

The mechanism driving the backlash effect—when repression increases protest—contrasts with the mechanism described in the literature in which backlash is driven by moral cascades by dissidents (Aytaç, Schiumerini and Stokes 2018, Chenoweth and Stephan 2011).[18] In the model, backlash occurs through a rationalist mechanism in which dissidents strategically anticipate future violence and protest as a means of potentially avoiding the costs associated with such violence. The model's finding for violent capabilities suggest a variation on the argument in Bueno de Mesquita (2010), in which a violent revolutionary vanguard induces nonviolent regime change. Particularly, a violent revolutionary vanguard may be effective only when it is small. When the vanguard is large it spoils protest: siphoning away too many protesters for the regime to change peacefully (Ryckman 2020). Furthermore, the incitement mechanism described here is different than in the model of Bueno de Mesquita. Rather than modifying beliefs about infrastructural power through violent attacks, support for a violent revolutionary movement in this model increases protest because of dissidents'

[18] Pearlman (2018) rules out mechanisms of likelihood of punishment and reputational cascades as drivers of backash to support the moral identity cascade mechanism.

adverse reactions to the prospect of increased militancy in the future.

The model suggests the need for theories and empirical analyses which consider the limitations of nonviolent action, particularly in authoritarian regimes. In developing these explanations and inferences, scholars will have a deeper understanding of why nonviolent dissent appears to be more successful observationally than violent dissent (Chenoweth and Stephan 2011), including but not limited to selection effects (Ritter and Conrad 2016). In terms of selection effects the model has two specific empirical implications. First, violent revolutionary movements are less likely to succeed because we observe them *after* protest has already failed, or after dissidents decided not to protest in the first place because they believed regime change was not possible. Second, increased repression is associated with the emergence of violent revolutionary movements not because repression causes an increase in violence, but because we only observe violent movements emerging when protests have failed under high repression. When protests succeed under lower repression, we do not observe the support for violence dissidents *would have* given if protests failed.

Another noteworthy aspect of the model and the outcomes is a pivotal dissident always triggers regime change. Because the model analyzes regime change at the individual level rather than the group level, and because the regime falls at a critical threshold, there is always a marginal shift in the proportion of dissidents protesting which makes dissent meet the critical threshold. Using the individual level of analysis is empirically grounded. During the Arab Spring, for example, dissidents on the periphery proved pivotal in resolving the coordination problem to induce regime change (Steinert-Threlkeld 2017). A model at the group level would omit the necessary role of peripheral dissidents.

Finally, I consider broader theoretical implications of the argument. By explicitly considering violent and nonviolent dissent in the same model, I suggest linking two literatures: the study of violent actors such as revolutionary vanguards and the study

of nonviolent dissidents, particularly in authoritarian regimes. Many rebel organizations, terrorist groups, and civil conflicts originate in protest movements which switch into violence (Braithwaite and Cunningham 2018). This model shows how an initial wave of mass nonviolent protest collapses under repression into a failed revolution followed by lingering violence as in the case of the Assad regime in Syria. Rebel organizations and terrorist groups are not exogenous: they are the product of interactions among dissident populations and with the regime which produce violent outcomes. As Tilly (2006) writes in discussing different strategies of contention in revolutions, "the distinction between revolutionary situations and civil wars begins to dissolve before our eyes" (163).

Appendix A

Religious Institutions and Resistance to Repression

A.1 A: Descriptive Statistics

1. Two-sample t-test for Argentines' agreement with the Catholic Church and with the armed forces, Table 1.A.1. Data are from a survey of 890 Argentine adults (Turner 1971).

2. Descriptive statistics, department level, Table 1.A.2.

3. Descriptive statistics, department-year level, Table 1.A.3.

4. Map detailing Catholic diocese boundaries in 1976, Figure 1.A.1.

5. List of bishops opposed to the regime, Table 1.A.4.

6. Coding criteria for bishop opposition.

Table 1.A.1: Two-Sample t-Test, Agreement with Argentine Institutions

Church Mean	Armed Forces Mean	t	df	p-value	CI Lower	CI Upper
35.18	24.56	7.46	1590.3	$1.37 \cdot 10^{-13}$	7.83	13.41

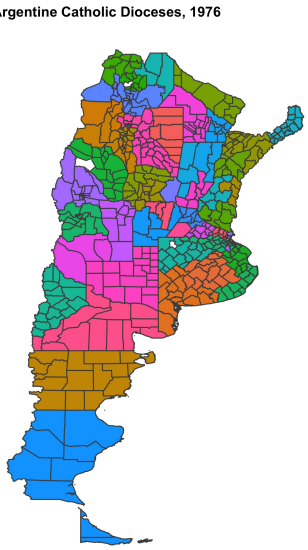
Table 1.A.2: Descriptive Statistics, Department Level

Variable	Minimum	Maximum	Mean	Median	Std. Dev	Missing
Population, 1970	427	2972453	47603.82	15439	158650.83	27
Literate Share, 1970	0.32	0.9	0.72	0.74	0.11	28
Foreign Residents, 1970	0	0.43	0.06	0.05	0.05	28
Child Mortality Rate, 1970	0.13	1.49	0.47	0.42	0.25	28
MSTM Priests, 1967	0	47	3.36	0	5.66	0
Archdiocese, 1976	0	1	0.21	0	0.41	0
Bishop Tenure, 1976	0	21	9.94	10	6.29	0
Peronist Vote, 1973	28.5	94.3	58.66	57	11.5	20
Spoiled Votes, 1973	0	6.7	0.93	0.8	0.55	20
Population, 1970	0	1	0.3	0	0.46	0

Table 1.A.3: Descriptive Statistics, Department-Year Level

Variable	Minimum	Maximum	Mean	Median	Std. Dev	Missing
Disappeared	0	796	1.69	0	20.93	48
Killed	0	129	0.43	0	4.17	48
Bishop Opposed	0	1	0.27	0	0.44	69
Bishop Age	41	76	61.88	62	6.5	4

Figure 1.A.1: Argentine Catholic Diocese Boundaries, 1976



Note: Figure depicts dioceses (colors) overlaid on department boundaries (lines) based on the Catholic dioceses existing in 1976 in Argentina.

Table 1.A.4: Argentine Bishops Opposed to the Regime, 1976-1983

Bishop	Diocese
Agustin Adolfo Herrera	San Francisco
Alberto Devoto	Goya
Alcides Jorge Pedro Casaretto	Rafaela
Alfredo Guillermo Disandro	Villa Maria
Antonio Alfredo Brasca	Rafaela
Antonio Maria Aguirre	San Isidro
Arcenio Raul Casado	Jujuy
Carlos Horacio Ponce de Leon	San Nicolas
Enrique A Angelelli	La Rioja
Gerardo Eusebio Sueldo	Oran
Jaime Francisco De Nevares	Neuquen
Jorge Kemerer	Posadas
Jorge Novak	Quilmes
Jose Agustin Marozzi	Resistencia
Juan Carlos Ferro	Concepcion
Juan Jose Iriarte	Reconquista
Justo Oscar Laguna	Moron
Luis Juan Tome	Mercedes-Lujan
Manuel Marengo	Azul
Miguel Esteban Hesayne	Viedma
Miguel Raspanti	Moron
Moises Julio Blanchoud	Rio Cuarto
Pedro Boxler	Gualeguaychu
Raul Marcelo Scozzina	Formosa
Vicente Faustino Zazpe	Santa Fe

Coding Criteria for Bishop Opposition

The following criteria were used to code a bishop as opposed to the regime.

- Did the bishop have a connection or involvement with the Movement of Priests for the Third World before the dictatorship began in 1976? If yes, label as potential opposed bishop.
- Did the bishop have a connection or involvement with other sectors of society which were targets of the dictatorship—such as student activists and organized labor—before the dictatorship? If yes, label as potential opposed bishop.
- Did the bishop take actions or make statements indicating opposition to repression before the coup on March 24, 1976? If so, label as potential opposed bishop.
- Was the bishop mentioned in Mignone (1986) as either a public or private opponent of the regime? If so, label as potential opposed bishop.
- Did the bishop have frequent associations and issue statements or take actions jointly with other bishops who are labeled as potential opposition bishops? If

so, label as potential opposed bishop.

- Did the bishop have at least two pieces of data which fulfill one or more of the above criteria, and is there no evidence that he discontinued this opposition after 1976? If so, validate and confirm coding as opposed bishop.

  See Appendix H for more details on the archival sources used.

A.2 B: Bishop Appointments

1. Test of endogenous bishop appointments Table 1.B.4.
2. Results of balance tests for opposed bishops at department-year level, Table 1.B.1.
3. Results of balance tests for opposed bishops at diocese level, Table 1.B.2.
4. Results of procedure testing for spatial clustering of bishop opposition, Figure 1.B.1.
5. Results of balance tests for opposed bishops at the diocese level based on whether bishops were appointed during the 1966-1973 military dictatorship, Table 1.B.3
6. Difference in means for opposed and unopposed bishops Figure 1.B.2.

Table 1.B.1: Results for Bishop Opposition Balance Test, by Department-Year

	Bishop Opposed						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log(1970 Pop.)	0.06						
	(0.04)						
Spoiled Vote Share		−0.00					
		(0.03)					
Literate Share			−0.05				
			(0.06)				
Child Mortality				−0.02			
				(0.04)			
Foreign Residents					0.03		
					(0.04)		
Peronist Vote						−0.04	
						(0.04)	
MSTM Priests							0.04
							(0.06)
Adj. R^2	0.02	−0.00	0.01	0.00	0.00	0.01	0.01
Observations	3835	3875	3819	3819	3819	3875	4051

$^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$

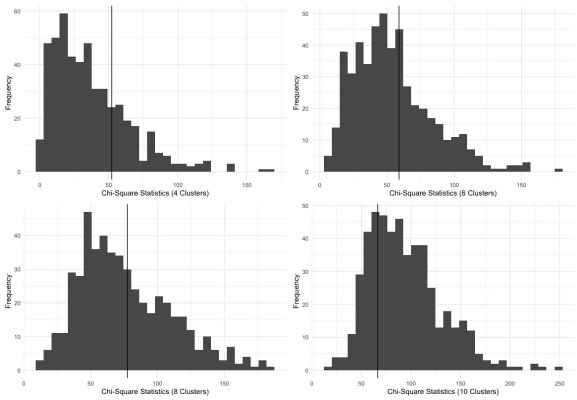
[1] *All standard errors clustered at the diocese level.*

Table 1.B.2: Results for Bishop Opposition Balance Test, by Bishop

	Bishop Opposed						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log(1970 Pop.)	−0.01						
	(0.05)						
Spoiled Vote Share		0.01					
		(0.05)					
Literate Share			−0.03				
			(0.05)				
Child Mortality				0.01			
				(0.05)			
Foreign Residents					0.06		
					(0.06)		
Peronist Vote						−0.03	
						(0.05)	
MSTM Priests							0.01
							(0.05)
Adj. R^2	−0.01	−0.01	−0.01	−0.01	0.00	−0.01	−0.01
Observations	78	78	78	78	78	78	79

$^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$

Figure 1.B.1: Test of Spatial Clustering by Bishop Opposition



Note: Figure depicts null distribution of χ^2 statistics for bishop opposition across four clustering schemes. Vertical line indicates position of the test statistic for the true assignment of opposition. In all four cases, the true assignment is not significant at the $p = 0.05$ level, suggesting no clustering.

Table 1.B.3: Results for Bishop Opposition Balance Test, 1966-1973 Regime, by Diocese

	Bishop Opposed
1966-1973 Appointment	−0.05
	(0.12)
Adj. R^2	-0.01
Num. obs.	79

$^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$

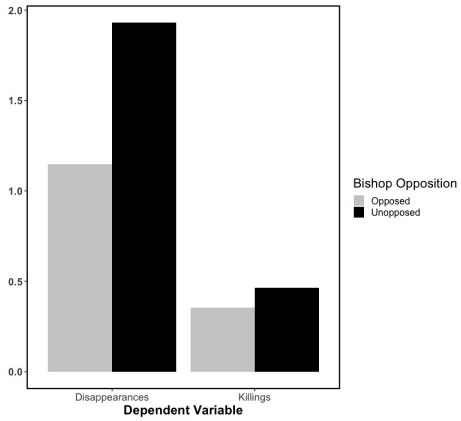
Table 1.B.4: Test of Endogenous Appointments of Regime-Opposed Bishop

	Disappearances, t				Killings, t			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ Bishop Opposed, $t+1$	−2.21	−0.63	0.05	−0.58	−0.37	0.00	0.12	−0.11
	(1.82)	(0.82)	(0.17)	(1.37)	(0.30)	(0.28)	(0.16)	(0.27)
Adj. R^2	0.12	0.75	0.89	0.10	0.14	0.42	0.84	0.11
Observations	3262	3262	3290	3290	3262	3262	3290	3290
Covariates	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Unit Trends	✓	✓	✓		✓	✓	✓	
Geographic FE	Zone	Subzone	Dept.		Zone	Subzone	Dept.	

$^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$

[1] *All standard errors clustered at the diocese level.*

Figure 1.B.2: Simple Difference in Means for Opposed and Unopposed Bishops



Note: Figure depicts group means for disappearances and killings between opposed and un-opposed bishops at the department-year level.

A.3 C: Robustness Tests

1. Main results with more strict coding criteria, Table 1.C.1.
2. Main results with looser coding criteria, Table 1.C.2.
3. Main results recoding bishops one at a time, Figure 1.C.1.
4. Main results dropping City of Buenos Aires, Table 1.C.3.
5. Main results dropping diocese of La Rioja, Table 1.C.4.
6. Main results dropping dioceses one at a time, Figure 1.C.2.
7. Main results with only data from 1976-1978, Table 1.C.5.
8. Negative binomial results for the main hypothesis, Table 1.C.6.
9. Main results with data aggregated to the diocese-year level, Table 1.C.7.
10. Consideration of localized spillovers, Tables 1.C.8 and 1.C.9.
11. Localized spillovers with autocovariate, Tables 1.C.10 and 1.C.11.
12. Placebo test results from randomization inference procedure, Figure 1.C.3.
13. Sensitivity analysis, Figure 1.C.4.

Table 1.C.1: More Strict Bishop Opposition Coding

	Disappearances				Killings			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bishop Opposed (Strict)	−4.17	−1.59**	−0.49	−3.08	−0.81*	−0.59**	−0.05	−0.57†
	(2.68)	(0.57)	(0.78)	(2.43)	(0.33)	(0.21)	(0.35)	(0.32)
Adj. R^2	0.12	0.75	0.89	0.09	0.13	0.41	0.82	0.10
Observations	3731	3731	3763	3763	3731	3731	3763	3763
Covariates	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Unit Trends	✓	✓	✓		✓	✓	✓	
Geographic FE	Zone	Subzone	Dept.		Zone	Subzone	Dept.	

$^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$

[1] *All standard errors clustered at the diocese level.*

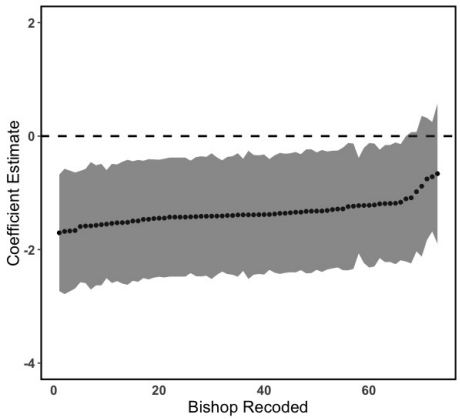
Table 1.C.2: More Loose Bishop Opposition Coding

	Disappearances				Killings			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bishop Opposed (Loose)	−1.37	−0.31	−0.76	−1.59	−0.38†	−0.21	−0.17	−0.39†
	(1.25)	(0.54)	(0.54)	(1.24)	(0.21)	(0.18)	(0.15)	(0.20)
Adj. R^2	0.11	0.75	0.89	0.09	0.13	0.41	0.82	0.10
Observations	3796	3796	3828	3828	3796	3796	3828	3828
Covariates	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Unit Trends	✓	✓	✓		✓	✓	✓	
Geographic FE	Zone	Subzone	Dept.		Zone	Subzone	Dept.	

$^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$

[1] *All standard errors clustered at the diocese level.*

Figure 1.C.1: Coefficient Estimates Recoding Bishops One at a Time



Note: Figure depicts coefficient estimates and confidence intervals for the effect of opposed bishops on disappearances from specifications including subzone and year fixed effects, when recoding each bishop's opposition in turn. Bishops are ordered according to resulting coefficient estimate magnitude.

Table 1.C.3: Effects of Bishop Opposition, Dropping City of Buenos Aires

	Disappearances				Killings			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bishop Opposed	−1.65**	−1.42**	−1.11	−1.68*	−0.59**	−0.52**	−0.30	−0.51*
	(0.55)	(0.52)	(0.56)	(0.68)	(0.21)	(0.19)	(0.26)	(0.21)
Adj. R^2	0.14	0.19	0.81	0.09	0.12	0.17	0.77	0.08
Observations	3723	3723	3755	3755	3723	3723	3755	3755
Covariates	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Unit Trends	✓	✓	✓		✓	✓	✓	
Geographic FE	Zone	Subzone	Dept.		Zone	Subzone	Dept.	

$^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$

[1] *All standard errors clustered at the diocese level.*

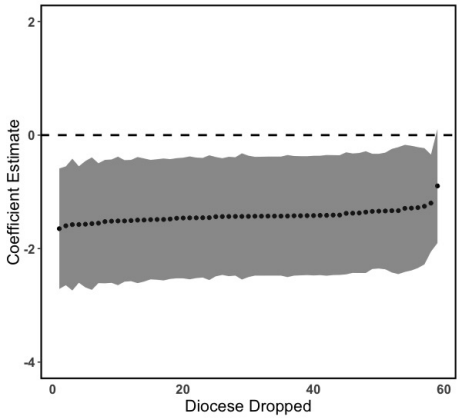
Table 1.C.4: Effects of Bishop Opposition, Dropping Diocese of La Rioja

	Disappearances				Killings			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bishop Opposed	$-3.93^{\dagger}$	-1.45^{*}	-1.64^{*}	-3.59	-0.83^{**}	-0.52^{*}	-0.37	-0.69^{*}
	(2.33)	(0.55)	(0.79)	(2.27)	(0.30)	(0.20)	(0.32)	(0.30)
Adj. R^2	0.12	0.75	0.89	0.09	0.13	0.41	0.82	0.10
Observations	3603	3603	3635	3635	3603	3603	3635	3635
Covariates	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Unit Trends	✓	✓	✓		✓	✓	✓	
Geographic FE	Zone	Subzone	Dept.		Zone	Subzone	Dept.	

$^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$

[1] *All standard errors clustered at the diocese level.*

Figure 1.C.2: Coefficient Estimates Dropping Dioceses One at a Time



Note: Figure depicts coefficient estimates and confidence intervals for the effect of opposed bishops on disappearances from specifications including subzone and year fixed effects, when dropping each diocese in turn. Dioceses are ordered according to resulting coefficient estimate magnitude.

Table 1.C.5: Effects of Bishop Opposition, 1976-1978 Only

	Disappearances			Killings		
	(1)	(2)	(3)	(4)	(5)	(6)
Bishop Opposed	-9.60	-3.14^{*}	-9.27	-1.97^{**}	-1.26^{**}	-1.74^{*}
	(5.91)	(1.26)	(6.17)	(0.73)	(0.43)	(0.78)
Adj. R^2	0.23	0.80	0.20	0.24	0.46	0.20
Observations	1410	1410	1422	1410	1410	1422
Covariates	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Unit Trends	✓	✓		✓	✓	
Geographic FE	Zone	Subzone		Zone	Subzone	

$^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$

[1] *All standard errors clustered at the diocese level.*

Table 1.C.6: Negative Binomial Estimates, Bishop Opposition

	Disappearances		Killings	
	(1)	(2)	(3)	(4)
Bishop Opposed	-0.57^{**}	0.15	-0.69^{**}	-0.02
	(0.07)	(0.10)	(0.01)	(0.28)
Observations	4003	3763	4003	3763
Covariates	N	Y	N	Y
Time Trend	Y	Y	Y	Y

$^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$

Table 1.C.7: Effects of Bishop Opposition, Aggregated to Diocese-Year Level

	Disappearances		Killings	
	(1)	(2)	(3)	(4)
Bishop Opposed	−12.45*	−17.29*	−2.92*	−2.25
	(5.42)	(7.82)	(1.24)	(1.67)
Adj. R^2	0.22	0.38	0.28	0.34
Observations	445	321	445	321
Covariates		✓		✓
Year FE	✓	✓	✓	✓
Unit Trends	✓	✓	✓	✓
Geographic FE	Zone	Zone	Zone	Zone

$^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$

Table 1.C.8: Localized Spillovers, 50 km Radius

	Disappearances			Killings		
	(1)	(2)	(3)	(4)	(5)	(6)
Bishop Opposed	−4.95*	−1.39*	−0.96	−1.00**	−0.52*	−0.29
	(2.40)	(0.62)	(0.80)	(0.31)	(0.23)	(0.29)
Opp. Depts. in 50 km	0.18	0.02	0.02	0.03*	0.01	0.00
	(0.14)	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)
Bishop Opp. * Opp. Depts	−0.06	−0.01	−0.02	−0.01	−0.00	−0.00
	(0.09)	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)
Adj. R^2	0.13	0.75	0.89	0.14	0.41	0.82
Observations	3739	3739	3771	3739	3739	3771
Covariates	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Unit Trends	✓	✓		✓	✓	
Geographic FE	Zone	Subzone		Zone	Subzone	

$^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$

[1] *All standard errors clustered at the diocese level.*

Table 1.C.9: Localized Spillovers Test, 100 km Radius

	Disappearances			Killings		
	(1)	(2)	(3)	(4)	(5)	(6)
Bishop Opposed	−3.98	−0.40	−0.89	−0.79**	−0.22	−0.21
	(2.15)	(0.83)	(0.80)	(0.29)	(0.30)	(0.29)
Opp. Depts. in 100 km	0.05	0.00	0.01	0.01*	0.00	0.00
	(0.04)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
Bishop Opposed * Opp. Depts.	−0.02	−0.02	−0.01	−0.00	−0.01	−0.00
	(0.03)	(0.02)	(0.01)	(0.00)	(0.00)	(0.00)
Adj. R^2	0.12	0.75	0.89	0.14	0.41	0.82
Observations	3739	3739	3771	3739	3739	3771
Covariates	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Unit Trends	✓	✓		✓	✓	
Geographic FE	Zone	Subzone		Zone	Subzone	

$^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$

[1] *All standard errors clustered at the diocese level.*

Table 1.C.10: Localized Spillovers Test, 50 km with Autocovariate

	Disappearances			Killings		
	(1)	(2)	(3)	(4)	(5)	(6)
Bishop Opposed	−4.66*	−1.25*	−4.03	−0.94**	−0.48*	−0.77**
	(2.28)	(0.61)	(2.02)	(0.30)	(0.23)	(0.28)
Opp. Depts. in 50 km	0.13	−0.00	0.15	0.02	−0.00	0.02
	(0.12)	(0.03)	(0.14)	(0.01)	(0.01)	(0.01)
Bishop Opposed * Opp. Depts.	−0.07	−0.02	−0.08	−0.01	−0.01	−0.01
	(0.09)	(0.03)	(0.10)	(0.01)	(0.01)	(0.01)
Adj. R^2	0.13	0.75	0.11	0.14	0.41	0.11
Observations	3715	3715	3715	3715	3715	3715
Autocovariate	Y	Y	Y	Y	Y	Y
Covariates	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Unit Trends	✓	✓		✓	✓	
Geographic FE	Zone	Subzone		Zone	Subzone	

$^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$

[1] *All standard errors clustered at the diocese level.*

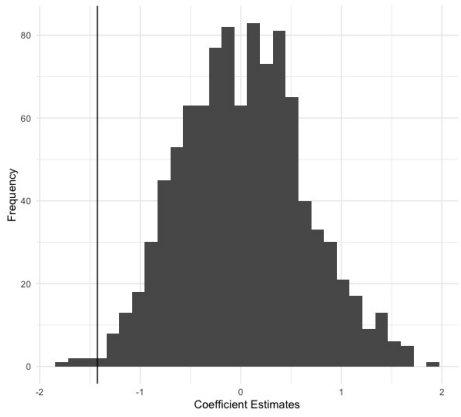
Table 1.C.11: Localized Spillovers Test, 100km with Autocovariate

	Disappearances			Killings		
	(1)	(2)	(3)	(4)	(5)	(6)
Bishop Opposed	−2.37	−0.19	−2.35	−0.55	−0.17	−0.49
	(1.53)	(0.86)	(1.65)	(0.28)	(0.31)	(0.27)
Opp. Depts. in 100 km	−0.03	−0.01	−0.02	−0.00	−0.00	−0.00
	(0.03)	(0.01)	(0.03)	(0.00)	(0.00)	(0.01)
Bishop Opposed Opp. Depts.	−0.04	−0.02	−0.03	−0.01	−0.01	−0.01
	(0.04)	(0.02)	(0.04)	(0.00)	(0.00)	(0.00)
Adj. R^2	0.13	0.75	0.10	0.14	0.41	0.11
Observations	3715	3715	3715	3715	3715	3715
Autocovariate	Y	Y	Y	Y	Y	Y
Covariates	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Unit Trends	✓	✓		✓	✓	
Geographic FE	Zone	Subzone		Zone	Subzone	

$^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$

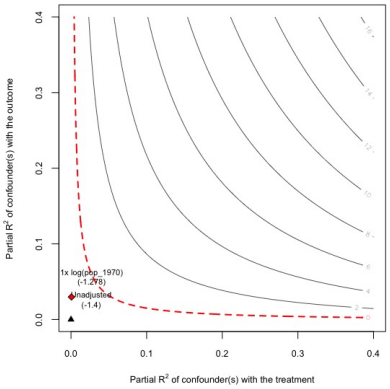
[1] *All standard errors clustered at the diocese level.*

Figure 1.C.3: Coefficient Estimates from Placebo Tests

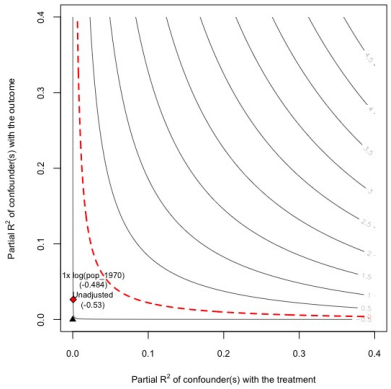


Note: Figure depicts coefficient estimates from models using 1000 random reassignments of bishops to different dioceses. Vertical line represents the coefficient estimate from the model regressing disappearances on bishop opposition—including covariates, year effects, and unit-specific polynomial trends with the actual data, significant at the $p = 0.05$ level.

Figure 1.C.4: Sensitivity Analyses



(a) Disappearances Models



(b) Killings Models

A.4 D: Instrumental Variables Analysis

1. Results for instrumental variables for bishops by papal appointment, Table 1.D.1.
2. Diagnostics for instrumental variables analysis, Table 1.D.2.
3. Test from Tuñón (2018) verifying no negative relationship between pope year of tenure and bishop appointee age, Table 1.D.3.
4. Compliance plot, similar to Tuñón (2018) showing bishop compliance with institutional retirement rule, Figure 1.D.1.

Table 1.D.1: Instrumented Bishop Opposition by Papal Appointment

	Disappearances	Killings
	(1)	(2)
$\widehat{\text{Bishop Opposed}}$	$-4.06^{\dagger}$	$-1.16^{\dagger}$
	(2.28)	(0.63)
Year	-1.03^{**}	-0.28^{**}
	(0.34)	(0.07)
(Intercept)	2049.84	546.46
	(680.91)	(144.77)
Observations	3801	3801

$^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$

[1] *All standard errors clustered at the diocese level.*

Table 1.D.2: Instrumental Variables Diagnostics

	Df1	Df2	Statistic	P-value
Weak Instruments	1	3801	73.45	0.00
Wu-Hausman	1	3800	0.24	0.63

Table 1.D.3: Papal Tenure and Age of Bishop Appointees

	Age of Bishop Appointee	
	(John XXIII)	(Paul VI)
Papal Tenure	−2.50	0.37^{*}
	(1.93)	(0.16)
(Intercept)	4950.33	−674.90
	(3788.96)	(307.77)
Adj. R^2	0.04	0.09
Observations	9	45

$^{**}p < 0.01$; $^{*}p < 0.05$, $^{\dagger}p < 0.10$

Figure 1.D.1: Bishop Compliance with Institutional Retirement Rule

Note: Figure depicts bishop compliance with institutional retirement rule of age 75 beginning in 1976. This forms the basis for exogenous vacancies filled by Pope John Paul II beginning in October 1978.

A.5 E: Alternative Explanation

1. Test of alternative explanation with supporting bishops, Table 1.E.1.
2. Test of alternative explanation with opposed bishops dropped, Table 1.E.2.
3. Verification the above results are not an artifact of the reduced sample, Figure 1.E.1.
4. Comparing opposed bishops with only supporting bishops, Table 1.E.3.

Table 1.E.1: Comparing Effects of Opposed and Supporting Bishops

	Disappearances				Killings			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bishop Opposed	−4.16	−1.93**	−1.09	−3.69	−0.78*	−0.59**	−0.37	−0.63†
	(2.47)	(0.58)	(0.96)	(2.43)	(0.33)	(0.21)	(0.38)	(0.32)
Bishop Support	−0.47	−0.66	0.15	−0.08	0.04	−0.08	−0.04	0.11
	(1.33)	(0.60)	(0.78)	(1.21)	(0.25)	(0.19)	(0.25)	(0.21)
Adj. R^2	0.12	0.75	0.89	0.09	0.13	0.41	0.82	0.10
Observations	3731	3731	3763	3763	3731	3731	3763	3763
Covariates	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Unit Trends	✓	✓	✓		✓	✓	✓	
Geographic FE	Zone	Subzone	Dept.		Zone	Subzone	Dept.	

$^{**}p < 0.01$; $^{*}p < 0.05$, $^{\dagger}p < 0.10$

[1] *All standard errors clustered at the diocese level.*

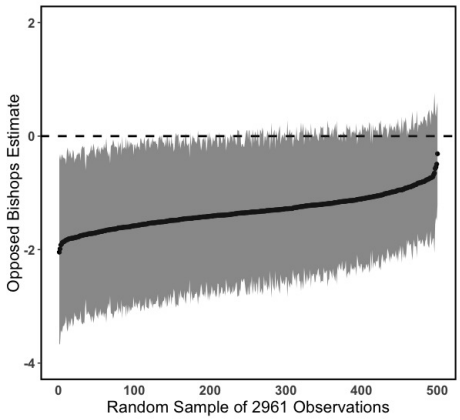
Table 1.E.2: Supporting Bishops and Repression

	Disappearances				Killings			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bishop Support	0.61	−0.93	−1.49	0.83	0.15	−0.15	−0.64	0.21
	(1.92)	(0.63)	(1.05)	(1.79)	(0.31)	(0.17)	(0.43)	(0.25)
Adj. R^2	0.14	0.76	0.88	0.12	0.14	0.43	0.82	0.11
Observations	2697	2697	2717	2717	2697	2697	2717	2717
Covariates	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Unit Trends	✓	✓	✓		✓	✓	✓	
Geographic FE	Zone	Subzone	Dept.		Zone	Subzone	Dept.	

$^{**}p < 0.01$; $^{*}p < 0.05$, $^{\dagger}p < 0.10$

[1] *All standard errors clustered at the diocese level.*

Figure 1.E.1: Coefficient Estimates with 500 Random Samples of 2961 Observations



Note: Figure depicts coefficient estimates and confidence intervals for the effect of opposed bishops on disappearances, when taking random samples of equivalent size to the test of the alternative explanation.

Table 1.E.3: Comparing Opposed with Supporting Bishops

	Disappearances				Killings			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bishop Opposed	-1.75^{**}	-1.29^{*}	-1.31	-1.73^{*}	-0.69^{*}	-0.69^{*}	-0.36	-0.56^{*}
	(0.61)	(0.61)	(0.80)	(0.72)	(0.26)	(0.26)	(0.39)	(0.25)
Adj. R^2	0.18	0.25	0.81	0.11	0.16	0.16	0.79	0.11
Observations	2733	2733	2765	2765	2733	2733	2765	2765
Covariates	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Unit Trends	✓	✓	✓		✓	✓	✓	
Geographic FE	Zone	Subzone	Dept.		Zone	Subzone	Dept.	

$^{**}p < 0.01$; $^{*}p < 0.05$, $^{\dagger}p < 0.10$

[1] *All standard errors clustered at the diocese level.*

A.6 F: Mechanism Tests

A.6.1 Local Assistance

1. Test for bishop-priest complementarities, Table 1.F.1.

Table 1.F.1: Priest-Bishop Interaction Effects

	Disappearances				Killings			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bishop Opposed	0.41	−1.15	−1.11	0.26	−0.22	−0.31	−0.23	−0.18
	(1.65)	(0.68)	(0.71)	(1.62)	(0.25)	(0.20)	(0.27)	(0.25)
MSTM Priests	1.27	0.12	0.20**	1.29	0.18*	0.06*	0.35***	0.18
	(0.91)	(0.10)	(0.06)	(0.94)	(0.09)	(0.03)	(0.06)	(0.09)
MSTM * Bishop Opp.	−1.23	−0.08	−0.02	−1.18	−0.17*	−0.06*	−0.03†	−0.16†
	(0.86)	(0.09)	(0.06)	(0.88)	(0.08)	(0.03)	(0.02)	(0.08)
Adj. R^2	0.14	0.75	0.89	0.11	0.14	0.41	0.82	0.11
Observations	3731	3731	3763	3763	3731	3731	3763	3763
Covariates	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Unit Trends	✓	✓	✓		✓	✓	✓	
Geographic FE	Zone	Subzone	Dept.		Zone	Subzone	Dept.	

** $p < 0.01$; * $p < 0.05$, † $p < 0.10$

[1] *All standard errors clustered at the diocese level.*

A.6.2 Confirming Local Assistance: Religious Orders

1. Enumeration of parishes in Buenos Aires with opposed religious orders, Table 1.F.2.
2. Test indicating CCDs are a valid proxy for repression, Table 1.F.3.
3. Test of explanation in which religious orders generally drive the results, Table 1.F.4.
4. Test of opposed religious order parishes' distance to police stations not converted to CCDs, Table 1.F.5.

Table 1.F.2: Opposed Religious Orders in Buenos Aires

Parish	Order
San Pedro Apostol	Salesianos
San Juan Evangelista	Salesianos
San Carlos Borromeo	Salesianos
San Juan Bosco	Salesianos
Nrta. Sra. de los Remedios	Salesianos
San Patricio	Palotinos
Santa Isabel de Hungría	Palotinos
Santa Cruz	Pasionistas
Ntra Sra. de las Mercedes	Asuncionista
San Martín de Tours	Asuncionista
Santo Cristo	Sagrados Corazones
Jesus Salvador	Sagrados Corazones
Ntra. Sra. de Fátima	Sagrados Corazones
Ntra. Sra. de los Dolores	Sagrados Corazones
Santa Maria Magdelena de Betania	Sagrados Corazones

Table 1.F.3: CCDs As a Valid Proxy for Repression

	Disappearances		Killings	
	(1)	(2)	(3)	(4)
CCDs	5.87**	5.13**	1.00**	0.97**
	(1.97)	(1.71)	(0.15)	(0.16)
Adj. R^2	0.50	0.57	0.39	0.40
Observations	3828	3796	3828	3796
Fixed Effects	Subzone		Subzone	
Covariates	Y	Y	Y	Y

$^{**}p < 0.01$; $^{*}p < 0.05$, $^{\dagger}p < 0.10$

[1] *All standard errors clustered at the diocese level.*

Table 1.F.4: Religious Orders and Nearest CCD Distance

	Distance to Nearest CCD (km)		
	(1)	(2)	(3)
Any Order	0.13	0.15	0.08
	(0.10)	(0.09)	(0.07)
Adj. R^2	0.01	0.15	0.34
Observations	154	154	154
Fixed Effects		Zone	Neighborhood

$^{**}p < 0.01$; $^{*}p < 0.05$, $^{\dagger}p < 0.10$

Table 1.F.5: Religious Orders and Nearest Unconverted CCD Distance

	Nearest Unconverted CCD (km)		
	(1)	(2)	(3)
Opposed Order	-0.34^{*}	-0.28	-0.20
	(0.15)	(0.18)	(0.19)
Adj. R^2	0.01	0.06	0.62
Observations	154	154	154
Fixed Effects		Zone	Neighborhood

$^{**}p < 0.01$; $^{*}p < 0.05$, $^{\dagger}p < 0.10$

A.6.3 Public Appeals

1. Outcome test for bishop opposition's relationship with Catholic adherence in each diocese, Table 1.F.6.

Table 1.F.6: Relationship of Bishop Opposition with Post-Regime Catholicity

	Δ Catholic Adherence, 1970-1988		
	(1)	(2)	(3)
Bishop Opposed	5.61^{*}	$5.31^{\dagger}$	4.87^{*}
	(2.19)	(2.90)	(1.90)
Adj. R^2	0.52	0.23	0.56
Observations	472	459	455
Fixed Effects	Subzone		Subzone
Covariates	N	Y	Y

$^{**}p < 0.01$; $^{*}p < 0.05$, $^{\dagger}p < 0.10$

[1] *All standard errors clustered at the diocese level.*

A.6.4 Backlash Mobilization

1. Test for bishop-ecclesial base community complementarities, Table 1.F.7. This test interacts the indicator of whether a diocese had ecclesial base communities in 1976. Ecclesial base communities were under the authority of diocesan bishops, and were hubs of activity for the progressive church during the dictatorship

and involved in resisting the Dirty War.

Table 1.F.7: Opposed Bishops and Ecclesial Base Communities

	Base Community Support			
	(1)	(2)	(3)	(4)
Bishop Opposed	0.12	0.15*	−0.09	0.12†
	(0.08)	(0.07)	(0.08)	(0.07)
Adj. R^2	0.27	0.43	0.91	0.23
Observations	3763	3763	3795	3795
Covariates	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Unit Trends	✓	✓	✓	
Geographic FE	Zone	Subzone	Dept.	

$^{**}p < 0.01$; $^{*}p < 0.05$, $^{\dagger}p < 0.10$

[1] *All standard errors clustered at the diocese level.*

A.6.5 Private Appeals

1. Test for bishop-episcopal conference complementarities, Table 1.F.8. This test interacts an episcopal conference indicator—whether a bishop was an active member of the Argentine episcopal conference—with the main indicator for a resisting bishop (Manzanares 1988).

2. Test for persuadable regime officers, Table 1.F.9. This test interacts an indicator for whether a given department was under the jurisdiction of the Argentine cavalry during the regime with the opposed bishop indicator. Cavalry officers were considered the most reluctant to carry out the Dirty War, and thus could be more willing to reduce repression under persuasion from an opposed bishop (Scharpf 2018).

Table 1.F.8: Opposed Bishop Heterogeneous Effects by Episcopal Conference Service

	Disappearances			Killings		
	(1)	(2)	(3)	(4)	(5)	(6)
Bishop Opposed	−3.35	−1.57*	−0.55	−0.82*	−0.54*	−0.28
	(2.06)	(0.66)	(0.70)	(0.31)	(0.21)	(0.29)
Episcopal Conference	0.71	−0.89	0.88	−0.09	−0.14	0.02
	(2.02)	(1.14)	(0.54)	(0.35)	(0.29)	(0.22)
Bishop Opposed * Conference	−1.98	−0.17	−4.03*	0.04	−0.13	−0.67
	(3.56)	(1.21)	(1.71)	(0.49)	(0.41)	(0.47)
Adj. R^2	0.12	0.75	0.89	0.13	0.41	0.82
Observations	3731	3731	3763	3731	3731	3763
Covariates	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Unit Trends	✓	✓		✓	✓	
Geographic FE	Zone	Subzone		Zone	Subzone	

$^{**}p < 0.01$; $^{*}p < 0.05$, $^{\dagger}p < 0.10$

[1] *All standard errors clustered at the diocese level.*

Table 1.F.9: Opposed Bishop Heterogeneous Effects by Cavalry Officers

	Disappearances			Killings		
	(1)	(2)	(3)	(4)	(5)	(6)
Bishop Opposed	−3.62	−1.36*	−1.17	−0.81**	−0.50*	−0.32
	(2.09)	(0.55)	(0.73)	(0.27)	(0.20)	(0.28)
Cavalry Officer	−3.37	−0.26	0.72	−0.83*	−0.21	0.48*
	(2.26)	(0.64)	(0.59)	(0.35)	(0.20)	(0.19)
Bishop Opp. * Cavalry	−12.44	−1.28	−0.51	−1.85*	−0.62*	−0.38
	(8.48)	(1.10)	(0.81)	(0.85)	(0.28)	(0.27)
Adj. R^2	0.13	0.75	0.89	0.14	0.41	0.82
Observations	3731	3731	3763	3731	3731	3763
Covariates	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Unit Trends	✓	✓		✓	✓	
Geographic FE	Zone	Subzone		Zone	Subzone	

$^{**}p < 0.01$; $^{*}p < 0.05$, $^{\dagger}p < 0.10$

[1] *All standard errors clustered at the diocese level.*

A.7 G: Archival Evidence

Table 1.G.1: Summary of Archival Documents Used in Coding

Source	No. Docs Used[a]	Type	Topics
ANM[b]	137	Press, reports, testimonials	Bishop activities, process of repression
CELS[c]	158	Press, military memos, academic studies	Bishop activities, repression, human rights
Conti Library	6	Books	Ecclesiastical law, MSTM, military strategy
CyR[d]	31	Magazines	Bishop activities, MSTM activities
Criterio[e]	12	Magazines	Bishop activities
Cabildo[f]	23	Magazine	Bishop activities, regime strategy
U.S. govt.[g]	232	Memos, publications intel. briefs	Bishop activities, human rights, repression

[a] This represents not total documents consulted but documents from which coding decisions were made.

[b] Archive of National Memory.

[c] Center for Legal and Social Studies.

[d] Cristianismo y Revolucion, a progressive Catholic magazine published in Argentina 1966-1971.

[e] A mainstream Catholic magazine.

[f] A conservative Catholic magazine, using editions published from 1973-1982.

[g] From the Argentine Declassification Project; consulted using keyword searches.

A.8 H: Scope Conditions

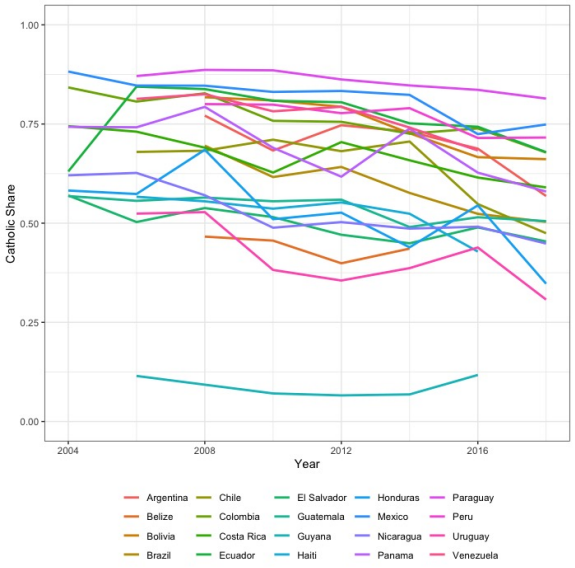
1. Evidence of Catholic Church influence in Argentina. Respondents indicated agreement with domestic institutions on a 100-point scale (Turner 1971), Table 1.H.1.

2. Catholic share in twenty Latin America countries, 2004-2018, Figure 1.H.1. Data are from the Latin American Public Opinion Project (2020).

Table 1.H.1: Public Agreement with Argentine Institutions, 1971

Institution	Mean Agreement
Unions	41.4
Church	35.2
Industrial Firms	32.8
Politicians	30.5
Military	24.6

Figure 1.H.1: Catholic Share in Latin America, 2004-2018



Appendix B

Political Competition and Authoritarian Repression

B.1 A: Description of Data

1. Descriptive statistics for municipality-level variables, Table 2.A.1.

2. Descriptive statistics for individual-level variables, Table 2.A.2.

Table 2.A.1: Descriptive Statistics, Municipality Level

Variable	Minimum	Maximum	Mean	Median	Std. Dev	Missing
Total Victims	0	121	2.42	0	8.45	0
Non-Dissident Victims	0	72	1.4	0	5.48	0
Close Election, 1973	0	1	0.16	0	0.37	9
Log(Total Votes, 1970)	1.61	12.23	7.68	7.63	1.5	3
Infant Mortality, 1972	0	77.78	10.06	8.75	7.91	2
Santiago	0	1	0.1	0	0.31	0
Army Base	0	1	0.02	0	0.15	1

Table 2.A.2: Descriptive Statistics, Individual Level

Variable	Minimum	Maximum	Mean	Median	Std. Dev	Missing
Age	1	72	29.1	27	10.45	71
Female	0	1	0.04	0	0.2	0
Leftist Partisan	0	1	0.34	0	0.48	132
Mass Killing Victim	0	1	0.25	0	0.44	3
Date of Death	0	111	24.97	22	22.84	4

B.2 B: Robustness Tests

1. Main results using a close election explanatory variable constructed according to Balcells (2010) in Table 2.B.1. This measurement uses the following formula, which ranges from zero (election not competitive) to one (election maximally competitive). A dummy variable using the top quintile of the Balcells index is taken for comparability with the primary close election measure in the body of the text.

$$1 - \left(\frac{\text{Popular Unity Vote} - \text{Non-Popular Unity Vote}}{100} \right)^2$$

2. Main results with a three-point ordinal scale of regime opposition, Table 2.B.2. This scale takes the value of 1 if the UP had less than or equal to 47% of the vote, a value of 2 if there was a close election, and a value of 3 if the municipality had a UP vote share greater than or equal to 53%. To capture the expected shape of the relationship, I estimate a second-order polynomial for the regime opposition scale. If the theory is correct the quadratic term will be negative, suggesting a negative curvilinear relationship between regime opposition and frequency of repression as well as the targeting of non-dissidents. The estimate for the quadratic term on the ordinal scale is negative and statistically significant, consistent with expectations.

3. Main results using a continuous Popular Unity vote margin explanatory variable instead of a close election dummy. This variable takes a lower value if the election was more competitive. The theory does not predict differences in political killings and non-dissident victims based on vote margin when elections are not close. Using a continuous measure therefore attenuates the relationship. Results are in Table 2.B.3, with vote margin logged due to right skew.

4. Negative binomial models for main results, Table 2.B.4.

5. Main results using varying operationalizations of non-dissidents, in Table 2.B.5. In the more restrictive operationalization, students are reclassified as dissidents rather than non-dissidents. In the less restrictive operationalization, rank-and-file party members are reclassified as non-dissidents rather than dissidents.

6. Varying electoral margins, 52%-59% UP vote share for total victims, Table 2.B.6.

7. Varying electoral margins, 60%-67% UP vote share for total victims, Table 2.B.7.

8. Varying electoral margins, 52%-59% UP vote share for non-dissident victims, Table 2.B.8.

9. Varying electoral margins, 60%-67% UP vote share for non-dissident victims, Table 2.B.9.

10. Subset of data to September 13 and later for main results, Table 2.B.10.

11. Sensitivity analysis for main results: total victims, Table 2.B.11.

12. Sensitivity analysis for main results: non-dissident victims, Table 2.B.12.

13. Replacing Santiago variable with region level dummy variables, Table 2.B.13.

Table 2.B.1: Main Results with Balcells (2010) Close Election Measure

	Dependent variable:			
	Total Victims		Non-Dissident Victims	
	(1)	(2)	(3)	(4)
Balcells Close Election	3.39^{**}	2.03^{*}	1.91^{*}	$1.13^{\dagger}$
	(1.20)	(1.02)	(0.84)	(0.68)
Infant Mortality, 1972		0.03		0.02
		(0.02)		(0.01)
Log(Total Votes, 1970)		1.37^{**}		0.78^{**}
		(0.47)		(0.27)
Santiago		11.11^{***}		8.19^{***}
		(2.25)		(1.57)
Army Base		5.77^{*}		0.98
		(2.34)		(1.19)
(Intercept)	1.73^{***}	-10.19^{**}	1.02^{***}	-5.91^{**}
	(0.40)	(3.41)	(0.25)	(2.03)
Adj. R^2	0.02	0.34	0.02	0.34
Observations	453	453	453	453

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$,$^{\dagger}p < 0.1$

Table 2.B.2: Political Killings with Ordinal Scale of Opposition Support

	Dependent variable:			
	Total Victims		Non-Dissident Victims	
	(1)	(2)	(3)	(4)
Opposition Support	1.24	4.95	0.68	2.84
	(6.45)	(5.20)	(4.38)	(3.54)
Opposition Support, Sq.	-28.83^{**}	-18.75^{*}	-16.12^{*}	$-10.45^{\dagger}$
	(10.51)	(8.36)	(7.40)	(5.73)
Infant Mortality, 1972		0.03		0.02
		(0.02)		(0.01)
Log(Total Votes, 1970)		1.37^{**}		0.78^{**}
		(0.46)		(0.27)
Santiago		11.15^{***}		8.22^{***}
		(2.23)		(1.56)
Army Base		5.85^{*}		1.02
		(2.33)		(1.20)
(Intercept)	2.40^{***}	-9.74^{**}	1.40^{***}	-5.66^{**}
	(0.39)	(3.43)	(0.26)	(2.04)
Adj. R^2	0.02	0.34	0.01	0.34
Observations	453	453	453	453

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$,$^{\dagger}p < 0.1$

Table 2.B.3: Main Results with Log(Vote Margin)

	Dependent variable:			
	Total Victims		Non-Dissident Victims	
	(1)	(2)	(3)	(4)
Log(Margin)	-1.50^{***}	-0.47	-0.84^{**}	-0.24
	(0.42)	(0.46)	(0.29)	(0.28)
Infant Mortality, 1972		0.02		0.02
		(0.02)		(0.01)
Log(Total Votes, 1970)		1.37^{**}		0.78^{**}
		(0.51)		(0.30)
Santiago		11.06^{***}		8.16^{***}
		(2.25)		(1.57)
Army Base		5.84^{*}		1.01
		(2.34)		(1.16)
(Intercept)	5.64^{***}	-8.71	3.23^{***}	-5.18
	(1.09)	(4.54)	(0.76)	(2.68)
Adj. R^2	0.03	0.33	0.02	0.34
Observations	453	453	453	453

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$,$^{\dagger}p < 0.1$

Table 2.B.4: Political Killings and Political Competition (Count)

	Dependent variable:			
	Total Victims		Non-Dissident Victims	
	(1)	(2)	(3)	(4)
Close Election, 1973	1.10^{**}	1.06^{***}	1.06^{*}	1.02^{***}
	(0.38)	(0.21)	(0.44)	(0.24)
Log(Total Votes, 1970)		0.92^{***}		0.80^{***}
		(0.08)		(0.09)
Infant Mortality, 1972		0.03^{*}		0.02
		(0.01)		(0.01)
Santiago		1.81^{***}		2.25^{***}
		(0.25)		(0.26)
Army Base		0.83		0.80
		(0.44)		(0.45)
(Intercept)	0.60^{***}	-8.53^{***}	0.07	-8.26^{***}
	(0.15)	(0.71)	(0.18)	(0.81)
Log Likelihood	-646.43	-518.97	-486.74	-376.90
Observations	453	453	453	453

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$,$^{\dagger}p < 0.1$

Table 2.B.5: Varying Operationalizations of Non-Dissident Victims

	Dependent variable:			
	Non-Dissident Victims (Less Strict)		Non-Dissident Victims (More Strict)	
	(1)	(2)	(3)	(4)
Close Election, 1973	3.40^{*}	2.34^{*}	2.42^{*}	1.63
	(1.33)	(1.07)	(1.14)	(0.91)
Infant Mortality, 1972		0.03		0.02
		(0.02)		(0.01)
Log(Total Votes, 1970)		1.26^{**}		0.92^{**}
		(0.41)		(0.34)
Santiago		10.68^{***}		9.84^{***}
		(2.12)		(1.89)
Army Base		5.08^{*}		1.95
		(2.10)		(1.42)
(Intercept)	1.69^{***}	-9.37^{**}	1.28^{***}	-7.01^{**}
	(0.36)	(3.06)	(0.31)	(2.55)
Adj. R^2	0.02	0.35	0.02	0.34
Observations	453	453	453	453

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 2.B.6: Changing Margins for Close Election Variable, 52-48 to 59-41 UP Vote Share Ranges (Total Killings)

	Dependent variable:							
	Political Killing Victims							
	52-48	53-47	54-46	55-45	56-44	57-43	58-42	51-49
Close Elec. (2 pt)	3.18^{**}							
	(1.03)							
Close Elec. (3 pt)		2.50^{**}						
		(0.90)						
Close Elec. (4 pt)			2.06^{*}					
			(0.83)					
Close Elec. (5 pt)				1.56^{*}				
				(0.76)				
Close Elec. (6 pt)					1.24			
					(0.73)			
Close Elec. (7 pt)						0.49		
						(0.70)		
Close Elec. (8 pt)							0.53	
							(0.69)	
Close Elec. (9 pt)								0.35
								(0.68)
Infant Mort.	0.03	0.03	0.03	0.03	0.03	0.02	0.02	0.02
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Log(Votes)	1.57^{***}	1.57^{***}	1.57^{***}	1.58^{***}	1.59^{***}	1.64^{***}	1.64^{***}	1.65^{***}
	(0.25)	(0.25)	(0.25)	(0.25)	(0.25)	(0.25)	(0.25)	(0.25)
Santiago	10.62^{***}	10.71^{***}	10.68^{***}	10.77^{***}	10.75^{***}	10.56^{***}	10.56^{***}	10.56^{***}
	(1.17)	(1.18)	(1.18)	(1.19)	(1.19)	(1.19)	(1.19)	(1.19)
Army Base	5.61^{*}	5.84^{**}	5.77^{**}	5.80^{**}	5.89^{**}	5.84^{**}	5.88^{**}	5.84^{**}
	(2.20)	(2.20)	(2.20)	(2.21)	(2.21)	(2.22)	(2.22)	(2.22)
(Intercept)	-11.48^{***}	-11.53^{***}	-11.57^{***}	-11.61^{***}	-11.70^{***}	-11.78^{***}	-11.77^{***}	-11.78^{***}
	(1.96)	(1.96)	(1.97)	(1.97)	(1.97)	(1.98)	(1.98)	(1.98)
Adj. R^2	0.33	0.33	0.33	0.32	0.32	0.32	0.32	0.32
Observations	453	453	453	453	453	453	453	453

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$,$^{\dagger}p < 0.1$

Table 2.B.7: Changing Margins for Close Election Variable, 60-40 to 67-33 UP Vote Share Ranges (Total Killings)

	Dependent variable:							
	Political Killing Victims							
	60-40	61-39	62-38	63-37	64-36	65-35	66-34	67-33
Close Elec. (10 pt)	-0.23							
	(0.68)							
Close Elec. (11 pt)		-0.31						
		(0.68)						
Close Elec. (12 pt)			-0.40					
			(0.69)					
Close Elec. (13 pt)				-0.58				
				(0.69)				
Close Elec. (14 pt)					-0.77			
					(0.70)			
Close Elec. (15 pt)						0.30		
						(0.73)		
Close Elec. (16 pt)							0.13	
							(0.75)	
Close Elec. (17 pt)								0.11
								(0.78)
Infant Mort.	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Log(Votes)	1.69^{***}	1.70^{***}	1.71^{***}	1.73^{***}	1.75^{***}	1.64^{***}	1.66^{***}	1.66^{***}
	(0.25)	(0.25)	(0.26)	(0.26)	(0.26)	(0.26)	(0.26)	(0.26)
Santiago	10.58^{***}	10.58^{***}	10.60^{***}	10.59^{***}	10.57^{***}	10.57^{***}	10.58^{***}	10.57^{***}
	(1.19)	(1.19)	(1.19)	(1.19)	(1.19)	(1.19)	(1.19)	(1.19)
Army Base	5.91^{**}	5.90^{**}	5.89^{**}	5.87^{**}	5.91^{**}	5.85^{**}	5.86^{**}	5.87^{**}
	(2.22)	(2.22)	(2.22)	(2.21)	(2.21)	(2.22)	(2.22)	(2.22)
(Intercept)	-11.89^{***}	-11.92^{***}	-11.93^{***}	-11.99^{***}	-12.01^{***}	-11.78^{***}	-11.82^{***}	-11.84^{***}
	(1.98)	(1.98)	(1.98)	(1.98)	(1.98)	(1.98)	(1.98)	(1.98)
Adj. R^2	0.32	0.32	0.32	0.32	0.32	0.32	0.32	0.32
Observations	453	453	453	453	453	453	453	453

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$,$^{\dagger}p < 0.1$

Table 2.B.8: Changing Margins for Close Election Variable, 52-48 to 59-41 UP Vote Share Ranges (Non-Dissident Victims)

	Dependent variable:							
	Non-Dissident Victims							
	52-48	53-47	54-46	55-45	56-44	57-43	58-42	51-49
Close Elec. (2pt)	1.82**							
	(0.66)							
Close Elec. (3pt)		1.39*						
		(0.58)						
Close Elec. (4pt)			1.13*					
			(0.54)					
Close Elec. (5pt)				0.85				
				(0.49)				
Close Elec. (6pt)					0.66			
					(0.47)			
Close Elec. (7pt)						0.13		
						(0.45)		
Close Elec. (8pt)							0.23	
							(0.44)	
Close Elec. (9pt)								0.24
								(0.44)
Infant Mort.	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Log(Votes)	0.81***	0.81***	0.81***	0.82***	0.83***	0.86***	0.85***	0.85***
	(0.16)	(0.16)	(0.16)	(0.16)	(0.16)	(0.16)	(0.16)	(0.16)
Santiago	8.09***	8.14***	8.12***	8.16***	8.15***	8.05***	8.05***	8.05***
	(0.76)	(0.76)	(0.76)	(0.76)	(0.77)	(0.76)	(0.76)	(0.76)
Army Base	0.88	1.01	0.98	0.99	1.04	1.02	1.03	1.01
	(1.43)	(1.43)	(1.43)	(1.43)	(1.44)	(1.44)	(1.44)	(1.44)
(Intercept)	−6.09***	−6.12***	−6.14***	−6.17***	−6.22***	−6.28***	−6.26***	−6.25***
	(1.26)	(1.27)	(1.27)	(1.27)	(1.27)	(1.27)	(1.27)	(1.28)
Adj. R^2	0.35	0.34	0.34	0.34	0.34	0.34	0.34	0.34
Observations	453	453	453	453	453	453	453	453

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$,$^{\dagger}p < 0.1$

Table 2.B.9: Changing Margins for Close Election Variable, 60-40 to 67-33 UP Vote Share Ranges (Non-Dissident Victims)

	Dependent variable:							
	Non-Dissident Victims							
	60-40	61-39	62-38	63-37	64-36	65-35	66-34	67-33
Close Elec. (10 pt)	−0.12							
	(0.44)							
Close Elec. (11 pt)		−0.19						
		(0.44)						
Close Elec. (12 pt)			−0.22					
			(0.44)					
Close Elec. (13 pt)				−0.32				
				(0.45)				
Close Elec. (14 pt)					−0.44			
					(0.45)			
Close Elec. (15 pt)						0.15		
						(0.47)		
Close Elec. (16 pt)							0.09	
							(0.49)	
Close Elec. (17 pt)								0.06
								(0.51)
Infant Mort.	0.02	0.02	0.02	0.02	0.02	0.01	0.01	0.02
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Log(Votes)	0.88***	0.89***	0.89***	0.90***	0.91***	0.85***	0.86***	0.86***
	(0.16)	(0.16)	(0.16)	(0.17)	(0.17)	(0.17)	(0.17)	(0.17)
Santiago	8.07***	8.07***	8.07***	8.07***	8.06***	8.06***	8.06***	8.06***
	(0.77)	(0.77)	(0.77)	(0.76)	(0.76)	(0.76)	(0.77)	(0.76)
Army Base	1.05	1.05	1.04	1.03	1.05	1.02	1.03	1.03
	(1.44)	(1.44)	(1.44)	(1.44)	(1.44)	(1.44)	(1.44)	(1.44)
(Intercept)	−6.32***	−6.34***	−6.34***	−6.37***	−6.39***	−6.26***	−6.28***	−6.29***
	(1.28)	(1.28)	(1.28)	(1.28)	(1.28)	(1.28)	(1.28)	(1.27)
Adj. R^2	0.34	0.34	0.34	0.34	0.34	0.34	0.34	0.34
Observations	453	453	453	453	453	453	453	453

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$,$^{\dagger}p < 0.1$

Table 2.B.10: Political Killing Victims and Political Competition, September 13 and Later

	Dependent variable:			
	Total Victims		Non-Dissident Victims	
	(1)	(2)	(3)	(4)
Close Election, 1973	3.49^{**}	2.39^{*}	1.94^{*}	$1.33^{\dagger}$
	(1.31)	(1.04)	(0.91)	(0.71)
Infant Mortality, 1972		0.03		0.02
		(0.02)		(0.01)
Log(Total Votes, 1970)		1.23^{**}		0.68^{**}
		(0.37)		(0.21)
Santiago		10.33^{***}		7.70^{***}
		(2.00)		(1.41)
Army Base		6.05^{**}		1.15
		(2.19)		(1.06)
(Intercept)	1.69^{***}	-9.06^{**}	1.00^{***}	-5.14^{**}
	(0.34)	(2.74)	(0.21)	(1.60)
Adj. R^2	0.03	0.37	0.02	0.37
Observations	448	448	448	448

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$,$^{\dagger}p < 0.1$

Table 2.B.11: Sensitivity Analysis, Political Killings

	Variable	Estimate	Std. Error	Statistic	P.value	Impact to Change Value	Impact
1	(Intercept)	-10.13	2.02	-5.02	0.00		
2	Close Election, 1973	2.49	0.89	2.79	0.01	0.24	
3	Infant Mortality, 1972	0.03	0.04	0.65	0.52		0.01
4	Log(Total Votes, 1970)	1.37	0.26	5.30	0.00		0.07
5	Santiago	11.15	1.18	9.44	0.00		0.01
6	Army Base	5.84	2.20	2.66	0.01		0.01

Table 2.B.12: Sensitivity Analysis, Non-Dissident Victims

	Variable	Estimate	Std. Error	Statistic	P.value	Impact to Change Value	Impact
1	(Intercept)	-5.88	1.31	-4.48	0.00		
2	Close Election, 1973	1.39	0.58	2.40	0.02	0.24	
3	Infant Mortality, 1972	0.02	0.03	0.66	0.51		0.01
4	Log(Total Votes, 1970)	0.78	0.17	4.63	0.00		0.06
5	Santiago	8.21	0.77	10.69	0.00		0.01
6	Army Base	1.01	1.43	0.71	0.48		0.00

Table 2.B.13: Results Including Region Dummies

	Dependent variable:			
	Total Victims		Non-Dissident Victims	
	(1)	(2)	(3)	(4)
Close Election, 1973	3.56^{**}	2.59^{*}	1.93^{*}	$1.42^{\dagger}$
	(1.24)	(1.20)	(0.83)	(0.82)
Infant Mortality, 1972		0.03		0.01
		(0.02)		(0.01)
Log(Total Votes, 1970)		1.71^{**}		0.98^{**}
		(0.55)		(0.33)
Army Base		4.54		0.32
		(2.60)		(1.33)
(Intercept)	0.96	-10.36^{**}	0.15	-6.17^{**}
	(0.61)	(3.42)	(0.15)	(2.05)
Region Dummies	Y	Y	Y	Y
Adj. R^2	0.24	0.32	0.27	0.31
Observations	453	453	453	453

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$,$^{\dagger}p < 0.1$

B.3 C: Mechanism Tests

1. Interaction of close elections and army bases, Table 2.C.1.

2. Interaction of close elections and marriages, Table 2.C.2.

3. Anti-regime votes in 1988 plebiscite, Table 2.C.3. Results are only for the subset of municipalities which match 1973 territorial divisions and 1988 territorial divisions.

4. Difference in means between municipalities with and and without civilian collaboration in repression, Table 2.C.4. For the eight municipalities identified in the Truth and Reconciliation report in which civilians collaborated with repression, there are descriptively closer elections, more non-dissident victims, and more total victims.

Table 2.C.1: Interaction of Close Elections and Army Bases

	Dependent variable:			
	Total Victims		Non-Dissident Victims	
	(1)	(2)	(3)	(4)
Close Election, 1973	3.49^{*}	2.43^{*}	2.10^{*}	$1.45^{\dagger}$
	(1.44)	(1.18)	(1.02)	(0.80)
Army Base	9.38^{**}	5.42	3.75^{*}	1.47
	(3.33)	(2.92)	(1.47)	(1.25)
Close Election x Army Base	-1.16	1.56	-3.52	-1.69
	(4.02)	(3.25)	(2.99)	(2.55)
Infant Mortality, 1972		0.03		0.02
		(0.02)		(0.01)
Log(Total Votes, 1970)		1.37^{**}		0.77^{**}
		(0.46)		(0.27)
Santiago		11.16^{***}		8.20^{***}
		(2.25)		(1.57)
(Intercept)	1.62^{***}	-10.14^{**}	1.00^{***}	-5.87^{**}
	(0.39)	(3.39)	(0.25)	(2.02)
Adj. R^2	0.05	0.34	0.02	0.34
Observations	453	453	453	453

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$,$^{\dagger}p < 0.1$

Table 2.C.2: Interaction of Close Elections and Marriages

	Dependent variable:			
	Total Victims		Non-Dissident Victims	
	(1)	(2)	(3)	(4)
Log(Marriages, 1972)	2.21^{***}	-0.13	1.35^{***}	-0.09
	(0.60)	(0.54)	(0.37)	(0.37)
Close Election, 1973	-12.80^{*}	-8.97	-8.86^{*}	-6.09
	(5.91)	(5.76)	(4.50)	(4.19)
Close Election x Marriages	3.03^{*}	$2.36^{\dagger}$	$2.03^{\dagger}$	1.54
	(1.42)	(1.39)	(1.08)	(1.01)
Infant Mortality, 1972		0.02		0.01
		(0.02)		(0.01)
Log(Total Votes, 1970)		1.38		0.78
		(0.96)		(0.59)
Santiago		10.28^{***}		7.70^{***}
		(2.47)		(1.63)
Army Base		5.06^{*}		0.53
		(2.39)		(1.32)
(Intercept)	-7.38^{***}	-9.49	-4.56^{***}	-5.40
	(2.20)	(5.28)	(1.36)	(3.20)
Adj. R^2	0.26	0.36	0.24	0.37
Observations	434	434	434	434

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$,$^{\dagger}p < 0.1$

Table 2.C.3: Close Elections and 1988 Plebiscite Votes

	Dependent variable:	
	1988 "No" Vote Share	
	(1)	(2)
Close Election, 1973	3.31^{**}	2.05^{*}
	(1.27)	(0.99)
UP 1973 Vote Share	0.56^{***}	0.47^{***}
	(0.05)	(0.04)
Infant Mortality, 1972		-0.10
		(0.11)
Log(Total Votes, 1970)		4.37^{***}
		(0.52)
Santiago		2.70^{*}
		(1.17)
Army Base		-2.13
		(1.98)
(Intercept)	22.39^{***}	-8.99
	(2.32)	(5.20)
Adj. R^2	0.37	0.63
Observations	304	304

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

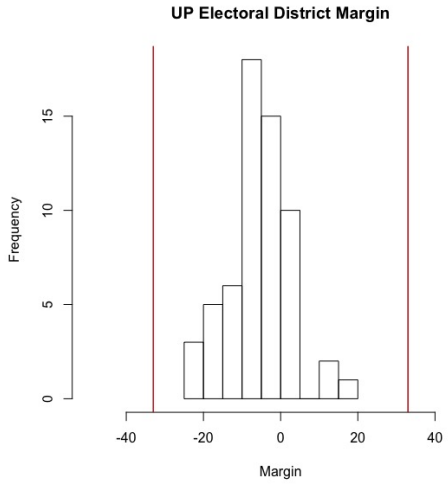
Table 2.C.4: Difference in Municipalities with Civilian Participation in Repression

Variable	Participation Mean	No Participation Mean	p-value
UP Vote Share	48.15%	42.63%	0.058
Total Victims	5.63	2.37	0.181
Non-Dissident Victims	4.25	1.35	0.158

B.4 D: Alternative Explanation: Regime Strategy

1. Histogram of UP vote margins at electoral district level, Figure 2.D.1.
2. District-level data on political killings and political competition, Table 2.D.1.
3. District-level data on political killings and political competition (negative binomial), Table 2.D.2.
4. Caravan of Death and victim type, Table 2.D.3.
5. Caravan of Death and victim type (negative binomial), Table 2.D.4.

Figure 2.D.1: Histogram of UP Vote Margins at District Level



Note: Figure shows distribution of vote margins for UP in March 1973 elections at the future electoral district level. Vertical lines indicate two-seat D'Hondt thresholds for two-coalition competition at which the UP would either lose or gain a seat.

Table 2.D.1: District-Level Variation in Political Killings by Political Competition

	Dependent variable:			
	Total Victims		Non-Dissident Victims	
	(1)	(2)	(3)	(4)
Close Election, 1973	13.18	7.60	9.11	5.96
	(7.70)	(6.58)	(5.93)	(4.52)
Infant Mortality, 1972		0.23		0.59
		(1.19)		(0.79)
Log(Total Votes, 1970)		16.89*		10.72*
		(7.15)		(4.35)
Santiago		27.66***		23.29***
		(7.45)		(5.12)
(Intercept)	13.93***	−173.46*	7.68***	−116.57*
	(3.46)	(73.93)	(2.15)	(46.58)
Adj. R^2	0.04	0.51	0.04	0.54
Observations	60	58	60	58

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$,$^{\dagger}p < 0.1$

Table 2.D.2: District-Level Variation in Political Killings by Political Competition (Count)

	Dependent variable:			
	Total Victims		Non-Dissident Victims	
	(1)	(2)	(3)	(4)
Close Election, 1973	0.67*	0.53*	0.78*	0.51
	(0.31)	(0.24)	(0.39)	(0.28)
Log(Total Votes, 1970)		0.78***		0.91***
		(0.20)		(0.24)
Infant Mortality, 1972		0.05		0.16**
		(0.05)		(0.05)
Santiago		1.24***		2.16***
		(0.32)		(0.37)
(Intercept)	2.63***	−6.77**	2.04***	−10.12***
	(0.18)	(2.15)	(0.22)	(2.50)
Log Likelihood	-231.93	-203.25	-193.79	-162.58
Observations	60	58	60	58

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$,$^{\dagger}p < 0.1$

Table 2.D.3: Caravan of Death and Victim Type

	Dependent variable:			
	Dissident Victims		Non-Dissident Victims	
	(1)	(2)	(3)	(4)
Caravan of Death	3.96**	2.80*	1.03	0.35
	(1.23)	(1.34)	(0.67)	(0.67)
Infant Mortality, 1972		0.01		0.01
		(0.01)		(0.01)
Log(Total Votes, 1970)		0.36**		0.77**
		(0.12)		(0.26)
Santiago		1.65**		8.24***
		(0.56)		(1.57)
Army Base		2.79		0.98
		(1.63)		(1.08)
(Intercept)	0.54***	−2.48**	1.33***	−5.58**
	(0.10)	(0.87)	(0.27)	(1.95)
Adj. R^2	0.11	0.23	-0.00	0.33
Observations	461	459	461	459

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$,$^{\dagger}p < 0.1$

Table 2.D.4: Caravan of Death and Victim Type (Count)

	Dependent variable:			
	Dissident Victims		Non-Dissident Victims	
	(1)	(2)	(3)	(4)
Caravan of Death	2.12^{***}	1.63^{***}	0.57	0.72
	(0.62)	(0.45)	(0.76)	(0.41)
Log(Total Votes, 1970)		0.88^{***}		0.83^{***}
		(0.11)		(0.10)
Infant Mortality, 1972		0.03^{*}		0.02
		(0.01)		(0.01)
Santiago		1.02^{**}		2.23^{***}
		(0.35)		(0.29)
Army Base		0.30		0.46
		(0.59)		(0.50)
(Intercept)	-0.61^{***}	-8.76^{***}	0.29	-8.30^{***}
	(0.15)	(0.96)	(0.17)	(0.84)
Log Likelihood	-409.66	-344.23	-496.89	-386.21
Observations	461	459	461	459

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$,$^{\dagger}p < 0.1$

B.5 E: Alternative Explanation: Different Victim Behavior

1. Exclusion of dissident party leaders and officials from sample, Table 2.E.1.

2. Exclusion of Santiago and other urban areas, Tables 2.E.2 and 2.E.3.

Table 2.E.1: Results with Sample Excluding Party Leaders and Officials

	Dependent variable:	
	Total Victims (Alternate)	
	(1)	(2)
Close Election, 1973	3.06^{*}	2.20^{*}
	(1.29)	(1.01)
UP 1973 Vote Share		-0.02
		(0.02)
Infant Mortality, 1972		0.02
		(0.02)
Log(Total Votes, 1970)		1.19^{**}
		(0.45)
Santiago		10.47^{***}
		(2.07)
Army Base		3.82
		(2.07)
(Intercept)	1.55^{***}	-8.22^{**}
	(0.35)	(2.71)
Adj. R^2	0.02	0.33
Observations	453	453

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table 2.E.2: Results with Sample Excluding Santiago

	Dependent variable:			
	Total Victims		Non-Dissident Victims	
	(1)	(2)	(3)	(4)
Close Election, 1973	2.34^{**}	1.89^{**}	0.82^{*}	0.68^{*}
	(0.80)	(0.69)	(0.36)	(0.34)
UP 1973 Vote Share		-0.01		-0.01^{**}
		(0.00)		(0.00)
Infant Mortality, 1972		0.01		0.00
		(0.01)		(0.01)
Log(Total Votes, 1970)		0.61^{***}		0.24^{***}
		(0.14)		(0.06)
Army Base		7.32^{***}		2.44^{**}
		(1.94)		(0.93)
(Intercept)	0.57^{***}	-3.81^{***}	0.24^{***}	-1.37^{***}
	(0.10)	(0.92)	(0.05)	(0.40)
Adj. R^2	0.07	0.32	0.04	0.20
Observations	405	405	405	405

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table 2.E.3: Results with Sample Excluding Urban Municipalities

	Dependent variable:			
	Total Victims		Non-Dissident Victims	
	(1)	(2)	(3)	(4)
Close Election, 1973	1.99*	1.37†	0.61	0.37
	(0.87)	(0.83)	(0.42)	(0.45)
UP 1973 Vote Share		−0.04		−0.02
		(0.03)		(0.02)
Infant Mortality, 1972		−0.01		−0.01
		(0.02)		(0.01)
Log(Total Votes, 1970)		1.51		0.78
		(0.90)		(0.54)
Army Base		4.57		0.79
		(3.33)		(1.85)
(Intercept)	0.92*	−8.79	0.45*	−4.35
	(0.37)	(4.99)	(0.21)	(2.97)
Adj. R^2	0.01	0.12	0.00	0.07
Observations	406	406	406	406

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Appendix C

Protest in the Shadow of Repression and Extremist Violence

C.1 Proof of Proposition 1

Consider the utility of violence for dissident i: $U_i(\text{Violence}) = s(\theta-\omega)+\psi_i-d(r)$. Note that $s = Pr(s(\theta-\omega)+\psi_i-d(r) > 0)$ which is the probability that violence is preferred to doing nothing in the second round. Rewriting: $s = Pr(\psi_i > d(r) - s(\theta-\omega))$ or $s = 1 - Pr(\psi_i \leq d(r) - s(\theta-\omega))$. Because $\psi \sim N(\mu, \alpha^2)$, this can be rewritten:

$$s = 1 - \Phi(\frac{d(r) - s(\theta-\omega) - \mu}{\alpha^2}) \tag{C.1}$$

Note (7) follows the form $f(s) = s$ which allows us to determine if there is a single-crossing property for the function f at which a unique exogenous value of the signal x corresponds to a unique endogenous value of s. Note this corresponds to the solution procedure for the primary model in which a unique endogenous cutoff threshold k is identified as corresponding to an exogenous signal x (Morris and Shin 2006). The expression $h(s) = 1 - \Phi(\frac{d(r)-s(\theta-\omega)-\mu}{\alpha^2}) - s$ should be strictly increasing in θ and decreasing in s.

To be strictly increasing in x means:

$$\frac{\partial}{\partial x}\{1-\Phi(\frac{d(r)-s(\theta-\omega)-\mu}{\alpha^2})-s\}>0$$

By the chain rule and dividing both sides by -1:

$$\frac{-s}{\alpha^2}\cdot\phi(\frac{d(r)-s(\theta-\omega)-\mu}{\alpha^2})<0$$

Since the second term is always positive by definition of pdf, the first term determines the sign of the expression. Since $s>0$ by definition of pdf the condition always holds.

To be strictly decreasing in s means:

$$\frac{\partial}{\partial s}\{1-\Phi(\frac{d(r)-s(\theta-\omega)-\mu}{\alpha^2})-s\}<0$$

By the chain rule and adding 1 to both sides:

$$\frac{x-\omega}{\alpha^2}\cdot\phi(\frac{d(r)-s(\theta-\omega)-\mu}{\alpha^2})<1$$

Note the second term is less than 1 by definition of pdf. The numerator of the first term is less than 1 by definition since $x\in[0,1]$ and $\omega\in[0,x]$. Thus a sufficient condition for the expression to decrease strictly in s is $\alpha>1$.

With monotonicity shown, for the expression in (C.1) to have a unique solution it must be the case that $h(s)=1-\Phi(\frac{d(r)-s(\theta-\omega)-\mu}{\alpha^2})-s$ crosses the horizontal axis in the interval $[0,1]$. To see this consider that:

$$h(1)=1-\Phi(\frac{d(r)-s(\theta-\omega)-\mu}{\alpha^2})-1=-\Phi(\frac{d(r)-s(\theta-\omega)-\mu}{\alpha^2})<0$$

By definition of CDF. Also consider, also by definition of CDF that:

$$h(0) = 1 - \Phi(\frac{d(r) - s(\theta - \omega) - \mu}{\alpha^2}) - 0 = 1 - \Phi(\frac{d(r) - s(\theta - \omega) - \mu}{\alpha^2}) > 0$$

By the intermediate value theorem, there exists an s such that $h(s) = 0$. Furthermore, by the monotonicity in s and x proven above this solution is unique. □

C.2 Payoffs

Here I offer the full formalization of the payoff equations (3) and (4) which were presented non-formally for clarity in the body of the paper.

Formal expression for equation (3). Note this expression is modified in the first round by $(1 - p)$ for Not Protest and by $(1 - p)(1 - r)$ for Protest:

$$EU_i[\text{Second Round}] = EU_i[\text{Violence}]Pr(\text{Violence}) + EU_i[\text{Nothing}]Pr(\text{Nothing})$$

$$= E[\text{Violence}] \cdot Pr(s(\theta - \omega) + \psi - d(r) \geq 0) + 0$$

$$= (E[s(x_i - \omega) + \psi - d(r)] \cdot Pr(s(\theta - \omega) + \psi - d(r) \geq 0)$$

Note the expected value of violence contains two cases through the Law of Iterated Expectations based on s. Recall that $s^* = Pr(s(\theta - \omega) + \psi - d(r) \geq 0)$ is characterized by the result from Proposition 1.

$$EU_i[\text{Second Round}] = E[s^*(x_i - \omega) + \psi - d(r)] \cdot s^*$$

$$= [s^*(\theta - \omega) + \psi - d(r)] \cdot s^*$$

Formal expression for equation (4):

$$EU_i[\text{Protest}] = EU_i[\text{Regime Change}]Pr(\text{RC}) + EU_i[\text{No Regime Change}]Pr(\text{No RC})$$

$$= E[x_i - c(r)]Pr(\text{RC}) + E[-c(r)(1-r)EU(\text{Violence})Pr(\text{Violence})](1 - Pr(\text{RC}))$$

$$= E[x_i]Pr(\text{RC}) - E[c(r)] + (1-p)(1-r)EU(\text{Violence})Pr(\text{Violence}) \quad \text{simplifying}$$

$$= p \cdot \theta + (1-p)(1-r)[s^*(\theta - \omega) + \mu - d(r)] \cdot s^*$$

Where the last step follows from the expectation of the infrastructural power of the state. The probability expression for p is the probability the proportion of dissidents who protest is greater than the regime survival threshold ρ:

$$p = Pr(1 - \Phi(\frac{k - x_i}{\sqrt{2}\sigma}) > \rho)) \quad \text{by assumption.}$$

$$= Pr(1 - \rho > \Phi(\frac{k - x_i}{\sqrt{2}\sigma} > \rho))$$

$$= Pr(\Phi^{-1}(1-\rho) > \frac{k - x_i}{\sqrt{2}\sigma})$$

$$= Pr(\sqrt{2}\sigma\Phi^{-1}(1-\rho) - k > -x_i)$$

$$= Pr(k - \sqrt{2}\sigma\Phi^{-1}(1-\rho) < x_i)$$

$$= 1 - \Phi(\frac{k - \sqrt{2}\sigma\Phi^{-1}(1-\rho) - x_i}{\sigma}) \quad \text{by distribution of signals.}$$

Thus, $1 - p = \Phi(\frac{k - \sqrt{2}\sigma\Phi^{-1}(1-\rho) - x_i}{\sigma})$. These are expressed as p and $1 - p$ except when noted for proofs.

C.3 Proof of Proposition 2

Consider the indifference point between protesting and not protesting for each dissident based on the above payoff expressions:

$$p \cdot x_i + (1-p)(1-r)[s^*(x_i - \omega) + \mu - d(r)] \cdot s^* = (1-p)[s^*(x_i - \omega) + \psi - d(r)] \cdot s^*$$

This can be rearranged to the form of a function $h(x) = x - f(x)$:

$$x_i - \frac{r(1-p)[s^*(x_i - \omega) + \mu - d(r)] \cdot s^* + c(r)}{p} = 0 \tag{C.2}$$

Now define $b(k)$ to be the unique value of x which solves the equation above. That is, for any given switching strategy with cutoff k, what is the value of x (private signal for each dissident) for which protest will be a best response? This value $x = b(k)$ becomes the new switching strategy in place of k, then solve for $b(b(k))$ and continue iterating by the same procedure until the best response converges. If there is a unique solution, the function will converge to the unique solution beginning at $k = 1$. In particular, solve for $x = b(1)$.

Lemma 1: *Expression (5) is strictly increasing in x and strictly decreasing in k when:*

$$\frac{-c(r)}{r} < s^*(x_i - \omega) + \mu - d(r) < 0 \tag{C.3}$$

Observe that a sufficient condition for uniqueness is that the expression (5) strictly increases in x and decreases in k (Morris and Shin 2006). Using the quotient and product rule to find the change in the expression with x and k yields the derivative, of which the expression should strictly increase in x. The expression increases in x if:

$$1 - \frac{\partial}{\partial x}(\frac{r(1-p)[s^*(x_i - \omega) + \mu - d(r)] \cdot s^* + c(r)}{p}) > 0$$

Applying the quotient rule, and focusing on the sign of the numerator given that $p^2 > 0$ *and rearranging, the expression increases in x if:*

$$\begin{aligned} s^*[s^*(x_i - \omega) + \mu - d(r)]\{p \cdot [\frac{\partial}{\partial x} r(1-p)] - r(1-p)(\frac{\partial}{\partial x} p) \\ < -rp(1-p)(\frac{\partial}{\partial x} s^* x_i) + c(r)(s^* \frac{\partial}{\partial x} p + 1) \end{aligned}$$

Rearranging:

$$\frac{\partial}{\partial x} p[-r(s^*(x_i - \omega) + \mu - d(r)) - c(r)] < \frac{1}{s^*}(-rp(1-p) \cdot \frac{\partial}{\partial x}(s^*)^2 x) + 1)$$

Under the assumption that $s^*(x_i - \omega) + \mu - d(r) < 0$*, which means the utility of violence for the mean dissident is negative, or less than the utility of doing nothing, and the assumption that* $s^*(x_i - \omega) + \mu - d(r)) > \frac{-c(r)}{r}$ *this becomes:*

$$\frac{\partial}{\partial x} p > \frac{-rp(1-p) \cdot ((s^*)^2 + 2s^* \frac{\partial}{\partial x}(s^*) + 1)}{-rs^*(s^*(x_i - \omega) + \mu - d(r)) - s^* c(r)} \tag{C.4}$$

The denominator is strictly negative under the assumptions. The numerator is strictly positive given that $rp(1-p) \cdot ((s^*)^2 + 2s^* \frac{\partial}{\partial x}(s^*)) < 1$ *always under restrictions on parameter values and the value of the normal pdf which composes* $\frac{\partial}{\partial x}(s^*)$*. Since* $\frac{\partial}{\partial x} p > 0$*, the condition in (12) is always met.*

Note the function decreases in k if:

$$\frac{\partial}{\partial k}(\frac{r(1-p)[s^*(x_i - \omega) + \mu - d(r)] \cdot s^* + c(r)}{p}) > 0$$

Using the quotient rule and focusing on the sign of the numerator since $p^2 > 0$:

$$p \cdot \frac{\partial}{\partial k}\{r(1-p)[s^*(x_i-\omega)+\mu-d(r)]s^*+c(r)\}-$$
$$\{r(1-p)[s^*(x_i-\omega)+\mu-d(r)]s^*+c(r)\}\cdot\frac{\partial}{\partial k}p>0$$

Rearranging and simplifying:

$$-rs^*[s^*(x_i-\omega)+\mu-d(r)]-c(r)<0$$

This is true by the previous assumption: $s^*(x_i-\omega)+\mu-d(r))>\frac{-c(r)}{r}$.

With monotonicity shown, for the expression in (C.2) to have a unique solution it must be the case that $h(x)=x_i-\frac{r(1-p)[s^*(x_i-\omega)+\mu-d(r)]\cdot s^*+c(r)}{p}$ crosses the horizontal axis in the interval $[0,1]$. To see this consider that:

$$h(0)=0-\frac{r(1-p)[s^*(-\omega)+\mu-d(r)]\cdot s^*+c(r)}{p}<0$$

Because $\frac{-c(r)}{r}<(1-p)s^*[s^*(0-\omega)+\mu-d(r)]$ under the assumptions for monotonicity.

$$h(1)=1-\frac{r(1-p)[s^*(x_i-\omega)+\mu-d(r)]\cdot s^*+c(r)}{p}>0$$

This requires that $\frac{p-c(r)}{r(1-p)s^*}>s^*(1-\omega)+\mu-d(r)$ which holds under the assumptions for monotonicity if $p(1)>c(r)$, meaning that under the maximum infrastructural power, the mean dissident would choose to protest.

By the Intermediate Value Theorem there exists a solution for h(x) = 0 in the interval. Combined with the proof of monotonicity above, the best response for any k is therefore unique (Morris and Shin 2006) and may be found through the process of fixed point iteration. The fixed point iteration converges to the unique cutoff which

is the switching strategy for all dissidents. There is a unique equilibrium in switching strategies in which θ^* is the unique cutoff. □

C.4 Proof of Proposition 3

Note the equilibrium cutoff dissidents choose is denoted θ^*. Therefore the proportion of dissidents who protest is:

$$Pr(\theta > \theta^*) = 1 - Pr(\theta \leq \theta^*)$$

Given the regime strength distribution:

$$1 - Pr(\theta \leq \theta^*) = 1 - \Phi(\frac{\theta^* - \theta}{\sigma})$$

So the proportion who choose not to protest (violence or nothing) is $\Phi(\frac{\theta^* - \theta}{\sigma})$. This proportion is divided in the last period depending on the probability that violence exceeds the value of doing nothing within not protesting, which is s^*.

Among those who do not protest, the proportion of all dissidents choosing violence is:

$$\Phi(\frac{\theta^* - \theta}{\sigma})s^*$$

Among those who protest, the proportion of all dissidents who support violence is:

$$(1 - \Phi(\frac{\theta^* - \theta}{\sigma}))(1 - r)s^*$$

Combining the expressions, the total proportion who support violence is:

$$(1 - r)s^* + rs^*\Phi(\frac{\theta^* - \theta}{\sigma})$$

Among those who do not protest, the proportion of all dissidents who do nothing

is:

$$\Phi(\frac{\theta^* - \theta}{\sigma})(1 - s^*)$$

Among those who protest, the proportion of all dissidents who do nothing is:

$$(1 - \Phi(\frac{\theta^* - \theta}{\sigma}))(1 - r)(1 - s^*)$$

Combining the expressions, the total proportion who do nothing is:

$$(1 - r)(1 - s^*) + r(1 - s^*)\Phi(\frac{\theta^* - \theta}{\sigma})$$

Note these three proportions can be computed based on the model parameters. □

C.5 Proof of Proposition 4

Consider the expression in equation (C.2). Note we can name an implicit function f such that $f(r, k) = c$ where $c = 0$. If $\frac{\partial}{\partial r} f(r, k)$ and $\frac{\partial}{\partial k} f(r, k)$ are continuous and $\frac{\partial}{\partial k} f(r, k) \neq 0$, then by the Implicit Function Theorem there is an explicit function $g(r) = k$ such that:

$$g'(r) = \frac{-\frac{\partial}{\partial r} f(r, k)}{\frac{\partial}{\partial k} f(r, k)}$$

When $g'(r) < 0$, repression decreases the threshold in switching strategies k, which increases protest. When $g'(r) > 0$, repression increases the threshold which decreases protest. Therefore to determine the sign of $g'(r)$, determine the signs of the derivatives in the numerator and the denominator. Note that:

$$-\frac{\partial}{\partial r} f(r, k) = -\frac{\partial}{\partial r}(\theta - \frac{r(1 - p)[s^*(\theta - \omega) + \mu - d(r)] \cdot s^* + c(r)}{p})$$

$$= \frac{1}{p}[r(1-p)(\theta-\omega)2s^*\frac{\partial}{\partial}rs^* + (1-p)(s^*)^2(\theta-\omega) + r(1-p)\mu\frac{\partial}{\partial r}s^* +$$
$$(1-p)\mu s^* - d'(r)r(1-p)s^* - d(r)(1-p)s^* - d(r)r(1-p)\frac{\partial}{\partial r}s^* + c'(r)]$$

So the numerator of the fraction which characterizes the derivative of explicit function is positive if the above quantity is positive. Rearranging, the condition becomes:

$$\frac{-c'(r)}{1-p} < r\frac{\partial}{\partial r}s^*[2s^*(\theta-\omega) + \mu - d(r)s^*] + s^*[s^*(\theta-\omega) + \mu - d(r)] - d'(r)rs^*$$

Isolating $\frac{\partial}{\partial r}s^*$, there are two cases based on the divisor: $2s^*(\theta-\omega) + \mu - d(r)s^*$. If the quantity is negative, then the condition becomes:

$$\frac{\partial}{\partial r}s^* \leq \frac{s^*[d'(r)r - (s^*(\theta-\omega) + \mu - d(r))] - c'(r)\cdot\frac{1}{1-p}}{r[2s^*(\theta-\omega) + \mu - d(r)s^*]}$$

Given the denominator is negative, and the numerator is negative if:

$$s^* < \frac{c'(r)}{d'(r)r + \frac{c(r)}{r}(1-p)}$$

then the case is positive. Because $\frac{\partial}{\partial r}s^* < 0$ (repression has a negative effect on the proportion of dissidents choosing violence all else equal), the case is satisfied always.

Given the denominator is positive, and the numerator is negative under the same condition as before, then the case is negative. This is only satisfied if:

$$\frac{\partial}{\partial r}s^* \geq \frac{s^*[d'(r)r - (s^*(\theta-\omega) + \mu - d(r))] - c'(r)\cdot\frac{1}{1-p}}{r[2s^*(\theta-\omega) + \mu - d(r)s^*]} \tag{C.5}$$

Now compute the denominator of the derivative of the explicit function:

Lemma 2: $\frac{\partial}{\partial k}f(r,k) < 0$.

$$\frac{\partial}{\partial k} f(r,k) = \frac{\partial}{\partial k}(\theta - \frac{r(1-p)[s^*(\theta-\omega)+\mu-d(r)]\cdot s^* + c(r)}{p})$$

Applying the quotient rule, and given that $\frac{\partial}{\partial k} p < 0$*, meaning an increasing threshold decreases the probability of regime change, the result matches that of Lemma 1, in which the fraction in the second term increases in* k*, meaning when multiplied by* -1 *the entire expression decreases in* k*. This also makes intuitive sense since an increasing threshold should decrease the benefit of protest through decreasing the probability of regime change.*

Therefore the sign of $g'(r)$ is determined inversely by the numerator. We can find the conditions under which the above expression is satisfied or not to determine when k decreases and increases in r, respectively. When k decreases, protest increases, and vice versa. □

C.6 Proof of Proposition 5

Consider the expression in equation (C.2). Note we can name an implicit function f such that $f(\mu,k) = c$ where $c = 0$. If $\frac{\partial}{\partial \mu} f(\mu,k)$ and $\frac{\partial}{\partial k} f(\mu,k)$ are continuous and $\frac{\partial}{\partial k} f(\mu,k) \neq 0$, then by the Implicit Function Theorem there is an explicit function $g(\mu) = k$ such that:

$$g'(\mu) = \frac{-\frac{\partial}{\partial \mu} f(\mu,k)}{\frac{\partial}{\partial k} f(\mu,k)}$$

When $g'(\mu) < 0$, violent capabilities decrease the threshold in switching strategies k, which increases protest. When $g'(\mu) > 0$, violent capabilities increases the threshold which decreases protest. Therefore to determine the sign of $g'(\mu)$, determine the signs of the derivatives in the numerator and the denominator. Note from Lemma

2 the sign of the denominator is negative. So the sign of the numerator inversely determines the sign of $g'(\mu)$. By similar arguments to Proposition 3, the following is $-\frac{\partial}{\partial\mu}f(\mu,k)$:

$$\frac{r(1-p)}{p}(2s^*\frac{\partial}{\partial\mu}s^*\cdot(\theta-\omega)+s^*+\mu\cdot\frac{\partial}{\partial\mu}s^*-d(r)\frac{\partial}{\partial\mu}s^*)$$

The numerator is positive if, and following some rearranging:

$$0\leq\frac{\partial}{\partial\mu}s^*(2s^*(\theta-\omega)+\mu-d(r))+s^*$$

This creates two cases. First, if $2s^*(\theta-\omega)+\mu-d(r)>0$, then for the numerator of $g'(\mu)$ to be positive it is necessary that:

$$\frac{\partial}{\partial\mu}s^*\geq\frac{-s^*}{2s^*(\theta-\omega)+\mu-d(r))}$$

Which is trivially satisfied because the left-hand side is positive given that violent capabilities increase the proportion of dissidents choosing violence and the right-hand side is negative. Second, if $2s^*(\theta-\omega)+\mu-d(r)<0$, then for the numerator of $g'(\mu)$ to be positive it is necessary that:

$$\frac{\partial}{\partial\mu}s^*\leq\frac{-s^*}{2s^*(\theta-\omega)+\mu-d(r))}$$

So we have determined conditions for the sign of the numerator of $g'(\mu)$, which inversely determines the sign of the derivative. We then find the conditions under which the above expression is met to determine when k decreases and increases in r, respectively. □

C.7 Note on Comparative Static Simulations

To simulate comparative statics from the model, I computed using fixed point iteration the unique threshold in switching strategies (θ^*) and then simulated the proportion of dissidents choosing each action based on the proportions in Proposition 2 across a range of exogenous parameter values. I used the function form $c(r) = r^4$ for the concave repression cost function for protest, and $d(r) = r^2$ for the concave repression cost function for violence.

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