# Repression and Reelection in Democracies

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Submitted to the Graduate Faculty of

the Dietrich School of Arts and Sciences in partial fulfillment

of the requirements for the degree of

**Doctor of Philosophy**

University of Pittsburgh

2021

UNIVERSITY OF PITTSBURGH

DIETRICH SCHOOL OF ARTS AND SCIENCES

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University of Pittsburgh, 2021

What is the relationship between elections and repression? How do voters evaluate repression, and what causes them to change their beliefs about rights violations? In this dissertation, I examine patterns of repression across democracies. I theorize that individuals' reactions to human rights violations vary based on their perceptions about the group that has been targeted by state violence. When the public perceives a target to threaten domestic security and stability, they are more likely to support leaders who perpetrate repression in order to provide (real or perceived) security. Yet individuals are also likely to feel threatened by repression that targets their in-group members, which can inhibit their own safety and well-being. In such cases, voters may punish their leaders for repression at the ballot box.

In a series of three papers, I explore the implications of this argument at several different levels of analysis. In the first paper, I examine whether leaders shift their repressive behavior over election cycles, presumably to gain electoral benefits. In cross-national analysis of democratic elections from 1995-2019, I find that leaders increase repression of threatening groups as elections approach, but only during periods of domestic violence. In the second paper, I evaluate decision-making at the individual level using a conjoint experiment in the United States. I find that only individuals who identify as an in-group with recent repression victims incorporate repression into their vote choice by punishing repressive candidates. Finally, in the third paper I assess whether the international community can intervene to shift individual attitudes about human rights and repressive leaders. Using a vignette experiment in the United States, I find that only those who view repression targets as non-threatening and do not share a party with the repressive leader increase their opposition to repressive leaders as a result of international naming and shaming. Taken together, these three papers provide novel theoretical frameworks and empirical evidence to evaluate the effectiveness of elections for constraining repression in democratic countries.

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## Preface

I am thankful for many professors, mentors, friends, and family members who provided unfailing support during graduate school. At the University of Pittsburgh, a sincere thank you to each of my committee members – Burcu Savun, Michael Colaresi, Steven Finkel, and Courtenay Conrad – for sharing big ideas and careful feedback through many iterations of this project. Your insight will strengthen my research for years to come. Thank you also to the DISS-IR-TATE group, including Michaël Aklin, Kevin Greene, Jude Hays, and Will Spaniel, for brainstorming with me during early phases of the writing process. Thank you to Danny Choi, Laura Paler, and Emily West, who shared helpful comments on early drafts, and Jabari Cook and Salina Pressimone, who provided fantastic research assistance. Most importantly, I am immensely grateful for direction and support from the best mentor, advisor, and chair. To Burcu Savun, thank you for your unwavering commitment to this dissertation and my development as a scholar. I am a better researcher and person for your guidance.

I am lucky to have made many wonderful friends through Pitt's graduate program. To my cohort members, José Incio, Minsu Jang, Rian Litchard, Max Peterson, and Jacob Schiller, as well as Brianna Howell, Josh Kaasik, Merve Keskin, Ignacio Mamone, Jacob Otto, Anne Russell, Çiğdem Ünal, and Louis Wilson: thank you for bringing joy to many challenging years. I am especially grateful for two dear friends who provided encouragement, laughter, and selfless friendship that sustained me through my graduate training. To Sara Fox and Junghyun Lim, my favorite IR women, I am so lucky to have shared this journey with you.

Thank you to my California "tribe," my extended family, and Allan Gendelman for cheering me on. Thank you also to many long-distance friends, especially Emma Liem Beckett, Tyler Beckett, Emily Collins, Joanne David, Hillary Finnegan, Anna McVae, Pamela Meza, Bailey Millsap, Liza Myers, Larissa Purnell, Sarah Scotti, Courtney Steinhauser, and Moriah Vincent, for loving me generously during these years of writing. In Pittsburgh, thank you to Amy Foster and Lorna Williams, for sharing your friendship and home with me. I also want to thank the Steinhauser family, for welcoming me to your family and to Pittsburgh. I am

especially grateful for my first advisor and mentor, Steve Swindle: thank you for believing in me from the very beginning and supporting me throughout this journey.

Finally, thank you to Leo Gentil-Fernandes: this dissertation is better for your creativity, critical thinking, and encouraging eye on every draft. I am so lucky to have had you and Alex to cheer me on every step of the way. Lastly, thank you to my parents and sisters: Tom, Linda, Mandy, and Amy Morrison, for encouraging me and praying over me through every high and low in life. This accomplishment is yours to share, and I dedicate this dissertation to you.

## 1.0 Introduction

Why do democratic leaders repress? How do repressive actions influence leaders' electoral prospects? The literature on the domestic democratic peace reveals a strong correlation between regime type and repression. Democratic institutions, like free and fair elections, independent judiciaries, and strong legislatures, are associated with relatively low levels of rights violations. In general, each these institutions introduce checks on leaders' behavior, which raise the cost of committing human rights abuses. However, while institutional constraints undoubtedly curb overall repression, the cost they impose on leaders varies across political contexts depending on various actors' willingness to hold leaders accountable. To better understand the strengths and weaknesses of democratic institutions for preventing repression, it is important to study the mechanisms through which each institution facilitates human rights protections, and the moments when they may fall short. To further this research agenda, this dissertation explores the limitations of one foundational democratic institution, elections, for keeping leaders from repression.

Across three papers, I focus on a core factor that moderates the effectiveness of elections for constraining repression: the threat that voters perceive from repression targets. Drawing from literature on the security-civil liberties trade-off, which explores how individuals make difficult choices between rights protections and security, I argue that individuals in democracies react differently to repression perpetrated against non-threatening groups and groups that put domestic security at risk. Specifically, I anticipate that voters will support leaders who repress threatening groups in an effort to provide domestic security, particularly in contexts of political crisis. This argument has implications for leaders' behavior over the course of election cycles, individuals' decision-making at the ballot box, and international organizations' ability to persuade democratic citizens to shift their support for human rights and rights-abusing leaders. I explore each of these issues in the three papers of the dissertation.

To begin, the first paper analyzes how leaders' incentives to target particular groups with repression vary over the course of an election cycle. Building from the expectation that voters reward leaders who repress threatening groups, I expect that leaders will engage in

higher levels of targeted repression against domestic threats as elections approach. Further, I theorize that these tendencies will be especially strong in countries in the midst of domestic violence, when security is likely to be a salient election issue. Using cross-national data on democratic elections from 1995-2019 and a novel measure of repression, I find support for this theory. However, I find no evidence that leaders' probability of winning reelection shifts as a result of their repressive behavior. In the second paper, I turn to the individual level of analysis, assessing whether and when voters consider repression in their vote choice. Using a representative conjoint experiment in the United States, I find that individuals who identify as in-groups with recent victims of repression punish candidates who support repression of their group. However, all other subsets of voters remain indifferent to repression at the ballot box, and no group of voters rewards repressive candidates. In the third paper, I evaluate whether international advocacy organizations can shift individuals' support for repression. The results of a representative vignette experiment in the United States indicate that threat perception influences individuals' susceptibility to naming and shaming. Specifically, I find that international advocacy increases opposition to repression leaders only among voters who perceive repression victims to be non-threatening and do not share a party with the repressive leader.

Taken together, the three papers of the dissertation provide unique insight into the interaction between elections and patterns of repression across democracies. I find that repression does shift during electoral cycles, but only in countries embroiled in domestic conflict. Further, I uncover greater indifference to repression on the part of voters than may have been expected by previous research. While some voters who have personal experience with repression are likely to consider repression in their vote choice, many others privilege issues like economic policy, party identification, and candidates' demographic features when choosing between candidates. Finally, the dissertation helps ascertain whether and how members of the international community can shift these individual-level tendencies. The results show that some individuals are amenable to even short informational treatments about human rights violations. Others, however, remain intransigent in their beliefs about human rights protections in the domestic sphere. These findings in particular have strong relevance to policymakers and human rights advocates who hope to increase domestic support

for human rights. Overall, the theories and findings presented in this dissertation have broad implications for the relationship between democratic institutions and human rights, as well as the individual-level choices that shape patterns of repression across democracies.

## 2.0 Paper 1: Election Timing and Targeted Repression

On October 14, 2019, Spain's Supreme Court found nine Catalan independence leaders guilty of sedition and misuse of public funds for their role in organizing the 2017 Catalan independence referendum (BBC News, 2019). After the leaders were sentenced to nine to 13 years in prison, it did not take long for protesters to fill the streets. By week's end, 350,000 people had gathered to protest the ruling throughout Catalonia, resulting in the closing of the Barcelona-El Prat airport, blockages of major roadways, and the postponement of the clásico match between Real Madrid and FC Barcelona (The Sydney Morning Herald, 2019; The Local, 2019). In reaction to the uprising, Prime Minister Pedro Sánchez assured the country that he would uphold the rulings, stating that "the government would work to guarantee public order" (Rolfe and McAuley, 2019). One means of securing public order was violent repression, as the police used foam bullets, batons, and other crowd control tactics to subdue the protesters, resulting in injuries to 593 people (BBC News, 2019). Even following the use of these repressive tactics, the Prime Minister maintained public support. Public opinion for the ruling party stayed the same throughout the fall (Politico, 2021), and Sánchez won an easy reelection in November with a strong plurality of 28% of the vote (Gutiérrez, 2019).

Spain's violent repression of Catalan protesters raises important questions about the relationship between elections, dissent, and repression.[1] How do elections shape leaders' incentives to respond to domestic threats with repression? When leaders do turn to repression, how do these tactics shape their performance in elections? On the one hand, research on the domestic democratic peace finds that democratic institutions – like elections – lead to relatively low levels of repression (Davenport, 2007*b*).[2] Yet this research provides limited insight

[1] *Repression* or *rights violations* are "coercive actions political authorities take to inhibit the will or capacity of people within their jurisdiction to influence political outcomes" (Ritter, 2014, 145). I focus on specific acts of repression that can be attributed to elected leaders, while recognizing that leaders often repress covertly (Colaresi, 2014; Krcmaric, 2019), that delegation problems can obscure leaders' responsibility for repression (DeMeritt, 2015; Tiberiu Dragu, 2018; Eck, 2015), and that repression is often embedded in political systems.

[2] I use a minimalist definition of *democracy*: "a regime in which those who govern are selected through contested elections" (Przeworski et al., 2000, 15). Figure A.1 in the Appendix shows patterns of repression

into how incentives to repress vary over the course of an election cycle and how repression changes leaders' likelihood of winning reelection. Further, numerous bodies of research – on the civil liberties - security trade-off, counterterrorism, and group-based conflict, for instance – suggest that individuals and groups often *support* the use of violence against threatening groups, particularly during periods of conflict (Conrad et al., 2018; Conrad, Hill Jr and Moore, 2018). Anecdotally, cases like Spanish repression in Catalonia demonstrate that repression might not always carry electoral costs. In light of these patterns, existing literature needs a cohesive account about how leaders' incentives to repress particular groups wax and wane over the course of an election cycle and how repression influences leaders' likelihood of staying in power.

This paper evaluates – theoretically and empirically – whether and how impending elections shift leaders' incentives to repress. I argue that leaders have increasing incentive to repress threatening groups as elections approach based on two mechanisms. First, because individuals and groups have a preference that leaders respond to threatening outgroups with repression, leaders face pressure from the electorate to repress threatening targets as elections approach. These preferences are further entrenched in countries facing violent domestic conflict or sustained threat. In such cases, security is especially likely to be a pivotal issue in elections, resulting in increased incentive for leaders to highlight their ability to provide security as elections approach. In a second causal mechanism, the leader can also increase the loyalty of their winning coalition by using repression. Especially in conflict-prone societies, targeted repression against threatening outgroups can prime in-group identification with the leader, increasing the likelihood that in-group members will support the leader by turning out to vote during elections. Two observable implications of these dynamics are that leaders should be increasingly likely to use targeted repression against threatening groups as elections approach and that voters should recognize these efforts, meaning that leaders who repress threatening groups should be more likely to win reelection than those who do not. These relationships should be especially strong during periods of elevated societal threat, when security is highly likely to be a critical election issue.

by regime type, revealing that repression is more frequent in autocracies but not uncommon in democracies. See also Haschke (2018).

To analyze the implications of this theory, I create a new measure of repressive events using data from the International Crisis Early Warning System (ICEWS). This data is uniquely suited to my interest in the relationship between election timing, targeted repression, and leaders' probability of winning reelection. Unlike standards-based measures of repression, which provide countries with a score of respect for various political and physical integrity rights in each year, events data captures the precise timing and target of specific repressive actions taken by governments. The analysis reveals that, during conflict, leaders' probability of repressing threatening groups increases as elections approach, particularly societies under threat. However, there is only limited evidence that pre-election repression of threatening groups increases leaders' probability of winning reelection, regardless of conflict status.

The first major contribution of this study is to generate a theoretical and empirical account of patterns of repression over the course of an election cycle in a large cross-national sample for an extended time series. Such an effort is crucial for understanding not only how democratic institutions influence patterns of repression at the aggregate level, but also when countries (and particular groups) are most vulnerable to repression. Another contribution is to introduce a new measure of repression using comprehensive events data, as captured in ICEWS. Standards-based measures help to identify general trends in patterns of repression across countries and over time. However, events data is necessary to isolate the precise timing and targeting of repressive actions. My measure thus contributes to growing efforts to disaggregate the study of repression (Bhasin and Gandhi, 2013; Conrad, Haglund and Moore, 2014; Cordell et al., 2021; Salehyan et al., 2012; Sullivan, 2015). Finally, this study identifies how threats – from particular groups and in particular contexts – change the relationship between elections and repression. While past work theorizes that elections provide less effective constraints against repression during conflict, it has not analyzed the moments at which this relationship breaks down and the groups that are most affected by the weaknesses of elections. In contrast, this study identifies the precise limitations of elections, generating critical insight that can help protect vulnerable groups from repression.

The paper proceeds as follows. The first section reviews the existing literature linking elections to repression. The second section reviews several bodies of literature which suggest that individual- and group-level preferences provide leaders with motive to repress as elec-

tions approach. The third section outlines a theoretical framework to explain how impending elections shape patterns of repression and how repression changes a leader's probability of winning reelection. The fourth section presents the research design and the fifth section tests the hypotheses. The last section concludes.

### 2.1 Elections and Repression

Democratic institutions, including elections, are associated with relatively low levels of repression (Davenport, 2007*a*; DeMeritt, 2016). Domestic democratic peace theory proposes that democratic institutions provide institutional constraints that raise the cost of repression vis-a-vis alternative governance strategies.[3] Regularly held free and fair elections mean that voters have the option to cast out leaders with disagreeable attributes. Assuming that voters prefer their leaders refrain from repression (Beer and Mitchell, 2004; Cingranelli and Filippov, 2010; Gurr, 1986; Richards, 1999; Richards and Gelleny, 2007), democratic leaders who repress should expect electoral sanctions. The implication of these arguments is that leaders in countries with regularly-held elections should repress less than those in countries without elections.

Existing literature adopts two empirical strategies to evaluate the relationship between elections and repression. First, some researchers analyze the correlation between the presence of the *institution* of elections and levels of repression. These studies typically find a negative association between indicators of electoral democracy and rights violations. For instance, Davenport (2007*b*) finds that countries with higher levels of "voice," operationalized as levels of electoral competition and participation, have lower levels of repression. In Mexico, Beer and Mitchell (2004) find a negative relationship between competitive elections and repression at the sub-national level. Electoral institutions that increase voters' ability to hold leaders accountable are also associated with lower levels of repression (Cingranelli and Filippov,

[3]Repression always carries costs, due to the need to employ and monitor coercive agents, for instance (Davenport, 2007*b*). I focus only on the institutional and electoral costs that may vary, considering these other costs to remain constant across cases. Democracies have many other options to deal with threats, including diversion, concessions, and illiberal tactics like censorship and surveillance (Cope, Crabtree and Lupu, 2018; Davies, 2016).

2010). Yet there is also ample evidence that this relationship is subject to limitations. Specifically, during periods of societal threat, "repression can be supported by at least part of the citizenry" (Davenport, 2007*b*, 27). As a result, the dampening effect of elections on repression diminishes in the presence of various kinds of conflict, including dissent, civil war, and interstate war (Davenport, 2007*b*). Democratic leaders are also less likely to stop torture spells during periods of violent threat (Conrad and Moore, 2010), and elections are likely to be less effective at constraining torture of threatening groups (Conrad et al., 2018; Conrad, Hill Jr and Moore, 2018).

Another empirical strategy is to assess whether repression increases or decreases in the periods surrounding the *events* of elections. This approach assesses the possibility that leaders' incentives to repress shift over the course of an election cycle. If it is the case that elections increase the costs of repression, then perhaps leaders experience these costs most acutely in the immediate proximity of an election. The evidence for this relationship, however, is mixed. For instance, Davenport (1997, 1998) finds in a sample of 49 countries (1948-1982) that national elections are associated with reduced repression during election years. However, these results are driven primarily by authoritarian countries (Davenport, 1997). Examining the country-month level of analysis among 12 consolidated democracies, Davenport (1997) further finds that national elections are not associated with any change in levels of repression in the 12 months preceding or following the election. Again focusing on national level trends, but disaggregating by election type, Richards and Gelleny (2007) find that legislative elections are associated with reduced levels of repression in the two years following an election, but that repression actually *increases* in the year following an executive election. Finally, in a sample of 74 randomly-chose countries over a three-year period, Richards (1999) finds no relationship between national election year and levels of repression.[4] In the most recent evaluation of this question, Bhasin and Gandhi (2013) find that leaders are increasingly likely to target opposition groups as elections approach, but their study is limited to autocratic countries.

Overall, the literature on the domestic democratic peace indicates that elections gener-

[4]These two branches of literature share similarities with research on election violence, which finds that elections generally reduce violence while causing spikes of violence in the period immediately surrounding an election by both the opposition and the incumbent (Harish and Little, 2017).

ally reduce repression, but this relationship is tempered by political context. In times of conflict, elections are less effective at constraining violence. Beyond these aggregate-level relationships, however, there remain many unanswered questions about how patterns of repression wax and wane over the course of an election cycle. For one, existing research on these dynamics tends to be limited in spatial and temporal scope. As a result, researchers lack generalizable insight about how patterns of repression vary as elections approach. Second, existing studies tend to use aggregate, standards-based measures of repression as their dependent variable. However, accurate identification of the sequencing of repressive events and electoral contests requires data that captures the precise timing and intensity of repressive actions. Further, studies on the timing of elections should analyze heterogeneous effects of the relationship between election timing and repression based on the threat of the target group. Given that elections prove to be less effective at deterring repression during conflict, it is worth evaluating whether this relationship is driven by spikes in repression at particular moments during the election cycle. Finally, one lingering question across all of these studies is how repression affects the outcomes of elections. To assess the degree to which elections constrain repression, if is important to consider the effect of repression on electoral success.

Ultimately, research on the effect of elections on repression would benefit from a more detailed account of how electoral contests influence leaders' incentives to repress over time. To understand this relationship, it is important to identify individual- and group-level preferences about repression. The next section reviews existing literature on these preferences.

## 2.2 Incentives to Repress

It is crucial to understand individual- and group-level preferences about repression in order to theorize leaders' incentives to utilize repression as elections approach. Several branches of literature provide insight into these preferences. First, research on the domestic democratic peace tends to assume that voters universally oppose repression (Beer and Mitchell, 2004; Cingranelli and Filippov, 2010; Gurr, 1986; Richards, 1999; Richards and Gelleny, 2007). However, more recent research at the individual level adds nuance to these assumptions.

In particular, the security-civil liberties trade-off demonstrates that individuals evaluate repression and civil liberties as competing goods (Davis and Silver, 2004; Davis, 2007). The trade-off suggests that, while individuals may value civil liberties in the abstract, they are often willing to sacrifice rights protections in order to guarantee security. In particular, individuals are willing to sacrifice an *outgroup's* liberties for the security of ingroup members or society as a whole (Waldron, 2003). Given that repression can provide security by inhibiting the ability of threatening groups to mobilize, these theories indicate that many individuals will support repression that generates security benefits. Recent survey research provides some empirical support for these ideas in showing that individuals are more willing to support torture of ethnic minority groups (Conrad et al., 2018; Kearns and Young, 2020; Piazza, 2015) and more willing to support rights restrictions during periods of high societal threat (Dietrich and Crabtree, 2019). The implication of these individual-level studies is that leaders may expect support from voters when they repress threatening groups, particularly during periods of high societal threat.[5]

One case that highlights these dynamics is the U.S. internment of Japanese Americans during World War II. In 1942, U.S. President Franklin D. Roosevelt authorized regional military commanders to designate military zones on the West Coast and remove any individuals deemed threatening from these areas. Soon after, commanders removed nearly 120,000 Japanese Americans from Western states to internment camps. Enacted only two months after the Japanese attacks on Pearl Harbor and two days preceding the bombing of Santa Barbara by Japanese submarines, the measures reflected public pressure to take action to protect them from a suspected fifth column of Japanese Americans who (they feared) could facilitate a Japanese invasion.[6] Indeed, a month after the order was signed, 59% of Americans supported the interment of American citizens of Japanese descent and 93% of Americans supported interning American residents (non-citizens) of Japanese descent (United States Holocaust Memorial Museum, N.d.). In this case, voters preferred their

[5]The literature on political tolerance also demonstrates that individuals are often less willing to support civil liberties protections for groups they dislike or perceive to be threatening as a result of their ideological beliefs, ethnic identity, or past behavior (Gibson and Gouws, 2000, 2003; Hajnal, Lajevardi and Nielson, 2019; Stouffer, 1955).

[6]Originating in the Spanish Civil War, a *fifth column* is a group within a country at war which is sympathetic to or working for its enemies. Note that there was no evidence, at the time or in the years since, that Japanese Americans were participating in activities to support Japanese war efforts (Reeves, 2015).

leaders repress given the context of security crisis (World War II) and the perception that the repression target (Japanese Americans) was a domestic threat.

Research on counterterrorism suggests similar mechanisms linking individual preferences for security to leaders' behavior around elections. Several such works build theories based on the assumption that voters support counterterrorism strategies because, even though such tactics may violate civil liberties, they increase security for society as a whole. For instance, Bueno de Mesquita (2007) use a formal model to demonstrate that electoral pressures push governments to increase observable counterterrorism measures as elections approach, for which they can expect to receive credit from voters. Similarly, in empirical analysis of Western European governments' counterterror strategies, Aksoy (2018) finds that right-wing governments increase their counterterrorism activities as elections approach. The theoretical underpinning for this finding is that counterterrorism is a public good that boosts electoral performance among demographics with hawkish values. Much like repression, counterterrorism measures often result in violations of the civil liberties of particular targets. Yet voters are typically willing to support violations of the rights of terrorists (or suspected terrorists) in order to avoid the possibility of an attack.

These findings generate interesting puzzles about other means that leaders may use to provide security to voters, including repression. Many counterterror strategies could be considered to be types of repression. In Aksoy's 2018 study, for example, counterterrorism is the number of government actions taken to combat terrorism in a particular month. Such actions include making arrests or taking armed action against designated terrorist organizations like the ETA in Spain and the IRA in Ireland. However, there are many other actions that leaders can take to combat threats in the pre-election period, including restrictions on freedoms of assembly, speech or physical integrity rights violations like torture or even targeted killing. Further, existing research on counterterrorism is limited to independent actions governments take to counter terror threats. But leaders also have incentive to respond to ongoing threats with repression. Finally, voters likely desire security from threatening groups that are not terrorist organizations. While terrorists certainly represent one salient threat, voters are also likely to be threatened by rebel groups, violent protesters, or even marginalized groups merely perceived to be threatening. Savun and Gineste (2019) provide evidence for the lat-

ter relationship, by showing that leaders face increasing incentives to use violence against refugees – a group often believed to be threatening – in order to demonstrate their ability to provide security in the wake of terrorist attacks. Similarly, Whitaker (2005) shows that leaders in Africa increasingly call into question the citizenship status of opponents during election cycles, fanning the flames of xenophobia and exclusionary nation-building strategies. In this study, I build on these previous works to evaluate leaders' incentives to target a wider range of repression targets and asses how leaders' incentives to provide security vary over the course of an election cycle.

Finally, group-level dynamics also influence leaders' propensity to repress as elections approach. Research on civil conflict demonstrates how a range of identity cleavages, sustained horizontal inequality, and ethnic discrimination can fuel violence between groups (Buhaug, Cederman and Gleditsch, 2014; Cederman, Gleditsch and Buhaug, 2013; Horowitz, 1985; Kalin and Sambanis, 2018). Far from preferring that leaders always protect individual rights, voters in divided societies often prefer that leaders enact repression and violence against rebel groups that pose a threat to the status quo. The aforementioned case of Spanish violence in Catalonia underscores some of these mechanisms. Part of the reason that Prime Minister Sánchez did not suffer loss of reelection as a result of repression against Catalan protesters was likely because the median Spanish voter did not support the *cause* of the protests: polling from 2014 suggested that only 12.3% of Spaniards favored Catalonian independence (Hernández, 2014). It is likely that the identity-based cleavage separating the median Spanish voter from the average Catalan protester shaped Sánchez's performance in the subsequent election. The quintessential case of group-based ethnic conflict, between Israel and Palestine, also demonstrates how democratic leaders can enact consistent repressive tactics without suffering electoral defeat (Amnesty International, 2021; Human Rights Watch, 2021). Indeed, research at the individual-level shows how Israelis and Palestinians have historically been unwilling to compromise (Shamir and Shikaki, 2002) and how Israelis who perceive greater threat from Palestinians are less likely to support protections for this outgroup (Maoz and McCauley, 2009). Overall, conflict typically serves to exacerbate group-level cleavages and increase hostility throughout society. Together, these dynamics have the potential to shift leaders' propensity to repress for electoral gain in response to individual- and group-level

pressures. The malleability of identity, particularly during periods of group-level strife, also introduce incentives for leaders to manipulate the loyalty of in-group members during elections. I discuss these bottom-up and top-down incentives to repress around elections in the next section.

### 2.3 Pre-Election Repression and Reelection

This section explains how leaders shift their patterns of repression as elections approach and how repressive tactics shape leaders' reelection prospects. Although elections produce relatively low levels of repression in democratic countries, elections also introduce incentives for leaders to target threatening groups, particularly when conflict heightens the importance of security provision to leadership selection.[7] Two mechanisms support the theoretical expectations. First, individual- and group-level preferences for security put pressure on leaders to target threatening groups with repression as elections approach. Because the median voter is likely to support repression of threatening groups, leaders should be increasingly likely to demonstrate their commitment to this preference as elections draw closer. Second, leaders can utilize repression against threatening outgroups to increase the loyalty of ingroup members during elections. Both of these mechanisms are intensified during conflict or societal threat, when security is a highly salient and consequential leadership attribute. In what follows, I present further details about each of these mechanisms and identify testable hypotheses about leaders' propensity to repress around elections and the effect that pre-election repression will have on leaders' reelection prospects.

First, leaders can face pressure from the electorate to utilize repression against threatening targets. Literature on the security-civil liberties trade-off shows that voters often prefer leaders use repression to protect them from threatening targets, rather than prioritizing civil liberties protections for all groups. When threatened, voters should reward leaders who pro-

[7]It is possible that leaders who utilize especially high levels of repression will face electoral backlash. However, I expect that leaders will generally not cross this threshold in democratic countries. Descriptive evidence of patterns of repression in this sample reveals that leaders in democracies do use far less repression than autocrats. Empirically, a negative relationship between election timing and repression would indicate some support for this possibility.

tect them by using repression to provide security. Considering group-level dynamics, when leaders' ingroup members face a sustained threat from an outgroup, these voters will also react favorably to a leader who provides them with security against an outgroup threat. Together, these preferences mean that leaders who are beholden to voters' interests via regular elections have an incentive to demonstrate policies that provide security even if these policies violate the civil liberties of some groups. One policy that can demonstrate this ability is repression. Specifically, repression targeted at *threatening* groups can reveal to voters that leaders have both the capacity and the will to provide them with security. The nature of the target is crucial to this assertion. The median voter is unlikely to support leaders who use widespread repression – which could inhibit their own security – or arbitrary repression, against groups that are not believed to pose a threat to aggregate-level security, as domestic democratic peace theory suggests. However, when leaders target repression against specific groups that pose a threat to society, repression demonstrates security without threatening the median voter's own rights.

It is important to recognize that voters' perception of the threat of particular groups is what matters for leaders' incentives to provide security via repression. Voters may prefer that leaders repress a specific group either because that group represents a real *or* perceived threat to society. On the one hand, there are a number of violent groups that actually pose a threat to the average voter's security. Some examples would be the threat of targeted Palestinian violence for the median Israeli voter, the threat of violence or kidnapping from rebel groups in Colombia, or the threat of targeted ethnic violence from aggrieved minority groups, as perpetrated by the Kurds in Turkey, for instance. In all of these cases, voters are likely to place a premium on security provision by leaders because such actions can protect their safety and well-being from ongoing violence. Various forms of repression, like arbitrary arrests, killings, or restrictions on free movement and assembly, can inhibit the ability of threatening groups to mobilize and, at the very least, increase voters' perceptions that leaders are able to keep them safe. In other cases, leaders may face bottom-up pressure to provide security against groups that are merely *perceived* to be threatening. The example of perceived threat from refugees provides one example of these dynamics. Savun and Gineste (2019) explain how voters support leaders who use targeted repression against refugees because they believe that

such actions provide them with security, even as refugees pose little real threat to domestic security. Conrad, Hill Jr and Moore (2018) summarize these dynamics: "leaders subject to removal via the ballot box can have positive incentives to (permit their coercive agents to) pursue visible 'security' measures vis-a-vis members of marginalized groups deemed to be threatening outsiders" (4).

Admittedly, voters' perceptions of the threat posed by different groups varies across countries and time periods. Here, I define repression targets as *threatening groups* based on two factors. First, for a target to be a threatening group they must be identifiable *as a group*. In other words, repression should be discriminate, based on identifiable membership in a particular coalition or class of people.[8] This feature distinguishes targeted repression from indiscriminate or widespread repression, which could presumably affect every voter and would be unlikely to be rewarded at the ballot box. Next, I categorize a group as threatening based on the perception of that group by the median voter. For perceptions of threat to influence a leader's behavior leading up to an election, it must be the case that the median voter perceives the group to be threatening. A group could be perceived as threatening by the median voter based on the group's *behavior* or *identity*. The first factor encompasses dissident groups, like protesters, rebels, gangs, or criminal organizations. The median voter is likely to believe these groups to be threatening because they utilize tactics that challenge domestic security (as when they use violence) or the status quo. The second encompasses marginalized groups, like refugees, which are often believed to be threatening due to their minority status (Horowitz, 1985; Savun and Gineste, 2019; Tajfel and Turner, 1979).

While preferences for security imply consistent pressure for leaders to target threatening groups with repression, the utility of repression for leaders varies over the course of an election cycle. This is because voters do not consider all of a leader's actions when they cast their ballots. Rather, voters tend to focus only on actions the leader takes in the immediate proximity to an election when evaluating candidates (Bartels, 2008; Clarke, Stewart and Whiteley, 1998; Fair, 1978, 1996; Kirchgässner, 1985; Kramer, 1971; Lewis-Beck, 1986).[9]

[8]Kalyvas (2006) provides a useful distinction between discriminate and indiscriminate violence in the context of civil war.

[9]There are many mechanisms that explain voters' propensity to focus on recent events: either they simply cannot remember conditions early in a leader's term (Sarafidis, 2007), they may believe that more recent indicators are more informative about a leader's capabilities (MacKuen, Erikson and Stimson, 1992), or they

The literature on retrospective voting has primarily evaluated voters' tendency toward myopia with regard to economic performance in the United States, though there is also some evidence that this pattern applies across democracies (Duch and Stevenson, 2006) and policy areas (Bechtel and Hainmueller, 2011). In one of the most recent evaluations of this question, for instance, Achen and Bartels (2016) find that economic growth in the last quarters of the leader's term provide the best explanation for election outcomes. In more conservative estimates, Wlezien (2015) finds that a correlation between economic indicators and incumbent victory may extend as far as two years before the election. Regardless of the precise amount of time, a striking empirical regularity is that more recent economic factors have the strongest impact on incumbents' chances of reelection (Franzese, 2002, 375).

The literature on retrospective voting has not extensively evaluated whether these patterns apply to security provision policies.[10] However, it is likely that voters' tendency to focus on recent events should be especially pronounced with regard to repression. For one, although it is the case that recent economic indicators are more readily available than information about the through the whole of a leader's tenure, it is not too difficult for voters to gain access to aggregate economic indicators. Further, statistics marking economic success, such as GDP growth, inflation, and unemployment, are readily agreed upon. This universally accepted standards make it easy for voters to gather information about a leader's performance over the course of their full tenure. In contrast, coverage of repression is typically more limited. Though news outlets may evaluate a leader's patterns of repression as they are perpetrated, the media is unlikely to dwell on a leader's record of repression for long. Further, it is difficult for voters to evaluate repression even if they are able to access information about a leader's actions over the full course of their term. Unlike economic indicators, there are not commonly accepted standards for leaders' ability to provide security. As a result, voters should consider recent repression, rather than repression from the leader's full term, when casting their ballots. Another dynamic to consider is that the mechanisms driving voters' desire for security operate in the short-term. If voters perceive a threat from a par-

substitute information that is readily available for a full picture of a leader's performance (Anderson, 2007; Healy and Lenz, 2014; Huber, Hill and Lenz, 2012; Kahneman, 2003).

[10]One exception would be from Aksoy (2018), who finds that right-wing leaders in Western European countries have increasing propensity to utilize counterterrorism tactics as elections approach.

ticular group, the real or perceived danger from this target results in immediate feelings of insecurity. The provision of security in the short-term also provides immediate relief from the threat of violence, which is likely to shape voters' decision making at the ballot box. Overall, the implication of all of these factors is that voters are more likely to consider repression perpetrated closer to an election than repression perpetrated further from an election when they are voting.

Voters' preference for security from threatening groups and their tendency to focus on the immediate pre-election period when casting their ballots means that only repression perpetrated in the run-up to an election is likely to influence a leader's electoral performance. If repression carried no costs, then these dynamics would not necessarily link election timing to patterns of repression. Leaders could utilize repression whenever they needed to, without changing their behavior as elections approached. However, repression *is* costly. For one, repression carries material costs: leaders must employ and monitor coercive agents in order to perpetrate repressive actions (Carey, 2006; Davenport, 2007*b*). Additionally, leaders recognize that extensive repression has the potential to trigger backlash effects. Aggrieved groups could double down in their opposition if the government utilizes tactics that are disproportionate to a target's offenses. Together, these factors mean that a leader has a limited repression budget and must act strategically when deciding whether or not to respond to threats with repression. Keeping office is the primary goal of all leaders, and democratic leaders achieve this goal by winning reelection (Bueno De Mesquita et al., 2005). So, leaders should take costly actions that voters favor when they are most likely to receive credit for those actions. This logic guides leaders' decision to repress: leaders should be more likely to repress threatening groups (a costly action favored by voters) when they are most likely to receive credit for this action (in the immediate pre-election period). In other words, as elections approach, leaders should become increasingly likely to target repression against threatening groups.

These dynamics should shape leaders' behavior across a range of countries and time periods. However, the mechanisms that guide the relationship between election timing and targeted repression of threatening groups should also intensify during periods of societal threat. In such contexts, candidates' ability to provide security is especially salient to vote

choice. Consider again two cases of ongoing civil conflict and societal threat: Colombia and Israel. In both cases, candidates' position on security policy is one of the driving forces behind candidates' appeals to voters. Security is more likely to be on the electoral agenda for a given election when there is a lack of security for the country as a whole, as during conflict. As a result, candidates should be more likely to demonstrate their ability to provide security through repression. Another factor to consider is that a context of high societal threat – as occurs following a recent terrorist attack or during civil and interstate war – increases the salience of group-based divisions. During conflict, voters tend to divide society into "us versus them" categories, making it easier to identify outgroups and increasing the likelihood that they will perceive outgroups to be threatening. When these group-based divisions are primed by conflict, leaders will find it easy to identify targets that the median voter will believe to be threatening that they can repress. Further, because violence is more common in societies under threat, voters are increasingly willing to trade rights protections for security as compared to contexts of peace.[11]

The discussion so far has focused on one mechanism linking election timing to repression: individual- and group-level preferences for security against threatening groups should increase leaders' likelihood of repressing as elections approach. Yet another mechanism originates with the leader's behavior. There is a possibility that leaders can shift the attitudes of voters by using repression. Targeted violence tends to highlight cleavages between groups and, as a result, increases the salience of group-based divisions. Research in political psychology, for instance, demonstrates that individuals who are exposed to targeted repression identify more strongly with their ingroup and experience higher levels of polarization (Nugent, 2020). Because polarization facilitates outgroup antipathy (Brewer, 1999), repression can facilitate a cycle in which individuals view those who have been targeted as threatening – and therefore deserving of repression – after leaders commit repression. The result of this cycle is that repression can increase the loyalty of in-group members by increasing animosity toward out-group members.[12] As Dickson and Scheve (2006) explain, "politicians can raise

[11]Carey (2010) also expects that state terror, one form of repression, will be more common during political instability given that governments will "be particularly sensitive to perceiving any dissent activities as threatening" during these periods (174).

[12]Mele and Siegel (2019) outline a similar mechanism, finding that governments at times have an incentive to hinder minority groups' attempt to assimilate in order to facilitate their ability to identify minority groups

the salience of social identities via campaign rhetoric to shore up support among voting members of favorably disposed social groups. Priming identity can offer candidates greater slack in selecting a political platform that diverges from the material interests of some voters, who privilege the concerns of the invoked social identity" (quote from (Kalin and Sambanis, 2018, 244).). One implication of these patterns is that leaders can use repression to increase the salience of identity, which could also increase the loyalty of the winning coalition that identifies the leader as an ingroup. Repression of outgroup threats can be an effective strategy to mobilize support from voters. As with bottom-up pressure, leaders' personal incentives to target threatening groups to mobilize supporters will also intensify during conflict. In these circumstances, voters have heightened perceptions of group-based cleavages and are especially susceptible to leaders' efforts to manipulate their loyalty to their ingroup. Taken together, both mechanisms increase the likelihood that leaders utilize targeted repression against threatening groups as elections approach.

> **Hypothesis 1**: Leaders become increasingly likely to repress threatening groups as elections approach, particularly in contexts of high societal threat.

The first hypothesis captures the strategic incentives for leaders. If it is the case that elections increase leaders' incentives to repress out of electoral concerns, then patterns of pre-election repression should also shape the *outcomes* of elections. If the median voter is likely to prefer the leader target threatening groups with repression, particularly during conflict, then leaders who take such actions should be more likely to win reelection than those who do not. There is some existing research linking patterns of repression to election outcomes. First, Cordell (2021) studies how the revelation of a countries' involvement in the United State's extraordinary rendition program influences electoral outcomes in subsequent elections. She finds that left-wing governments face electoral sanctions when this information is revealed, but right-wing governments face no cost. Cooperation in extraordinary rendition can perhaps be considered support for one kind of repression, since the program entailed torture of suspected terrorists. However, there is reason to believe that voters will react differently to support for foreign torture as protection against domestic threats. Domestic threats are likely to be more salient to vote choice, since they represent immediate threats to

to target with repression.

voters' safety and well-being. Repression of these groups provides clear and immediate benefits to voters, while past cooperation in extraordinary rendition programs has less obvious benefits for domestic constituencies. Bryant and Esarey (2019) also examine the influence of repression on voting behavior in the United States. In a vignette experiment, the authors find that repression has no effect on electoral outcomes. However, this experiment does not differentiate across different repression targets and does not consider how voters' preferences may shift as elections approach.

In contrast to these works, the repression of interest in this paper occurs in the pre-election period against groups that represent a threat to security. I expect that the median voter will have favorable attitudes toward such repression, particularly during periods of societal threat, which explains why leaders should be more likely to use targeted repression as elections approach. The downstream consequence of these preferences is that leaders who respond to these interests should increase their likelihood of winning reelection. Leaders boost their reelection prospects when they enact favorable policies during the period preceding an election (Franzese, 2002; Healy and Malhotra, 2013). I expect the same dynamic to hold for leaders' efforts to increase security by targeting threatening groups.[13]

**Hypothesis 2**: Leaders who repress threatening groups are more likely to win reelection than those who do not, particularly in contexts of high societal threat.

## 2.4 Research Design

To test the hypotheses, I analyze patterns of repression and reelection across democratic countries from 1995 to 2019. I focus on a sample of democratic countries given that voters do not hold sway over the outcomes of elections in autocratic regimes. Since I am interested in the effectiveness of the electoral constraint on repression and not the effect of other institutions associated with democracy, I adopt a minimalist definition of democracy.[14] I restrict

[13]These hypotheses to apply to the low- and medium-intensity repression that is commonly observed in democracies.

[14]My account intentionally differs from previous theories that posit that a country that represses cannot be a democracy (e.g., Acemoglu and Robinson (2006)). I find it problematic both empirically and conceptually to define as a democracy as a country that does not repress, mostly because it would render the set of true

my sample to countries coded as liberal democracies or electoral democracies according to the Regimes of the World (RoW) index in the Varieties of Democracy (V-Dem) dataset (Coppedge et al., 2020). The RoW measure classifies regimes based on overall competitiveness of access to power as well as liberal principles. *Electoral democracies* are countries that have "de-facto free and fair multiparty elections and a minimum level of Dahl's institutional prerequisites for polyarchy," but lack full liberal guarantees. *Liberal democracies* are countries that meet Dahl's criteria for polyarchy and guarantee "access to justice, transparent law enforcement and the liberal principles of respect for personal liberties, rule of law, and judicial as well as legislative constraints on the executive satisfied."

I also restrict my analysis to executive elections. While authority over national security is typically divided between legislatures and executives (Damrosh, 1995), the executive holds control over the armed forces as well as the agencies that implement security policy. As such, constitutions primarily task executives with ascertaining the appropriate balance between civil liberties and security (Luban, 2007; Posner and Vermeule, 2007). Further, most constitutions allocate a broad set of emergency powers to the executive (Ferejohn and Pasquino, 2004; Gross and Aoláin, 2006). These powers provide executives with both the means and justification to restrict individual rights to protect citizens against real or manufactured threats (Dragu and Fan, 2020; Lührmann and Rooney, 2020). Given this broad control over the security sector, I expect that voters are most likely to hold executives accountable for repression, as compared to other political leaders. *Executive elections* include presidential, semipresidential, and parliamentary elections as defined by the Democratic Electoral Systems Around the World data (Bormann and Golder, 2013).[15]

### 2.4.1 Repression and Target Identity

I measure repression using data from the International Crisis Early Warning System (ICEWS), which tracks a variety of socio-political events from over 6,000 international, national, regional, and local news sources in English, Spanish, Portuguese, and Arabic (Boschee

democracies empty. See Przeworski et al. (2000) for a similar discussion (14).

[15]I extend Bormann and Golder's coding for 218 elections that (1) were considered democracies by V-Dem but not DES and therefore not coded in DES or (2) occurred after 2016. This data and coding justifications for each election is available upon request.

et al., 2015). Most commonly-used data sources on repression, like the Cingranelli and Richards (CIRI) human rights data project (Cingranelli, Richards and Clay, 2014) and the Political Terror Scale (PTS) (Gibney et al., 2019), create an ordinal score to measure each country's respect for human rights in each year. These measures are inappropriate for this project, however, given that theory refers not to a country's overall human rights performance, but to the specific timing and target of each human rights violation.

There is a long debate about the relative merits of standards-based and events-based measures in human rights research (Chenoweth, Perkoski and Kang, 2017; Fariss, 2014; Schoultz, 1981; Stohl, Carleton and Johnson, 1984; Stohl et al., 1986). The main weakness of events data to study repression is the likelihood that not all events will be captured by the data. Events data use information provided by newspapers and typically skew to cover larger events from urban areas in English-speaking democracies (Chenoweth, Perkoski and Kang, 2017). In light of this bias, many propose that standards-based indicators, which place countries into broad categories of respect for human rights, are more reliable. In this study, however, the biases associated with events data reflect the theoretical focus. This paper explores leaders' propensity to utilize repression for electoral gain, which is repression that is *observable* to voters. Further, in order to react to repression at the ballot box, as predicted in the second hypothesis, individuals must be aware of whether or not repression has taken place. In light of these conditions, it is appropriate to utilize data from newspapers, which reflects the information that is readily available to voters. It is also the case that the sample of countries used in this analysis (democracies) are likely to have regular coverage of events features in the news sources analyzed by ICEWS. Finally, it should be noted that ICEWS improves upon past events data sources by analyzing a wide array of national and regional newspapers in multiple languages.

To utilize the ICEWS data, I parse the full data set of 18 million political events to collect events that represent repressive actions. I use several coding decisions to extract repressive events, drawing from classical conceptions of repression from Davenport (2007*a*), Goldstein (1978), and Ritter (2014). To qualify as repression, an event must be a coercive action that is perpetrated by political authorities who represent the executive branch of an independent state. Further, repressive events must take place in the perpetrator's territorial

jurisdiction and must be targeted against individuals or groups that belong to the same state as the perpetrator. In Section A.2 of the Appendix, I include a thorough description of the coding rules for this measure. Section A.3 of the Appendix provides an empirical comparison between my new measure and existing standards-based metrics of repression. Overall, there is a strong correlation between the ICEWS measure and existing measures of repression in the sample of interest.

The hypotheses refer to repressive actions targeted against threatening groups. Threat perception can be subjective and likely varies by country, making it admittedly difficult to measure. However, I propose that the median voter is likely to perceive some groups to be threatening across cases and that leaders can more easily persuade the public that certain targets are threatening. To categorize repression as targeting threatening groups, I utilize ICEWS' sector dictionary, which assigns the target of each event to one or more of 591 sector categories (Schrodt and Yilmaz, 2007). I consider repression to be targeted at a *threatening* group if the target was a dissident, national ethnic minority, refugee or displaced group, religious minority, international dissident, immigrant, illegal immigrant, or exile. In all of these instances, I expect that median voter to perceive that the target of repression in threatening, while acknowledging that it is difficult to measure perceptions of threat in a purely objective way.[16] See Table A.2 for the frequency of types of repression perpetrated against threatening groups and Figure A.3 for the relative frequency of repression against each of the threatening targets. By far the most common target type identified as threatening was dissidents, making up 98% of the target types.

This measure of repression serves as the dependent variable in the tests of Hypothesis 1 and the independent variable in the tests of Hypothesis 2. To test the first hypothesis, I use a count measure of the number of repressive events perpetrated against a threatening group in a given month. To test the second hypothesis, I aggregate the full count of repressive actions in order to link them to specific elections. I code the number of repressive events occurring in the months preceding each election, aggregating by targets who are likely to be

[16]Note that each target can belong to multiple sectors. In cases where the target belongs to both a non-threatening and a threatening sector, I code the target as threatening. For example, if a minority ethnic group also represents a political party, this group would be coded as threatening. I expect that threat perception for the groups I have identified as threatening will outweigh the other sectors to which the group belongs.

perceived as threatening. The main measure counts the number of events in the five months preceding the election, but the results are typically robust to other aggregations within the 6-month pre-election period. I focus on a relatively short pre-election time span given that research on retrospective voting finds that voters tend to focus on current conditions when casting their votes rather than the incumbent's performance over their full tenure (Bartels, 2008; Fair, 1978, 1996; Kirchgässner, 1985; Kramer, 1971; Lewis-Beck, 1986).

### 2.4.2 Other Variables

The independent variable in the first hypothesis is the *time to election*. I operationalize this variable as the number of months until an executive election. Recall that the sample includes only executive elections in regimes classified as democracies. I restrict the sample to the 2 years preceding an election to account for variation in the length of election cycles across countries.

The dependent variable in the second hypothesis is incumbent *party win*. I operationalize this variable as the incumbent's party rather than the incumbent himself given that most democracies enforce term limits on the executive office. I utilize V-Dem's coding of turnover in the executive office for my dependent variable (Coppedge et al., 2020). The coding for this variable depends on the system of government in place and the type of election at hand. In presidential and semipresidential elections, there is executive turnover when the new president is a different person representing a different party or an independent candidate is elected. In parliamentary systems, turnover is when the ruling party or the ruling coalition of parties lost the election and the prime minister represents a different party or coalition. My dependent variable is a dummy variable coded as "1" when there is no turnover (the incumbent's party stayed in office) and "0" otherwise. Figure A.4 shows descriptive information for the ratio of wins and losses in each election type in my sample. Note that because I am interested in whether a leader's party survives in office, I measure the outcome as whether the party stays in office, not whether the party increases their vote share compared to previous elections. In other words, my outcome of interest is survival, not change in vote share over time.

Both Hypothesis 1 and 2 expect that the relationships of interest will be moderated by the level of societal threat. In the first hypothesis, I expect that leaders will be more likely to target threatening groups with repression as elections approach when society is undergoing threat in order to demonstrate their capacity to provide security to voters. In the second hypothesis, I expect that voters will be particularly likely to reward these efforts in contexts of elevated societal threat. I use three variables to measure different types of ongoing domestic threat. Each of these variables are used as interaction terms to evaluate whether societal threat moderates the relationships of interest.

The first variable, *intrastate conflict* is coded "1" if there is an active intrastate or internationalized intrastate conflict in a given month according to the UCDP/PRIO Armed Conflict Dataset, Version 20.1 (Gleditsch et al., 2002; Pettersson and Öberg, 2020).[17] The second measure, *insurgency*, is coded "1" for country-months when there is an ongoing violent campaign according to the Nonviolent and Violent Campaigns and Outcomes (NAVCO) Data Project, Version 2.0 (Chenoweth and Lewis, 2013). Campaigns are coded as violent when the primary method of resistance for the insurgency is violent in a given year. The third variable, *attack*, is coded as "1" in country-months in which a terrorist attack took place in a given country according to the Global Terrorism Database (GTD) from the National Consortium for the Study of Terrorism and Responses to Terrorism (START) at the University of Maryland. I only code attacks where the nationality of the perpetrator matched the location of the attack, given that the government can only use repression against domestic threats.

I include a number of control variables to account for confounding factors that may influence the relationship between elections and repression in both models. I first control for the regime type of each country, using V-Dem's *polyarchy* measure.[18] I also control for the quality of the approaching election (*free/fair election*), to account for the fact that executives are likely to behave differently in anticipation of unfair compared to fair elections. For this

[17] This coding is based on the start of each conflict episode (when the 25th battle-related death is identified) and the end of an episode (the date of the end of hostilities in the year preceding the end of the conflict episode). I code the end date as December 31 of the last year of conflict if no specific date is listed.

[18] The results are robust to substituting this measure for V-Dem's liberal democracy measure (v2x_libdem) and polity2. I prefer to include a minimalist measure of democracy, given that liberal categorizations of democracy typically include levels of repression in the categorization of regime type.

measure I use V-Dem's primary measure of electoral integrity (v2elfrfair). I also control for a country's *population*, given that larger countries tend to exhibit higher levels of repression, as well as *GDP per capita* and *GDP growth*, since wealthier countries tend to have lower levels repression. These variables come from the Maddison Project (Bolt et al., 2018) and the World Bank's Development Indicators.[19] Next I control for the number of protests each month, using a measure from the ICEWS data which counts the number of events involving peaceful protest, violent protest, or threats of protest.[20] Excluding the measure of election quality, all of these variables are lagged by one month to avoid post-treatment bias. Finally, I include a one-month lag of the number of repressive events targeted against threatening groups, to account for the path-dependent nature of repression. Tables A.3 and A.4 provides summary statistics for all the variables utilized in the tests of the first and second hypotheses.

In the tests for the second hypothesis, I add additional control variables that could influence leaders' patterns of repression as well as their likelihood of winning reelection. First, I control for two election-specific variables, which influence the likelihood that a leader will win reelection. These include the *margin of majority*, which is a dummy variable indicating whether the executive had control of the relevant houses of congress, taken from the Database on Political Institutions (DPI) (Cruz, Keefer and Scartascini, 2018). Leaders with more strength in the legislature are less likely to be voted out of office and more likely to execute their preferred policies. I also include the number of years that the incumbent's party has stayed in office (*years in office*), given that parties who have won before are more likely to win again (Ashworth, 2012), also from the DPI. Given the importance of executive systems in ascribing accountability to particular leaders, I also control for the executive system (presidential, semi-presidential, and parliamentary), given that past research indicates that voters are likely to evaluate leaders differently across different executive systems (Anderson, 2007; Franzese, 2002; Powell and Whitten, 1993; Tufte, 1978). I include dummy variables for *presidential* and *semi-presidential* elections and omit the category for *parliamentary* elections. Finally, I consider constraints on the executive from other branches, including *judicial*

[19] I merge these variable's from V-Dem's data, Version 10.

[20] Specifically, this measure counts events with the following CAMEO codes: threaten with political dissent (133), engage in political dissent (140), demonstrate or rally (141, 1411, 1412, 1413, 1414), conduct hunger strike (142, 1421, 1422, 1423, 1424), conduct strike/boycott (143, 1431, 1432, 1433, 1434), obstruct passage, block (144, 1441, 1442, 1443, 1444), and protest violently, riot (145, 1451, 1452, 1453, 1454).

*constraints* and *legislative constraints*, using data from V-Dem (v2x_jucon and v2xlg_legcon, respectively). These parallel institutions may affect the electorates' likelihood of holding the executive accountable for his actions at the ballot box (Franzese, 2002; Tufte, 1978).

## 2.5 Analysis and Discussion

To test the first hypothesis, I regress the count of repressive actions taken by the government on the months until the subsequent election and the battery of control variables discussed previously. The sample for these hypothesis tests is democratic country-months in the 2-year period preceding each election. This time-frame allows for some consistency across countries, which all have different lengths in their electoral cycles. Further, while two years is long enough to track variation in patterns of repression, it is not so long as to include time periods for which elections would not be expected to have an effect. I use a Poisson link given that the outcome variable is a count measure and cluster standard errors by country. Table 2.1 presents the main results. In Model 1, which presents the bivariate relationship between months to election and repression, there is a positive and statistically significant relationship. In contrast to my first hypothesis, this model demonstrates that the count of repressive actions *decreases* as an election approaches. Since the independent variable counts the number of months until an election, the positive coefficient suggests that levels of repression against threatening groups are significantly higher with further distance from the election. In Model 2, however, we see that this significant effect dissipates with the inclusion of confounding variables that also drive patterns of repression. In general these variables behave as expected: more democratic countries repress less, countries with economic growth repress less, leaders repress more when there are higher levels of protest the preceding month, and repression is highly path-dependent, with a strongly significant predictor of repression being the lag of number of repressive events perpetrated in the previous month.[21]

[21]Some surprising coefficients were the positive effect of the quality of elections (countries with more free elections used higher levels of repression) and GDP per capita (countries with higher levels of wealth used higher levels of repression). It is possible that these results reflect my theoretical expectations, in that leaders beholden to fairer elections are more likely to repress threatening groups to demonstrate their capacity to provide security. The positive relationship between GDP per capita and repression might reflect the capacity

While the results of Model 1 and Model 2 did not conform to my theoretical expectations, part of the reason for these results could be that these models do not account for the presence or absence of ongoing conflict. I expected that leaders would be especially likely to repress threatening groups in the pre-election period when conflict is underway, in order to demonstrate their ability to provide security to voters. Models 3-5 test this possibility using three measures to assess different forms of domestic security threats: civil conflict, violent insurgency, and terrorism. In these models, I interact each of the dummy variables with the number of months to an election to evaluate whether leaders' propensity to repress varies as elections approaches and whether this likelihood shifts when conflict is ongoing. The first thing to note from these interactions is that all three interaction terms (which measure the effect of the time to election during conflict) are negative and statistically significant. These coefficients indicate that, during conflict, leaders are increasingly likely to repress threatening groups as elections approach. In contrast, the coefficients on the constituent term for the number of months to an election are positive and statistically significant. These coefficients show the effect of election timing on repression in times of peace, and demonstrate that leaders are *less* likely to target repression against threatening groups as elections approach.

To better visualize these interaction effects, Figure 2.1 shows the predicted levels of repression as the time to an election decreases, based on whether conflict is ongoing or not.[22] The important takeaway from these figures is that, in times of peace (represented by the blue line), there is a significant decrease in levels of repression as elections approach. These results reflect the conventional wisdom from domestic democratic peace theory about the deterrent effect of elections. Even for repression targeted against threatening groups, leaders seem to be less willing to repress during election periods in times of peace. Whether or not domestic audiences are actually willing to punish repression, it does seem that leaders are hesitant to utilize repressive tactics when they may be held accountable for this behavior in a subsequent election. In contrast to this seeming deterrence during peacetime, leaders are *increasingly* likely to repress threatening groups as elections approach during conflict

of leaders to maintain a coercive apparatus.

[22] I reverse the x-axis to better visualize the relationship between time to election (counting down) and repression. As a result, the direction of the coefficients in the tabular results is opposite to the visualized slope in the figure.

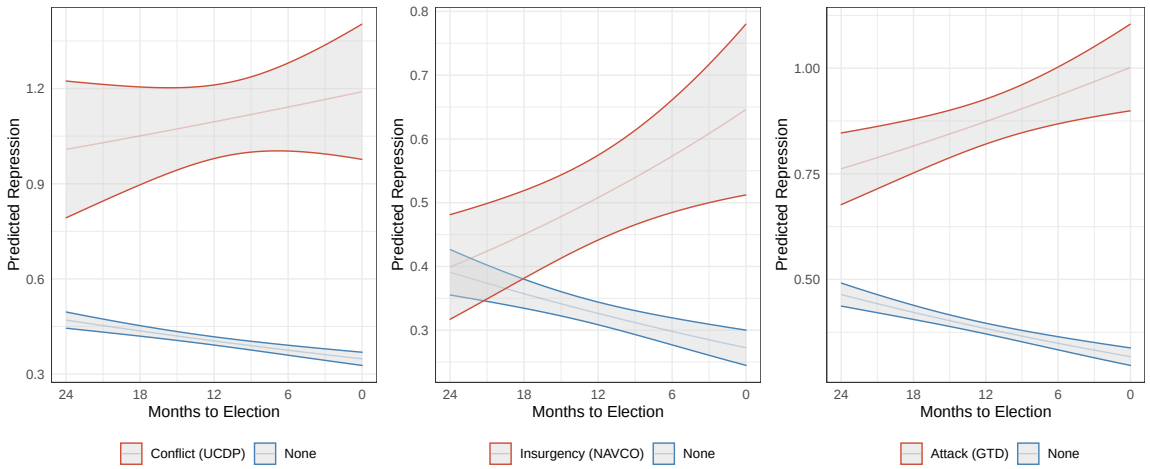
Table 2.1: Election Timing and Repression

| | Bivariate | Controls | Conflict | Insurgency | Terrorism |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Months to Election | 0.01*** | 0.01 | 0.01** | 0.02** | 0.02** |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| Conflict (UCDP) | | | 1.23*** | | |
| | | | (0.30) | | |
| Months to Election * Conflict | | | −0.02** | | |
| | | | (0.01) | | |
| Insurgency (NAVCO) | | | | 0.86*** | |
| | | | | (0.25) | |
| Months to Election * Insurgency | | | | −0.04* | |
| | | | | (0.02) | |
| Attack (GTD) | | | | | 1.15*** |
| | | | | | (0.23) |
| Months to Election * Attack | | | | | −0.03* |
| | | | | | (0.02) |
| Polyarchy | | −4.91** | −4.04** | −7.20*** | −4.58*** |
| | | (1.92) | (1.87) | (1.22) | (1.62) |
| Free/Fair Election | | 0.60*** | 0.56*** | 0.86*** | 0.55*** |
| | | (0.23) | (0.21) | (0.20) | (0.21) |
| Population | | 0.00 | 0.00 | 0.00 | 0.00 |
| | | (0.00) | (0.00) | (0.00) | (0.00) |
| GDP per Capita | | 0.46** | 0.43* | 0.61*** | 0.45** |
| | | (0.23) | (0.22) | (0.23) | (0.21) |
| GDP Growth | | −5.93*** | −5.53*** | −3.85* | −5.37*** |
| | | (1.94) | (1.96) | (2.06) | (1.89) |
| Protests | | 0.01* | 0.01*** | 0.03*** | 0.01*** |
| | | (0.01) | (0.00) | (0.00) | (0.00) |
| $\text{Repression}_{t-1}$ | | 0.03*** | 0.03*** | 0.02*** | 0.03*** |
| | | (0.01) | (0.01) | (0.00) | (0.00) |
| Constant | −0.82*** | −2.44* | −2.87** | −3.06** | −2.75** |
| | (0.23) | (1.25) | (1.25) | (1.56) | (1.22) |
| Observations | 14,121 | 10,701 | 10,701 | 5,248 | 10,701 |

*Notes*: Dependent variable is the count of the number of repressive actions in each month. Standard errors clustered by country. Sample is the 24 months before an executive election (presidential, parliamentary, semipresidential) in a democracy. ***, **, * significant at .01, .05, .10, respectively.

(represented by the red line). The slightly positive effect of election timing in these models is striking when compared to the negative effect during peacetime. The deterrent effect of elections on repression evaporates across a wide range of conflict conditions, and elections even increase the likelihood of targeted repression of threatening groups during these periods at the margins. Of course, the coefficients for the relationship between election timing and repression are relatively small, especially in comparison to the other confounders included in the model. However, given that the outcome variable is the number of repressive actions in a given *month*, even one added act of repression (imposition of a curfew, an act of torture, or a physical assault, for instance), has substantive importance.

Figure 2.1: Predicted Repression, Table 2.1, Models 3-5




*Notes*: Predicted levels of repression based on the months to the next executive election and the presence of ongoing conflict. 95% confidence intervals. All control variable set to means. Outcome variable: count of repressive actions perpetrated by the government in each month.

Turning to the second hypothesis, Table 2.2 shows the results of the regression of whether a party won a particular election on the levels of pre-election repression targeted against threatening groups. These models use a logit link function given the dichotomous nature of the dependent variable and include clustered standard errors by country. Models 1 and 2 both show a positive relationship between repression of threatening groups and a party's likelihood of winning reelection. These results do lend support to the second hypothesis that targeted repression of threatening groups would increase a leader's likelihood of winning reelection in *general*. However, as with the first set of hypothesis tests, the more interesting

quantity is the relationship between repression and election outcomes when interacted with the presence of ongoing conflict. In Hypothesis 2, I expected that targeted repression of threatening groups would have an even stronger effect on a party's probability of winning reelection in the presence of elevated societal threat. However, the results for the interaction terms in Models 3-5 do not tell a clear story.

Considering first Model 3, both the interaction term and the constituent term for conflict are statistically significant. The constituent term on conflict represents the effect of civil conflict on the probability of reelection when a leader has not used pre-election repression. This coefficient is negative and large, indicating that leaders are less likely to win reelection during conflict, as might be expected. Considering the interaction term, predicted probabilities plots in Appendix Figure A.5 reveal that pre-election repression has a positive effect on the likelihood of winning reelection in both conflict and peacetime, but this effect is statistically significant during conflict and indistinguishable from zero during peacetime. So, Model 3 does lend some support for Hypothesis 2 in that the positive effect of pre-election repression on reelection seems to be driven by leaders' behavior during conflict. Models 4 and 5 however, provide mixed evidence. In Model 4, which interacts insurgency with pre-election, none of the constituent terms are significant predictors of leaders' probability of reelection. Model 4 yields a positive and statistically significant coefficient on the constituent term for repression, showing that pre-election repression has a slight positive effect on reelection prospects when there has *not* been a terrorist attack, in contrast to the predictions of the second hypothesis. The constituent term for attack is negative and statistically significant, showing that leaders are less likely to win reelection when there has been a recent terrorist attack compared to when there has not been a recent terrorist attack and the leader has not used repression. The interaction term in this model is insignificant.

Together, Models 3-5 suggest that leaders' baseline probability of winning reelection is relatively low during conflict and when there has been a recent terrorist attack compared to when there is no conflict and there has not been a recent terrorist attack. These findings indicate that leaders' tenure is more unstable when they face domestic violence. Theoretically, I expected the pre-election repression would increase the likelihood of winning reelection for leaders embroiled in such conflict. Model 3 provides some support for this idea: pre-election

Table 2.2: Repression and Reelection Prospects

| | Bivariate | Controls | Conflict | Insurgency | Terrorism |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Repression | 0.02** | 0.03* | 0.03 | 0.01 | 0.03* |
| | (0.01) | (0.02) | (0.02) | (0.04) | (0.02) |
| Conflict | | | −2.25** | | |
| | | | (0.94) | | |
| Repression * Conflict | | | 0.14* | | |
| | | | (0.08) | | |
| Insurgency (NAVCO) | | | | −1.35 | |
| | | | | (0.84) | |
| Repression * Insurgency | | | | −0.05 | |
| | | | | (0.13) | |
| Attack (GTD) | | | | | −0.72** |
| | | | | | (0.37) |
| Repression * Attack | | | | | 0.01 |
| | | | | | (0.03) |
| Polyarchy | | 3.64 | 2.88 | 0.30 | 3.37 |
| | | (2.37) | (2.46) | (3.43) | (2.31) |
| Free/Fair Election | | −0.74** | −0.72** | −0.58 | −0.70** |
| | | (0.34) | (0.35) | (0.48) | (0.33) |
| Population | | −0.00 | 0.00 | 0.00 | 0.00 |
| | | (0.00) | (0.00) | (0.00) | (0.00) |
| GDP per Capita | | 0.01 | 0.02 | 0.05 | 0.02 |
| | | (0.20) | (0.21) | (0.31) | (0.20) |
| GDP Growth | | −0.58 | −0.98 | −3.59 | −1.18 |
| | | (3.41) | (3.46) | (5.10) | (3.31) |
| Protests | | −0.00 | −0.01 | 0.01 | −0.00 |
| | | (0.01) | (0.01) | (0.02) | (0.01) |
| Margin of Majority | | 0.31 | 0.29 | 0.67* | 0.34 |
| | | (0.23) | (0.23) | (0.38) | (0.23) |
| Years in Office | | −0.04 | −0.04 | −0.06 | −0.04 |
| | | (0.03) | (0.03) | (0.05) | (0.03) |
| Presidential Election | | 0.03 | 0.06 | 0.24 | 0.05 |
| | | (0.35) | (0.37) | (0.50) | (0.34) |
| Semi-presidential Election | | −0.82** | −0.82** | −0.49 | −0.85** |
| | | (0.35) | (0.35) | (0.51) | (0.33) |
| Judicial Constraints | | 2.47** | 2.82*** | 6.14*** | 2.49** |
| | | (1.00) | (1.03) | (2.05) | (1.03) |
| Legislative Constraints | | −1.45 | −1.51 | 0.19 | −1.51 |
| | | (1.28) | (1.28) | (1.30) | (1.21) |
| Constant | −0.26** | −2.56 | −2.39 | −5.32** | −2.45 |
| | (0.11) | (1.67) | (1.76) | (2.61) | (1.56) |
| Observations | 590 | 452 | 452 | 226 | 452 |

*Notes*: Dependent variable is whether the incumbent party won reelection. Standard errors clustered by country. Sample is executive elections (presidential, semipresidential, and parliamentary) in democracies. ***, **, * significant at .01, .05, .10, respectively.

repression of threatening groups has a positive effect on the likelihood of reelection during conflict that is distinguishable from zero. However, Model 4 yields no clear evidence about the relationship between insurgency, repression, and reelection, while Model 5 indicates the pre-election repression increases reelection, but this effect does not seem to be conditional on the presence or absence of conflict. One possible interpretation of these findings is that the dichotomous outcome variable is too rough to capture the true relationship between repression and reelection. To evaluate this possibility, in supplemental analysis I assess the results using an alternative dependent variable – vote share – on a sub-sample of elections.[23] However, the results from this analysis are also null. It is possible that there may be some observable implications of the theory with a larger sample or with a finer-grained measure of leader support like public opinion polling. Ultimately, though, while there do seem to be some general trends linking repression to reelection prospects, this relationship is fairly weak.

To summarize the results of this analysis, the strongest findings support the first hypothesis. In periods of peace, elections function as domestic democratic peace theory expects, deterring targeted repression in the pre-election period. In contrast, this deterrent effect disintegrates during various forms of conflict. In such times of elevated societal threat, approaching elections have a positive and weakly significant effect on leaders' propensity to repress threatening groups. In contrast to the support for the first hypothesis, the results for the second hypothesis are relatively weak. In general, targeted repression of threatening seems not to have a consistent effect on leaders' probability of keeping office. While there is some evidence that repression of threatening groups has a positive effect on the likelihood of reelection, the significance of this relationship varies, and there does not seem to be consistent heterogeneous effects based on the presence of absence of high levels of societal threat. One question that emerges from this analysis is how to make sense of these two sets of results. While it seems that leaders are more likely to repress threatening groups as elections approach during conflict, it is not necessarily the case that leaders are rewarded for this behavior. Two main explanations could explain these results. First, it could be that

[23] I collect this data from Erdem Aytaç (2017), who codes candidate vote share across democratic countries up to 2014, omitting post-communist countries. In total, the overlap between this sample and my sample is 213 elections.

leaders seek to repress threatening groups for electoral gain, but that these efforts go unnoticed by voters. Leaders cannot perfectly predict whether and the electorate will respond to their behavior, and all of their efforts to win reelection might not result in higher reelection prospects. Second, it is possible that leaders *are* rewarded by voters for repressing threatening groups during conflict, but that the effect of this behavior is not large enough to change leaders' probability of winning reelection, a relatively noisy outcome measure. Again, future research should evaluate how other indicators of leaders' popularity, including vote share or public opinion polling, vary as a result of pre-election repression targeted at threatening groups.

### 2.5.1 Additional Analysis

In this section, I undertake several analytical strategic to increase confidence in the strongest results from the empirical analysis: the findings in support of Hypothesis 1 from Models 3-5 of the main results table. First, I re-run the analysis with additional control variables. I first control for two variables that provide added insight into the economic state of a country. It is possible that the leader's pre-election repressive strategies will vary along with their confidence in their job performance and the success of the economy. To account for this possibilities, I add control variables for *unemployment* and *inflation*. Unemployment is the percentage of the total labor force that is unemployed but searching for work. Inflation is the percent change in the consumer price index, which is the annual change in the cost to the average consumer of acquiring a basket of goods and services. Data for these economic indicators comes from the World Bank's World Development Indicators (World Bank, 2019).

I also control for the average years of *education* among citizens 15 years and older, aggregated by V-Dem (Coppedge et al., 2020). If the population is more educated, they may be less accepting of human rights violations or less persuaded by the leader's justifications for repression. Finally, I add a control variable for *freedom of expression*, an index which includes the availability of alternative sources of information. This variable measures expert responses to the following question from V-Dem: "to what extent does government respect press and media freedom, the freedom of ordinary people to discuss political matters at

home and in the public sphere, as well as the freedom of academic and cultural expression?" (Coppedge et al., 2020). I include this variable given that Eck and Fariss (2018) demonstrate that information about variation varies heavily across countries and that (Ashworth, 2012) has shown that leaders are more likely to be responsive to voter preferences when voters have access to more precise information about leaders' performances (184). All of these variables are lagged one year. For the most part, the results in Table A.5 and Figure A.6 reveal substantively similar results with these additional control variables.

In the next set of analyses, I limit the sample to pre-election periods that precede elections categorized as free and fair by V-Dem. One benefit of the first set of hypothesis tests is that the time to the election is for the most part exogenous: in democratic countries, elections are typically planned in advance and recur on a set schedule. For this reason, leaders do not typically manipulate the time until the election based on their preferences for repression, increasing confidence that the analysis reveals the true relationship between election timing and pre-election repression. However, it is also the case that leaders could manipulate the quality of the election, even in a democracy, to reduce the likelihood that they would be punished or rewarded for their repressive behavior. To account for this possibility, I re-run the analysis with only elections that were considered to be free and fair in V-Dem's ordinal measure of election quality. The results, in Table A.6 and Figure A.7, are robust to the reduction of the sample to including only these elections.

The final analytical strategy I present in this section is to analyze whether the relationship between election timing and repression varies based on a leader's ideological values. There is some evidence that liberal leaders are more likely to be punished for utilizing repression, or that liberal leaders would have a lower baseline propensity to repression (Cordell, 2021). In contrast, ideologically conservative leaders may reap increased electoral benefits as a result of repressive strategies from voters who prefer a hard-line stance against threatening groups (Aksoy, 2018). To evaluate whether the result are driven by incumbents from a particular political ideology, I re-run the analysis in two different sub-samples, one for right-wing leaders and one for left-wing leaders. I use the classification from the Database of Political Institutions (DPI) (Cruz, Keefer and Scartascini, 2018) which codes incumbent parties as left, right, or center based on their economic policies. Right-wing parties are

conservative, Christian democratic, or right-wing, while left-wing parties are communist, socialist, social democratic or left-wing. Respect for human rights may not perfectly correlate with economic policy preferences, but these values do tend to align within parties.

The first set of results, in the sample of countries with a right-wing incumbent, can be found in Table A.7 and Figure A.8. Here the results are almost the same as the results in the full sample: the interaction terms are negative and highly statistically significant for conflict and insurgency, though the interaction term for domestic terrorist is insignificant. The predicted probability plots reveal similar trends compared to the main analysis, though the confidence intervals are larger at lower levels of repression, likely due to the smaller sample size. The results in the sub-sample of countries with left-wing incumbents can be found in Table A.8 and Figure A.9. Here the results are more mixed. The constituent terms are not significant in the tabular results, but the predicted probabilities plots do reveal similar trends to the main analysis: a slight negative effect of time to election on repression during peacetime, and a higher level of predicted repression during conflict, without a clear relationship between time to election and repression. Overall, the results suggest that right-wing leaders may align their behavior more closely with the theoretical framework provided in this paper, but that left-wing leaders do not behave too differently in their pre-election patterns of repression.

Another possible interpretation of the results for the first hypothesis relates to bias in media reporting. It is possible that the media is more likely to cover repressive events as elections approach, and this increased amount of coverage could lead to an increase in reports of repression by ICEWS. In this case, the observed results would stem from an increase in *reporting* of repressive events rather than an increase in the *occurrence* of repressive events. If this interpretation is correct, one would expect to see an increase in reports of all repressive events, regardless of the target type, around elections. In contrast, my theory anticipates that only repression of threatening groups should increase around elections: voters have no reason to reward repression of non-threatening groups. To evaluate these dynamics, I re-analyze my hypotheses using the alternative dependent variable of the count of repressive events against non-threatening groups in each month. These results are shown in Table A.9. What they reveal is that there is no statistically significant relationship between time to

election and repression of non-threatening groups during periods of conflict, adding support to the theoretical underpinnings of the findings in the main analysis. In some models, there is a small negative effect of election timing on repression of non-threatening groups during peacetime, indicating that reports of repression of non-threatening groups increase as elections approach in non-conflict periods. These results contrast with the main findings, which show that, in peacetime, reports of repression of threatening groups *decrease* as elections approach. If anything, the potential media bias during peacetime means that the results for this sub-sample of countries are understated in the main analysis.

Another possible interpretation of the results is that domestic opposition groups are more likely to utilize violence as elections approach (Harish and Little, 2017). Then, an observed increase in repression as elections approach could be a result of leaders' reaction to the presence of insurgency during this time periods, rather than an increase of repression due to electoral concerns, as I theorize in the paper. To analyze this possibility, I conduct analysis with separate outcome variables for each of the types of conflict that I utilize as interaction terms in the main analysis: civil conflict, violent insurgency, and domestic terrorist attacks with the standard battery of lagged controls, which are also predictors of conflict onset. These results are shown in Appendix Table A.10. They show that there is not a statistically significant effect of election timing on any of the types of conflict used in the main analysis.

## 2.6 Conclusion

This paper considers how elections shape leaders' propensity to utilize repression and their performance at the ballot box. Theoretically, I propose that leaders are increasingly likely to target repression against threatening groups in the run-up to elections. Particularly during periods of elevated societal threat, leaders have an incentive to demonstrate their capacity to provide security to voters. The analysis of the relationship between election timing and patterns of repression, which uses a novel measure of targeted repression from the ICEWS dataset, supports this theory. While elections deter targeted repression during periods of peace, leaders are increasingly likely to repress threatening groups as elections

approach during civil conflict, in the presence of a violent insurgency, or in the aftermath of a domestic terrorist attack. I also expected that leaders' *performance* in elections would vary with their use of pre-election repression and the status of ongoing conflict. For the most part, however, the results did not substantiate this hypothesis. Targeted repression seemed to have little effect on leaders' propensity to win reelection, regardless of the presence of ongoing threats.

The theory and empirical results lend important insight into the relationship between democratic institutions and repression. While previous studies find a positive correlation between democracy and human rights, this paper focuses on the effectiveness and limitations of one particular democratic institution: elections. I explain why elections are likely to provide ineffective constraints against some targeted repression: leaders have strategic incentives to repress threatening groups to demonstrate their capacity to provide security during conflict. The results support this idea. While elections deter targeted repression during peacetime, they increase repression of threatening groups during conflict. In contrast to the micro-foundations of the domestic democratic peace, the second main finding is that even leaders who utilize relatively high levels of targeted repression do not seem to suffer electoral consequences. Overall, this paper supports the idea that elections enact relatively weak constraints against targeted repression, given that electorates evaluate repression based (in part) on the security that it can provide them against threatening groups. Instead, parallel institutions, such as strong legislatures and independent judiciaries, are likely to provide the primary bulwarks against targeted repression in democracies.

## 3.0 Paper 2: Why Voters (Sometimes) Punish Repression

Each year, democratic leaders use a variety of repressive tactics against those within their borders: they arbitrarily arrest dissidents, employ violence against protesters, torture, kill, and limit basic freedoms of speech, press, and movement.[1] Though such actions are certainly more prevalent in autocracies, they are not uncommon in democracies (Haschke, 2018). This behavior can be puzzling in light of the robust literature linking democratic institutions to relatively low levels of repression (Davenport, 2007*a*), defined as "coercive actions political authorities take to inhibit the will or capacity of people within their jurisdiction to influence political outcomes" (Ritter, 2014, 145). In spite of the range of institutional constraints against repression in democracies – from independent judiciaries, legislative bodies, and the electorate – leaders often believe that the benefits of repression outweigh its potential costs.

One way to understand these patterns is to evaluate when and why democratic institutions break down in keeping leaders from repressing. Under what conditions are the costs imposed by democratic institutions too low to deter leaders from utilizing repressive tactics? To shed light on this question, this paper evaluates the strengths and weakness of the foundational democratic institution – elections – for keeping leaders from repression. A common assumption in the literature on the domestic democratic peace is that voters prefer their leaders refrain from repression and are willing to punish repression in future elections (Cingranelli and Filippov, 2010; Gurr, 1986; Richards, 1999; Richards and Gelleny, 2007). The major implication of this assumption is that elections can keep candidates from repressing for fear of losing office. However, there is little research at the individual level to anticipate whether and when voters actually behave in this way. For elections to impose a cost on repression, and thus counteract leaders' incentives to repress, individuals must both be averse to repression *and* be willing to punish candidates who utilize repressive tactics.

[1]For some examples of these patterns see Aytaç, Schiumerini and Stokes (2017); Easterly, Gatti and Kurlat (2006); Haschke (2018); International Network of Civil Liberties Organizations (2013); Rejali (2007) and Varieties of Democracy Institute (2021). The experimental design in this paper was approved by the University of Pittsburgh's Institutional Review Board (Study 20070262) and pre-registered with the Open Science Foundation (`https://osf.io/kvsyt/?view_only=2eed643d73354b30926616ceb9d8360e`) before data collection. I acknowledge financial support for this project from the School of Arts and Sciences at the University of Pittsburgh, the Richard Cottam Prize, and the Hayek Fund.

Without knowing when and how these conditions take effect, however, researchers are left with limited insight about the effectiveness of elections for curtailing repression.

This paper introduces a theoretical framework to understand when individual attitudes toward repression influence vote choice. It proposes that voters vary in their propensity to consider repression based on the *target* of repression and their perceptions about the *threat* that target poses to domestic security. I argue that voters are primarily self-interested when it comes to considering repression at the ballot box. As a result, they should punish repression perpetrated against groups to which they belong and reward repression that generates security benefits against groups they perceive to be threatening. Alternatively, if repression is targeted against an out-group that poses no threat, voters will privilege other issues in their vote choice. The implication of these individual-level tendencies is that candidates should experience electoral losses among groups that have been targeted by repression, while they should experience electoral gains among groups who perceive the targets of repression to be threatening.[2]

To evaluate this theory, I conduct a conjoint experiment that isolates the causal effect of repression on candidates' vote share and generates insight into the importance of repression vis-a-vis other salient issues. Using the United States as a test case, I find in a representative sample of registered voters that repression does at times lower candidates' expected vote share. However, only certain groups punish repression. In particular, only those who identify as an in-group with Black Lives Matter (BLM) – a frequent target of repression in the United States – incorporate repression into their vote choice. Among this group, candidates can expect their vote share to decrease by 5.2% when they support repression of BLM compared to when they support everyone's right to protest. Substantially, this treatment effect mirrors the influence of candidates' partisan identification, their stance on gun rights, and their position on COVID-19 restrictions. Among other groups, however, repression consistently has a null effect on candidates' vote share.[3] While the results do not suggest

[2]There is individual-level research about the conditions under which individuals will oppose various forms of human rights abuses n the abstract (Conrad et al., 2018; Davis and Silver, 2004; Edwards and Arnon, Forthcoming; Gibson and Gouws, 2003; Kearns and Young, 2020; Piazza, 2015). However, this paper is one of the first to explain how these *attitudes* translate into voting *behavior* (see Bryant and Esarey (2019) for an exception).

[3]For the most part, power analysis rules out the possibility that these results are the result of a small sample size. See the discussion in the analysis and Section B.5 in the Appendix.

that any group will *reward* repression, they do show consistent indifference to repression among all demographics except BLM supporters.

These findings yield important insight into the strength of elections for curbing repression. In the simulated elections of this experiment, candidates can expect electoral losses for repression only among a group that has been the frequent victim of past violence. Problematically, however, candidates are not punished by other demographics for supporting repression. The result is that candidates can repress with impunity if they are not accountable to the groups that do punish repression. Extrapolating beyond the U.S. case, these results indicate that, absent other institutional constraints, elected leaders have little reason to avoid targeting groups that do not belong to their winning coalition. Such dynamics might help to explain why leaders in electoral democracies – like India, Brazil, Turkey, or Mexico – often repress marginalized groups with seeming impunity at the ballot box. This paper also extends existing knowledge on the domestic democratic peace, by bolstering past studies which suggest that the strongest institutional constraints are not elections but rather horizontal institutions like an independent judiciary that can prosecute repressive actions and legislatures that can balance out the power of the executive to implement repressive policies (Conrad and Moore, 2010; Conrad, Hill Jr and Moore, 2018; Conrad et al., 2018; Davenport, 2007*b*). This paper's results indicate that strengthening these parallel institutions is crucial for promoting rights protections for all groups.

The paper proceeds as follows. The first section reviews the literature on the security-civil liberties trade-off, which generates insight into the drivers of individual support (and opposition to) rights protections in the abstract. The second section explains how these individual-level attitudes influence voting behavior and derives testable hypotheses about the probability that repression will shape candidates' expected vote share. The third section introduces the conjoint experiment, sample characteristics, and randomization scheme. The fourth section tests the hypotheses and explores alternative explanations for the relationship between repression and vote share. The last section concludes.

### 3.1 The Security-Civil Liberties Trade-off

The protection of individual rights and maintenance of collective security often represent conflicting goals (Davis, 2007). It is common for governments of all regime types to restrict rights and liberties for the sake of promoting the security of the country as a whole. Such restrictions vary in intensity. At one end of the spectrum, governments routinely implement boundaries on rights to free speech. It is well understood that one cannot yell "fire" in a crowded theater and many forms of hate speech are not tolerated in democracies. During the Covid-19 pandemic, governments required the use of masks in public spaces to promote collective health, one kind of security. Governments also invoke more extreme violations of physical integrity rights to protect against perceived or actual threats. The interment of Japanese Americans during World War II provides one such example, as the U.S. government enacted harsh repression to the threat of a fifth column of Japanese support following the attacks on Pearl Harbor (Reeves, 2015). In a similar vein, violent action to disperse protesters and torture of individuals suspected of facilitating terrorist attacks represent rights restrictions to ensure order and stability for the community as a whole. Overall, the trade-off between collective security and individual freedoms is one of the major dilemmas of modern governance (Waldron, 2003).

Individuals vary in their attitudes about the use of various forms of repression to promote increased collective security. In general, those with more liberal political ideologies are less likely to support civil liberties restrictions, as are those who have low levels of trust in the government (Davis and Silver, 2004; Jenkins-Smith and Herron, 2009; Mondak and Hurwitz, 2012) and those who have been victimized by crime (Bateson, 2012). Similarly, demographic features like education, gender, religiosity, age, and race influence the extent to which individuals value civil liberties or security when asked to trade one value for the other (Davis, 2007; Malka and Soto, 2011; Sullivan, Piereson and Marcus, 1982; Wemlinger, 2013). Personality features like right-wing authoritarianism also influence preferences for the security-civil liberties trade-off (Crowson and DeBacker, 2008).

Beyond these individual-level factors, however, the *context* in which governments repress also influences the extent to which individuals will support rights violations in the abstract.

As Davis (2007) explains, few see civil liberties or security as absolute goals. Rather, "support for civil liberties should be seen as situational, with people picking and choosing which values they are willing to concede and the type of security it might provide" (220). A variety of contextual factors influence attitudes toward the security-civil liberties trade-off, including the effectiveness of rights restrictions in promoting security (Garcia and Geva, 2016; Kearns and Young, 2020), the kinds of rights restrictions that are implemented (Richards, Morrill and Anderson, 2012), and the levels of sociotropic threat a country is experiencing when governments implement repressive policies (Davis and Silver, 2004; Davis, 2007; Dietrich and Crabtree, 2019). The latter factor is especially salient: threat perception helps explain why individuals are willing to support repressive leaders in countries plagued by civil conflict and terrorist attacks.

One important contextual factor is attitudes toward the group that has been targeted by rights restrictions. Individuals typically have supportive attitudes toward rights restrictions of groups they dislike or perceive to be threatening compared to the rights of groups they like, respect, or belong to. In the context of the security-civil liberties trade-off, one can understand individuals' desire to restrict the rights of out-groups as relating to their interest in increasing their own security and the security of their in-group members, broadly speaking. Individuals are willing to trade away the rights of groups that could threaten their physical safety, but also the dominance of their group's ideals and values. For instance, respondents in Russia, South Africa and the United States supported rights restrictions of disliked groups, including homosexuals, atheists, communists and political extremists (Gibson, 1998, 2008; Gibson and Gouws, 2003; Sullivan, Piereson and Marcus, 1982). Each of these groups can be perceived as threatening in one way or another, at times to domestic security, but also to traditional understanding of gender roles and some religious values.

Individuals are also willing to restrict the rights of groups who do not share their ethnic or racial identity, both because of dislike and increased threat perception. For instance, respondents in the United States are more likely to support arbitrary detention (Piazza, 2015) and the use of enhanced interrogation techniques against people with Arabic-sounding names (Conrad et al., 2018) or foreign nationalities (Kearns and Young, 2020), in part because they are more likely to see such individuals as threatening than in-group members

(Huddy et al., 2005). Recent survey experiments in Israel and the United States also find that respondents were more likely to support repression of those who did not belong to their identity group (Edwards and Arnon, Forthcoming). In India, respondents are less likely to support the civil liberties of out-groups when primed to consider the threat of electoral violence (Deglow and Fjelde, 2020). Similarly, in post-war Sri Lanka, Tamils and Sinhalese Sri Lankans were less likely to grant an out-group member the right to protest compared to a member of their own ethnic group (Kijewski and Rapp, 2019).

As a whole, past research provides important insight into the determinants of individual *preferences* about repression. However, there exists little evidence for the link between attitudes about repression and political behavior in the form of vote choice. The next section provides a theoretical framework to understand how voters evaluate repression at the ballot box.

### 3.2 Repressive Leaders at the Ballot Box

How do attitudes about repression affect voters' evaluations of political candidates?[4] Past research provides only suggestive evidence to answer this question. Theoretical work on the domestic democratic peace, for instance, often assumes that voters are willing to punish repression in elections (Beer and Mitchell, 2004; Cingranelli and Filippov, 2010; Davies, 2016; Gurr, 1986; Richards, 1999; Richards and Gelleny, 2007). Yet this assumption has not been empirically tested, and it fails to consider literature on the security-civil liberties trade-off, which complicates our understanding of attitudes toward repression. In the latter body of research, individuals often tacitly support repression against threatening groups. Research examining abstract attitudes about repression does at times extrapolate its findings to hypothesize about the effect that attitudes may have on voting behavior. However, it has not explicitly considered – theoretically or empirically – how attitudes shape vote choice.

Such an examination is critical given that individuals cannot consider all of their prefer-

[4]This framework diverges from literature on authoritarian governance, in which autocrats often use repression to intimidate voters and fix election results (Gandhi and Przeworski, 2007). It applies only to democratic elections, where election results are not pre-determined.

ences when they vote, meaning that abstract attitudes rarely translate directly into voting behavior (Achen and Bartels, 2016; Anderson, 2007; Healy and Malhotra, 2013; Tversky and Kahneman, 1974). Instead, voters must decide how to weigh their support for (or opposition to) rights protections in comparison to other salient issues. This process is consequential for our understanding of the effectiveness of elections for constraining repression. If voters react favorably to certain kinds of repression at the ballot box, then leaders have an incentive to undertake repressive action in the run-up to the election. On the other hand, if voters are willing to punish leaders for some kinds of repression, then elections will serve to keep leaders from engaging in rights violations. Ultimately, existing literature lacks a coherent account for the influence of repression not just on attitudes, but on voting behavior and, consequentially, leaders' electoral performance.

The next sections remedy these gaps by introducing a cohesive account of the influence of repression on individuals' *voting* decisions. I propose that individuals' evaluations of repression at the ballot box vary based on the *target* of repression and their perceptions about the *threat* posed by that target. Two mechanisms underscore this argument. First, voters typically evaluate repression based on its effects on their own and their group's well-being. Second, voters are more likely to support repressive candidates when they believe that repression increases security. As a result, they will reward repression against threatening groups.

### 3.2.1 Group-Based Interests

Voters typically evaluate repression based on its effect on their own well-being and the well-being of other in-group members. Given that it is cognitively difficult to assess a leader's overall performance, individuals often vote according to the impact that a leader's performance has on their own welfare (Fiorina, 1981; Key Jr., 1966). As Kinder and Kiewiet (1981) explain with regard to economic voting: "personal experiences [...] are compelling in ways that vicarious experiences cannot be" (130).[5] Self- and group-focused considerations should

[5]Admittedly, the empirical support for pocketbook voting is mixed (Healy, Persson and Snowberg, 2017; Kinder and Kiewiet, 1981; Rogers and Tyszler, 2018). In large part this is because it is difficult to distinguish egocentric and sociotropic motivations for vote choice given that individual and aggregate prosperity are intimately tied. Yet repression does not have this feature: repressive actions are typically targeted at

also play a role in how individuals evaluate government repression. All forms of repression restrict the rights and liberties of a subset of each country's population. While some restrictions affect almost everyone – like surveillance measures that infringe on everyone's right to privacy – most repressive actions in democracies are targeted against members of specific groups. For instance, leaders typically torture members of groups suspected of engaging in terrorist activity. Similarly, repression of protesters affects those who choose to protest, but not those who stay home. A variety of other types of repressive actions, including regulations of land-use, temporary detention, and restrictions on patterns of dress, target minority populations, like indigenous groups in Latin America, Kurdish communities in Turkey, and Muslim women in France.

The possibility of discriminate repression means that repression affects each voter differently. While the cost of repression to the immediate victims is often quite high, for many groups it is negligible. For instance, most voters are unlikely to be tortured during a given leader's tenure and are also unlikely to have a personal relationship with someone who has been tortured. Similarly, those who have never protested and never planned to attend a protest are unlikely to experience any costs for government violence against protesters. Anderson (2007) argues that individuals rely on heuristics not only of their personal well-being but of the well-being of similarly situated individuals in order to form evaluations of the economy. More recent work also finds that voters act according to self-interest as well as concern for other in-group members (Haselswerdt, 2020).

When casting their ballots, voters first evaluate repression based on whether it has impact on their own well-being and the well-being of other in-group members. The immediate victims of repression, as well as their friends and family, are unlikely to vote for leaders who support repression of their group. In one sense, this prediction is trivial: leaders are unlikely to repress their own supporters, so those targeted were not likely to support repressive leaders in the first place. Yet this paper's experimental design evaluates more nuance around this expectation. By holding constant all other policy positions and demographic features, the conjoint experiment analyzes whether voters would support a candidate who repressed their own group even if all the candidate's other attributes were favorable. Given that repression

particular groups, meaning that the costs of repression are carried only by specific populations.

presents a threat to the safety and well-being of an individual and his or her group, it should play a strong role in vote choice for victims, similar to other policies that have a direct effect on the voter's well-being.

Compared to those who identify as in-group members with the targets of repression, voters who have *not* been targeted, do not know those who have been targeted, or do not fear future targeting are generally less likely to consider repression in their vote choice. Rather, these voters will tend to cast their ballots based on policies that have a direct effect on their own lives and the lives of those close to them.[6] Kearns and Young (2020) quote one government official in the United States who provides a revealing reflection on this dynamic with regard to enhanced interrogation techniques: "people's support for harsh punishment would change if someone like them — their in-group member — is impacted by it" (121).[7]

To summarize the above expectations: voters are unlikely to elect candidates who support repression of their in-group members, but they will typically be indifferent toward repression of out-groups. While this discussion accounts for the uneven distribution of the *costs* of repression, however, it has not considered the actual or perceived *benefits* that repression may provide. The next section evaluates a second mechanism linking repression to vote choice: voters' perceptions about the security benefits of repression.

### 3.2.2 Threat Perception

Individuals are often willing to trade rights protections for security. This trade-off is driven by the fact that, while repression always imposes costs on its immediate victims, it can also provide real or perceived security benefits to the collective community. Consider torture, for instance. Most voters experience no personal costs when the government tortures, but many believe that torture provides collective benefits in the form of intelligence against terrorist attacks. Similarly, individuals who have not been repressed while protest-

[6] A counterargument is that voters who hold liberal values may punish repression for ideological reasons, even if repression does not affect their personal well-being. Cordell (Forthcoming), for instance, finds that liberal parties are sanctioned for engaging in covert torture in future elections. I discuss the potential for heterogeneous effects based on political ideology in the results section and in Appendix Section B.8.

[7] Conrad et al. (2018) also make this assumption: "We do not assume that the electorate inherently values rights protection for the population at large. Instead, we make the relatively innocuous assumption that each voter prefer that the state not violate his/her own rights" (7).

ing experience no cost to repression of other protesters. But, these same individuals may experience benefits to repression that is targeted against protesters who threaten their safety and/or ideals. In all of these cases, individuals can choose to trade the rights of out-groups for the security of their own group (Cole, 2003; Waldron, 2003).

The factor that determines whether or not voters experience benefits to repression is the perception they have about the threat posed by the target of repression. If voters perceive the target of repression to be non-threatening, then repression provides no cost but also no benefits. In contrast, if a voter believes the target to be threatening, then repression can provide security benefits by defusing an ongoing threat. How do these dynamics link to voting behavior? All else equal, voters are unlikely to incorporate out-group repression into their vote choice, instead focusing on issues more salient to their own well-being and the well-being of their group members. Yet there is an exception to this tendency when voters perceive a threat from the target of repression. In such cases, voters should favor leaders who support repression: repression increases security against an ongoing threat while a failure to act leaves voters vulnerable. In other words, among voters who perceive a threat from a particular group, candidates who support repression should collect a higher vote share compared to candidates who do not.

It is important to note that there are many reasons why voters may perceive a group targeted by repression to be threatening. I define threatening in a broad sense, given that evaluations of threat vary across identity cleavages across countries and political contexts (Tajfel, 1978; Tajfel and Turner, 1979; Brewer, 1999). Voters may believe specific groups to be threatening for their use of violence, ideals and values that threaten their own way of life, or their perceptions about the features of a particular ethnic group. The experiment measures threat perception directly by asking respondents to rank the threat they perceive from a variety of groups.

Notably, evaluations of threat need not be based in reality. Media portrayals tend to amplify negative stereotypes, particularly regarding the level of threat that out-groups pose to society. Kearns and Young (2020) note that media accounts often capitalize on stereotypes that highlight racial and cultural differences in coverage of terrorism and mass violence (31). In the United States, for instance, terrorist attacks receive 357% more coverage when

the perpetrator is Muslim and this coverage is more likely to mention terrorism (Kearns, Betus and Lemieux, 2019). Another relevant case is repression of members of the Mapuche community in Chile. Here, political elites and the media capitalized on acts of violence perpetrated by some Mapuche members to paint the community as a whole as violent and threatening. These narratives fueled public perception that the Mapuche posed a threat to Chilean society, and justified the passage of an anti-terrorism law restricting the rights of Mapuche citizens and other forms of government repression (Amnesty International, 2018). Both real and perceived threats can influence political behavior. As Davis (2007) explains, "that there might be a discrepancy between an actual and perceived threat does not make the fear of it any less relevant" (10).

### 3.2.3 Hypotheses

This discussion leads to testable hypotheses about the link between different forms of repression and individual evaluations of repression at the ballot box. Two individual-level mechanisms guide the argument: voters will consider the group targeted by repression and the threat posed by that group when incorporating repression into their vote choice. Holding all other candidate characteristics equal, voters are *less* likely to support candidates who favor in-group repression, *equally* likely to support candidates who favor repression of non-threatening out-groups, and *more* likely to support candidates who favor repression of threatening out-groups.

One outcome of interest that results from these individual-level decisions is candidates' performance in elections. Theoretically, I am interested both in how individuals make decisions during elections as well as how these decisions aggregate to shape the likelihood that a leader will win votes at the ballot box. A conjoint experiment allows for empirical evaluation of both of these outcomes. Using data on individual-level preferences, the conjoint design aggregates a series of candidate selections by individual respondents into a generalizable link between policies and vote share. Specifically, the conjoint design assesses changes in the probability that an individual will choose a candidate as well as the expected change in each candidates' vote share as a result of the attribute of interest (Bansak et al., 2020), averaging

over all other candidate attributes. In other words, the conjoint design creates a direct empirical link between individual-level choices and the candidate-level outcome of expected vote share. In light of these design nuances, the following hypotheses make predictions about how repressive policy positions influence candidates' vote share. This candidate-level outcome stems directly from the individual-level theory outlined above.

**Hypothesis 1**: Candidates who support repression of voters' *in-group members* will have a *lower* vote share compared to candidates who do not support repression.

**Hypothesis 2**: Candidates who support repression of *non-threatening out-groups* will have *no difference* in vote share compared to candidates who do not support repression.

**Hypothesis 3**: Candidates who support repression of *threatening out-groups* will have a *higher* vote share compared to candidates who do not support repression.

### 3.3 Experimental Design

I use a choice-based conjoint experiment to evaluate my hypotheses. Conjoint experiments have grown in popularity in political science for the insight they provide into multidimensional preferences (Bansak et al., 2019*b*). A conjoint is especially useful in this case given that it can assess the magnitude of the effect of repression on vote choice relative to other candidate attributes. Further, given that social desirability bias likely obscures respondents' true preferences for repressive candidates, a conjoint design has the advantage of decreasing the likelihood the respondents will perceive the sensitive nature of survey questions (Horiuchi, Markovich and Yamamoto, 2018).

I conduct my experiment in the United States in March-April 2021 using Lucid, a survey firm based in the United States.[8] I survey a representative sample of 750 U.S. registered voters to approximate the effect that candidate positions would have on actual elections.[9]

[8]Recent studies using Lucid have been published in the *American Political Science Review* (Graham and Svolik, 2020; Tomz and Weeks, 2020; Williamson et al., 2020), *American Journal of Political Science* (Costa, 2020), and *Journal of Politics* (Lajevardi, 2020; Levy, 2020).

[9]The sample is representative on party identification, race, gender, income, and region. The pre-analysis plan specified a sample of 500. However, power analysis with this sample indicated that it was too small for some models to detect significant effects, so I increased the sample size for the final analysis.

The sample is restricted to individuals who voted in the 2020 elections to maximize external validity: it is the decision-making of voters that is likely to drive candidate behavior.

The United States represents a compelling case to evaluate the theory. Though it has been classified as a consolidated democracy, the United States also has a long history of repression against minority ethnic and ideological groups (Davenport, 2015, 2010; Gibson, 1988, 1989; Goldstein, 1978; Komisarchik, Sen and Velez, 2021). As such, this case exemplifies an important tension: democratic institutions may have limited effectiveness in constraining repression against groups the electorate perceives to be threatening. Concerns about the strength of U.S. democracy have also intensified in recent years (Hyde, 2020; Ingraham, 2020; Kaufman and Haggard, 2019; Levitsky and Ziblatt, 2018) as individuals (Graham and Svolik, 2020) and party leaders (Lührmann et al., 2020) have become less committed to democratic principles. Repression has become increasingly relevant to public discourse, and the experiment evaluates how voters reacted to these tendencies in the 2020 election cycle. The theory also suggests that voters must perceive some groups to be threatening for politicians to reap electoral rewards for repression. Because threat perception is often linked to ethnic identity, the diversity and salience of ethnic cleavages in U.S. politics and society present an interesting test case. Finally, much recent experimental research on repression has been conducted in the United States (Bryant and Esarey, 2019; Conrad et al., 2018; Dietrich and Crabtree, 2019; Edwards and Arnon, Forthcoming; Kearns and Young, 2020; Piazza, 2015). This study adds important nuance to these previous findings.[10]

### 3.3.1 Conjoint Set-Up

Following a standard conjoint design to measure vote choice, respondents must choose between two candidates for political office, in this case a gubernatorial race. This level of office is appropriate for two reasons. First, while authority over security policy is typically

[10]Bryant and Esarey's (2019) study is the most similar to this experiment, though there are several distinctions. First, Bryant and Esarey (2019) use a factorial design to randomize candidates' support for repression, while I use a conjoint design to randomize all candidate features. Further, while respondents in Bryant and Esarey's (2019) study always choose between a Republican and a Democrat, respondents in this experiment also evaluate pairs of candidates with the same party identification, so they need not defect from their own party to punish or reward repression. This conjoint design also evaluate a unique form of repression (restrictions on the right to protest) and the heterogeneous effects of group identification and threat perception.

divided between legislatures and executives, the executive holds control over the agencies that implement security policy and must ascertain the appropriate balance between civil liberties and security (Posner and Vermeule, 2007). Most executives also have a variety of emergency powers (Gross and Aoláin, 2006) that provide them with the means and justification to restrict individual rights to protect citizens against real or manufactured threats. Given these dynamics, voters are most likely to hold executives accountable for repression as compared to other political leaders. Further, a gubernatorial race is preferential to a presidential contest, given the high name recognition and polarization associated with presidential elections.

After first soliciting responses for a standard battery of demographic questions, the survey reveals a screen with the profiles of two candidates as displayed in Table 3.1.[11] The instructions ask respondents to "consider a choice between candidates for the governor of your state." Further, they explain to respondents that "some of these candidates will seem similar to actual candidates and others may seem unusual. That's okay. Just make the best choice about which candidate you would prefer." I include these caveats given that a conjoint task inevitably includes unrealistic candidate profiles. In order to hold constant unobservable features about the candidates' personalities, the instructions also tell respondents to "presume that both candidates are equally qualified to hold office in terms of character, temperament, and other personal and moral qualities."' Every respondent will evaluate ten pairs of candidates, each displayed on a new screen.[12] Attribute ordering is randomly varies across respondents to avoid any ordering effects, but it is consistent across tasks for each respondent to avoid confusion (Bansak et al., 2019*b*).

There are thirteen candidate attributes in each conjoint task, well below the threshold that would result in satisficing (Bansak et al., 2019*a*). There are two kinds of candidate attributes: demographic features and policy positions. These include demographic features that have been shown to influence individual vote choice, including *Ideology*, *Race*, *Gender*, and *Current Job*. The design also includes candidates' positions on several policies salient to the 2020 election. Preceding the experiment, I reviewed the platforms of all 2020 gubernato-

[11] I collect demographic information about respondents' *Gender*, *Income*, *Education*, *Age*, *Race/Ethnicity*, *Community (Urban/Rural)*, *State*, *Party Identification*, and *Political Ideology*. See Appendix Section B.2 for coding rules and Section B.9 for the full survey instrument.

[12] Bansak et al. (2018) find that respondents can complete up to 30 tasks before the response quality of the conjoint design degrades.

Figure 3.1: Sample Choice Task

| | Candidate A | Candidate B |
|---|---|---|
| **Gender** | Male | Female |
| **Race** | Black | Black |
| **Position on the Economy** | Reduce regulations and restrictions | Reduce regulations and restrictions |
| **Position on Guns** | Everyone has the right to bear arms | Support commonsense gun safety measures |
| **Current Job** | State Representative | State Representative |
| **Position on Protests** | Deny permits to Black Lives Matter protesters | Support everyone's right to protest |
| **Position on Abortion** | Abortion should be safe, legal, and rare | Support universal access to abortions |
| **Position on Healthcare** | Support a free market health care system | Expand access to quality, affordable healthcare |
| **Party** | Democrat | Independent |
| **Position on Education** | No public funding for private education | No public funding for private education |
| **Position on Immigration** | Keep dangerous aliens out of our communities | Create a path to legal residence for immigrants without visas |
| **Position on the Environment** | Reduce greenhouse gas emissions | Reduce greenhouse gas emissions |
| **Position on COVID-19** | Oppose mass vaccinations and stay-at-home orders | Support mask mandates and limited reopening |

Task Instructions: "Consider a choice between candidates for the governor of your state. Some of these candidates will seem similar to actual candidates and others may seem unusual. That's okay. Just make the best choice about which candidate you would prefer. When making your choice, presume that both candidates are equally qualified to hold office in terms of character, temperament, and other personal and moral qualities."

rial candidates (N = 122).[13] Table B.1 in the Appendix lists all the issues that were included in at least five candidates' platforms and the number of candidates who referenced each issue. From the full sample of issues, the conjoint includes the policies that were included in at least ten campaign platforms and which had some level of ideological divergence. These are candidate positions on *Education*, the *Economy*, *Healthcare*, *Guns*, the *Environment*, *COVID-19*, and *Abortion*.[14] In addition to these frequently-mentioned issues, the conjoint includes the candidates' position on *Immigration*.[15]

For each attribute listed above, the conjoint tasks include the most common demographic categories and policy positions. For the substantive issues, I randomize both a liberal and conservative position, where appropriate. To maximize external validity, the text of each attribute value draws from the actual campaign platforms of gubernatorial candidates. Table 3.1 lists each of the attributes ascribed to the candidates as well as the values that each of these attributes can take.

### 3.3.2 Repression Treatments

The conjoint uses two repression treatments: (1) violence against protesters and (2) denial of protest permits. To maximize external validity, these treatments reflect repression that is both relevant to the 2020 U.S. gubernatorial elections and prevalent across democracies. Most importantly, repression of protesters was the only form of repression that was discussed by candidates in the 2020 U.S. gubernatorial race.[16] In addition to mirroring actual candidate positions, the repression treatments represent opposite poles on the spectrum

[13]Gubernatorial races took place in Delaware, Indiana, Missouri, Montana, New Hampshire, North Carolina, North Dakota, Utah, Vermont, Washington, and West Virginia in 2020. I collect platform information for all candidates with campaign websites. Appendix Section B.1 discusses in more detail.

[14]There was not ideological divergence for *Jobs* (all candidates promised more) and *Corruption* (all candidates promised less). Positions on *Taxes* were also eliminated, as they had similar positions to the *Economy*.

[15]Although immigration was not a top issue in the 2020 gubernatorial race, this is likely because no states that border Mexico had gubernatorial elections this year. I include this variable to avoid masking effects, since a candidate's stance on immigration may be correlated with their propensity to implement repressive policies (Bansak et al., 2019*a*).

[16]For instance, Dave Bosco (R, Delaware) stated that "when a protest turns to violence or even into looting and rioting then law enforcement needs to step up and regain control." Tim Eyman (R, Washington) argued that "people using violence to advance political ends should suffer the consequences." Doug Bergum (R, North Dakota) stated that he was proud to work with those who protested against the Dakota Access Pipeline. Cairo D'Almeida (D, Washington) argued that the governor should pay greater respect to the civil liberties of protesters.

Table 3.1: Attributes for Candidate Profiles in Conjoint Experiment

| **Attribute** | **Levels** |
|---|---|
| Ideology | Conservative |
| | Liberal |
| Race | White |
| | Black |
| | Latino |
| Gender | Male |
| | Female |
| Current job | Businessperson |
| | Incumbent |
| | State Representative |
| | Doctor |
| Position on Education | No public funding for private education |
| | Increase parent choice about education |
| | End standardized testing and Common Core |
| Position on the Economy | Create a strong economy for all, not just those at the top |
| | Reduce regulations and restrictions |
| Position on Healthcare | Expand access to affordable healthcare |
| | Support a free market healthcare system |
| Position on Guns | Support commonsense gun safety measures |
| | Everyone has the right to bear arms |
| Position on the Environment | Drastically reduce greenhouse gas emissions |
| | Avoid increased regulations |
| Position on COVID-19 | Support mask mandates and limited reopening |
| | Oppose mask mandates and stay-at-home orders |
| Position on Abortion | Proudly pro-life |
| | Abortion should be safe, legal, and rare |
| | Support universal access to abortions |
| Position on Immigration | Create a path to legal residence for residents without a visa |
| | Keep dangerous aliens out of our communities |
| Position on Protests | Support everyone's right to protest |
| | Deny permits to Black Lives Matter protesters |
| | Use tear gas and rubber bullets against Black Lives Matter protesters |
| | Deny permits to white nationalist protesters |
| | Use tear gas and rubber bullets against white nationalist protesters |

of repression types: violent/nonviolent and proactive/reactive (Davenport, 2007*a*). The hypotheses do not distinguish between the treatment effects for the different types of repression. However, I expect that individuals are likely to have greater aversion to more egregious human rights violations, as indicated by some past studies (Heinrich and Kobayashi, 2020; Putnam and Shapiro, 2017). I discuss the difference in treatment effects across nonviolent and violent violations in the analysis section.

In addition to providing external validity, the treatment conditions build on previous studies that examine respondents' reaction to restrictions of the right to protest (Edwards and Arnon, Forthcoming; Gibson, 1989, 2008). However, it is worth considering whether the results would apply to other repression types. On the one hand, voters may be especially likely to consider repression of protesters in their vote choice, given that this action violates a first amendment right. Also, because many respondents will likely have protested in the past or know someone who has, they may be less willing to ignore this rights violation in their vote choice since it may affect their personal well-being. At the same time, past research on political tolerance demonstrates that even liberal voters are often willing to restrict the first amendment rights, including the right to assemble, of disliked and threatening groups (Gibson and Gouws, 2003; Gibson, 2008). Further, repression of protesters represents a fairly mild type of rights violation, especially compared to physical integrity rights violations like extrajudicial killing and disappearances. From this perspective, voters may be less likely to consider repression of protesters in their vote choice compared to other repression types. Ultimately, more research will be necessary to understand how voters evaluate different kinds of repression.

### 3.3.3 Group Identification and Threat Perception

The experiment identifies two groups as repression targets: Black Lives Matter and white nationalists. Representing opposite ends of the ideological spectrum, these groups could be perceived as threatening for a range of reasons, including real/perceived use of violence, ideological values, and racial identification. Both groups protested around the 2020

elections across state lines.[17] The last section of Table 3.1 includes the text for the repression attributes for each of these groups. The control condition is that the candidate will "Support everyone's right to protest."

To measure respondents' identification with these groups, the survey asks respondents whether they or someone close to them (a close friend or family member) would consider themselves a member of the group. The question references close relations, rather than respondents themselves, to mitigate social desirability bias. Further, the theory anticipates that respondents will punish candidates who support repression of groups to which their friends and family belong. The survey measures threat perceptions by asking respondents to rank the threat they perceive from each group on a scale of 1 to 10, 1 being not threatening at all and 10 being the most threatening. For each respondent, the variable *Threatening* takes a value of "1" when the respondent codes the group as higher than 7 on the threat scale and "0" otherwise.[18] The survey does not specifically define the term *threat* for respondents: because threat perception may cover a range of dimensions, it allows respondents to evaluate this term for themselves and indicate their own *perceptions* about group threat.[19] In the Appendix, Figure B.4 shows the correlations between measures of in-group identification and threat perception. Section B.4.3 discusses the demographic correlates with group identification and threat perceptions, shown in Table B.9.

### 3.3.4 Outcome

Following each choice task, respondents must identify which candidate they would support if they had to choose only one of the profiles. The primary outcome measure, *Candidate Preferred* takes a value of "1" if the respondent prefers one candidate and "0" otherwise.

[17]Beneficially, there are similar groups of focus in a recent study by Edwards and Arnon (Forthcoming), who examine respondent reaction to repression of Black Lives Matter and white nationalists. By referencing the same groups, this experiment answers a lingering question from Edwards and Arnon's study: how does respondent reaction to repression correlate with voting behavior?

[18]The meter for threat perceptions starts at 5, so respondents must move the meter two spaces toward a more threatening position in order to be coded as perceiving the group to be threatening.

[19]It is possible that social desirability bias hindered respondents from answering honestly about their group identification and threat perception. To alleviate some concern, descriptive data from the sample reveals variation on both of these questions, as shown in Appendix Tables B.1, B.2, and B.3. Ultimately, this design choice captures only those with strong group identification and especially high levels of perceived threat. These are the individuals who should be most likely to consider repression in their vote choice.

The forced choice task is advantageous in that it requires respondents to make trade-offs between the different candidate attributes in the list, as in an actual election (Bansak et al., 2019*b*). In the robustness checks, I assess results for an alternative outcome variable, which asks respondents to rank their favorability toward each candidate on a scale of 1 to 10.

### 3.3.5 Treatment Assignment

The conjoint design is fully randomized. Since each respondent has an equal probability of seeing each of the repression treatments ($\frac{1}{5}$) and these treatments are randomly assigned, the effect of this variable is causally identified. While the causal identification of the effect of repression is straightforward, I use a block randomization scheme to identify the causal effects of group identification and threat perception. Measuring heterogeneous effects of these values based on raw scores risks subjecting the results to the usual problems of omitted variable bias (Acharya, Blackwell and Sen, 2018). Instead, I randomly assign the conjoint tasks for three blocks of respondents: those who identify as an in-group member with the target of repression (to test Hypothesis 1), those who perceive the target as a non-threatening out-group (to test Hypothesis 2), and those who perceive the target as a threatening out-group (to test Hypothesis 3). Because there are also two groups that are randomized in the repression treatments (Black Lives Matter and white nationalists), there are a total of nine blocks ($3^2$). The main results combine these categories for each group of interest. Since the probability of assignment to each treatment condition is uniform across blocks, combining the blocks does not bias estimates of the treatment effects (Gerber and Green, 2012, 76).[20]

Section B.3 discusses a range of balance checks to assess balance along in-group identification, threat perception, and a range of demographic features (listed in Appendix Section B.2). Specifically, Table B.2 shows that in-group identification and threat perception were not significant predictors of treatment status, indicating the effectiveness of the block randomization. Tables B.3, B.4, and B.5 indicate which demographic features are imbalanced

[20]One difficulty with this block randomization scheme is that it requires asking respondents about their threat perception before they complete the conjoint tasks. While this ordering avoids post-treatment bias, it may also heighten respondents' awareness of the threat posed by the groups of interest. To reduce the effects of priming, I ask respondents about their threat perceptions of groups that will not be included in the study (refugees, Islamic extremists, anti-mask advocates, animal rights activists, Communists, and illegal immigrants). Since these questions influence all voters, they will not bias the results of the hypothesis tests.

in the sub-samples. Tables B.6 and B.7 reveal that the results are robust when controlling for all respondent demographic features and the imbalanced demographic features.

### 3.3.6 Estimation

The estimation of treatment effects in a conjoint design is straightforward given that each attribute is randomized independently of all other attributes. Following the estimation strategy developed by Hainmueller, Hopkins and Yamamoto (2014), I use the Average Marginal Component Effects (AMCEs) of each attribute to identify the effect of each attribute on respondents' preferences for a given candidate. The AMCE represents the degree to which a given value of each attribute increases (or decreases) respondents' favorability to a candidate with reference to the baseline category, after averaging over all possible combinations of other candidate attributes (Bansak et al., 2019*b*; Hainmueller, Hopkins and Yamamoto, 2014; Leeper, Hobolt and Tilley, 2020). Given that attributes are randomly assigned, profiles with one value of an attribute will have the same distribution of other qualities compared to profiles with another value, on average. As such, the AMCE can be estimated using a regression of the primary outcome variable, *Candidate Preferred*, on each attribute value, while omitting a baseline condition for each attribute. The coefficients on each of the other attribute values represent the change in expected vote share for candidate's who adopt each value compared to the baseline condition (Bansak et al., 2020). Note that because each respondent completes ten conjoint tasks observations are not independent. Therefore, I cluster standard errors by respondent. Because the hypotheses concern treatment effects in three separate sub-groups, I estimate the conditional AMCE (Leeper, Hobolt and Tilley, 2020) in separate samples based on respondents' group identification and threat perception. Again, the block randomization scheme ensures that these estimates are causally identified.

It is important to take care in interpreting the AMCE. The AMCE does *not* reveal the effect of a particular policy on a candidates' likelihood of winning and does *not* imply that a majority of respondents prefer a candidate with one value of an attribute compared to another (Abramson, Kocak and Magazinnik, 2019). Rather, the AMCE takes into account both the direction and magnitude of respondent preferences and indicates the effect of changing

the attribute value from the baseline to the value of interest on the candidate's *expected vote share*, averaging over the randomization distribution of the profiles (Bansak et al., 2020). It is also important to note that the AMCE represents the causal effect of attribute values *with relation to* the (arbitrarily selected) baseline category. As such, the AMCE must be interpreted as a causal effect, not a descriptive representation of each respondent's level of support for a candidate contingent on that attribute (Leeper, Hobolt and Tilley, 2020).

To test each of my hypotheses, I regress the outcome variable (*Candidate Preferred*) on the four repression treatments in a sample of all candidates.[21] I use a logit link function given that the outcome variable is dichotomous and plot the results for ease of interpretation.[22] The omitted baseline category represents candidates who support the rights of everyone to protest. As such, the coefficients on the repression treatments represent the expected change in vote share when a particular candidate changes from a non-repressive position to supporting a particular forms of repression. The model takes the following form:

$$\begin{aligned} Y = \alpha + \beta_1 \text{ Deny BLM Permits} + \beta_2 \text{ Violence Against BLM+} \\ \beta_3 \text{ Deny White Nationalists Permits} + \beta_4 \text{ Violence Against White Nationalists} + e \end{aligned} \quad (1)$$

### 3.4 Results

To analyze the hypotheses, I run the above model in different samples based on respondents' group identification and threat perceptions. Table 3.2 shows the main results for each of the different sub-samples, with the gray boxes highlighting the coefficients used to test each of the hypotheses. To test the first hypothesis, I analyze the relationship between different forms of repression and a candidate's vote share for those who identify as in-group members with Black Lives Matter (Model 1) and white nationalists (Model 2). Here I expected that in-group members would be less likely to support a candidate who repressed in-group

[21] Since I have 750 respondents and each respondent evaluates 10 total pairs of candidates, my total sample for the conjoint analysis is 7,500. This full sample is divided into sub-samples for each hypothesis test as detailed below.

[22] The results are substantially similar if I use a linear model, as shown in Appendix Table B.11.

members, resulting in negative, significant coefficients for the gray boxes in Models 1 and 2. The results of Model 1 support my hypothesis: candidates who deny permits to Black Lives Matter protesters and who support the use of violence against protesters are likely to receive a lower vote share than those who support everyone's right to protest. Model 1 also reveals that respondents who identified BLM as an in-group were willing to punish violent repression of white nationalists, though not denial of protest permits to white nationalists. In other words, the propensity of BLM to punish repression expands beyond their own group, but only for the most egregious form of repression. This finding can perhaps be explained by the fact that Black Lives Matter protesters have been the frequent victims of police violence. As a result, perhaps these individuals are willing to punish candidates who use violence against any group, even a group to which they do not belong (and to which they likely have ideological opposition). Model 1 also reveals some support for the idea that voters are especially likely to oppose violent repression. While a linear hypothesis test shows that the coefficients for denial of permits to BLM and violence against BLM are not distinguishable from zero (p = .568), BLM in-group members are willing to punish violence even against out-groups.

Table 3.2: Effect of Repression on Vote Share

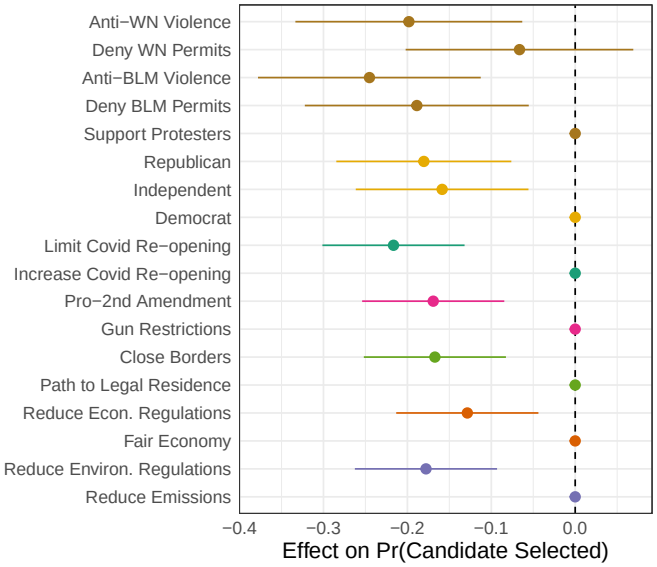
| | *In-Group* | | *Out-Group Non-Threatening* | | *Out-Group Threatening* | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | BLM | WN | BLM | WN | BLM | WN |
| Deny Permits to BLM | −0.19** | −0.02 | −0.10 | −0.03 | 0.11 | −0.18** |
| | (0.08) | (0.11) | (0.09) | (0.08) | (0.11) | (0.08) |
| Use Violence Against BLM | −0.23*** | −0.06 | −0.11 | −0.01 | 0.10 | −0.24*** |
| | (0.09) | (0.11) | (0.08) | (0.08) | (0.11) | (0.08) |
| Deny Permits to WN | −0.07 | −0.06 | −0.06 | −0.07 | −0.15 | −0.11 |
| | (0.08) | (0.12) | (0.09) | (0.08) | (0.11) | (0.08) |
| Use Violence Against WN | −0.19** | −0.12 | −0.05 | −0.10 | −0.10 | −0.14 |
| | (0.08) | (0.11) | (0.09) | (0.08) | (0.11) | (0.09) |
| Constant | 0.14*** | 0.05 | 0.07 | 0.04 | 0.01 | 0.14** |
| | (0.05) | (0.07) | (0.05) | (0.05) | (0.07) | (0.05) |
| Observations | 6,120 | 2,800 | 5,440 | 6,140 | 3,440 | 6,060 |

*Notes*: Dependent variable is whether a candidate was selected. Standard errors clustered by respondent. Sample is BLM in-group (M1), white nationalists in-group (M2), BLM out-group / non-threatening (M3), white nationalists out-group / non-threatening (M4), BLM out-group / threatening (M5), white nationalists out-group / threatening (M6). ***, **, * significant at .01, .05, .10, respectively. Gray boxes represent coefficients used for hypothesis tests.

The substantive effects from Model 1 are also interesting. Appendix Table B.8, replicates Table 3.2 while including controls for all other candidate attributes. These results allow for comparison between the treatments of interest (repression) and other influential candidate

attributes. Figure 3.2 shows the treatment effects for repression and the other significant predictors of vote share from Model 1. The figure demonstrates that the treatment effects were not only statistically significant, but also substantially important. A comparison of predicted probabilities reveals that, among BLM in-group members, candidates who repress BLM can expect their vote share to decrease by about 5.2% compared to candidates who do not repress BLM. Figure 3.2 reveals that the size of this treatment effect mirrors the influence of other salient candidate features, including partisanship, and candidates' positioning on COVID-19, the 2nd Amendment, border protections, the economy, and the environment. Perhaps even more interesting, among this sub-sample, the substantive effect of repression is *greater than* the effect of candidate attributes like race, gender, abortion, education, and healthcare policy. Overall, these results reveal that repression is both significantly and substantially important for BLM in-group members when deciding which candidate to support.

Figure 3.2: Substantive Effects from Table 3.2, Model 1




*Notes*: Coefficients for statistically significant coefficients in model with all control candidate attributes. 90% confidence intervals. Baseline categories set to 0. Sample is BLM in-group members (N = 6,120). Outcome variable: forced choice between candidates.

Model 2 shows an alternate test for the first hypothesis using the sample of those who identify white nationalists as an in-group. In contrast to the results for BLM in-group members, these results show that repression did not have a significant effect on candidates'

vote share among white nationalist in-group members. Although the coefficients are negative, they are small and indistinguishable from zero. There may be some concern that these results are under-powered, given that the sample of respondents who identified white nationalists as in-group members was the smallest of the group (N = 280 respondents). Section B.5 of the Appendix uses power analysis to evaluate this possibility. The results in Table B.10 reveal that, when setting power to 0.9 and alpha to 0.10, the required sample size to detect a significant effect of .05 would be 8,522, far above the observed sample here. However, the required sample size to detect a significant effect of 0.10 would be 2,099, below the observed sample size for Model 2. What these results indicate is that it is likely that the coefficient for violence against white nationalists is not significant, but it may be that the sample size is too small to detect a significant effect for the coefficient on denial of protest permits. Overall, it does seem that repression is not as salient in the vote choice of those who identify white nationalists as an in-group.

One explanation for these results could be that white nationalists have not been regularly targeted by repression in recent years, meaning that repression may not provide as great of a threat to the expected well-being of in-group members with white nationalists. Another issue is the difficulty of identifying those who truly identify as in-group members with white nationalists. In the Appendix, I plot the number of respondents who identify as in-group members with various groups (Figure B.1) and model the relationship between demographic indicators and in-group status (Table B.9). The results of this analysis demonstrate that a substantial number of respondents do identify white nationalists as an in-group, more than those who identify Communists, Antifa, and Islamic Extremists as in-groups. However, some the determinants of in-group identification are surprising. In particular, Republicans were significantly *less* likely to identify white nationalists as an in-group than were Democrats. This could be a true relationship, or it could reflect a tendency for Democrats to be more likely to apply the label of white nationalist to individuals with racist views, even among friends and family. If this logic holds, it may be that this sample does not include all of those who are really in-group members with white nationalists and/or the sample may include individuals who identify white nationalists as friends and family, but also have strong ideological opposition to members of this group. Either possibility would dilute the theoret-

ical relevance of this sample, perhaps explaining the insignificant coefficients for repression of in-group members in Model 2.

Models 3 and 4 in Table 3.2 show the results for my second hypothesis. Hypothesis 2 predicted that individuals who identify the repression targets as non-threatening out-groups will not consider repression in their vote choice. Across both models there is strong support for this hypothesis. Within the samples who perceive Black Lives Matter and white nationalists to be non-threatening out-groups (Models 3 and 4), repression has no significant treatment effect on a candidates' vote share. Instead, other candidate features have a stronger effect on the selection of candidates, as predicted in my second hypothesis and theoretical framework. Table B.8 shows that individuals in these sub-samples put a much greater weight on factors like party identification, COVID re-opening policies, and economic policy rather than repression. The theorized mechanism – that these policies have a greater influence on individuals' well-being in these samples – can certainly explain the insignificant treatment effects for repression. Further, power analysis in Table B.10 reveals that the results in Model 3 are not the result of a low sample size, as the smallest relevant coefficient of 0.09 would be detected in a sample as small as 2,601, smaller than the observed sample of 5,060. In Model 4 however, both coefficients would be too small to be detected in a sample of 5,780. As a result, I take Models 3 and 4 to lend tentative support for the second hypothesis.

Finally, Models 5 and 6 test the third hypothesis, which predicts that candidates who repress threatening groups will reap electoral benefits. Model 5 shows the results for those who see Black Lives Matter as a threatening out-group. Though both of these coefficients are positive, as expected, they are statistically indistinguishable from zero and relatively small. In Model 6, which samples respondents who perceive white nationalists to be a threatening out-group, the results are negative but, again, statistically insignificant. Power analysis in Table B.10 shows that the sample in Model 5 might be too small to detect a significant effect, but that the sample in Model 6 is large enough to detect significant effects if they were to exist. Overall, it seems that even those who view the repression targets as threatening are more likely to react to repression with indifference than support. One caveat to keep in mind while interpreting these results for Hypothesis 3 is that there are certainly groups that

represent a more immediate threat to national security than do white nationalists and BLM. As a result, those who see these groups as threatening may not be crossing the threshold to support repression in the form of restrictions of a basic political freedom in the right to protest. However, perhaps voters would reward repression of even more threatening groups, like terrorist or rebel organizations. These results do not suggest that voters will *never* reward any kind of repression. However, support for the right to protest in the United States seems to be especially sticky, as even sub-samples of voters who perceive these groups to be threatening will not cross the line to rewarding repression.

Overall, the experimental results lend the strongest support to Hypotheses 1 and 2, while demonstrating interesting variation across the different groups of interest. The main take-away is that Black Lives Matter in-group members are willing to punish in-group repression as well as violent repression against out-groups. Among most other sub-samples, however, voters meet repression with indifference, and candidates' vote share is much more likely to be influenced by their demographic features and positions on other policy issues other than repression. Even those who perceive the targets of repression to be threatening tend to privilege other campaign issues over repression. I interpret the results as reflecting the current political context, in which Black Lives Matter has been a frequent target of repression and, as a result, the threat of repression is likely to be especially salient to the individuals who identify as an in-group with BLM. These individuals will punish repression, but, for others, other policy issues have a greater impact on their well-being and their vote.

### 3.4.1 Robustness Checks

This section outlines several empirical strategies to assess the robustness of the main hypothesis tests.[23] First, Tables B.12 - B.20 assess the possibility of ordering effects by replicating the main results while limiting the sample to only the first task, only the first two tasks, only the first three tasks, etc. It is important to note a trade-off with this analysis: while the results in the earlier tasks may exhibit less decision fatigue, they are also smaller sample sizes. Ultimately, the main significant findings replicate for the first 8 tasks and the

[23]Appendix Section B.6 provides a more in-depth discussion of these results.

first 9 tasks, while the negative relationship between violence against BLM and lower vote share among BLM in-group members in all but three of the sub-samples. The results for Hypothesis 2 are almost always insignificant, as in the main hypothesis tests. Interestingly, in the first two tasks there is a positive relationship between viewing BLM as a threatening out-group and rewarding repression, perhaps because the social desirability bias against rewarding repression had not solidified in the earlier tasks as respondents are less familiar with the treatment conditions.

Tables B.21 - B.24 replicate the tests of the second and third hypotheses with alternative thresholds for threat perception. The cut-off for threat perception at greater than or equal to 7 in the main analysis is somewhat arbitrary, and these results allow for evaluation as to whether this decision influenced the results. These results should be treated as suggestive, given that the block randomization scheme used 7 as the cut-off. The results for Hypothesis 2 are consistently null across these alternative thresholds, as expected, except for the last cut-off at threatening = 10, where those who view white nationalists as a non-threatening out-group punished violence against white nationalists. Likely these results are driven by the BLM in-group members, who would show up in this group that encompasses almost the full sample of respondents. For the tests of Hypothesis 3, there is never a positive effect of repression on vote share even with the alternative cut-off points. There are some models where those who view white nationalists as a threatening out-group punish various kinds of repression. Likely these results are also driven by the incorporation of BLM in-group members into the larger samples.

Finally, Table B.25 replicates the main hypothesis tests using an alternative outcome variables in which respondents must rank the candidates rather than choose between them. I use linear models to analyze the effect of repression on candidate rankings from 1 to 10, where 1 means the respondent definitely *would not* vote for the candidate and 10 means the respondent definitely *would* vote for the candidate. These results prove interesting because many more of the repression coefficients are negative and statistically significant. What these results reveal is that the forced choice conjoint task achieved its purpose in eliciting tough decisions from respondents about how to incorporate repression into their selection of candidates. In the ranking task, which did not involve a trade-off, candidates

received a lower vote share for at least one type of repression in nearly all respondent subcategories. However, when forced to make a trade-off, the main hypothesis tests reveal that many respondents did not incorporate repression into their evaluation of candidates. The comparison between these two measures reflects the importance of using a conjoint to assess to evaluate the role of repression in vote choice. In the abstract, most respondents viewed repression as an undesirable quality. However, when forced to make decisions about this quality vis-a-vis other candidate features, all but BLM in-group members made their choice around attributes other than repression.

### 3.4.2 Causal Mechanisms

Appendix Section B.7 provides analysis of the causal mechanisms that support the logic of the hypotheses. First, this section evaluates whether respondents who identify as an in-group with a repression target and/or consider one of the targets to be threatening are more likely to incorporate repression into their vote choice. Specifically, I ask respondents to mark which attributes they consider when selecting a candidate, to specify the importance of each of the repression attributes, and to answer an open-ended question indicating whether there were any attributes that disqualified a candidate from office. Descriptive data from these questions (Figures B.5 and Figure B.6) shows that repression ranks similarly to other salient issues in the 2020 election cycle, like abortion and COVID-19. Table B.26 regresses these measures on respondent's demographic features, their in-group identification, and their threat perceptions of the repression targets. Compared to the baseline category of non-threatening out-group, those who perceived BLM to be a non-threatening in-group and a threatening out-group were significantly more likely to consider repression and rank repression as having greater importance in their vote choice, as expected. In contrast, those who identified white nationalists as an in-group were significantly less likely to consider repression, but these results may reflect some of the difficulty with the measurement for in-group identification for white nationalists. Finally, there is a weakly significant positive relationship between those who view white nationalists as a threatening out-group and consideration of repression and likelihood that repression disqualified the candidate. In all,

there is some evidence across these models that, at least with relation to Black Lives Matter, in-group identification and threat perception increase the likelihood that a respondent will incorporate repression into their vote choice.

In another set of questions, I ask respondents to identify how they typically choose between candidates. In one question, I ask respondents whether their main consideration when selecting candidates is (1) how a candidates' policies affect their own well-being, (2) how the candidates' policies affect the well-being of their friends and family, (3) how the candidates' policies affect the country as a whole, (4) the candidates' party, or (5) the candidates' position on a specific policy. I also ask respondents to rank these categories from most important to least important. The theory suggests that voters tend to make decisions based on how a particular policy affects their own well-being or the well-being of other in-group members. While not directly evaluating this dynamic, the descriptive evidence in Figures B.7 and B.8 suggest that a substantial number of voters do privilege the effect that policies have on in-group members when making their vote choice, even beyond considerations of party identification and specific issues.

For a third and final set of questions to evaluate causal mechanisms, I ask respondents about their attitudes toward the civil liberties - security trade-off.[24] Table B.27 shows the relationship between respondents' group identification, threat perception, demographic features and the relative importance of civil liberties versus security. In these models, positive coefficients indicate relatively higher support for civil liberties relative to the omitted category, and negative coefficients represent relatively higher support for security. I expected that the coefficients for in-group identification would be positive and statistically significant: those who identify with one of the repression targets should place a relatively higher weight on civil liberties than security compared to out-group members. In contrast, I expected that the coefficients for threat perception would negative and statistically significant, signalling that those who view one of the repression targets as threatening will be more likely to support security over civil liberties.

The results in Table B.27 do not reflect my expectations. First, there is not significant relationships between the measures of threat perception and the outcomes. Second, the

[24]Figure B.9 shows descriptive statistics for these questions.

results show that those who identify BLM and white nationalists as an in-group are significantly *less* likely to mark a response that privileges protection of civil liberties. Rather, respondents who are in-group members with repression targets are more likely to agree that the government should protect citizens from threats, even if it means violating civil liberties, and more likely to agree that sometimes the government must violate civil liberties in order to maintain security. One way to make sense of these relationships is that those who identified as an in-group with one repression target may feel threatened by the other target. As a result, they may have had the other group in mind when answering the questions, particularly if they were unlikely to view their own group as a threat. For instance, someone who identified white nationalists as an in-group may not see their own group as threatening, but they may be more likely to see BLM as threatening. In that case, they may answer the questions with BLM in mind and believe that it is important to protect from threats, even if it means violating the civil liberties of *another* group. The last finding is that those who view BLM as an in-group were more likely to agree that it is important that the rights of those with unpopular views be protected. Perhaps in the question that specifically measured protests from unpopular views, BLM in-group members did respond to the question with their own group in mind.

### 3.4.3 The Influence of Party Identification

Finally, Appendix Section B.8 examines the relationship between party identification and the role of repression in vote choice. This paper's theory focuses on group identification and threat perception as primary drivers for the incorporation of repression into vote choice. However, ideology and – in the United States especially – party identification, are also likely to play a role in how voters evaluate repression at the ballot box. Many past studies indicate that voters with liberal values are more likely to prioritize human rights protections over security (Davis and Silver, 2004; Davis, 2007; Finkelstein et al., 2017; Jenkins-Smith and Herron, 2009; McFarland and Mathews, 2005) and these values may also influence how repressive policies influence parties' performance in elections (Aksoy, 2018; Cordell, 2021). As such, it is important to evaluate the extent to which partisanship may confound the results

of the main hypothesis tests and/or provide an additional explanation for the relationship between repression and vote share.

The first takeaway from this analysis is that in-group identification, threat perception, and party identification *are* correlated. However, these concepts are not perfectly aligned. Figure B.10 shows the correlations between these concepts and Table B.9 shows regressions for in-group identification and threat perceptions on respondents' demographic features, including party identification and ideology. For the most part, these results reflect conventional wisdom that, compared to Democrats, Republicans are more likely to view BLM as threatening, less likely to identify BLM as an in-group and less likely to view white nationalists as threatening.[25] These results suggest that the relationship between in-group identification and threat perception in the main hypothesis tests is not *purely* driven by party identification, as there is variation in group membership and threat perception in each of the partisan sub-samples.

Given the correlation between party identification and the concepts of interest, however, it is important to identify whether political party is a confounding variable. The balance checks in B.3 rule out this possibility. In the sub-samples, treatment assignment is balanced for political party membership in all but three instances.[26] Yet, even when controlling for political party and ideology in all models (Table B.6) and when these variables are imbalanced (Table B.7), the results for the hypothesis tests are robust.

While party identification does not confound the results, many readers may still be interested to know whether individuals who belong to different parties vary in their propensity to punish repression. Table B.28 evaluates this possibility by analyzing the influence of repression on vote share in three partisan sub-samples: Democrats, Republicans, and Non-Partisans. Interestingly, the results do somewhat align with the hypothesis tests: among

[25]One surprising finding from this analysis is that, compared to Democrats, Republicans are significantly *less* likely to identify white nationalists as an in-group. As mentioned previously, this finding may stem from variation in each party's propensity to label the same attitudes and behaviors as exhibiting white nationalist views.

[26]In the BLM in-group sample, Independents were more likely to receive the treatment where the candidate denies white nationalists permits; in the WN in-group sample, Republicans were more likely to receive the treatment where the candidate denies BLM permits; and in the sample of respondents who view BLM as a non-threatening out0group, Independents were more likely to receive the treatment where the candidate supported restricting permits for BLM protesters.

Democrats, candidates receive a significantly lower vote share for repression of Black Lives Matter as well as violent repression of white nationalists. The reverse holds in the Republican sample: candidates are only punished for denial of protest permits to white nationalists, though this relationship is only significant at the .10 level. Repression is not punished among non-partisans. Overall, these results suggest that Democrats are more likely to punish repression, but this tendency varies by target and repression type.

Finally, it is worthwhile to consider whether the main results are driven by the members of one particular party. To assess this possibility, Tables B.29, B.30, and B.31 replicate the main hypothesis tests for sub-samples of Democrats, Republicans, and non-partisans. These results should be treated as suggestive, given that the treatment is not randomized along party lines and the sub-samples are quite small. Still, they provide some interesting insight. Among Democrats, the results are almost identical to the main hypothesis tests, though even those who identify BLM and white nationalists as non-threatening out-groups punish some repression. Among Republicans, even BLM in-group members are not willing to punish repression, but white nationalist in-group members punish violence against white nationalists, and those who view white nationalists as threatening out-groups punish denial of permits to white nationalists. Among non-partisans the results are somewhat convoluted given the heterogeneous nature of the sample and small sample sizes. Here, in-group members with white nationalists *reward* denial of permits to white nationalists, though this sample includes only 16 respondents.

The main takeaway from these analyses is that the primary significant results in the main hypothesis tests, in which BLM in-group members punish repression of BLM, seems to be driven by Democratic respondents. At the same time, there *is* a unique effect of in-group identification, above and beyond the role of party identification, as demonstrated in the models with controls to adjust for imbalance along this variable. There is also some interesting heterogeneity across the parties, as the results suggest that Democrats are not necessarily universally more supportive of rights protections. In the in-group, partisan sub-samples, for instance, only Democrats who identified as an in-group with BLM punished repression of BLM, while only Republicans who identified as an in-group with BLM punish violent repression of white nationalists. Across the sub-samples, Democrats seemed to be

more willing to punish repression of BLM, while Republicans seemed to be more willing to punish repression of white nationalists, suggesting that party identification does seem to indicate some group-level affiliation. Ultimately, these results do not undermine the strong influence of in-group identification with BLM as shaping incorporation of repression into vote choice. However, they do round out our understanding of the role of party identification in showing that both in-group identity and party affiliation likely play a role in influencing how a particular individual evaluates repression at the polls.

## 3.5 Conclusion

This paper provides novel experimental evidence to evaluate a central question in the literature linking democratic institutions and repression: how does repression shape candidates' performance in elections? The assumption that voters can and will punish repression in democracies supports a range of studies arguing for the deterrent effect of elections on repression. These studies surmise that, if voters are willing to throw repressive leaders out of office, then leaders accountable to voters will be deterred from repression (Cingranelli and Filippov, 2010; Gurr, 1986; Richards, 1999; Richards and Gelleny, 2007). My results tell a different story. Using a unique conjoint design that forces respondents to make trade-offs between repression and other salient campaign issues – as occurs in actual elections – I find that only one group of voters is likely to punish repression. In these simulated U.S. elections, candidates only suffered electoral losses among Black Lives Matter in-group members, and only for violent repression and targeted repression of BLM. In contrast, among most other sub-groups of the electorate, candidates fared just as well when they advocated targeted repression as when they supported the right to protest for all citizens. The implication of this finding is that elections are likely to provide ineffective constraints against many types of repression, for many candidates. In particular, for candidates who were not accountable to BLM, repression had little impact on their expected vote share.

These results have concerning implications for democratic theory. In the context of this experimental setting, elections are only likely to inhibit leaders from repressing if they are

beholden to the interests of certain repression targets, in this case, Black Lives Matter. For candidates who do not count this group as part of their winning coalition, however, repression likely carries few costs. Optimistically, no candidates are rewarded for repression across the different sub-groups, even among those who view the repression target as threatening. However, the indifference of most voters to repression can allow many candidates to support repression with seeming impunity. These findings are important both for existing research and for their policy implications. For one, this conjoint design allows me to uncover the tendency toward indifference regarding repression among voters who must evaluate repression in comparison to other issues at the ballot, providing important insight into the relationship between political attitudes and behavior. The findings reveal that many respondents who oppose human rights in the abstract (as demonstrated in the rank-choice task) still refrain from incorporating repression into their vote (as demonstrated in the forced-choice task). As a result, researchers should take care to extrapolate abstract aversion to human rights violations to real-life voting behavior. From a policy perspective, my findings suggest that strengthening norms against repression and horizontal institutions to raise the costs of repression are crucial endeavors to hold leaders to account for rights violations.

Of course, these results are limited to respondents' behavior in the experimental setting of one country at a particular political moment. Repression, particularly of minority groups, was a salient topic of conversation throughout the 2020 election cycle in the United States. As a result, one might expect voters to be particularly attune to this issue and particularly willing to incorporate repression into their vote choice. If anything, given the increased attention to the protest-repression nexus at this moment in American politics, I expected the treatment effects to be stronger-than-average. Yet even in this heightened political context, only Black Lives Matter in-group members incorporated repression into their vote. The results of this somewhat easy test should invite further caution about the propensity of voters to consider repression when voting. Beyond this case, more research is certainly needed to assess whether these treatment effects travel to other democracies. On the one hand, the United States has strong democratic institutions and strong norms in favor of protecting civil liberties. On the other, repression of minority communities in the United States has a long history, and voters may be more reticent to punish repression of out-groups in this political

context. Ultimately there is a lack of cross-national research to assess variation in baseline support and opposition to physical integrity rights violations across countries, though some recent work is making progress on this front (Clay et al., 2018). Future studies should further analyze the micro-foundations of the domestic democratic peace by identifying the extent to which voters in other countries incorporate repression into their vote choice.

## 4.0 Paper 3: Variation in the Efficacy of International Advocacy

Since the 1970s, transnational advocacy networks (TANs) have engaged in far-reaching and costly public information campaigns to name and shame leaders who perpetrate human rights abuses (Clark, 2001).[1] At the root of these efforts is the desire to increase government respect for human rights. In spite of this clear goal, there is mixed evidence for the effectiveness of international advocacy. Some studies find that naming and shaming reduces certain forms of human rights rights violations (Murdie and Davis, 2012), like government killings (DeMeritt, 2012) and restrictions of political rights (Hafner-Burton, 2008). Other research, however, finds that naming and shaming is correlated with increased physical integrity rights violations (Hafner-Burton, 2008) and results in a substitution effect, in which states reduce some kinds of violations while increasing others (DeMeritt and Conrad, 2019). Naming and shaming may also result in a backlash, from audiences who view international advocacy as encroaching on domestic values and traditions (Snyder, 2020; Terman, 2017). Still other studies find that naming and shaming campaigns can improve human rights, but only among certain types of countries (Franklin, 2008; Hendrix and Wong, 2013).

In part, the mixed findings in this extant research stem from a lack of consensus about the mechanisms through which international advocacy decreases human rights violations, each of which may function effectively only under particular circumstances. Naming and shaming may directly pressure governments to change their behavior, but it can also shift third party behavior or the preferences of domestic audiences. Each of these pathways may have different effects on leaders' overall respect for human rights. In one recent study, for instance, Allendoerfer, Murdie and Welch (2020) find that naming and shaming decreases

[1] *Transnational advocacy networks* are "forms of organization characterized by voluntary, reciprocal, and horizontal patterns of communication and exchange. [...] They are organized to promote causes, principled ideas, and norms and they often involve individuals advocating policy changes that cannot be easily linked to a rationalist understanding of their "interests."" (Keck and Sikkink, 1998, 8-9). *Shaming* is when HROs "use information about human rights abuses in the popular media to pressure or "shame" a state regarding its human rights record" (Davis, Murdie and Steinmetz, 2012, 204). The experimental design for this study approved by the University of Pittsburgh's Institutional Review Board (Study 21060195) and pre-registered with the Open Science Foundation (`https://osf.io/rbnya/?view_only=b281bb47bf20457aa253754bfaf45e95`) before data collection. I acknowledge financial support from the School of Arts and Sciences at the University of Pittsburgh.

human rights violations in general, but it also spurs third party intervention that can have a deleterious effect on human rights. As a result of these divergent pressures, recent research has begun to separately evaluating the myriad links in the causal chain linking international advocacy to human rights practices. Such efforts provide clarity about how exactly naming and shaming campaigns function to change leaders' behavior.

One causal pathway that has gained increased attention in recent years is the effect of naming and shaming on public opinion. A core function of TANs is to raise awareness among domestic and international audiences about the presence of human rights abuses and change the frames through which the public evaluates leaders' behavior. As Keck and Sikkink (1998) explain, TANs "[frame] issues to make them comprehensible to target audiences," "attract attention and encourage action," "bring new ideas, norms, and discourses into policy debates," and serve as alternative "sources of information and testimony" (2-3). By highlighting the importance of human rights, TANs can mobilize the public to either pressure their own government to abandon rights abuses (in the case of domestic audiences) or pressure foreign states to change behavior (in the case of international audiences).[2] Several recent studies have evaluated whether international naming and shaming is effective at initiating this bottom-up process of accountability. Ausderan (2014) and Davis, Murdie and Steinmetz (2012), for instance, find that international naming and shaming has a positive effect on public perceptions that human rights abuses are occurring. Several other studies examine variation in effectiveness of advocacy campaigns based on different framing strategies (Arves and Braun, 2019; Haines et al., 2020; McEntire, Leiby and Krain, 2015, 2017) and the interaction between human rights campaigns and government counterarguments (Bracic and Murdie, 2020; Williamson and Malik, 2020).

What is lacking from these empirical accounts, however, is consideration of variation in how different domestic audiences are likely to perceive human rights campaigns. Existing theoretical work recognizes that individuals react differently to accusations about human rights abuses (Snyder, 2020; Terman, 2017; Risse, Ropp and Sikkink, 1999). While some

[2]Certainly, international HROs are not the only organizations that can initiate this process. Domestic media sources can also provide information about repression and increase the salience of repression in a given election. However, international HROs – as specialists in human rights abuses around the world – will be especially effective at shifting public opinion about the justifiability of repression.

individuals update their beliefs about human rights as a result of informational campaigns, others may double down in their support for the repressive leader in backlash to perceived international encroachment. Still others may be persuaded by leaders' counterarguments that repression is necessary to protect citizens from ongoing threats (Bracic and Murdie, 2020; Williamson and Malik, 2020; Risse, Ropp and Sikkink, 1999). Ultimately, research on the effectiveness of naming and shaming requires a cohesive theoretical account for these diverse responses. Tests for the average treatment effect of naming and shaming overlook the range of individual reactions to new information, masking possible heterogeneous effects and raising issues for human rights activists who rely on scholarly work to determine where their limited resources will yield the most benefit.

This paper presents a framework to account for heterogeneity in individual responses to international advocacy. Individuals filter new information about human rights based on their preconceptions about and trust in the actors involved in the human rights violations and advocacy campaign. During advocacy campaigns, individuals hear two competing narratives: that of the leader, who typically justifies repression with appeals to domestic security, and that of the human rights advocacy organization, which makes personalist appeals to humanize the victims of repression (Bracic and Murdie, 2020; McEntire, Leiby and Krain, 2015). Individuals will process this dialogue differently depending on their baseline levels of trust in the competing sources of information. Specifically, individuals' propensity to update their opinions about human rights – and their support for repressive leaders – varies based on their preconceptions about the victims of human rights abuses, their pre-existing support for the leader, and their trust in the international community. First, individuals who view the victim of human rights violations as threatening are less likely to be persuaded by naming and shaming than those who do not. Those who feel threatened by repression victims are more likely to be persuaded by the leader's appeals to domestic security and support restrictions on the mobilization potential of these groups. Second, individuals who are strong supporters of the leader are more likely to sympathize with the leader's excuses for their behavior and less likely to update their beliefs following international advocacy. Finally, individuals with low levels of trust in international organizations will be less susceptible to cues from the international community about human rights.

I use a vignette experiment to test the argument about the heterogeneity of effectiveness of human rights campaigns. The experiment, conducted with a representative sample of U.S. adults, first describes a hypothetical leader's pattern of human rights abuses (violence against protesters) and the leader's justifications for this behavior (such efforts are necessary to increase domestic security). The experiment then randomly assigns further information about a naming and shaming campaign from Amnesty International that criticizes the leader for his behavior. This campaign features a personalist appeal from a victim of the human rights abuse, given that such forms of advocacy have been shown to be the most effective at eliciting attitudinal change (McEntire, Leiby and Krain, 2015). The experiment uses a block randomization scheme to assess the causal effect of this treatment among three subcategories of respondents: those who perceive the victim to be threatening, those who are likely to trust the leader, and those who are likely to be skeptical of appeals from the international community. This design allows for internal validity in assessing the effect of international advocacy among these different populations, revealing whether individuals react differently to human rights advocacy based on their preconceptions about the actors. The results show that individuals are unlikely to shift their generalized attitudes about domestic human rights protections, regardless of their orientation to the victims, leaders, and human rights advocacy organizations. However, there is a positive effect of naming and shaming on opposition to repressive *leaders* among those who do not perceive victims to be threatening and among those who do not share a party with the repressive leader.

This research makes several contributions to the existing literature on naming and shaming, public opinion, and human rights. First, this research builds on a growing body of literature evaluating one critical link in the pathway between transnational advocacy and human rights protections: public opinion. While existing research has empirically evaluated whether naming and shaming shifts individual attitudes and whether certain messages are more effective than others, it has not accounted for the variation in whether and how individuals update their beliefs as a result of transnational advocacy. This paper considers three important factors that should moderate the influence naming and shaming campaigns on shifts in public opinion: an individual's relationship to the victim of repression, the target of the human rights campaign, and source of the information. In so doing, it integrates diverse

accounts about the effectiveness of international cues in order to provide a cohesive framework for predicting how individuals will respond to new information about human rights. The survey experiment then isolates the causal effect of naming and shaming within these different subgroups, providing critical insight for human rights organizations (HROs) about which individuals will be most likely to respond favorably to their messages and which individuals might have a negative reaction. Overall, this theory and findings helps scholars and activists better understand the process by which international HROs can successfully reduce state violations of human rights.

The paper proceeds as follows. In the first section, I review two major theoretical frameworks that link transnational advocacy to changes in human rights practices: the spiral model and the boomerang model. This section also highlights the role of information and public opinion in each of these models and reviews existing work that has evaluated this causal link. The next section presents an original argument to explain heterogeneity in individual responses to international advocacy. The following sections present the experimental design, results, and conclusion.

## 4.1 Transnational Advocacy and Public Opinion

There are many ways through which non-governmental HROs can pressure rights abusing governments to change their behavior. Two primary theoretical models categorize these processes. First, in the *boomerang model* (Keck and Sikkink, 1998, 12-13), non-governmental organizations (NGOs) seek to pressure their own states to stop rights abuses, but lack political and judicial forums to voice their concerns. In light of these domestic blockages, NGOs publicize government rights abuses through campaigns that raise awareness in a broad transnational advocacy network. Such campaigns share information about state behavior, categorize rights abuses, and provide testimonials from victims to humanize patterns of abuse. If successful, human rights campaigns mobilize support from foreign NGOs, governments, and international organizations, who in turn can put pressure on the target state. This pressure can take the form of appeals to international norms, operating from a logic of

appropriateness, or material incentives, such as promises of aid or threats to impose economic sanctions. Overall, the success of these appeals depends on the presence and receptiveness of international actors, who must place a high enough value on rights protections to mobilize against the target state.

In the *spiral model*, Risse, Ropp and Sikkink (1999, 20) add theoretical nuance to the boomerang model, specifying five phases through which states move toward norm implementation. The first phase begins with domestic repression and the activation of a network of domestic activists, who share information with the international community about rights abuses. In the second phase, international actors recognize these abuses as part of the international human rights agenda. Following this spike in attention, states typically deny that rights violations are taking place, but sustained campaigning from domestic and international activists can also shift the attitudes of the public and policymakers in favor of human rights protections. During the third phase, states make tactical concessions in order to pacify domestic and international opposition. While material concerns typically drive changes at this stage, the fourth stage involves larger-scale changes, such as signing international human rights conventions or institutionalizing human rights into domestic law or constitutions. Then, human rights norms begin to take root as normative standards, even if state behavior does not always reach such goals. Finally, respect for international human rights norms becomes a habitual practice in the fifth phase, and domestic rule of law enforces these internalized values.

Both the boomerang and spiral models highlight multiple causal pathways through which different actors can pressure states to respect human rights. Notably, the first step in each of these frameworks is the provision of relevant and actionable information about rights abuses from domestic HROs. Without information about what the target state has done, there is no way to initiate the processes that lead to changes in state behavior (McEntire, Leiby and Krain, 2015, 408). As Davis, Murdie and Steinmetz (2012) explain, "The theoretical literature [on naming and shaming] hinges on first convincing an international and domestic audience that human rights abuses are occurring" (208). Given the pivotal role of information in both models, it is important to recognize that different actors and demographics are likely to react differently to coverage of human rights abuses. While information provi-

sion is the starting point across all causal pathways, the intended audience of informational campaigns varies. In both major theories of transnational advocacy, the primary target of informational campaigns is international elites, such as leaders of HROs, foreign governments, and international organizations. Once members of the international community recognize the presence of rights abuses, they have many tools at their disposal to put pressure on abusive governments.[3] However, the spiral model also highlights the role of foreign and domestic audiences in initiating long-term behavioral changes. Information campaigns can spur domestic mobilization, which facilitates pressure from below for leaders to respect human rights. In democracies, the public has many avenues to pressure the government: through elections, public protest, the formation of new political parties, etc. However, domestic audiences can also threaten leaders' survival in autocracies (Bueno De Mesquita et al., 2003; Kadivar, 2018), and widespread protest is common in many authoritarian countries, particularly hybrid regimes (Trejo, 2014; Weidmann and Rød, 2019). International advocacy can also generate international outrage, which increases the pressure on foreign governments to take action against other states who have abused human rights.

The second phase of the spiral model highlights these processes by emphasizing how activists can use information to shift the opinion of both policymakers and the public. A crucial aspect of this precarious phase is that activists must mount a resistance that is large enough to keep an issue on the international agenda and maintain the focus of the target government. One way to do this is by enraging a domestic audience that may have been unaware or indifferent to patterns of rights abuses. By successfully reframing and highlighting patterns of rights abuses, activists can raise the pressure from domestic audiences. While many rights abusing states do not have venues for domestic opposition to voice its concern, even autocratic leaders are susceptible to pressure from widespread protest or disillusionment from members of their winning coalition (Bueno De Mesquita et al., 2003; Kadivar, 2018; Trejo, 2014; Weidmann and Rød, 2019). In rights abusing states with regular elections, dissatisfaction from the electorate can also put pressure on leaders to shift tactics for fear of electoral sanctions. In a similar process, international audiences can put bottom-up pressure

[3]See, for instance Barry, Clay and Flynn (2013); Dietrich and Murdie (2017); Esarey and DeMeritt (2017); Murdie and Peksen (2013, 2014); Peksen (2009) and Wood (2008).

on their own leaders to hold foreign governments accountable through threats of electoral sanctions or widespread protest. For instance, international audiences are more supportive of foreign aid to countries that do not abuse human rights (Allendoerfer, 2017).

There is some recent research on the causal pathway linking HRO activism to shifts in domestic and international public opinion. A first branch of empirical work on this issue considers whether sharing information about human rights abuses increases the public's awareness that rights abuses are occurring. Given that rights abuses often take place in obscurity, and repressive leaders may shield the public from their abusive behavior, a first step in initiating bottom-up pressure is to inform the public of the presence of human rights violations. Davis, Murdie and Steinmetz (2012), for instance, use survey data to show a negative correlation between naming and shaming and domestic perceptions about government respect for human rights. In a similar study, Ausderan (2014) uses both experimental and observational analysis to evaluate the link between human rights campaigns and awareness of human rights abuses. Observational analysis in this research shows a positive link between naming and shaming and perceptions of human rights abuses, while a survey experiment randomly varying the presence of naming and shaming shows no statistically significant relationship between information provision and perceptions about repression. While this initial finding casts doubt on the effectiveness of naming and shaming, it evaluates public reaction to only one kind of human rights abuse using a convenience sample and does not consider whether certain individuals may be more susceptible to international advocacy than others. Also, it evaluates *awareness* of human rights abuses, which may already be high preceding exposure to the treatment, rather than *support* for human rights.

Taking this agenda a step further, recent survey experiments have evaluated whether certain types of information are particularly effective at garnering public support for human rights. McEntire, Leiby and Krain (2015), for instance, collect information about all of Amnesty International's campaigns and create a typology to categorize the organization's messaging into informational, personal, and motivational frames. Using a survey experiment, the authors show that personalist frames are the most effective at initiating attitudinal and behavioral changes against sleep deprivation torture techniques. In a follow-up study, McEntire, Leiby and Krain (2017) show that personalist frames used in conjunction with

the other two frames are just as effective as personalist frames used alone. Adding nuance to these findings, Bracic and Murdie (2020) also show that personalist campaigns are effective, but that respondents are less receptive to these efforts when the government has labeled the victim of human rights abuse a terrorist. Haines et al. (2020) find that personalist campaigns can increase sympathy for victims while also increasing support for retributive violence. Finally, Williamson and Malik (2020) use a survey experiment in Egypt to reveal that HROs can counteract governments' smear campaigns against human rights victims.

Overall, recent research on the relationship between international advocacy and public opinion shows that naming and shaming campaigns – particularly those using personalist appeals – can be effective at garnering support for human rights. They also reveal that the discursive relationship between governments and HROs impacts individuals' evaluations of developing events: governments can reduce support for human rights when they defend their behavior based on appeals to national security, as Risse, Ropp and Sikkink (1999) predict in the spiral model. While this research does make important progress in evaluating the causal link between information provision and attitudinal changes, it also leaves unanswered questions about how individuals filter new information about human rights and whether some individuals are more susceptible to HRO targeting than others. The next section presents a theoretical framework to understand why individuals react differently to naming and shaming based on their preconceptions about and trust in the different actors involved in the rights violation and advocacy campaign.

## 4.2 Heterogeneous Responses to Naming and Shaming

As Risse, Ropp and Sikkink (1999) point out in their spiral model, the phase of informational advocacy during which international organizations and governments engage in a back-and-forth dialogue about the justifiability of rights abuses is quite fragile. HROs and leaders are engaged in a competition to establish the dominant narrative about unfolding events as rights abuses or necessary efforts to protect domestic security. One reason for the fragility of this phase is that international advocacy is unlikely to persuade everyone to

support human rights. Domestic audiences will not have a uniform reaction to human rights campaigns, and many will maintain their support for the repressive leader even in the face of allegations of rights abuses. Certainly, individuals' demographic features explain some variation in response to international advocacy. While some individuals have a predisposition to support human rights, others are innately skeptical of international encroachment into domestic affairs and the broader human rights agenda (Gronke et al., 2010; Richards, Morrill and Anderson, 2012; Snyder, 2020; Terman, 2017). Research on public opinion about human rights and civil liberties shows that baseline attitudes toward human rights vary with education, wealth, authoritarianism and other demographic features (Carlson and Listhaug, 2007; Davis, 2007; Gronke et al., 2010; Haider-Markel and Vieux, 2008; Malka and Soto, 2011; Richards, Morrill and Anderson, 2012). Further, recent experimental research suggests that personalist frames may only be effective at shifting support for torture among ideological liberals (Arves and Braun, 2019).

Yet these demographic features do not reveal the full picture. Beyond these factors, individuals will react differently to new information about human rights based on their baseline levels of trust, preconceptions, and support for the actors involved in the rights violation and the informational campaign. Faced with competing messages about government behavior, individuals will process new information about human rights based on their relation to the actors involved. After hearing different narratives about human rights, individuals rely on their pre-existing reservoirs of trust to determine whether to support a repressive leader – who typically appeals to domestic security to justify repression – or the advocacy of international actors, who advocate protecting the human rights of all. This section accounts for these dynamics by reviewing individuals' relations to the three primary actors involved in human rights violations and advocacy campaigns: the human rights abuse victims, the repressive leader, and the international community. I expect that individuals' baseline support for each of these actors will determine whether they are receptive to naming and shaming from international organizations. It is important to consider these factors given that the success of a human rights campaign depends on HROs' ability to persuade a critical mass of individuals to support human rights and abandon support for the repressive leader. One implication of this theory is that the feasibility of this task varies across contexts, depending

on the population's preconceptions about victims, leaders, and the international community.

### 4.2.1 Relation to the Victim

The first factor that moderates the effect of international advocacy is individuals' relation to the victim of human rights abuses. One of the central missions of international human rights advocacy is to build sympathy for victims of government repression. Such efforts are critical given that governments often successfully characterize human rights abuse victims as enemies of the state or as terrorists (Bracic and Murdie, 2020; Brysk, 1993; Williamson and Malik, 2020). As Bracic and Murdie (2020) explain, "HROs are not operating in a political vacuum; repressive governments often try to spin information about abuses and the abused to their advantage" (879). As with many foreign policy issues, leaders operate at an informational advantage regarding the true threat that domestic groups pose to national security and the policy alternatives that could mitigate these threats (Baum and Groeling, 2010; Davies, 2016; Grieco et al., 2011). Leaders also have private interests in misrepresenting the necessity of repression to the public (Dragu, 2011). Further, repressive tactics that restrict the freedoms of threatening groups often do increase security for the country as a whole (Davis and Silver, 2004; Waldron, 2003). In light of these factors, leaders can often persuade domestic audiences that there are compelling reasons to support repression, particularly if they have pre-existing biases against repression victims.

The debate about the effectiveness and justifiability of torture to obtain information from suspected terrorists provides some insight into these dynamics. In many countries, there is a widespread belief that torture is effective at eliciting information that will help to divert a terrorist attack (Amnesty International, 2014; Gronke et al., 2010). Because of its perceived benefits, some argue that torture, though it would be abhorrent in normal times, can be morally justified for the sake of providing security (Dershowitz, 2002). Still, evidence suggests that torture is unlikely to provide such benefits. Rejali (2007) demonstrates that torture – when it is needed most – hardly ever provides the type of intelligence that is actually helpful for governments looking to prevent future attacks (478). Brecher (2007) argues that because it is rarely effective, torture is not morally justifiable, even in extreme cases. So, why

do so many believe that torture can often be justified to promote security in spite of evidence to the contrary? One reason why is that most individuals – who will never be tortured – have few reasons to doubt a leader's justifications for torture and may readily accept stereotypes about torture victims. In other words, the information environment surrounding torture often favors the status quo, which is a leader's justifications for his own behavior.

These patterns explain why personalist appeals are typically the most effective type of international advocacy for shifting support toward human rights protections (McEntire, Leiby and Krain, 2015, 2017). Those who initially have little sympathy for repression victims can at times be persuaded to view victims with compassion when advocacy campaigns reframe repressive tactics as rights abuses rather than domestic security initiatives. Still, these efforts are unlikely to be equally effective for all audiences. Specifically, those who view repression victims as especially threatening to domestic security are unlikely to shift their baseline levels of support for human rights as a result of advocacy efforts. Individuals in this category will be more likely to privilege the government's justifications for repression and remain intransigent even in the face of personalist appeals for human rights. Because of their preconceptions about the threat posed by repression victims, they are more likely to accept human rights abuses as reasonable efforts to promote domestic security. In contrast, those who do not view repression victims as threatening may be more amenable to the appeals of human rights groups. Individuals in this category may identify repression victims as ingroup members, perhaps as result of shared ethnic identity or ideological values, or they may have no strong opinions about the victims. For these individuals, personalist appeals from HROs may cut through the government's justifications of abuse and increase support for human rights and opposition to repressive leaders.

> **Hypothesis 1**: Individuals who perceive a victim of human rights abuses to be threatening are *less likely* to support human rights and oppose repressive leaders as a result of international advocacy than those who do not perceive a victim of human rights abuses to be threatening.

### 4.2.2 Support for the Leader

The second factor that drives variation in responses to international advocacy is an individuals' baseline level of support for the leader who has perpetrated the rights abuse. The dialogue between the government and HROs that ultimately guides individual attitudes about rights protections rests on the persuasive appeal of the leader's justifications of repression. In turn, individuals' propensity to trust these justifications depends on the level of baseline support they have for the leader himself. Those who support the leader for reasons other than their patterns of repression are more likely to trust the cues that this leader provides rather than update their beliefs as a result of international advocacy. As Lupia and McCubbins (1998) emphasize in their work on political endorsements, the effectiveness of cues depends on the receiver's perceptions about whether the cue giver is more informed about the "true" state of the world than they are. Further, cues are particularly effective when the cue giver and receiver share common interests.

Extrapolating this logic to issues of international security, Grieco et al. (2011) demonstrate that individuals are more receptive to international endorsements that contradict domestic foreign policy when they have low levels of trust in the leader himself. Similarly, Hayes and Guardino (2011) find that Americans were more susceptible to cues from foreign elites about the Iraq war if they were Democrats or Independents. In contrast, Republicans, who shared a party with the incumbent, were unlikely to shift their perspectives as a result of these cues. All of these findings have implications for the influence of international endorsements in the field of human rights. Specifically, those with high levels of trust in the leader should be less likely to be persuaded to update their beliefs by international organizations who challenge the leader's preferred narrative. Instead, they are likely to remain supportive of the leader and accept an account of repression that emphasizes the importance of domestic security. Ultimately, those who are most likely to be receptive to the leader's justifications are the leader's supporters. By extension, those privileging the leader's preferred account are less likely to increase their support for human rights following a naming and shaming campaign.

**Hypothesis 2**: Individuals who support the leader who perpetrates human rights abuses are *less likely* to support human rights and oppose repressive leaders as a result of interna-

tional advocacy than those who do not support the leader who perpetrates human rights abuses.

### 4.2.3 Trust in the Source

The third factor that influences the appeal of naming and shaming campaigns is individuals' level of trust for the international organizations that initiate these campaigns. A growing body of literature demonstrates that cues from international endorsements can successfully persuade individuals to shift their policy preferences in a number of issue areas. Because there is so much uncertainty around foreign policy issues, the public may be especially willing to seek out cues from experts or place greater weight to these cues than in other policy areas (Chapman, 2009, 734). However, these appeals cannot successfully persuade all audiences, particularly since there is high variation in support for international organizations due to factors like individual cosmopolitanism (Norris, 2000; Ecker-Ehrhardt, 2011), support for globalization (Bearce and Joliff Scott, 2019), personality traits (Schoen, 2007), and generalized trust (Brewer et al., 2004; Dellmuth and Tallberg, 2015; Torgler, 2008). In light of this variation, only individuals who have high levels of trust in international institutions are likely to be persuaded by their appeals (Linos, 2011). For instance, Anjum, Chilton and Usman (2021) finds conditional effects of United Nations (UN) endorsements for women rights based on baseline levels of support for the UN. Similarly, Grieco et al. (2011) find that cues from international organizations should be influential "for individuals who place intrinsic value on the endorsement of international institutions" (566). Bearce and Cook (2018) also uncover heterogeneous effects in responsiveness to cues from the World Trade Organization and the UN Security Council based on levels of support for these two international organizations.

In addition to these studies that reveal increased responsiveness to institutional endorsements from those who have high levels of trust in the institution, other work demonstrates that those who are skeptical of international intervention may backlash to human rights messaging (Bloomfield, 2016; Dixon, 2017; Epstein, 2012; Symons and Altman, 2015). Theoretical work from Terman (2017) suggests that individual perceptions about the credibility and legitimacy of the organization promoting a particular norm moderate the influence of international pressure. In particular, she expects that individuals will exhibit a defensive

reaction to international pressure "when they perceive transnational pressure to constitute symbolic domination: an illegitimate attempt to undermine the target's status, integrity, or interests by antagonistic actors for the purposes of infiltration or control" (6). Drawing from social identity theory, Terman (2017) expects that those who strongly identify with their their nation over the international community will be more likely to experience a sense of threat from the criticism of an outside actor like an international HRO. On the other hand, I expect that individuals who value international influence and advocacy should be relatively more likely to respond favorably to naming and shaming campaigns. Though Terman does not test this argument at the individual level, her theory implies that individuals will react differently to naming and shaming campaigns depending on their support for (or opposition to) the source of the campaign.

> **Hypothesis 3**: Individuals with low levels of trust in international organizations are *less likely* to support human rights and oppose repressive leaders as a result of international advocacy than those with high levels of trust in international organizations.

### 4.3 Research Design

I use a vignette experiment with a representative sample of 758 U.S. adults in July 2021 to test the implications of the theory.[4] Compared to other countries, individuals in the United States have average to below-average support for international organizations (Bearce and Joliff Scott, 2019; Norris, 2000). Survey data from the International Social Survey Programme's National Identity module in 1995, 2003, and 2013 show that survey respondents in the United States had similar attitudes to the cross-national mean in three measures of support for IOs (Bearce and Joliff Scott, 2019). For instance, in response to a question asking whether international organizations should be able to enforce solutions to certain problems, 56% of U.S. respondents answered in support compared to an average of 59.6%

[4]I use Lucid to conduct the survey experiment, a platform that is gaining increasing popularity in political science survey research. Recent studies using Lucid have been published in the *American Political Science Review* (Graham and Svolik, 2020; Tomz and Weeks, 2020; Williamson et al., 2020), the *American Journal of Political Science* (Costa, 2020), and the *Journal of Politics* (Lajevardi, 2020; Levy, 2020). The full survey instrument can be found in Appendix Section C.2. Appendix Section C.1 outlines a few areas of deviation from the pre-analysis plan.

across all countries in 2013. For this question, responses in the United States ranked within one percentage point of those from Hungary, Ireland, Japan, Russia, Slovakia, and the United Kingdom. Given these comparisons, the treatment effects of the survey experiment in the United States may be considered generally representative, though potentially below average, compared to the likely effects of naming and shaming across space. Another scope condition to consider is regime type, given that the effect of information provision about human rights varies across regimes (Hendrix and Wong, 2013). In democracies, HROs can initiate campaigns to *amplify* the attention to human rights abuses and influence the narrative through which individuals process rights violations. In contrast, advocacy in autocratic countries may provide individuals' first exposure to human rights violations that are kept hidden by leaders. Generally, the theoretical discussion, experimental design, and treatment effects in this paper represent the process of international advocacy in democratic countries, where citizens are typically aware of rights abuses and where bottom-up pressure is most likely to shift leaders' behavior.

To test the effects of naming and shaming, I select a rights abuse that has been the subject of various international advocacy campaigns targeting the United States over the last year: police violence against Black Lives Matter (BLM) protesters (Amnesty International, 2020*a*; Human Rights Watch, 2020; United Nations, 2021). This abuse is appropriate for the study given that it is both a salient and polarized issue in U.S. politics. Previous research suggests that international endorsements are most likely to be effective under high levels of polarization (Guisinger and Saunders, 2017). Further, police violence has been the focus of relatively little human rights research, which tends to evaluate shifts in attitudes toward other forms of physical integrity rights, like torture (Bracic and Murdie, 2020; Conrad et al., 2018; Kearns and Young, 2020; McEntire, Leiby and Krain, 2015, 2017; Piazza, 2015). The theory focuses on how international advocacy influences perceptions of human rights among a domestic audience, as well as how naming and shaming campaigns shape individuals' attitudes about the leaders who have perpetrated rights abuses. Whereas international audiences can mobilize against their own leaders to pressure foreign governments to abandon rights abuses, domestic audiences can often exert higher levels of pressure against their own repressive leaders through domestic mobilization strategies. Evaluation of this aspect of

the causal chain linking international advocacy to behavioral changes requires analysis of shifting attitudes among domestic audiences about their own leaders. As such, I select a human rights abuse that has occurred in the same country as the experimental setting.

The structure of the treatment vignette reflects the typical way that individuals would obtain information about domestic human rights abuses. The vignette first asks respondents to consider a scenario where an unnamed governor orders the police to use tear gas and rubber bullets against BLM protesters, reflecting a common news headline from the past year, when protests and police violence were common. Then, the leader issues a justification for his decision to utilize these tactics, emphasizing how the efforts are meant to prevent violence and deter future protests. Such language reflects common discourse from leaders who attempt to explain their patterns of rights abuses with appeals to domestic security (Bracic and Murdie, 2020; Williamson and Malik, 2020). The treatment is the random assignment of a second portion of the vignette, which explains how a prominent human rights organization, Amnesty International, has initiated a campaign to criticize the leader for his behavior. To maximize the likelihood that individuals will respond to the shaming treatment, this portion also includes a personalist appeal from one of the supposed victims of police violence, similar to a recent Amnesty International campaign (Amnesty International, 2020*b*). Existing research has shown that personalist appeals are most effective at garnering shifts in attitudes toward human rights (McEntire, Leiby and Krain, 2015, 2017). I include the vignette below, with the naming and shaming treatment in italics.

> Consider the following scenario. Black Lives Matter activists have been actively protesting in your state for the past several weekends against systemic racism and discrimination. The governor, a Republican, orders the police to use tear gas and rubber bullets against the protesters. He argues that the protests have begun to pose a threat to local businesses and the security of the state. Following a recent escalation in violence as a result of these new policing tactics, the governor explains in an interview that the measures are necessary to protect citizens, the local economy, and the safety of the rest of the state. He hopes that the measures will deter future protests.
> *In reaction to these events, Amnesty International, an international advocacy organization, initiates a campaign to criticize the governor. They shame the governor for using violence, arguing that these actions represent a violation of the human rights of the protesters. The campaign features accounts from multiple victims, who explain how police violence left them with welts on their arms and legs and chemical irritants in their eyes. One activist recounted how she was struck by a flash grenade while distributing food to other protesters. She was transported via stretcher to a private car, which took her to the hospital because no*

*ambulance would come to help. At the hospital, she suffered multiple cardiac arrests and was diagnosed with a concussion and whiplash. In the days since, she has had difficulty breathing while standing and becomes easily fatigued.*

The three hypotheses specify that the effect of the naming and shaming treatment will vary based on individuals' perceptions about the victims of human rights abuses, their support for the leader who committed the rights abuse, and their trust in the human rights advocacy organization. To measure the first variable, the survey includes a question that asks respondents how threatening they perceive various groups to be to domestic security on a scale from 1 (not at all threatening) to 10 (very threatening). To avoid priming respondents to the importance of the group that is the focus of the experiment, and to provide reference points that ground their rankings, there are 10 total groups included in the question.[5] I code the variable *Threatening* as 1 if individuals ranked BLM, the victim of repression in the vignette, as greater than 7 on the threat perception scale.[6] I expect that these individuals will be less likely to react to naming and shaming compared to those who do not perceive BLM to be threatening. The second independent variable of interest concerns respondents' trust in the leader who perpetrated the repression. I rely on a proxy measure of shared party identification to measure trust, given the high salience of party identification in U.S. politics. I code individuals as a 1 for the variable *Trust Leader* if they identify as a Republican and 0 otherwise.

Finally, the third hypothesis expects that individuals with high levels of trust in international organizations will be more likely to respond favorably to international endorsements than those who do not trust international organizations. I use three survey questions to measure individual attitudes about international HROs, and the international community in general. I adapted these questions from Pew's 2017 Global Attitudes Survey (Wike and Bishop, 2017), which capture the degree to which individuals support international HROs. From these three questions I create a dichotomous indicator for whether respondents *Don't*

[5]The groups in this section are the Republican Party, the Democratic Party, Black Lives Matter, Antifa, Refugees, Islamic extremists, White nationalists, Animal rights activists, Communists, and Illegal immigrants.

[6]The meter for threat perceptions starts at 5, so respondents must move the meter more than two spaces toward a more threatening position in order to be coded as perceiving the group to be threatening, so it is not the case that individuals would fall in this category if they did not move the meter or only moved the meter slightly.

*Support HROs.* Respondents must respond negatively to two of the three questions measuring support for international organizations to be coded as "1."[7] Negative responses include those who respond in the bottom two categories of the first question (bad, very bad), the bottom two categories of the second question (not at all important, slightly important) and answer that "international human rights organizations are primarily dedicated to promoting the interests of foreign groups" in the third question score a 1 on this indicator, while all others score a 0.[8] The text for the questions is below:

1. What kind of influence are international human rights organizations having on the way things are going in the United States? (5-point scale from "very good" to "very bad")
2. How important do you think it is for international human rights organizations to hold governments accountable for human rights issues? (5-point scale from "very important" to "not at all important")
3. Which of the following statements comes closest to your view: (1) international human rights organizations are primarily dedicated to protecting the rights of people in our country or (2) international human rights organizations are primarily dedicated to promoting the interests of foreign groups? (respondents choose one of these or can indicate "neither" or "don't know").

I use a block randomization strategy to identify the causal effect of the treatment among these sub-populations of respondents. The hypotheses predict that individuals will react differently to naming and shaming based on (1) their threat perception of the victim, (2) their trust in the leader, and (3) their support for international organizations. I create dichotomous variables for each of these categories, as detailed above. Given the combination

[7] I adjusted the coding for this variable following data collection given that only 15 respondents met my initial criteria to be coded as a "1." The changes in coding are detailed in section C.1 in the Appendix. Ultimately, the results are the same with either measure.

[8] As a robustness check, I use an additional measure based on individuals' support for Amnesty International. I expect general predisposition toward the international community to guide heterogeneous responses to naming and shaming campaigns. However, it is also possible that individuals react to new information based on their attitudes toward the specific organization in the vignette. To evaluate this possibility, I first ask respondents whether they are familiar with Amnesty International. For those who answer yes, I also ask them to use a feeling thermometer to indicate their general disposition toward the organization. In the alternative measure, I code *Don't Support AI* as a 1 for individuals who were familiar with Amnesty International and ranked the organization at or below a 2 on the feeling thermometer. Appendix Figure C.6 shows the results with this alternative measure.

of potential values of these variables, I randomize the treatment within eight blocks (2x2x2). In the analysis, I combine these categories for each of the groups of interest. Because the probability of assignments is uniform across the blocks, I can combine blocks without biasing the estimates of the treatment effects (Gerber and Green, 2012, 76).

To assess the effects of naming and shaming, I include two categories of outcome variables: attitudes about human rights and attitudes toward the repressive leader.[9] As a first step, I am interested in whether individuals shift their support for human rights abuses as a result of shaming as well as their intent to mobilize against human rights abuses in the future.[10] In the first of these measures, I ask respondents whether they agree or disagree that the use of tear gas and rubber bullets is an appropriate policing technique. This question assesses whether attitudes about the appropriateness of a particular repressive tactic, which could be considered as a measure to promote domestic security or a human rights violation, vary as a result of naming and shaming. Second, I ask respondents how upset they feel about the governor's use of tear gas and rubber bullets against protesters. Third, I include a general question about intent to mobilize, asking respondents about their likelihood of participating in a campaign to ban the use of tear gas and rubber bullets by police officers. Though this question only gauges hypothetical mobilization, it does elicit some information about respondent's intent to support human rights advocacy in a tangible way. Together, these three primary outcome measures assess both attitudinal changes as a result of international advocacy.

In the second set of outcome measures, I ask respondents to share how they rate the leader's job performance and their likelihood of supporting the leader for reelection. This analysis assesses a neglected aspect of the causal chain linking international advocacy to changes in patterns of repression: individuals' likelihood of shifting their voting behavior as a result of informational campaigns. In particular, the effect of naming and shaming on support for repressive leaders demonstrates whether individuals will defect from their party as a result of repression and/or international advocacy. In the first of these outcome measures,

[9] Appendix Section C.4 provides descriptive statistics for the outcome measures and independent variables of interest. It also shows the correlation between the different outcome measures.

[10] The first set of outcome measures mirror those used in recent survey experiments on attitudes about human rights as a result of naming and shaming, namely Bracic and Murdie (2020) and McEntire, Leiby and Krain (2015).

I simply ask respondents how they would describe their level of support for the Governor's reaction to the protests. Next, I ask respondents whether they approve or disapprove of the way the Governor is handling his job. Finally, I ask respondents how likely they would be to reelect this Governor in the next election.

I use a series of regressions to evaluate the heterogeneous treatment effects among the populations of interest. I regress the outcome measures on the treatment, interacted with the three moderating variables in separate models: *Threatening*, *Trust Leader*, and *Don't Support HROs*. I use linear models for all of the regressions, predicting that the treatments will have a stronger positive effect on human rights attitudes for those coded as 0 in the interaction variables compared to those coded as 1. Respondents who do not view the target as threatening, who do not trust the leader, and who support HROs should be more likely to shift their views in favor or human rights in reaction to international advocacy compared to those who view the target and threatening, who trust the leader, and who do not support HROs. Regarding the second set of outcome variables, I expect that the treatments will have a stronger negative effect on support for the repressive leader for individuals coded as a 0 in the interaction term compared to those coded as 1. Respondents who view the victim as threatening, who trust the leader, and who do not support HROs should be less likely to reduce their support for the leader than respondents who do not view the victim as threatening, who do not trust the leader, and who support HROs.[11]

Below I include the regression equation for the tests of the first hypothesis, which interacts the treatment with the indicator for whether the respondent perceives the victim of repression to be *Threatening*. In the other hypothesis tests, I simply substitute the indicator for threatening with the other dichotomous measures of support for the repressive leader and lack of support for international HROs. The hypotheses specify that the treatment effect of shaming for those who perceive the target to be threatening will be smaller than the effect of shaming for those who do not perceive the target to be threatening. In the equation, $\alpha$ represents baseline attitudes for those who do not perceive the victim to be threatening

[11]I include only indicators for the treatment and moderating variables in these models. I check for balance across several demographic features, including gender, income, education, age, race, community, region, and ideology. Coding for these variables is listed in Appendix Section C.3 and the results of the balance check can be found in Table C.1. All these covariates are balanced.

and are in the control group while $\beta_1$ represents attitudes for those who do not perceive the victim to be threatening and are exposed to international shaming. So, the treatment effect for those who perceive the target not to be threatening is $\beta_1$ - $\alpha$. Among those who perceive the victim to be threatening, $\beta_2$ is attitudes in the control group and $\beta_2 + \beta_3$ is attitudes in the treatment group. Then, the treatment effect for those who perceive the victim to be threatening is $\beta_2 + \beta_3$ - $\beta_2 = \beta_3$. Hypothesis 1 expects that the size of the *positive* treatment effect for those who perceive the victim to be threatening ($\beta_3$) is *less than* the treatment effect for those who perceive the victim to be non-threatening ($\beta_1$ - $\alpha$). Naming and shaming should yield greater improvements in human rights attitudes and greater opposition to repressive leaders among those who do not perceive the victim to be threatening compared those who do perceive the victim to be threatening.

$$Y_i = \alpha + \beta_1 Shaming + \beta_2 Threatening + \beta_3 Shaming * Threatening + e_i \quad (2)$$

### 4.4 Analysis and Discussion

To begin, I analyze the effect of naming and shaming on support for human rights and opposition to repressive leaders without using any interaction effects. These results can be found in Table 4.1. This analysis shows that naming and shaming has no statistically significant effect on support for human rights or opposition to repressive leaders in any of the models. Not only are these results significantly insignificant, but they are also substantively small. In contrast, the measures for threat perception of the repression victim and shared party with the leader are strong predictors of attitudes about human rights and opposition to the repressive leader. In particular, those who found the repression victim (BLM) to be threatening were less likely to support human rights in the abstract and less likely to oppose the repressive leader. Similarly, Republicans, who share a party with the repressive leader, were less likely to support human rights and more likely to oppose repressive leaders compared to respondents who belonged to other parties. Respondents who supported HROs

were more supportive of human rights in general than those who did not, but this variable did not have an effect on opposition to repressive leaders. In general, these results show the expected effects of the covariates of interest, but do not show a treatment effect for naming and shaming.

Table 4.1: Support for Human Rights and Opposition to Repressive Leaders

| | *Support Human Rights* | | | *Oppose Repressive Leader* | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Shaming | 0.01 | 0.03 | 0.03 | 0.08 | 0.05 | 0.04 |
| | (0.09) | (0.07) | (0.10) | (0.09) | (0.09) | (0.10) |
| BLM Threatening | −1.10*** | −0.69*** | −0.51*** | −0.94*** | −1.02*** | −1.03*** |
| | (0.10) | (0.08) | (0.11) | (0.10) | (0.10) | (0.11) |
| Republican | −0.57*** | −0.73*** | −0.84*** | −0.56*** | −0.56*** | −0.49*** |
| | (0.11) | (0.08) | (0.11) | (0.10) | (0.10) | (0.11) |
| Don't Support HROs | −0.18 | −0.53** | −0.89** | 0.01 | −0.22 | −0.06 |
| | (0.34) | (0.27) | (0.35) | (0.32) | (0.33) | (0.35) |
| Constant | 3.35*** | 2.92*** | 3.14*** | 3.31*** | 3.43*** | 3.42*** |
| | (0.08) | (0.06) | (0.08) | (0.07) | (0.08) | (0.08) |
| Observations | 758 | 758 | 758 | 758 | 758 | 758 |

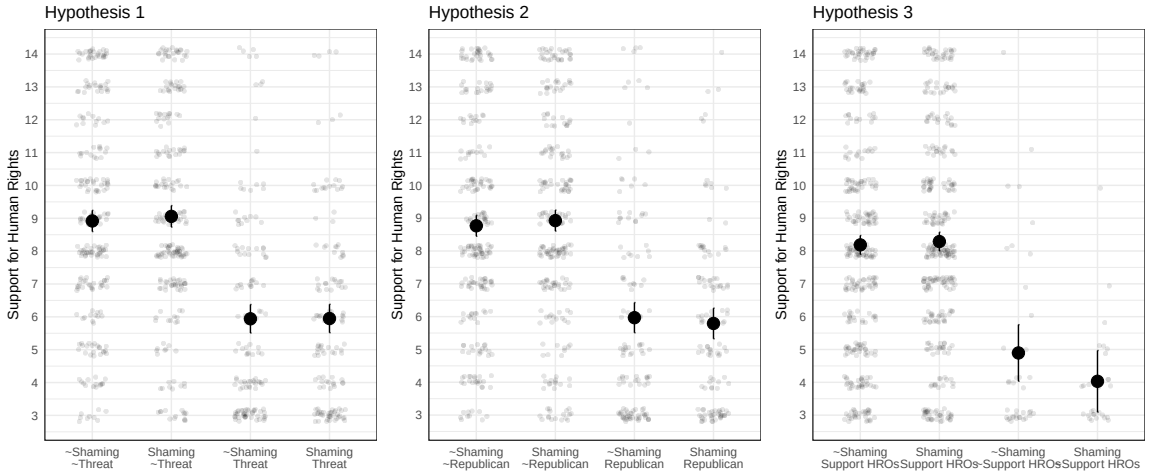
*Notes*: Linear models. Dependent variables are Likert scales for support for human rights and opposition to repressive leaders. Higher values indicate higher support for human rights and greater opposition to repressive leaders. ***, **, * significant at .01, .05, .10, respectively.

One theoretical explanation for the null effects of naming and shaming in these initial models is that different groups react differently to international advocacy. In particular, my hypotheses predict that the treatment effect of naming and shaming will vary based on individuals' relation to the victim of repression, the repressive leader, and the shaming organization. Thus, one interpretation of the null results of advocacy in Table 4.1 could be that the treatment effects in different sub-samples cancel each other out. To evaluate this possibility, it is necessary to interact the treatment with these different measures of respondents' orientation to the actors involved in human rights violations. I first assess these interaction effects using the outcome measures of support for human rights. Interestingly, these results show that naming and shaming has no significant effect on generalized attitudes about human rights among any of the sub-groups of interest. In Figure 4.1, I show these results with an aggregated measure of support for human rights.[12] As predicted, the figure demonstrates that the sub-groups who identify repression victims as non-threatening, do

[12]This outcome measure is the sum of the three measures of support for human rights discussed in the research design. I present results for the aggregated outcome for ease of interpretation, but the results in models using separate outcome measures are the same.

not share a party with the leader, and have high levels of support for HROs have higher levels of support for human rights compared with the respondents that fall outside of these categorizations. However, there is no significant treatment effect of naming and shaming within or outside of these sub-samples.

Figure 4.1: Effect of Shaming on Support for Human Rights




*Notes*: The effect of shaming on support for human rights by threat perception (panel 1), in-group identification with the leader (panel 2), and support for international organizations (panel 3). Dependent variable is the combined score of 3 measures of support for human rights. 90% confidence intervals. N = 758.

This first round of results for the hypothesis tests does not support my predictions. However, it should be noted that generalized attitudes about human rights are difficult to change, and they may be especially intransigent in an issue area that has been the subject of high levels of media attention and national debate in the months preceding the experiment. However, while naming and shaming might not change overall beliefs, perhaps it can influence individuals' opposition to the *leader* who has perpetrated a rights abuse. Given the potentially higher threshold for international advocacy to influence support for human rights in general, compared to repressive leaders in particular, it is important to also analyze the effect of naming and shaming on outcome variables measuring opposition to repressive leaders. Table 4.2 presents results using these outcome measures. Turning first to the results of the first hypothesis, Models 1-3 show support for the theorized relationships. Specifically, shaming has a positive and statistically significant effect on opposition to repressive leaders among those who do not perceive BLM to be threatening. In contrast, those who do view

BLM as threatening *decrease* their opposition (*increase* their support) for repressive leaders following naming and shaming, as can be seen by the negative and statistically significant coefficients on the interaction terms. These results show that naming and shaming can be effective among some sub-populations while resulting in a backlash effect among others. Models 4-6, which provide the results for the second hypothesis, show similar results. Shaming increases opposition to repressive leaders among respondents who do not share a party with the leader, but increases *support* for repressive leaders among respondents who share a party with the leader. Again, the results show that the effect of shaming varies based on individuals' orientation to one of the parties involved in the rights abuse: the repressive leader.[13] Finally, in contrast to my expectations, Models 7-9 do not show support for the third hypothesis. Shaming does not affect opposition to repressive leaders, regardless of individuals' baseline levels of support for HROs.

These results show that the factors of interest in this paper – threat perception of the victim, shared identity with the leader, and trust in the international community – do shape attitudes about repression. Across all the models, these variables are strong predictors of support for human rights and opposition to repressive leaders. Those who viewed the victim as non-threatening, did not share a party with the leader, and exhibited high levels of support for international HROs were more likely to support human rights compared to those who perceived the victim to be threatening, shared a party with the leader, or did not trust the international community. Regarding the results of the hypothesis tests, however, some of the results supported my theory while others diverged from my expectations. Specifically, international advocacy was unable to shift respondents' beliefs about human rights *in general*. However, international naming and shaming did increase opposition to repressive *leaders* within two specific sub-populations: those who perceived the victim to be non-threatening and those who did not share a party with the leader. The takeaway is that international advocacy can effectively mobilize opposition to repressive leaders, but only among some populations. Further, they reveal that generalized attitudes may be more difficult to shift through information sharing compared to attitudes about specific leaders.

[13]Linear hypothesis tests demonstrate that the effect of naming and shaming across sub-groups in Models 1-6 are statistically distinct from each other, as expected in the hypotheses.

Table 4.2: Effect of Shaming on Opposition to Repressive Leaders

| | *Hypothesis 1* | | | *Hypothesis 2* | | | *Hypothesis 3* | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Shaming | $0.20^{*}$ | $0.21^{*}$ | $0.20^{*}$ | $0.21^{*}$ | 0.17 | $0.22^{*}$ | 0.08 | 0.04 | 0.03 |
| | (0.11) | (0.12) | (0.12) | (0.11) | (0.12) | (0.12) | (0.10) | (0.11) | (0.11) |
| BLM Threatening | $-0.94^{***}$ | $-0.99^{***}$ | $-0.96^{***}$ | | | | | | |
| | (0.13) | (0.14) | (0.14) | | | | | | |
| Shaming * BLM Threatening | $-0.33^{*}$ | $-0.43^{**}$ | $-0.44^{**}$ | | | | | | |
| | (0.19) | (0.19) | (0.20) | | | | | | |
| Republican | | | | $-0.64^{***}$ | $-0.71^{***}$ | $-0.53^{***}$ | | | |
| | | | | (0.14) | (0.15) | (0.15) | | | |
| Shaming * Republican | | | | $-0.45^{**}$ | $-0.40^{*}$ | $-0.61^{***}$ | | | |
| | | | | (0.20) | (0.21) | (0.22) | | | |
| Don't Support HROs | | | | | | | $-0.71^{***}$ | $-0.77^{***}$ | $-0.76^{***}$ |
| | | | | | | | (0.23) | (0.24) | (0.25) |
| Shaming * Don't Support HROs | | | | | | | $-0.21$ | $-0.07$ | $-0.08$ |
| | | | | | | | (0.33) | (0.35) | (0.36) |
| Constant | $3.13^{***}$ | $3.23^{***}$ | $3.24^{***}$ | $3.01^{***}$ | $3.10^{***}$ | $3.07^{***}$ | $2.87^{***}$ | $2.95^{***}$ | $2.97^{***}$ |
| | (0.08) | (0.08) | (0.08) | (0.08) | (0.08) | (0.09) | (0.07) | (0.07) | (0.08) |
| Observations | 758 | 758 | 758 | 758 | 758 | 758 | 758 | 758 | 758 |

*Notes*: Linear models. Dependent variables are Likert scales for measures of opposition to repressive leaders: support of the governor's reaction (M1, M4, M7), job approval (M2, M5, M8), and intention to reelect the leader (M3, M6, M9). Higher values indicate greater opposition to repressive leaders. ***, **, * significant at .01, .05, .10, respectively.

## 4.5 Conclusion

A growing body of recent research evaluates how (and whether) international actors can shape public attitudes about human rights. One core activity through which the international community can influence individual beliefs is by utilizing international advocacy campaigns that name and shame leaders for committing acts of repression. These efforts may be especially useful in democratic countries, in which citizens have many institutional mechanisms to hold leaders accountable for abuse. In this study, I evaluate whether these international efforts vary in their efficacy across different groups of respondents. Specifically, I assess whether individuals are more amenable to human rights advocacy depending on their preconceptions and trust in the actors that were involved in the human rights violations: the victims, the repressive leader, and the international community. Using the United States as a test case, I find that naming and shaming does not change individual attitudes about human rights in general among any sub-population of respondents. In contrast, human rights advocacy can increase opposition to repressive *leaders* among those who do not see abuse victims as threatening and who do not share a party with the leader. Further, the experimental results show that those who *do* view the victims as threatening and share a party with the leader backlash to international advocacy. These individuals increase their support for repressive leaders following naming and shaming.

Overall, the results of the study suggest that international organizations may face difficulty when trying to change generalized attitudes about domestic repression. However, their campaigns may be effective at changing support for repressive leaders among some sub-sets of the populations who already sympathize with repression victims or are skeptical of repressive leaders. These findings are especially interesting when considered in comparison to other recent survey experiments that evaluate the effect of advocacy on human rights attitudes. Some of the studies have found a null result of naming and shaming on awareness of human rights abuses (Ausderan, 2014) and support for the human rights of domestic terrorist organizations (Williamson and Malik, 2020). Others show that international advocacy can increase support for human rights for victims of state killings (Haines et al., 2020) and sleep deprivation (McEntire, Leiby and Krain, 2015, 2017). One takeaway from this study is

that researchers should evaluate whether naming and shaming campaigns are more effective among different groups of respondents, as a null effect in the full sample could mask heterogeneous effects across groups. The mixed results across studies could also indicate that respondents react differently to new information about different *types* of abuses and that individuals in some countries are more receptive to human rights messaging than others. Ultimately, there is much room for future research to evaluate the conditions under which international naming and shaming campaigns change beliefs, as well as behavior.

# 5.0 Conclusion

This dissertation has explored the connection between elections and repression by evaluating patterns of repression across democracies, individuals' propensity to incorporate repression into their vote choice, and international organizations' efficacy at shifting public opinion about human rights and repressive leaders. Together, the three papers in the dissertation evaluate the extent to which threat perceptions shape individuals' attitudes about repression and the likelihood that democratic leaders will incorporate these preferences into their behavior around elections. The evidence across the papers lends some support to the theoretical framework while raising important questions for future study.

To summarize the main findings, the first paper shows that leaders increase repression as elections approach, but only in countries experiencing domestic violence. In contrast to expectations, the results do not show a significant relationship between pre-election repression and the likelihood that a leader will win reelection. Turning to the individual level, the second paper reveals that most voters are indifferent to repression and that even those who perceive repression victims to be threatening will not reward repressive candidates. Instead, only voters who identify with the victims of repression punish rights violations at the ballot box, at least regarding repression in the form of restrictions on the right to protest in the United States. Finally, the third paper shows that threat perception, as well as party identification, are important moderators for the effect of international shaming on attitudes about repressive leaders. Generally, the third paper shows that naming and shaming campaigns may be less effective at shifting generalized beliefs about a human rights issue that has already been the subject of widespread news coverage and debate.

These findings leave ample room for future research to delve deeper into the relationship between threat perceptions, elections, targeted repression, and voting behavior. To begin, the results of the first paper lay the foundation for three areas of future study. First, future research should consider whether patterns of targeted repression during pre-election periods shape other important outcomes besides leaders' overall probability of winning reelection. Survival in office is only one important metric of public support for leaders. Subsequent stud-

ies should analyze whether repression shapes leaders' vote share across countries, indicating the effect of repression on leaders' performance when they may have already had a high or low likelihood of winning reelection, and public opinion about leaders. In particular, public opinion data offers a finer-grained measure of reaction to repression across time. Second, future research can evaluate whether certain types of leaders are likely to commit repression based on their *expected* probability of winning reelection. Perhaps leaders who are secure in office refrain from resorting to repressive tactics, or, in contrast, perhaps leaders with a high likelihood of winning have greater freedom to engage in repressive behavior. Evidence for either of these possibilities would provide important insight into the conditions under which democracies are at greatest risk of experiencing human rights violations. A final area of study would evaluate the extent to which electoral considerations shape leaders' response to ongoing dissent. Past research has assessed a range of features that influence leaders' reaction to domestic challenges. However, further consideration of the electoral dynamics that may also shape leaders' response to dissent would add critical insight into the dissent-repression nexus.

The survey research in the second and third papers generate crucial insight into individual decision-making about one important form of repression – restrictions against the rights to protest – in the context of the United States, an established democracy. In addition delivering interesting findings, these papers also lay the foundation for a rich body of research that can further evaluate individual-level beliefs about repression across issue areas and political contexts. In particular, two areas of study would make important contributions. First, future work should evaluate voters' propensity to incorporate different forms of repression, perpetrated against a range of repression targets, into their vote choice. In particular, this research could examine whether voters react differently to more intense physical integrity rights violations, such as torture, disappearances, or extrajudicial killings, perpetrated against a wider range of victims, including members of terrorist groups or other marginalized communities. The range of repression that may influence vote share is also likely to vary widely across contexts. Thus, it is important to empirically evaluate the extent to which these results travel to countries involved in domestic conflict or developing democracies, in which repression is likely to be more widespread. Regarding the influence of

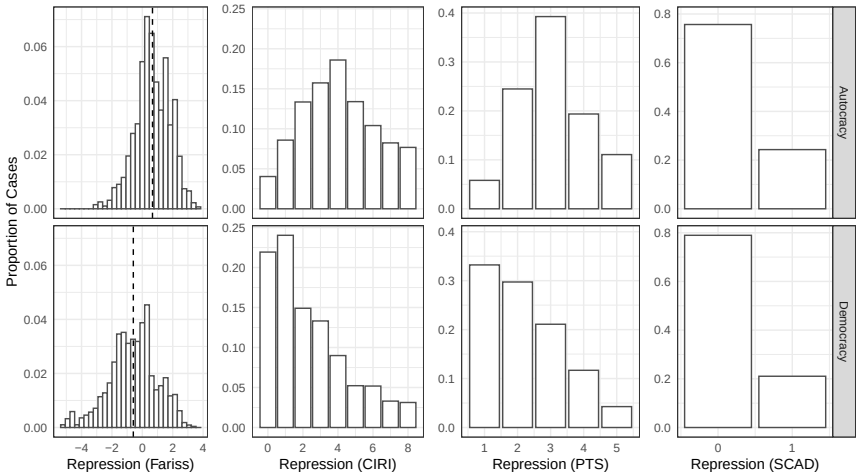
international advocacy, it is possible that individuals in the United States are particularly resistant to naming and shaming, especially in an issue area in which views may be crystallized as a result of widespread debate and media coverage. Future work can assess the effectiveness of advocacy campaigns in countries that may be more amenable to intervention from the international community and in issue areas where individuals may be less certain about the costs and benefits of repression.

Overall, this dissertation generates a research agenda with important contributions to existing academic debates and public policy. By focusing on one foundational democratic institution, elections, the dissertation adds nuance to extant findings about the domestic democratic peace. Prior research demonstrates that democracies are at lower risk for human rights violations than autocracies. Yet, rights violations in democracies are not uncommon, particularly against minority communities, protesters, political outsiders, and marginalized groups. The dissertation highlights one important explanation for this pattern: voters' acceptance of, or indifference to, rights violations against those they perceive to be threatening. The theoretical account highlights the moderating effect of threat perception, often induced by ongoing domestic conflict, on voting behavior and leaders' decision to engage in repression. In so doing, it integrates existing insight about the security-civil liberties trade-off into a better understanding of patterns of repression across democracies. Taken together, these contributions pave the way for a deeper understanding of the drivers of human rights violations across countries and some mechanisms to promote robust human rights regimes around the world.

# Appendix A Paper 1 Additional Material

## A.1 Repression by Regime Type

Figure A.1: Repression by Regime Type




*Notes*: Each panel shows the proportion of each category of repression for democracies and autocracies, as defined by Przeworski et al. (2000). Higher values on the x-axis indicate higher levels of repression. From left to right, data is from Fariss (2014) (1945-2016), the Cingranelli and Richards (CIRI) Human Rights Data Project (1980-2011) (Cingranelli, Richards and Clay, 2014), the Political Terror Scale (1976-2016) (Gibney et al., 2019), and the Social Conflict Analysis Database (1990-2016) (Salehyan et al., 2012). The last column plots the proportion of protest events to which the government responded with lethal or non-lethal repression.

## A.2 Measuring Repression from ICEWS

I make several coding decisions to extract repressive events from the larger database of all ICEWS events. These coding decisions are based on accepted definitions of repression from Davenport (2007*a*), Goldstein (1978), and Ritter (2014). Following Ritter (2014), *repression* is "coercive actions political authorities take to inhibit the will or capacity of people within their jurisdiction to influence political outcomes" (145). Below, I outline the coding rules for collecting repressive events.

1. First, events must be **coercive actions** that inhibit the will or capacity of people to influence political outcomes. I identify events that match this definition based on their CAMEO (Conflict and Mediation Event Observations) code, which is a systematized metric to identify event types (Schrodt and Yilmaz, 2007). The CAMEO codes that I identify as repressive events are below. More specific descriptions of these categories and examples of events that meet these criteria can be found in the CAMEO codebook.
    - 1233: reject request for rights
    - 1241: refuse to ease administrative sanctions
    - 132: threaten with administrative sanctions, not specified
    - 1321: threaten with restrictions on political freedom
    - 1322: threaten to ban political parties or politicians
    - 1323: threaten to impose curfew
    - 1324: threaten to impose state of emergency or martial law
    - 137: threaten with repression
    - 1383: threaten unconventional violence
    - 151: increase police alert status
    - 153: mobilize or increase police power
    - 170: coerce, not specified
    - 172: impose administrative sanctions, not specified
    - 1721: impose restrictions on political freedoms
    - 1722: ban political parties or politicians
    - 1723: impose curfew
    - 1724: impose state of emergency or martial law
    - 175: use tactics of violent repression
    - 180: use unconventional violence, not specified
    - 181: abduct, hijack, or take hostage
    - 182: physically assault, not specified
    - 1821: sexually assault
    - 1822: torture
    - 1823: kill by physical assault

- 185: attempt to assassinate
- 186: assassinate
- 200: use unconventional mass violence, not specified
- 201: engage in mass expulsion
- 202: engage in mass killings
- 203: engage in ethnic cleansing

2. Second, events must be perpetrated by political authorities **linked to a specific country**. ICEWS includes a variable that links the source of the event (the perpetrator) to a specific country, if possible. I first filter out all observations where this variable is missing. These are cases where the perpetrator could not be linked to a specific country, such as when the source is an international organization (e.g., Relief International, UN Women), a multi-national corporation (e.g., Tim Hortons, Netflix, Tesla Motors), or an individual (e.g., Amara Essy, Ronald Lauder). Next, I exclude all observations where the source was not a country identified by Correlates of War. These include occupied or overseas territories (e.g., Palestine, Aruba, Puerto Rico) and land not linked to a state (e.g., Holy See, Antarctica, Timor-Leste under UN control). Since I later tie repression to electoral outcomes at the national level, I only include acts associated with a source country.
3. Third, events must be perpetrated by **political authorities**. To satisfy this condition, I omit events that were perpetrated by actors that are associated with a particular country but do not represent the government. More specifically, because I will look at the results of executive elections, I only keep events that are linked to the executive branch. ICEWS uses a nested dictionary to identify the sector associated with each event's source. The master dictionary includes 591 sector types belonging to five nested levels. Each event can be associated with multiple sectors. I keep events that were associated with at least one of the following sectors: Government (if there are no lower nested categories listed), Executive (and all nested categories), Police (and all nested categories), Military (and all nested categories), Legislative / Parliamentary (and all nested categories), Government Religious (and all nested categories), Government Major Party (In Government), and Government Minor Party (In Government). I also keep cases where the sector was not

identified: following a standard unitary actor assumption, I assume that coercive actions taken by the country in general can be associated with the country's leader.

4. Fourth, events must be perpetrated **within the source country's jurisdiction**. For this condition I verify that either (1) the source country has the same COW code as the country in which the event occurred or (2) the location of the event was an overseas territory of the source country. For instance, I consider coercive actions taken by the United States both in the United States as well as in American Samoa, Guam, the Northern Mariana Islands, Puerto Rico, the U.S. Virgin Islands, and the U.S. Minor Outlying Islands to be repression. I drop observations where the location is neither a country nor an occupied territory (Antarctica, Holy See, and Timor-Leste under UN management).
5. Fifth, **the target must belong to the source country**. Here I reference expanded definitions of repression, which specify that repressive actions must be perpetrated against those **under** the source country's jurisdiction (Fariss, 2014), to control **citizens'** behavior (Ritter, 2014), or to "prevent people within the state from participating in their **own** governance" (Conrad and Ritter, 2019, emphasis added). These definitions all recognize that repression is limited to state actions taken against those who in some sense *belong* to the state's jurisdiction, not just events that take place *within* the state's territory. After limiting the sample according to the other coding rules, there remain cases in which the target country does not match the source/location country. These are cases where:
    - The target country is missing. This occurs when the target cannot be linked to a specific country. There are 713 distinct actors in this category, including international organizations (e.g., UN, EU, Peackeeping Troops, International Media), individuals (e.g., Aum Shinrikyo, Kofi Annan, Tom Steyer), NGOs (e.g., Human Rights Watch), regions (e.g., Latin America, Middle East, South Asia), and international ethnic and religious groups.
    - The target belongs to a country other than the source country. These are coercive actions taken by one country against actors associated with another country. Many of these events straddle the line between repression and interstate conflict. Rather than repression, such events better fit with states' sovereign right to monopolize

force in their own territories.

- The target is attempting to join the source country (asylum seekers, refugees, immigrants, illegal immigrants, and expatriates). These cases overlap with both of the above categories: sometimes they are linked to a specific country and other times not. These events represent a challenging coding decision given that the target does not belong to the source country but may consider the source country to be their own.

I keep cases that fall in the third category and eliminate all other cases where the source and the target do not match. Past definitions do not typically consider coercive action taken against individuals or groups that do not belong to the perpetrating case to be repression. Still, it is reasonable to consider immigrants and refugees to fall under their host country's jurisdiction. I code four variables to demarcate these categories: *refugees* (the target sector is "Refugees / Displaced"), *exiles* (the target actor is "Exiled Opposition" or "Exiles"), *immigrants* (the target actor is "Immigrant"), *illegal immigrants* (the target actor is "Illegal Immigrant").

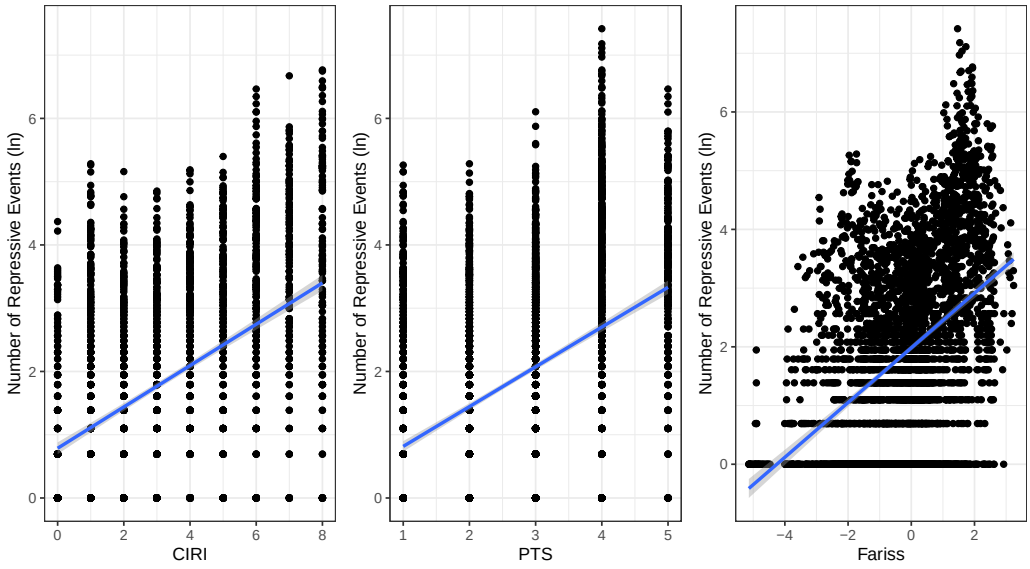
### A.3 Comparing ICEWS to Other Measures of Repression

The result of these coding decisions is a sample of 125,163 unique repressive events. How does this events-based measure of repression compare to existing standards-based measures? I briefly compare the measure of repression I have created using the ICEWS data with existing measures of repression: the Cingranelli and Richards Human Rights Data Project (CIRI), the Political Terror Scare (PTS), and Fariss' continuous measure. Where necessary, I rescale these variables so that higher values represent more repression. Figure A.2 shows that there is a positive correlation between each of these variables and my measure of repression. I also regress each of these measures on my measure, while controlling for variables known to be correlated with events coverage: whether the country is a *democracy* (as measured by Przeworski et al. (2000)), whether the country is a member of the *OECD*, and the country's *Population*. I use a Poisson link function given that the dependent variable is a count of the

total number of repressive events.

These results, presented in Table A.1, are promising. First, there is a positive and statistically significant relationship between each preexisting measure of repression and the ICEWS event count. Second, the control variables behave as I would expect for events data: countries that are more democratic, belong to the OECD, and have larger populations are likely to have more repressive events. Rather than suggesting that these countries are more repressive *per se*, these results indicate the well-known biases of events data: richer, larger, and more democratic countries are more likely to have higher news coverage. It is also possible that the results suggest something interesting about repression across regime types. While autocracies are likely to rank low on standards-based measures of repression, their levels of repression may also be based on a culture of fear that inhibits popular dissent. As a result, these countries may have fewer repressive *events*, even disregarding the bias in news coverage.[1] Overall, these results indicate that my sample of free and fair elections will have good coverage in the ICEWS data and that my measure has face validity in comparison to existing metrics.

Figure A.2: ICEWS v. Other Repression Measures




[1]Note that these regression results hold when I use Polity and V-Dem's polyarchy index instead of Przeworski's measure of democracy.

Table A.1: Predictors of ICEWS Repression

| | (1) | (2) | (3) |
|---|---|---|---|
| CIRI | 0.277*** | | |
| | (0.002) | | |
| PTS | | 0.441*** | |
| | | (0.004) | |
| Fariss | | | 0.394*** |
| | | | (0.003) |
| Democracy | 0.342*** | 0.298*** | 0.291*** |
| | (0.008) | (0.007) | (0.007) |
| OECD | 0.320*** | 0.185*** | 0.298*** |
| | (0.012) | (0.010) | (0.011) |
| Population | 0.411*** | 0.550*** | 0.520*** |
| | (0.003) | (0.002) | (0.002) |
| Constant | −5.156*** | −7.609*** | −5.876*** |
| | (0.041) | (0.032) | (0.035) |
| Observations | 2,746 | 3,745 | 3,753 |

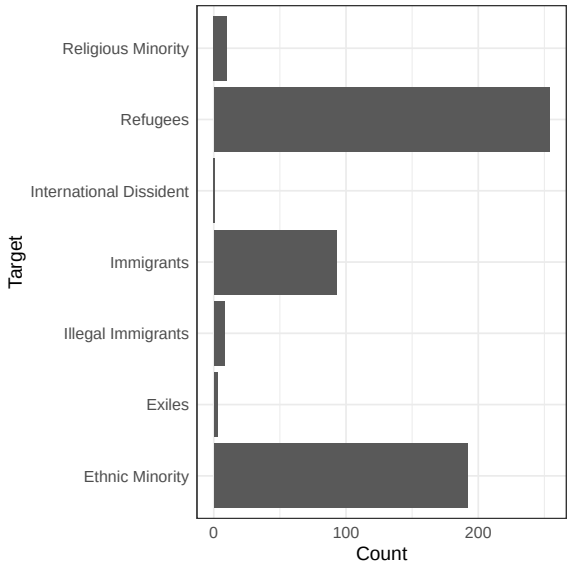
*Notes*: Poisson models. Dependent variable is the count of repressive events for each country-year. ***, **, * significant at .01, .05, .10, respectively.

## A.4 Repression of Threatening Groups

Table A.2: Repression of Threatening Groups

| Event Type | Count |
|---|---|
| Use tactics of violent repression | 22221 |
| Coerce | 3165 |
| Physically assault | 1785 |
| Use unconventional violence | 1429 |
| Impose administrative sanctions | 1415 |
| Assassinate | 1115 |
| Torture | 599 |
| Abduct, hijack, or take hostage | 345 |
| Impose restrictions on political freedoms | 344 |
| Threaten with repression | 269 |
| Ban political parties or politicians | 132 |
| Attempt to assassinate | 93 |
| Sexually assault | 92 |
| Kill by physical assault | 92 |
| Refuse to ease administrative sanctions | 89 |
| Engage in mass killings | 40 |
| Mobilize or increase police power | 31 |
| Threaten with administrative sanctions | 30 |
| Engage in mass expulsion | 21 |
| Increase police alert status | 16 |
| Threaten with restrictions on political freedoms | 15 |
| Threaten to ban political parties or politicians | 14 |
| Impose state of emergency or martial law | 10 |
| Impose curfew | 6 |
| Reject request for rights | 1 |

Figure A.3: Frequency of Threatening Groups Targeted by Repression (All Countries)




*Notes*: Frequency of repressive events perpetrated against groups identified as threatening in all countries (democracies and autocracies, 1995-2020). Note that by far the most common threatening target was dissidents, making up 98% of observations. I omit this category to better observe the relative frequency of the other categories.

## A.5 Descriptive Statistics

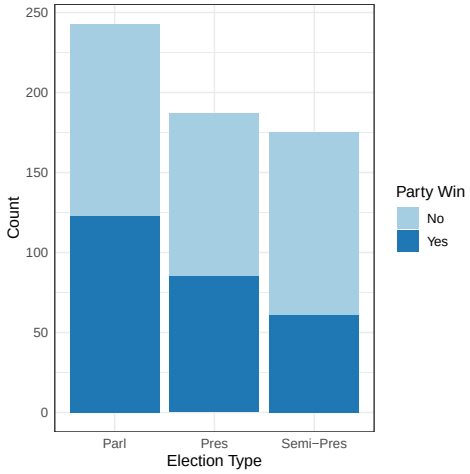
Table A.3: Summary Statistics (H1)

| Variable | N | Mean | St. Dev. | Min | Max |
|---|---|---|---|---|---|
| Repression | 14,121 | 0.52 | 2.60 | 0 | 141 |
| Months to Election | 14,121 | 11.97 | 7.00 | 0 | 24 |
| Polyarchy | 14,121 | 0.74 | 0.13 | 0.50 | 0.92 |
| Free/Fair Election | 11,674 | 1.35 | 0.75 | −0.93 | 2.74 |
| Population | 11,814 | 41,026,466.00 | 136,588,295.00 | 131,678.00 | 1,352,617,328.00 |
| GDP per Capita | 11,293 | 9.44 | 1.03 | 6.59 | 11.34 |
| GTD Growth | 11,293 | 0.03 | 0.04 | −0.55 | 0.21 |
| Protests | 14,121 | 7.47 | 26.06 | 0 | 573 |
| Conflict (UCDP) | 13,017 | 0.02 | 0.15 | 0 | 1 |
| Insurgency (NAVCO) | 5,676 | 0.07 | 0.26 | 0 | 1 |
| Attack (GTD) | 14,121 | 0.07 | 0.25 | 0 | 1 |

Table A.4: Summary Statistics (H2)

| Variable | N | Mean | St. Dev. | Min | Max |
|---|---|---|---|---|---|
| Party Win | 605 | 0.44 | 0.50 | 0 | 1 |
| Repression | 590 | 2.27 | 6.95 | 0 | 67 |
| Polyarchy | 605 | 0.74 | 0.13 | 0.50 | 0.92 |
| Free/Fair Election | 605 | 1.32 | 0.76 | −0.93 | 2.74 |
| Population | 560 | 36,641,403.00 | 121,962,391.00 | 133,806.00 | 1,295,604,184.00 |
| GDP per Capita | 536 | 9.42 | 1.03 | 6.59 | 11.34 |
| GDP Growth | 536 | 0.03 | 0.03 | −0.14 | 0.13 |
| Protests | 605 | 8.63 | 21.33 | 0 | 286 |
| Margin of Majority | 490 | 0.32 | 0.47 | 0.00 | 1.00 |
| Years in Office | 518 | 5.03 | 3.15 | 1.00 | 20.00 |
| Presidential Election | 605 | 0.31 | 0.46 | 0 | 1 |
| Semi-presidential Election | 605 | 0.29 | 0.45 | 0 | 1 |
| Judicial Constraints | 605 | 0.80 | 0.17 | 0.12 | 0.99 |
| Legislative Constraints | 605 | 0.79 | 0.17 | 0.10 | 0.98 |
| Conflict (UCDP) | 590 | 0.03 | 0.16 | 0 | 1 |
| Insurgency (NAVCO) | 258 | 0.08 | 0.27 | 0 | 1 |
| Attack (GTD) | 590 | 0.17 | 0.38 | 0 | 1 |

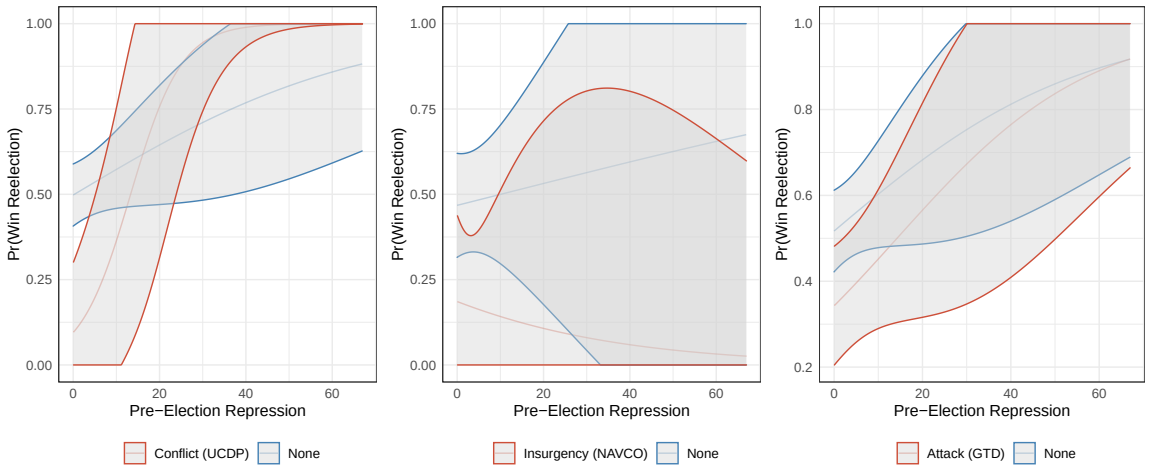
Figure A.4: Party Wins in Executive Elections




*Notes*: Ratio of party wins and losses in presidential, semipresidential, and parliamentary elections between 1995 and 2019. Party win is when the executive's party kept their position as head of government or head of state as a result of the election. N = 605.

## A.6 Predicted Probabilities for Hypothesis 2 Analysis

Figure A.5: Predicted Probability of Winning, Table 2.2, Models 3-5




*Notes*: Predicted probability of reelection based on the levels of repression in the pre-election period and the presence of ongoing conflict. 95% confidence intervals. All control variable set to means.

## A.7 Additional Analysis for Hypothesis 1

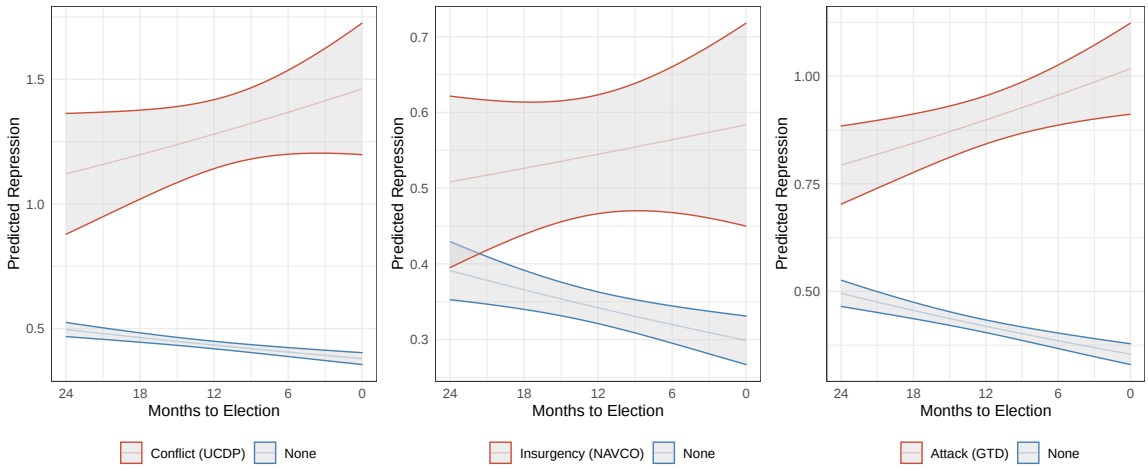
### A.7.1 Additional Control Variables

Table A.5: Election Timing and Repression: Additional Control Variables

| | Conflict | Insurgency | Terrorism |
|---|---|---|---|
| | (1) | (2) | (3) |
| Months to Election | 0.01* | 0.01* | 0.01** |
| | (0.01) | (0.01) | (0.01) |
| Conflict (UCDP) | 1.35*** | | |
| | (0.25) | | |
| Months to Election * Conflict | −0.02*** | | |
| | (0.01) | | |
| Insurgency (NAVCO) | | 0.67** | |
| | | (0.31) | |
| Months to Election * Insurgency | | −0.02 | |
| | | (0.02) | |
| Attack (GTD) | | | 1.06*** |
| | | | (0.25) |
| Months to Election * Attack | | | −0.02 |
| | | | (0.02) |
| Polyarchy | −3.26 | −7.34*** | −4.57 |
| | (3.21) | (2.57) | (3.67) |
| Election Free/Fair | 0.51* | 1.21*** | 0.48** |
| | (0.26) | (0.31) | (0.24) |
| Population | 0.00 | 0.00 | 0.00 |
| | (0.00) | (0.00) | (0.00) |
| GDP per Capita | 0.47 | 0.41 | 0.54 |
| | (0.32) | (0.31) | (0.33) |
| GDP Growth | −5.59*** | −3.46 | −5.48*** |
| | (2.04) | (2.78) | (2.01) |
| Protests | 0.01*** | 0.03*** | 0.01*** |
| | (0.00) | (0.01) | (0.00) |
| $\text{Repression}_{t-1}$ | 0.03*** | 0.01*** | 0.03*** |
| | (0.01) | (0.00) | (0.00) |
| Unemployment | 0.02 | 0.01 | 0.02 |
| | (0.02) | (0.02) | (0.02) |
| Inflation | −0.02** | −0.02*** | −0.01 |
| | (0.01) | (0.01) | (0.01) |
| Education | −0.01 | 0.06 | −0.02 |
| | (0.09) | (0.07) | (0.08) |
| Freedom of Expression | −2.11 | −3.62* | −0.90 |
| | (2.08) | (2.01) | (2.95) |
| Constant | −1.86 | 1.27 | −2.54 |
| | (2.24) | (2.16) | (2.66) |
| Observations | 9,021 | 4,424 | 9,021 |

*Notes*: Dependent variable is the count of the number of repressive actions in each month. Standard errors clustered by country. Sample is the 24 months before an executive election (presidential, parliamentary, semipresidential) in a democracy. ***, **, * significant at .01, .05, .10, respectively.

Figure A.6: Predicted Repression, Table A.5, Models 1-3




*Notes*: Predicted levels of repression based on the months to the next executive election and the presence of ongoing conflict. 95% confidence intervals. All control variable set to means. Outcome variable: count of repressive actions perpetrated by the government in each month.

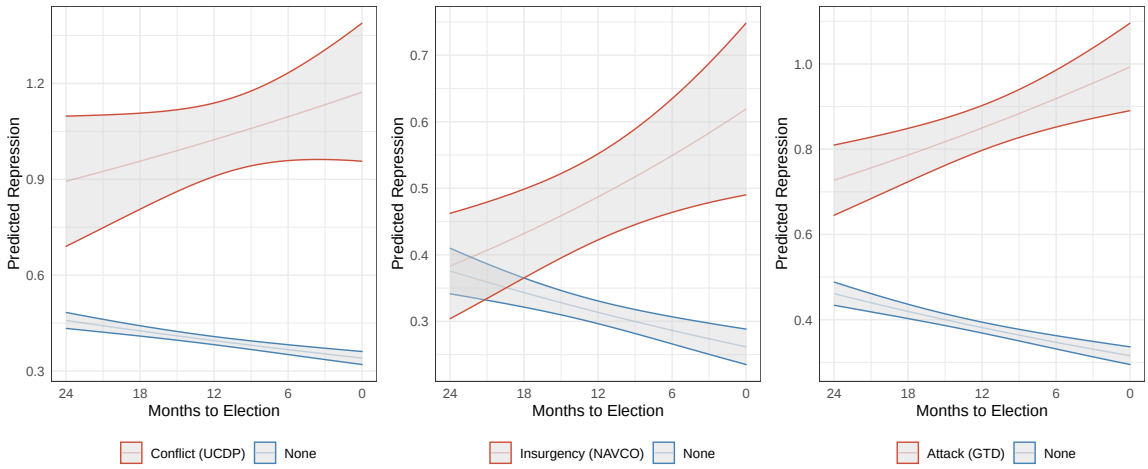
### A.7.2 Free/Fair Elections

Table A.6: Election Timing and Repression: Free/Fair Elections

| | Conflict | Insurgency | Terrorism |
|---|---|---|---|
| | (1) | (2) | (3) |
| Months to Election | 0.01* | 0.02** | 0.02** |
| | (0.01) | (0.01) | (0.01) |
| Conflict (UCDP) | 1.24*** | | |
| | (0.32) | | |
| Months to Election * Conflict | −0.02** | | |
| | (0.01) | | |
| Insurgency (NAVCO) | | 0.86*** | |
| | | (0.25) | |
| Months to Election * Insurgency | | −0.04* | |
| | | (0.02) | |
| Attack (GTD) | | | 1.15*** |
| | | | (0.24) |
| Months to Election * Attack | | | −0.03* |
| | | | (0.02) |
| Polyarchy | −4.13** | −7.16*** | −4.65*** |
| | (1.89) | (1.23) | (1.65) |
| Free/Fair Election | 0.58*** | 0.86*** | 0.58*** |
| | (0.23) | (0.21) | (0.21) |
| Population | 0.00 | 0.00 | 0.00 |
| | (0.00) | (0.00) | (0.00) |
| GDP per Capita | 0.44* | 0.61*** | 0.46** |
| | (0.23) | (0.23) | (0.22) |
| GDP Growth | −5.46*** | −3.87* | −5.25*** |
| | (1.98) | (2.05) | (1.91) |
| Protests | 0.01*** | 0.03*** | 0.01*** |
| | (0.00) | (0.00) | (0.00) |
| $\text{Repression}_{t-1}$ | 0.03*** | 0.02*** | 0.03*** |
| | (0.01) | (0.00) | (0.00) |
| Constant | −2.87** | −3.02* | −2.77** |
| | (1.28) | (1.57) | (1.25) |
| Observations | 9,021 | 4,424 | 9,021 |

*Notes*: Dependent variable is the count of the number of repressive actions in each month. Standard errors clustered by country. Sample is the 24 months before an executive election (presidential, parliamentary, semipresidential) in a democracy with free/fair elections. ***, **, * significant at .01, .05, .10, respectively.

Figure A.7: Predicted Repression, Table A.6, Models 1-3




*Notes*: Predicted levels of repression based on the months to the next executive election and the presence of ongoing conflict. 95% confidence intervals. All control variable set to means. Outcome variable: count of repressive actions perpetrated by the government in each month.

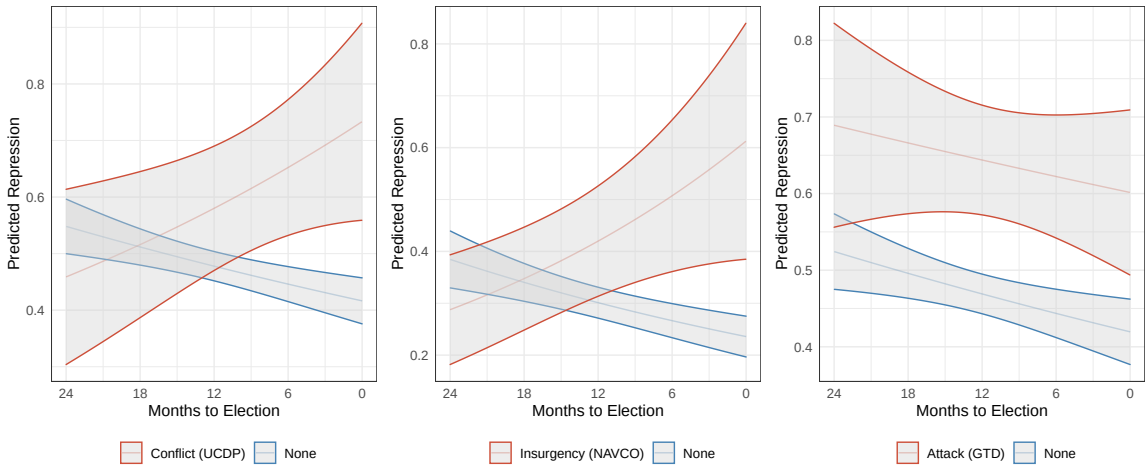
### A.7.3 Heterogeneous Effects by Incumbent Ideology: Right-Wing Leaders

Table A.7: Election Timing and Repression: Right-Wing Leaders

| | Conflict | Insurgency | Terrorism |
|---|---|---|---|
| | (1) | (2) | (3) |
| Months to Election | 0.01 | 0.02** | 0.01 |
| | (0.01) | (0.01) | (0.01) |
| Conflict (UCDP) | 0.57 | | |
| | (0.54) | | |
| Months to Election * Conflict | −0.03** | | |
| | (0.01) | | |
| Insurgency (NAVCO) | | 0.96** | |
| | | (0.38) | |
| Months to Election * Insurgency | | −0.05*** | |
| | | (0.02) | |
| Attack (GTD) | | | 0.36 |
| | | | (0.40) |
| Months to Election * Attack | | | −0.00 |
| | | | (0.02) |
| Polyarchy | −7.96** | −10.00*** | −8.41*** |
| | (3.39) | (1.96) | (2.56) |
| Free/Fair Election | 0.84** | 1.36*** | 0.86*** |
| | (0.37) | (0.29) | (0.33) |
| Population | 0.00 | 0.00 | 0.00** |
| | (0.00) | (0.00) | (0.00) |
| GDP per Capita | 0.82** | 1.01*** | 0.84** |
| | (0.41) | (0.35) | (0.39) |
| GDP Growth | −6.60*** | −8.58** | −6.67*** |
| | (2.21) | (3.47) | (2.40) |
| Protests | 0.02*** | 0.03*** | 0.02*** |
| | (0.00) | (0.00) | (0.00) |
| $\text{Repression}_{t-1}$ | 0.02*** | 0.01*** | 0.02*** |
| | (0.00) | (0.00) | (0.00) |
| Constant | −4.09 | −5.80** | −3.96 |
| | (2.55) | (2.48) | (2.54) |
| Observations | 3,317 | 1,828 | 3,317 |

*Notes*: Dependent variable is the count of the number of repressive actions in each month. Standard errors clustered by country. Sample is the 24 months before an executive election (presidential, parliamentary, semipresidential) in a democracy with a right-wing incumbent. ***, **, * significant at .01, .05, .10, respectively.

Figure A.8: Predicted Repression, Table A.7, Models 1-3




*Notes*: Predicted levels of repression based on the months to the next executive election and the presence of ongoing conflict. 95% confidence intervals. All control variable set to means. Outcome variable: count of repressive actions perpetrated by the government in each month.

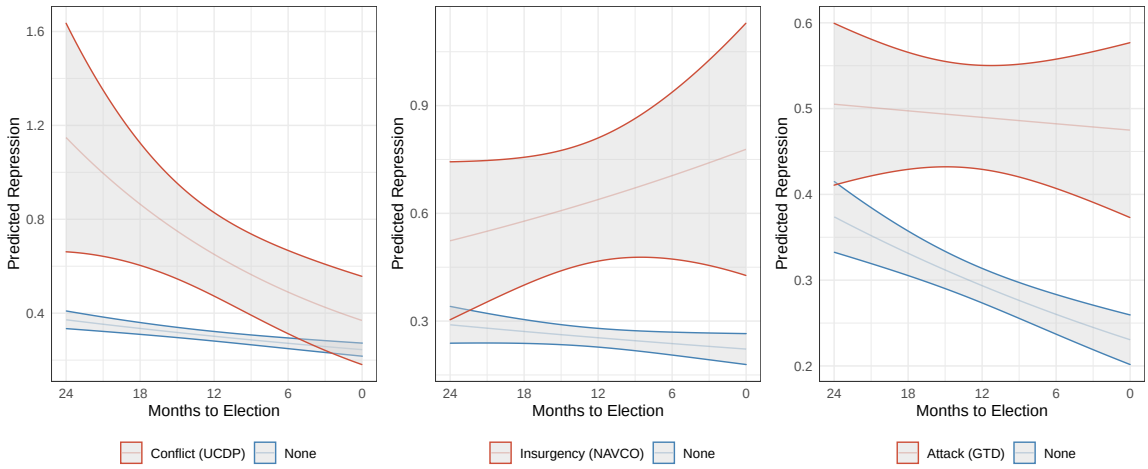
### A.7.4 Heterogeneous Effects by Incumbent Ideology: Left-Wing Leaders

Table A.8: Election Timing and Repression: Left-Wing Leaders

| | Conflict | Insurgency | Terrorism |
|---|---|---|---|
| | (1) | (2) | (3) |
| Months to Election | 0.02** | 0.01 | 0.02** |
| | (0.01) | (0.02) | (0.01) |
| Conflict (UCDP) | 0.41 | | |
| | (1.42) | | |
| Months to Election * Conflict | 0.03 | | |
| | (0.02) | | |
| Insurgency (NAVCO) | | 1.25*** | |
| | | (0.39) | |
| Months to Election * Insurgency | | −0.03 | |
| | | (0.02) | |
| Attack (GTD) | | | 0.72** |
| | | | (0.32) |
| Months to Election * Attack | | | −0.02 |
| | | | (0.02) |
| Polyarchy | −2.85 | −6.70*** | −2.78 |
| | (2.15) | (1.82) | (2.06) |
| Free/Fair Election | 0.92*** | 1.01*** | 0.92*** |
| | (0.25) | (0.32) | (0.25) |
| Population | 0.00 | 0.00 | 0.00** |
| | (0.00) | (0.00) | (0.00) |
| GDP per Capita | 0.30 | 0.53** | 0.26 |
| | (0.26) | (0.26) | (0.24) |
| GDP Growth | −10.03** | 0.66 | −10.09** |
| | (4.27) | (3.28) | (4.06) |
| Protests | 0.00 | 0.02** | 0.00** |
| | (0.00) | (0.01) | (0.00) |
| $\text{Repression}_{t-1}$ | 0.06*** | 0.09*** | 0.05*** |
| | (0.01) | (0.02) | (0.01) |
| Constant | −3.24* | −3.11** | −2.96* |
| | (1.71) | (1.53) | (1.65) |
| Observations | 3,626 | 1,972 | 3,626 |

*Notes*: Dependent variable is the count of the number of repressive actions in each month. Standard errors clustered by country. Sample is the 24 months before an executive election (presidential, parliamentary, semipresidential) in a democracy with a left-wing incumbent. ***, **, * significant at .01, .05, .10, respectively.

Figure A.9: Predicted Repression, Table A.8, Models 1-3




*Notes*: Predicted levels of repression based on the months to the next executive election and the presence of ongoing conflict. 95% confidence intervals. All control variable set to means. Outcome variable: count of repressive actions perpetrated by the government in each month.

## A.8 Repression of Non-Threatening Groups

Table A.9: Election Timing and Repression (of Non-Threatening Groups)

| | Bivariate | Controls | Conflict | Insurgency | Terrorism |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Months to Election | −0.01** | −0.01* | −0.01 | −0.02*** | −0.01 |
| | (0.00) | (0.00) | (0.01) | (0.01) | (0.01) |
| Conflict (UCDP) | | | 1.05** | | |
| | | | (0.47) | | |
| Months to Election * Conflict | | | −0.00 | | |
| | | | (0.01) | | |
| Insurgency (NAVCO) | | | | 0.38 | |
| | | | | (0.28) | |
| Months to Election * Insurgency | | | | 0.01 | |
| | | | | (0.01) | |
| Attacks (GTD) | | | | | 0.71*** |
| | | | | | (0.20) |
| Months to Election * Attack | | | | | −0.01 |
| | | | | | (0.01) |
| Polyarchy | | −5.89*** | −5.03*** | −6.86*** | −5.69*** |
| | | (1.36) | (1.40) | (1.65) | (1.32) |
| Free/Fair Election | | 0.53** | 0.47** | 0.62** | 0.50** |
| | | (0.23) | (0.22) | (0.25) | (0.22) |
| Population | | 0.00* | 0.00 | 0.00*** | 0.00 |
| | | (0.00) | (0.00) | (0.00) | (0.00) |
| GDP per capita | | 0.64*** | 0.64*** | 0.78*** | 0.64*** |
| | | (0.17) | (0.17) | (0.22) | (0.17) |
| GDP Growth | | −4.05** | −3.50** | −3.85* | −3.64** |
| | | (1.85) | (1.76) | (2.23) | (1.71) |
| Protests | | 0.00 | 0.00 | 0.02*** | 0.00 |
| | | (0.00) | (0.00) | (0.00) | (0.00) |
| $\text{Repression}_{t-1}$ | | 0.02* | 0.02** | 0.03*** | 0.02** |
| | | (0.01) | (0.01) | (0.00) | (0.01) |
| Constant | 0.68** | −2.07 | −2.70* | −2.97* | −2.29 |
| | (0.30) | (1.40) | (1.46) | (1.58) | (1.40) |
| Observations | 14,121 | 10,701 | 10,701 | 5,248 | 10,701 |

*Notes*: Dependent variable is the count of the number of repressive actions against **non-threatening groups** in each month. Standard errors clustered by country. Sample is the 24 months before an executive election (presidential, parliamentary, semipresidential) in a democracy. ***, **, * significant at .01, .05, .10, respectively.

## A.9 Effect of Timing on Conflict

Table A.10: Election Timing and Conflict Onset

| | Conflict | Insurgency | Attack |
|---|---|---|---|
| | (1) | (2) | (3) |
| Months to Election | −0.01 | −0.01 | −0.00 |
| | (0.02) | (0.01) | (0.01) |
| Polyarchy | −29.66*** | −15.96** | −4.26 |
| | (5.20) | (6.27) | (3.18) |
| Free/Fair Election | 0.73 | 0.84* | 0.39 |
| | (0.88) | (0.49) | (0.41) |
| Population | 0.00*** | 0.00** | 0.00*** |
| | (0.00) | (0.00) | (0.00) |
| GDP per Capita | 1.00*** | 0.81 | 0.36 |
| | (0.39) | (0.51) | (0.27) |
| GDP Growth | −18.18*** | −6.20 | −5.07** |
| | (6.74) | (6.52) | (2.44) |
| Protests | −0.04*** | −0.02 | 0.00 |
| | (0.02) | (0.01) | (0.02) |
| Repression | 0.08*** | 0.02 | 0.04 |
| | (0.02) | (0.02) | (0.06) |
| Constant | 5.33 | −0.24 | −3.33 |
| | (3.80) | (3.44) | (2.09) |
| Observations | 10,709 | 5,216 | 10,709 |

*Notes*: Dependent variable is the presence of civil conflict (M1), violent insurgency (M2), and domestic terrorism (M3). Standard errors clustered by country. Sample is the 24 months before an executive election (presidential, parliamentary, semipresidential) in a democracy. ***, **, * significant at .01, .05, .10, respectively.

# Appendix B Paper 2 Additional Material

## B.1 Top Issues in the 2020 Gubernatorial Races

Table B.1 shows the results of a survey of all candidate platforms for the 2020 gubernatorial elections (N = 122). Gubernatorial races took place in Delaware, Indiana, Missouri, Montana, New Hampshire, North Carolina, North Dakota, Utah, Vermont, Washington, and West Virginia. I reviewed every candidate's website and noted each issue that was featured in at least one of the candidates' platforms. The Table shows the issues that were mentioned in at least five platforms, with the total number of campaigns where the issue was featured listed in the right-hand column.

Table B.1: Top Issues in 2020 Gubernatorial Campaigns

| **Issue** | **Campaigns** |
|---|---|
| Education | 48 |
| Economy | 33 |
| Healthcare | 30 |
| Guns | 25 |
| Environment | 19 |
| Taxes | 19 |
| Jobs | 16 |
| Corruption | 15 |
| COVID-19 | 15 |
| Abortion | 11 |
| Homelessness | 10 |
| Transportation | 10 |
| Safety | 10 |
| Rural Areas | 9 |
| Opioid Crisis | 8 |
| Police | 8 |
| Public Lands | 8 |
| Voting | 8 |
| Budget | 7 |
| Infrastructure | 7 |
| Veterans | 7 |
| Climate Crisis | 6 |
| Efficiency | 6 |
| Energy | 6 |
| Racial Justice | 6 |
| Emergency Services | 5 |
| Family | 5 |
| Immigration | 5 |
| Workers | 5 |

*Notes*: Issues addressed in at least five gubernatorial platforms in the 2020 elections (out of 122 total candidates). Issues in gray included as attributes in the conjoint experiment.

## B.2 Coding of Demographic Control Variables

- *Gender*: male, other, female (omitted)
- *Income*: low (omitted: combines less than $30,000 and $30,000-$50,000), high (combines $50,000-$74,999 and $75,000 or more)
- *Education*: less than college (omitted: combines less than high school degree, high school degree or equivalent, some college but no degree), college (combines Bachelor's degree and Graduate degree)
- *Age*: young (omitted: 18-34), middle aged (combines 35-49 and 50-64), old (65+)
- *Race*: white (omitted), Black or African American, Hispanic/Latino, other
- *Community*: rural (omitted), large city, suburb near a large city, small city or town
- *Region*: Northeast (Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island, Vermont, New Jersey, New York, Pennsylvania), Midwest (Illinois, Indiana, Michigan, Ohio, Wisconsin, Iowa, Kansas, Minnesota, Missouri, Nebraska, North Dakota, South Dakota), South (omitted: Delaware, Florida, Georgia, Maryland, North Carolina, South Carolina, Virginia, District of Columbia, West Virginia, Alabama, Kentucky, Mississippi, Tennessee, Arkansas, Louisiana, Oklahoma, Texas), West (Arizona, Colorado, Idaho, Montana, Nevada, New Mexico, Utah, Wyoming, Alaska, California, Hawaii, Oregon, Washington)
- *Party ID*: Democrat (omitted), Republican, Independent, other (combines no preference, other party, and don't know)
- *Ideology*: liberal (omitted: combines very liberal and liberal), moderate, conservative (combines very conservative and conservative), other

## B.3 Balance Checks

All of the treatments in the conjoint design are fully randomized across candidate profiles. Therefore, on average, one would expect respondents' demographic features to be balanced across the treatments. However, there may be chance imbalance along two lines:

(1) individuals with particular demographic features may be more likely to receive a certain treatment and (2) some candidate attributes may be more likely to appear with other attributes (Hainmueller, Hopkins and Yamamoto, 2014). This section evaluates and addresses possible imbalance across these categories.

First, Table B.2 uses an ordered probit model to regress the five treatment categories on demographic features of respondents, using the control condition of support for all protests as the baseline category. I do not use this full sample for any of the hypothesis tests. However, the model evaluates whether any of the six blocks used for the hypothesis tests are significantly correlated with receiving one type of treatment over another. The results rule out this possibility. Compared to the omitted baseline categories (BlM in-group and WN in-group), the sub-samples for non-threatening out-groups and threatening out-groups are not significantly correlated with any of the four treatment conditions compared to the baseline condition. This analysis confirms the effectiveness of the block randomization scheme, allowing for sub-sample analysis of the hypothesis tests.

Second, Tables B.3, B.4, and B.5 again use ordered probit models to regress the treatment conditions on demographic features in the sub-samples used for the hypothesis tests: among in-groups, those who perceive the target to be a non-threatening out-group, and those who perceive the target to be a threatening out-group. There is some imbalance across a few demographic features in these samples, as would be expected from the relatively small sample sizes. To address the possibility that these imbalanced features could bias the results of the hypothesis tests, in Table B.6 I re-run the results including all demographic covariates. In Table B.7, I re-run the results including only those demographic features that were imbalanced in each of the sub-samples. Across these models, the results are robust.

Finally, Table B.8 replicates the main hypothesis tests while including indicators for all candidate attributes. The results also remain unchanged with these controls. This analysis has the added benefit of indicating the relative strength of the size of the effect for repression on candidates' vote share compared to other attributes of interest, as I discuss in the main paper.

### B.3.1 Treatment Assignment in Full Sample

Table B.2: Balance Check: Full Sample

| | Anti-BLM Violence | Anti-WN Violence | Deny BLM Permits | Deny WN Permits |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| BLM out/non-threat | 0.94 | 0.97 | 0.90 | 1.07 |
| | (0.07) | (0.07) | (0.07) | (0.07) |
| WN out/non-threat | 1.01 | 0.98 | 1.07 | 1.05 |
| | (0.08) | (0.08) | (0.08) | (0.08) |
| BLM out/threat | 0.95 | 0.93 | 0.94 | 1.01 |
| | (0.08) | (0.08) | (0.08) | (0.08) |
| WN out/threat | 1.09 | 1.08 | 0.99 | 1.07 |
| | (0.08) | (0.09) | (0.09) | (0.09) |
| Male | 0.99 | 1.08 | 0.99 | 0.93 |
| | (0.06) | (0.06) | (0.06) | (0.06) |
| Other Gender | 0.71 | 0.90 | 0.69 | 0.75 |
| | (0.51) | (0.49) | (0.49) | (0.51) |
| High Income | 1.04 | 0.96 | 1.00 | 0.96 |
| | (0.06) | (0.06) | (0.06) | (0.06) |
| College | 1.02 | 0.97 | 1.01 | 1.06 |
| | (0.06) | (0.06) | (0.06) | (0.06) |
| Middle Age | 0.88* | 0.92 | 0.90 | 0.84** |
| | (0.07) | (0.07) | (0.07) | (0.07) |
| Old Age | 0.98 | 0.95 | 0.94 | 0.90 |
| | (0.09) | (0.09) | (0.09) | (0.09) |
| Black | 0.82** | 0.90 | 0.83** | 0.99 |
| | (0.10) | (0.10) | (0.10) | (0.09) |
| Latino | 1.00 | 0.95 | 0.98 | 0.85* |
| | (0.09) | (0.09) | (0.09) | (0.09) |
| Other Race | 0.97 | 1.05 | 1.14 | 0.99 |
| | (0.11) | (0.11) | (0.11) | (0.11) |
| City | 1.08 | 1.05 | 0.97 | 1.02 |
| | (0.08) | (0.08) | (0.08) | (0.08) |
| Suburb | 0.94 | 0.91 | 0.87* | 0.94 |
| | (0.08) | (0.08) | (0.08) | (0.08) |
| Town | 1.02 | 0.97 | 0.92 | 1.00 |
| | (0.09) | (0.09) | (0.09) | (0.09) |
| Republican | 1.01 | 1.03 | 1.02 | 1.06 |
| | (0.07) | (0.07) | (0.07) | (0.07) |
| Independent | 1.01 | 1.10 | 1.17 | 0.95 |
| | (0.11) | (0.11) | (0.11) | (0.12) |
| Other Party | 1.02 | 0.89 | 1.12 | 0.79 |
| | (0.18) | (0.19) | (0.18) | (0.19) |
| Moderate | 0.99 | 1.08 | 0.99 | 1.05 |
| | (0.08) | (0.08) | (0.08) | (0.08) |
| Conservative | 1.00 | 1.06 | 0.98 | 1.05 |
| | (0.08) | (0.08) | (0.08) | (0.08) |
| Other Ideology | 1.15 | 1.20 | 1.05 | 1.31 |
| | (0.20) | (0.20) | (0.20) | (0.20) |
| Northeast | 1.05 | 1.10 | 1.08 | 1.03 |
| | (0.08) | (0.08) | (0.08) | (0.08) |
| Midwest | 1.08 | 1.03 | 1.03 | 0.92 |
| | (0.07) | (0.07) | (0.07) | (0.07) |
| West | 1.03 | 1.03 | 1.05 | 1.01 |
| | (0.07) | (0.07) | (0.07) | (0.07) |
| Constant | 1.06 | 0.96 | 1.14 | 1.03 |
| | (0.13) | (0.13) | (0.13) | (0.13) |

*Notes*: Dependent variable is treatment condition with baseline category as "support all protesters." Full sample of conjoint tasks for all respondents (N = 15000). Multinomial logit models with relative risk ratios reported. ***, **, * significant at .01, .05, .10, respectively. Omitted category for group identification is in-group members. Other omitted categories listed in Appendix Section B.2.

### B.3.2 Treatment Assignment in Sub-Samples

Table B.3: Balance Check: In-Group Samples

| | *BLM In-Group Members* | | | | *WN In-Group Members* | | | |
|---|---|---|---|---|---|---|---|---|
| | Anti-BLM Violence | Anti-WN Violence | Deny BLM Permits | Deny WN Permits | Anti-BLM Violence | Anti-WN Violence | Deny BLM Permits | Deny WN Permits |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Male | 1.04 | 1.11 | 1.00 | 0.97 | 1.06 | 0.84 | 1.30 | 0.94 |
| | (0.09) | (0.09) | (0.09) | (0.09) | (0.17) | (0.17) | (0.18) | (0.17) |
| Other Gend. | 0.38 | 0.93 | 0.82 | 0.50 | 1.00 | 1.00 | 1.00 | 1.00 |
| | (0.83) | (0.63) | (0.67) | (0.73) | (0.00) | | | |
| High Income | 0.97 | 0.94 | 0.94 | 0.90 | 0.94 | 0.90 | 1.08 | 1.08 |
| | (0.10) | (0.10) | (0.10) | (0.10) | (0.21) | (0.21) | (0.21) | (0.22) |
| College | 0.98 | 0.94 | 0.99 | 1.15 | 1.41* | 0.96 | 1.13 | 0.99 |
| | (0.10) | (0.10) | (0.10) | (0.10) | (0.20) | (0.20) | (0.20) | (0.21) |
| Middle Age | 0.81** | 0.87 | 0.80** | 0.78** | 0.65*** | 0.85 | 0.75* | 0.84 |
| | (0.09) | (0.09) | (0.09) | (0.09) | (0.15) | (0.16) | (0.16) | (0.16) |
| Old Age | 0.92 | 0.97 | 0.99 | 1.03 | 0.78 | 0.86 | 1.20 | 0.93 |
| | (0.16) | (0.17) | (0.16) | (0.16) | (0.40) | (0.41) | (0.38) | (0.40) |
| Black | 0.80** | 0.79** | 0.81* | 1.09 | 0.91 | 0.84 | 0.77 | 1.35 |
| | (0.11) | (0.12) | (0.12) | (0.11) | (0.27) | (0.27) | (0.28) | (0.26) |
| Latino | 0.97 | 0.89 | 1.04 | 0.97 | 1.50 | 0.55** | 1.04 | 0.72 |
| | (0.14) | (0.14) | (0.14) | (0.14) | (0.25) | (0.28) | (0.26) | (0.28) |
| Other Race | 0.79 | 1.12 | 1.10 | 1.07 | 0.67 | 1.00 | 2.17 | 1.90 |
| | (0.17) | (0.16) | (0.16) | (0.17) | (0.64) | (0.61) | (0.54) | (0.56) |
| City | 0.97 | 0.88 | 0.99 | 0.89 | 0.84 | 0.94 | 0.93 | 0.88 |
| | (0.13) | (0.13) | (0.13) | (0.13) | (0.18) | (0.19) | (0.19) | (0.19) |
| Suburb | 0.91 | 0.77* | 0.89 | 0.81 | 0.89 | 0.71 | 0.93 | 0.73 |
| | (0.13) | (0.13) | (0.13) | (0.13) | (0.21) | (0.21) | (0.21) | (0.22) |
| Town | 0.80 | 0.71** | 0.80 | 0.92 | 0.53** | 0.42*** | 0.56* | 1.15 |
| | (0.16) | (0.17) | (0.17) | (0.16) | (0.31) | (0.34) | (0.32) | (0.30) |
| Republican | 1.04 | 1.04 | 1.03 | 1.08 | 1.24 | 1.19 | 1.50** | 1.26 |
| | (0.11) | (0.11) | (0.11) | (0.11) | (0.16) | (0.16) | (0.16) | (0.16) |
| Independent | 0.76 | 0.83 | 0.92 | 0.67* | 0.82 | 0.86 | 1.31 | 0.71 |
| | (0.21) | (0.21) | (0.20) | (0.22) | (0.34) | (0.33) | (0.32) | (0.35) |
| Other Party | 0.81 | 1.07 | 0.96 | 1.07 | 1.08 | 1.25 | 1.33 | 2.30 |
| | (0.34) | (0.34) | (0.33) | (0.34) | (0.78) | (0.86) | (0.81) | (0.78) |
| Moderate | 0.99 | 1.16 | 1.11 | 1.01 | 1.03 | 0.99 | 1.01 | 1.01 |
| | (0.11) | (0.11) | (0.11) | (0.11) | (0.19) | (0.20) | (0.20) | (0.20) |
| Conservative | 0.94 | 1.01 | 1.03 | 1.04 | 1.01 | 1.00 | 0.95 | 0.89 |
| | (0.11) | (0.11) | (0.11) | (0.11) | (0.17) | (0.17) | (0.17) | (0.18) |
| Other Ideol. | 1.13 | 0.89 | 1.16 | 0.87 | 0.92 | 1.04 | 1.37 | 1.95 |
| | (0.32) | (0.33) | (0.31) | (0.33) | (0.50) | (0.50) | (0.48) | (0.46) |
| Northeast | 1.13 | 1.07 | 1.05 | 0.95 | 1.17 | 1.01 | 0.82 | 0.83 |
| | (0.12) | (0.12) | (0.12) | (0.12) | (0.20) | (0.20) | (0.20) | (0.21) |
| Midwest | 0.97 | 0.99 | 0.93 | 0.97 | 1.06 | 1.24 | 1.11 | 0.89 |
| | (0.13) | (0.13) | (0.13) | (0.13) | (0.20) | (0.20) | (0.20) | (0.21) |
| West | 1.08 | 0.91 | 1.01 | 1.01 | 1.21 | 1.15 | 0.99 | 1.06 |
| | (0.11) | (0.12) | (0.11) | (0.12) | (0.16) | (0.17) | (0.16) | (0.17) |
| Constant | 1.36* | 1.29 | 1.26 | 1.18 | 1.04 | 1.54 | 0.83 | 1.19 |
| | (0.18) | (0.18) | (0.18) | (0.18) | (0.32) | (0.32) | (0.32) | (0.32) |

*Notes*: Dependent variable is treatment condition with baseline category as "support all protesters." Samples of BLM in-group members for M1 - M4 (N = 6120) and WN in-group members for M5 - M8 (N = 2800). Multinomial logit models with relative risk ratios reported. ***, **, * significant at .01, .05, .10, respectively. Omitted categories listed in Appendix Section B.2.

Table B.4: Balance Check: Out-group / Non-threatening Samples

| | *BLM Out-Group / Non-Threatening* | | | | *WN Out-Group / Non-Threatening* | | | |
|---|---|---|---|---|---|---|---|---|
| | *BLM In-Group Members* | | | | *WN In-Group Members* | | | |
| | Anti-BLM Violence | Anti-WN Violence | Deny BLM Permits | Deny WN Permits | Anti-BLM Violence | Anti-WN Violence | Deny BLM Permits | Deny WN Permits |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Male | 0.95 | 1.06 | 1.00 | 0.94 | 0.97 | 1.14 | 0.94 | 0.91 |
| | (0.10) | (0.10) | (0.10) | (0.10) | (0.09) | (0.09) | (0.09) | (0.09) |
| Other Gend. | 0.67 | 0.55 | 0.59 | 0.80 | 0.56 | 0.87 | 0.70 | 0.87 |
| | (0.76) | (0.84) | (0.79) | (0.79) | (0.54) | (0.51) | (0.51) | (0.53) |
| High Income | 1.04 | 1.01 | 1.05 | 1.03 | 1.08 | 0.96 | 1.00 | 1.06 |
| | (0.10) | (0.10) | (0.10) | (0.10) | (0.10) | (0.10) | (0.10) | (0.10) |
| College | 1.00 | 0.95 | 1.06 | 1.09 | 1.04 | 0.96 | 1.00 | 0.97 |
| | (0.10) | (0.10) | (0.10) | (0.10) | (0.09) | (0.10) | (0.09) | (0.09) |
| Middle Age | 1.02 | 1.10 | 1.02 | 0.95 | 0.96 | 0.93 | 0.92 | 0.90 |
| | (0.13) | (0.13) | (0.13) | (0.13) | (0.11) | (0.11) | (0.10) | (0.10) |
| Old Age | 1.08 | 1.22 | 0.94 | 0.96 | 1.09 | 0.99 | 0.99 | 0.89 |
| | (0.14) | (0.15) | (0.15) | (0.15) | (0.13) | (0.13) | (0.13) | (0.13) |
| Black | 0.98 | 1.12 | 0.90 | 0.64* | 0.85 | 0.90 | 0.75** | 0.89 |
| | (0.25) | (0.24) | (0.26) | (0.27) | (0.14) | (0.15) | (0.14) | (0.14) |
| Latino | 1.04 | 0.88 | 0.83 | 0.63*** | 1.06 | 1.08 | 0.95 | 0.84 |
| | (0.15) | (0.16) | (0.16) | (0.16) | (0.14) | (0.15) | (0.14) | (0.15) |
| Other Race | 1.20 | 1.05 | 1.15 | 1.09 | 1.19 | 1.17 | 1.09 | 1.04 |
| | (0.18) | (0.18) | (0.18) | (0.18) | (0.17) | (0.17) | (0.17) | (0.17) |
| City | 1.03 | 1.05 | 0.83 | 0.94 | 1.08 | 1.06 | 0.95 | 1.13 |
| | (0.14) | (0.14) | (0.14) | (0.14) | (0.13) | (0.13) | (0.13) | (0.13) |
| Suburb | 0.94 | 0.96 | 0.85 | 0.97 | 1.06 | 1.04 | 0.89 | 1.05 |
| | (0.13) | (0.13) | (0.13) | (0.12) | (0.12) | (0.12) | (0.12) | (0.12) |
| Town | 1.03 | 1.05 | 0.80 | 0.85 | 1.00 | 1.01 | 0.89 | 1.09 |
| | (0.16) | (0.16) | (0.16) | (0.16) | (0.14) | (0.14) | (0.14) | (0.14) |
| Republican | 1.08 | 1.08 | 1.17 | 1.15 | 0.94 | 1.06 | 0.92 | 0.97 |
| | (0.11) | (0.12) | (0.12) | (0.11) | (0.11) | (0.11) | (0.10) | (0.10) |
| Independent | 1.26 | 1.37* | 1.45** | 1.16 | 1.01 | 1.17 | 1.26 | 0.91 |
| | (0.17) | (0.17) | (0.17) | (0.17) | (0.19) | (0.19) | (0.18) | (0.19) |
| Other Party | 1.23 | 0.95 | 1.44 | 0.75 | 1.21 | 1.00 | 1.42 | 0.70 |
| | (0.24) | (0.26) | (0.25) | (0.27) | (0.27) | (0.28) | (0.26) | (0.29) |
| Moderate | 0.85 | 0.89 | 0.72*** | 0.90 | 0.98 | 0.94 | 0.93 | 1.09 |
| | (0.12) | (0.12) | (0.12) | (0.12) | (0.13) | (0.13) | (0.13) | (0.13) |
| Conservative | 0.97 | 0.95 | 0.89 | 0.87 | 0.98 | 0.93 | 1.00 | 1.16 |
| | (0.14) | (0.14) | (0.14) | (0.14) | (0.13) | (0.14) | (0.13) | (0.14) |
| Other Ideol. | 1.03 | 1.29 | 0.91 | 1.49 | 1.18 | 1.10 | 0.76 | 1.28 |
| | (0.29) | (0.29) | (0.30) | (0.29) | (0.29) | (0.30) | (0.30) | (0.30) |
| Northeast | 1.03 | 1.07 | 1.13 | 1.15 | 0.97 | 1.03 | 1.15 | 1.05 |
| | (0.14) | (0.14) | (0.14) | (0.14) | (0.14) | (0.14) | (0.13) | (0.13) |
| Midwest | 1.32** | 1.13 | 1.20 | 0.99 | 1.14 | 0.99 | 1.06 | 0.96 |
| | (0.12) | (0.12) | (0.12) | (0.12) | (0.11) | (0.11) | (0.11) | (0.11) |
| West | 1.31** | 1.38*** | 1.29** | 1.15 | 1.07 | 1.03 | 0.99 | 0.96 |
| | (0.12) | (0.12) | (0.12) | (0.12) | (0.12) | (0.12) | (0.12) | (0.12) |
| Constant | 0.87 | 0.77 | 1.01 | 1.08 | 0.92 | 0.91 | 1.26 | 1.01 |
| | (0.19) | (0.19) | (0.19) | (0.19) | (0.18) | (0.18) | (0.18) | (0.18) |

*Notes*: Dependent variable is treatment condition with baseline category as "support all protesters." Samples of BLM out-group non-threatening for M1 - M4 (N = 5440) and WN out-group non-threatening for M5 - M8 (N = 6140). Multinomial logit models with relative risk ratios reported. ***, **, * significant at .01, .05, .10, respectively. Omitted categories listed in Appendix Section B.2.

Table B.5: Balance Check: Out-Group / Threatening Samples

| | *BLM Out-Group / Threatening* | | | | *WN Out-Group / Threatening* | | | |
|---|---|---|---|---|---|---|---|---|
| | *BLM In-Group Members* | | | | *WN In-Group Members* | | | |
| | Anti-BLM Violence | Anti-WN Violence | Deny BLM Permits | Deny WN Permits | Anti-BLM Violence | Anti-WN Violence | Deny BLM Permits | Deny WN Permits |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Male | 0.84 | 0.93 | 0.92 | 0.74*** | 0.99 | 1.06 | 0.96 | 0.97 |
| | (0.12) | (0.12) | (0.12) | (0.12) | (0.09) | (0.09) | (0.09) | (0.09) |
| Other Gender | 1.00 | 1.00 | 1.00 | 1.00 | 1.00 | 1.00 | 1.00 | 1.00 |
| | | (0.00) | | (0.00) | | | (0.00) | |
| High Income | 1.08 | 0.93 | 1.09 | 0.87 | 1.02 | 0.96 | 1.03 | 0.87 |
| | (0.13) | (0.13) | (0.13) | (0.13) | (0.09) | (0.09) | (0.09) | (0.09) |
| College | 1.12 | 1.07 | 0.97 | 0.89 | 0.97 | 1.01 | 1.03 | 1.12 |
| | (0.13) | (0.13) | (0.13) | (0.13) | (0.09) | (0.09) | (0.09) | (0.09) |
| Middle Age | 0.83 | 0.83 | 0.92 | 0.76* | 0.85 | 0.94 | 0.82* | 0.84 |
| | (0.17) | (0.17) | (0.17) | (0.17) | (0.11) | (0.11) | (0.11) | (0.11) |
| Old Age | 0.97 | 0.77 | 0.95 | 0.75* | 0.90 | 0.93 | 0.83 | 0.95 |
| | (0.17) | (0.17) | (0.17) | (0.17) | (0.13) | (0.13) | (0.13) | (0.13) |
| Black | 0.69 | 1.03 | 0.96 | 0.83 | 0.79 | 0.90 | 0.92 | 0.91 |
| | (0.29) | (0.28) | (0.28) | (0.28) | (0.16) | (0.16) | (0.16) | (0.16) |
| Latino | 1.11 | 1.19 | 1.18 | 0.95 | 0.84 | 0.97 | 0.94 | 0.87 |
| | (0.20) | (0.20) | (0.20) | (0.20) | (0.14) | (0.14) | (0.14) | (0.14) |
| Other Race | 1.15 | 1.11 | 1.23 | 0.71 | 0.79 | 0.96 | 1.10 | 0.94 |
| | (0.32) | (0.33) | (0.32) | (0.35) | (0.16) | (0.16) | (0.16) | (0.16) |
| City | 1.45* | 1.40* | 1.07 | 1.62** | 1.36** | 1.09 | 1.13 | 1.05 |
| | (0.19) | (0.19) | (0.20) | (0.20) | (0.13) | (0.14) | (0.14) | (0.14) |
| Suburb | 0.97 | 0.90 | 0.80 | 1.22 | 0.97 | 0.90 | 0.90 | 0.91 |
| | (0.16) | (0.16) | (0.16) | (0.16) | (0.12) | (0.12) | (0.12) | (0.12) |
| Town | 1.33* | 1.16 | 1.14 | 1.51** | 1.24 | 1.11 | 1.04 | 0.93 |
| | (0.17) | (0.17) | (0.17) | (0.17) | (0.14) | (0.14) | (0.14) | (0.14) |
| Republican | 0.93 | 1.22 | 0.96 | 1.12 | 0.92 | 0.86 | 0.96 | 0.97 |
| | (0.18) | (0.18) | (0.18) | (0.18) | (0.13) | (0.13) | (0.13) | (0.13) |
| Independent | 1.36 | 1.59 | 1.43 | 1.10 | 1.04 | 1.02 | 0.98 | 0.95 |
| | (0.32) | (0.33) | (0.32) | (0.35) | (0.17) | (0.17) | (0.18) | (0.18) |
| Other Party | 0.62 | 0.72 | 0.80 | 0.96 | 0.91 | 0.76 | 0.69 | 0.91 |
| | (0.52) | (0.56) | (0.52) | (0.54) | (0.29) | (0.30) | (0.30) | (0.30) |
| Moderate | 1.32 | 1.17 | 1.46 | 1.95** | 1.05 | 1.22* | 1.09 | 1.05 |
| | (0.26) | (0.27) | (0.27) | (0.30) | (0.11) | (0.12) | (0.12) | (0.12) |
| Conservative | 1.15 | 1.10 | 1.13 | 1.74* | 1.06 | 1.23 | 1.02 | 1.10 |
| | (0.26) | (0.27) | (0.26) | (0.29) | (0.14) | (0.15) | (0.15) | (0.15) |
| Other Ideology | 1.88 | 1.01 | 0.91 | 2.35 | 0.93 | 1.23 | 1.56 | 1.11 |
| | (0.60) | (0.67) | (0.66) | (0.63) | (0.37) | (0.37) | (0.36) | (0.37) |
| Northeast | 1.07 | 1.50* | 1.02 | 0.96 | 1.06 | 1.18 | 1.09 | 1.09 |
| | (0.25) | (0.25) | (0.26) | (0.26) | (0.13) | (0.13) | (0.14) | (0.13) |
| Midwest | 0.85 | 0.84 | 0.95 | 0.68** | 0.98 | 1.03 | 0.95 | 0.86 |
| | (0.15) | (0.15) | (0.15) | (0.15) | (0.12) | (0.12) | (0.12) | (0.12) |
| West | 0.67** | 0.81 | 0.81 | 0.84 | 0.94 | 1.03 | 1.08 | 1.00 |
| | (0.16) | (0.16) | (0.16) | (0.15) | (0.11) | (0.11) | (0.11) | (0.11) |
| Constant | 1.01 | 0.88 | 0.98 | 0.75 | 1.18 | 0.96 | 1.08 | 1.18 |
| | (0.31) | (0.31) | (0.31) | (0.34) | (0.17) | (0.17) | (0.17) | (0.17) |

*Notes*: Dependent variable is treatment condition with baseline category as "support all protesters." Samples of BLM out-group threatening for M1 - M4 (N = 3440) and WN out-group threatening for M5 - M8 (N = 6060). Multinomial logit models with relative risk ratios reported. ***, **, * significant at .01, .05, .10, respectively. Omitted categories listed in Appendix Section B.2.

### B.3.3 Controlling for Demographic Features

Table B.6: Effect of Repression on Vote Share with Controls for Respondent Demographics

| | *In-Group* | | *Out-Group Non-Threatening* | | *Out-Group Threatening* | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | BLM | WN | BLM | WN | BLM | WN |
| Deny BLM Permits | −0.17** | 0.00 | −0.10 | −0.01 | 0.14 | −0.16* |
| | (0.08) | (0.12) | (0.09) | (0.09) | (0.11) | (0.08) |
| Use Violence Against BLM | −0.23*** | −0.05 | −0.13 | −0.00 | 0.13 | −0.24*** |
| | (0.09) | (0.12) | (0.08) | (0.08) | (0.11) | (0.08) |
| Deny WN Permits | −0.06 | −0.07 | −0.06 | −0.07 | −0.16 | −0.11 |
| | (0.08) | (0.13) | (0.09) | (0.08) | (0.11) | (0.08) |
| Use Violence Against WN | −0.18** | −0.13 | −0.06 | −0.11 | −0.09 | −0.12 |
| | (0.08) | (0.11) | (0.09) | (0.08) | (0.12) | (0.09) |
| Male | 0.00 | −0.01 | −0.00 | 0.00 | −0.00 | −0.00 |
| | (0.00) | (0.01) | (0.00) | (0.00) | (0.01) | (0.00) |
| Other Gender | −0.01 | | −0.01 | 0.00 | | |
| | (0.01) | | (0.01) | (0.01) | | |
| High Income | −0.00 | −0.00 | 0.00 | −0.00 | −0.01* | 0.00 |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.01) | (0.00) |
| College | −0.00 | −0.00 | 0.00 | −0.00 | −0.00 | −0.00 |
| | (0.00) | (0.01) | (0.00) | (0.00) | (0.01) | (0.00) |
| Middle Age | −0.00 | −0.00 | 0.00 | −0.00 | −0.01 | −0.00 |
| | (0.00) | (0.01) | (0.00) | (0.00) | (0.01) | (0.00) |
| Old Age | −0.00 | −0.00 | 0.00 | −0.00 | −0.01* | −0.00 |
| | (0.00) | (0.01) | (0.00) | (0.00) | (0.01) | (0.00) |
| Black | −0.01** | 0.00 | −0.00 | 0.00 | 0.00 | −0.01 |
| | (0.00) | (0.01) | (0.01) | (0.00) | (0.01) | (0.00) |
| Latino | −0.00 | −0.01 | 0.00 | −0.00 | −0.01 | −0.00 |
| | (0.00) | (0.01) | (0.01) | (0.00) | (0.01) | (0.00) |
| Other Race | −0.00 | −0.01 | 0.00 | 0.00 | −0.02 | −0.00 |
| | (0.01) | (0.02) | (0.00) | (0.00) | (0.01) | (0.00) |
| City | −0.00 | −0.00 | −0.00 | 0.00 | 0.01 | 0.01* |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.01) | (0.00) |
| Suburb | −0.00 | −0.01 | −0.00 | 0.00 | 0.01 | −0.00 |
| | (0.00) | (0.01) | (0.00) | (0.00) | (0.01) | (0.00) |
| Town | −0.01* | −0.01 | −0.00 | 0.00 | 0.01 | 0.01 |
| | (0.01) | (0.02) | (0.00) | (0.00) | (0.01) | (0.00) |
| Republican | 0.00 | −0.00 | 0.00 | 0.00 | 0.01 | −0.00 |
| | (0.00) | (0.01) | (0.00) | (0.00) | (0.01) | (0.00) |
| Independent | −0.00 | −0.01 | 0.00 | 0.00 | −0.01 | 0.00 |
| | (0.01) | (0.01) | (0.00) | (0.00) | (0.01) | (0.00) |
| Other Party | −0.00 | 0.00 | 0.01 | −0.01 | 0.01 | −0.00 |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| Moderate | 0.00 | 0.00 | −0.00 | 0.00 | 0.00 | 0.00 |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.01) | (0.00) |
| Conservative | −0.00 | 0.00 | −0.00 | −0.00 | 0.01 | 0.00 |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.01) | (0.00) |
| Other Ideology | 0.00 | 0.00 | −0.00 | 0.00 | 0.01 | 0.00 |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.02) | (0.01) |
| Northeast | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| | (0.00) | (0.01) | (0.00) | (0.00) | (0.01) | (0.00) |
| Midwest | −0.00 | 0.00 | 0.00 | −0.00 | −0.01 | −0.00 |
| | (0.00) | (0.01) | (0.00) | (0.00) | (0.01) | (0.00) |
| West | 0.00 | 0.00 | 0.00 | −0.00 | 0.01 | −0.00 |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.01) | (0.00) |
| Constant | 0.14** | 0.06 | 0.07 | 0.04 | −0.01 | 0.13** |
| | (0.06) | (0.08) | (0.05) | (0.05) | (0.07) | (0.05) |
| Observations | 5,960 | 2,660 | 5,260 | 5,880 | 3,340 | 6,020 |

*Notes*: Dependent variable is candidate selection. Standard errors clustered by respondent. Sample is BLM in-group (M1), white nationalists in-group (M2), BLM out-group / non-threatening (M3), white nationalists out-group / non-threatening (M4), BLM out-group / threatening (M5), white nationalists out-group / threatening (M6). ***, **, * significant at .01, .05, .10, respectively. Omitted categories listed in Appendix Section B.2.

Table B.7: Effect of Repression on Vote Share with Controls for Imbalanced Respondent Demographics (see Tables B.3, B.4, and B.5 for Variable Selection)

| | *In-Group* | | *Out-Group Non-Threatening* | | *Out-Group Threatening* | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | BLM | WN | BLM | WN | BLM | WN |
| Deny BLM Permits | −0.17** | 0.00 | −0.09 | −0.03 | 0.13 | −0.17** |
| | (0.08) | (0.12) | (0.09) | (0.08) | (0.11) | (0.08) |
| Use Violence Against BLM | −0.22*** | −0.04 | −0.12 | −0.01 | 0.12 | −0.24*** |
| | (0.09) | (0.12) | (0.08) | (0.08) | (0.11) | (0.08) |
| Deny WN Permits | −0.06 | −0.06 | −0.06 | −0.07 | −0.16 | −0.11 |
| | (0.08) | (0.12) | (0.09) | (0.08) | (0.11) | (0.08) |
| Use Violence Against WN | −0.18** | −0.12 | −0.05 | −0.10 | −0.10 | −0.13 |
| | (0.08) | (0.11) | (0.09) | (0.08) | (0.11) | (0.09) |
| College | | −0.00 | | | | |
| | | (0.01) | | | | |
| Male | | | | | −0.01 | |
| | | | | | (0.01) | |
| Middle Age | −0.01 | −0.00 | | | −0.01 | −0.00 |
| | (0.00) | (0.01) | | | (0.01) | (0.00) |
| Old Age | −0.00 | −0.01 | | | −0.01 | −0.00 |
| | (0.00) | (0.01) | | | (0.01) | (0.00) |
| Black | −0.01** | 0.00 | −0.00 | 0.00 | | |
| | (0.00) | (0.01) | (0.01) | (0.00) | | |
| Latino | −0.00 | −0.01 | 0.00 | 0.00 | | |
| | (0.00) | (0.01) | (0.00) | (0.00) | | |
| Other Race | −0.00 | −0.00 | 0.00 | 0.00 | | |
| | (0.00) | (0.01) | (0.00) | (0.00) | | |
| City | 0.00 | −0.00 | | | 0.00 | 0.01 |
| | (0.00) | (0.00) | | | (0.01) | (0.00) |
| Suburb | −0.00 | −0.01 | | | 0.00 | −0.00 |
| | (0.00) | (0.01) | | | (0.01) | (0.00) |
| Town | −0.01 | −0.00 | | | | 0.01 |
| | (0.00) | (0.01) | | | | (0.00) |
| Republican | 0.00 | 0.00 | 0.00 | | | |
| | (0.00) | (0.00) | (0.00) | | | |
| Independent | −0.00 | −0.00 | 0.00 | | | |
| | (0.01) | (0.01) | (0.00) | | | |
| Other Party | −0.00 | 0.00 | 0.01 | | | |
| | (0.01) | (0.01) | (0.01) | | | |
| Moderate | | | −0.00 | | 0.01 | 0.00 |
| | | | (0.00) | | (0.01) | (0.00) |
| Conservative | | | −0.00 | | 0.02* | −0.00 |
| | | | (0.00) | | (0.01) | (0.00) |
| Other Ideology | | | −0.00 | | 0.02 | −0.00 |
| | | | (0.01) | | (0.02) | (0.01) |
| Northeast | | | 0.00 | | 0.00 | |
| | | | (0.00) | | (0.01) | |
| Midwest | | | 0.00 | | −0.01 | |
| | | | (0.00) | | (0.01) | |
| West | | | 0.00 | | 0.01 | |
| | | | (0.00) | | (0.01) | |
| Constant | 0.13** | 0.05 | 0.06 | 0.04 | −0.01 | 0.13** |
| | (0.05) | (0.08) | (0.05) | (0.05) | (0.07) | (0.05) |
| Observations | 6,040 | 2,680 | 5,360 | 6,140 | 3,360 | 6,040 |

*Notes*: Dependent variable is candidate selection. Standard errors clustered by respondent. Sample is BLM in-group (M1), white nationalists in-group (M2), BLM out-group / non-threatening (M3), white nationalists out-group / non-threatening (M4), BLM out-group / threatening (M5), white nationalists out-group / threatening (M6). ***, **, * significant at .01, .05, .10, respectively. Omitted categories listed in Appendix Section B.2.

### B.3.4 Controlling for Candidate Attributes

Table B.8: Effect of Repression on Vote Share with Controls for Candidate Attributes

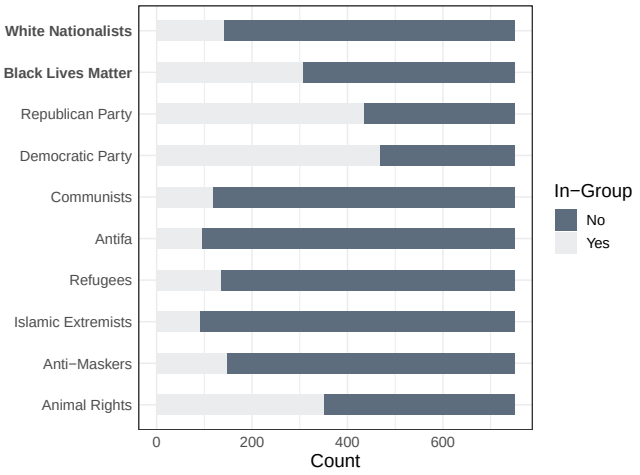
| | *In-Group* | | *Out-Group Non-Threatening* | | *Out-Group Threatening* | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | BLM | WN | BLM | WN | BLM | WN |
| Baseline: Support All Protests | | | | | | |
| Deny Permits to BLM | −0.20** | −0.02 | −0.12 | −0.04 | 0.12 | −0.18** |
| | (0.08) | (0.12) | (0.09) | (0.08) | (0.11) | (0.08) |
| Use Violence Against BLM | −0.25*** | −0.07 | −0.12 | −0.01 | 0.14 | −0.23*** |
| | (0.09) | (0.12) | (0.08) | (0.08) | (0.11) | (0.08) |
| Deny Permits to WN | −0.07 | −0.07 | −0.07 | −0.08 | −0.10 | −0.11 |
| | (0.08) | (0.13) | (0.09) | (0.08) | (0.11) | (0.08) |
| Use Violence Against WN | −0.20** | −0.12 | −0.06 | −0.11 | −0.10 | −0.14 |
| | (0.08) | (0.11) | (0.09) | (0.08) | (0.12) | (0.09) |
| Baseline: White | | | | | | |
| Black | 0.05 | −0.17** | −0.13** | −0.04 | −0.16** | −0.03 |
| | (0.05) | (0.08) | (0.06) | (0.05) | (0.07) | (0.06) |
| Baseline: Male | | | | | | |
| Female | −0.05 | −0.11 | 0.01 | −0.04 | −0.00 | 0.06 |
| | (0.05) | (0.08) | (0.06) | (0.05) | (0.08) | (0.05) |
| Baseline: Incumbent | | | | | | |
| Businessperson | 0.05 | 0.01 | 0.13* | 0.10 | 0.14* | 0.13** |
| | (0.06) | (0.09) | (0.07) | (0.06) | (0.08) | (0.06) |
| State Representative | 0.08 | 0.08 | 0.04 | −0.00 | 0.07 | 0.10* |
| | (0.06) | (0.09) | (0.07) | (0.07) | (0.08) | (0.06) |
| Baseline: Democrat | | | | | | |
| Independent | −0.16*** | −0.32*** | −0.15** | −0.03 | 0.19* | −0.03 |
| | (0.06) | (0.10) | (0.07) | (0.06) | (0.10) | (0.07) |
| Republican | −0.18** | −0.43*** | −0.29*** | 0.05 | 0.45*** | −0.07 |
| | (0.07) | (0.12) | (0.09) | (0.08) | (0.11) | (0.07) |
| Baseline: Pro-Choice | | | | | | |
| Safe/Legal/Rare | −0.04 | 0.05 | 0.12* | 0.10 | 0.37*** | 0.15** |
| | (0.07) | (0.09) | (0.07) | (0.06) | (0.09) | (0.07) |
| Pro-Life | −0.11 | 0.12 | 0.03 | 0.18** | 0.70*** | 0.08 |
| | (0.07) | (0.10) | (0.08) | (0.07) | (0.11) | (0.09) |
| Baseline: Limit COVID Re-opening | | | | | | |
| Increase COVID Re-opening | −0.22*** | −0.00 | −0.33*** | −0.12** | −0.08 | −0.43*** |
| | (0.06) | (0.07) | (0.06) | (0.06) | (0.07) | (0.06) |
| Baseline: Support Public Schools | | | | | | |
| No Common Core | 0.12* | 0.03 | 0.03 | 0.03 | 0.07 | 0.14** |
| | (0.07) | (0.10) | (0.06) | (0.07) | (0.09) | (0.06) |
| More Parent Choice | 0.01 | 0.06 | 0.02 | 0.05 | 0.13 | 0.00 |
| | (0.06) | (0.09) | (0.07) | (0.06) | (0.09) | (0.07) |
| Baseline: Gun Restrictions | | | | | | |
| Pro 2nd Amendment | −0.16*** | 0.02 | −0.12** | −0.01 | 0.38*** | −0.05 |
| | (0.05) | (0.07) | (0.06) | (0.05) | (0.07) | (0.06) |
| Baseline: Universal Healthcare | | | | | | |
| Free Market Healthcare | −0.06 | 0.02 | −0.03 | −0.01 | 0.11 | −0.03 |
| | (0.05) | (0.07) | (0.06) | (0.06) | (0.08) | (0.06) |
| Baseline: Path to Legal Residence | | | | | | |
| Close Borders | −0.17*** | −0.04 | −0.05 | −0.00 | 0.41*** | 0.04 |
| | (0.05) | (0.07) | (0.06) | (0.06) | (0.07) | (0.06) |
| Baseline: Fair Economy | | | | | | |
| Reduce Econ. Regulations | −0.13** | 0.05 | −0.11* | −0.17*** | −0.03 | −0.08 |
| | (0.05) | (0.08) | (0.06) | (0.05) | (0.07) | (0.06) |
| Baseline: Reduce Emissions | | | | | | |
| Reduce Environ. Regulations | −0.18*** | −0.15** | −0.11* | −0.03 | 0.10 | −0.12** |
| | (0.05) | (0.07) | (0.06) | (0.06) | (0.07) | (0.06) |
| Constant | 0.67*** | 0.38** | 0.52*** | 0.09 | −1.07*** | 0.28** |
| | (0.12) | (0.17) | (0.14) | (0.13) | (0.19) | (0.14) |
| Observations | 6,120 | 2,800 | 5,440 | 6,140 | 3,440 | 6,060 |

*Notes*: Dependent variable is candidate selection. Standard errors clustered by respondent. Sample is BLM in-group (M1), white nationalists in-group (M2), BLM out-group / non-threatening (M3), white nationalists out-group / non-threatening (M4), BLM out-group / threatening (M5), white nationalists out-group / threatening (M6). ***, **, * significant at .01, .05, .10, respectively.

## B.4 Group Identification and Threat Perception

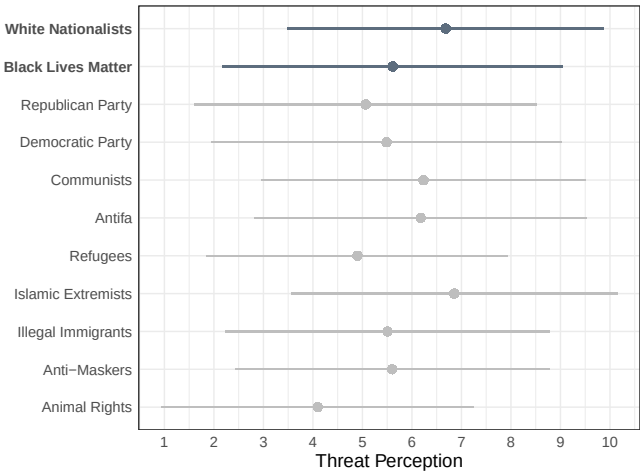
### B.4.1 Descriptive Statistics

Figure B.1: In-Group Identification Across Groups



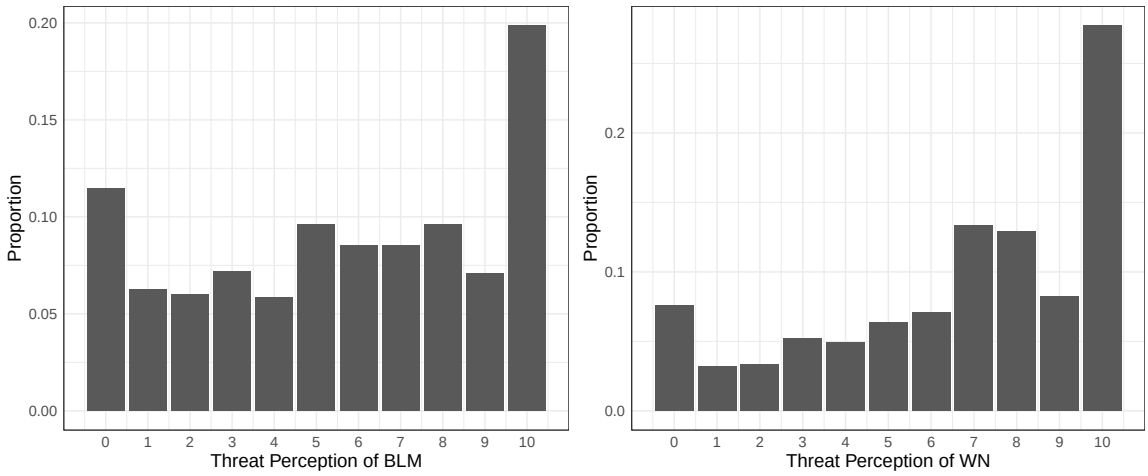

*Notes*: Group identification for main groups in experimental analysis. N = 750. Survey question: "Please indicate whether your or someone close to you (a close friend or family member) would consider themselves a member of the following groups."

Figure B.2: Threat Perception Across Groups




*Notes*: Threat perception for main groups in experimental analysis. N = 750. Groups of interest in bold/blue. Survey question: "On a scale of 1 to 10, 1 being the LEAST threatening and 10 being the MOST threatening, how threatening do you perceive the following groups to be to U.S. society?" Means / 95% confidence intervals shown.

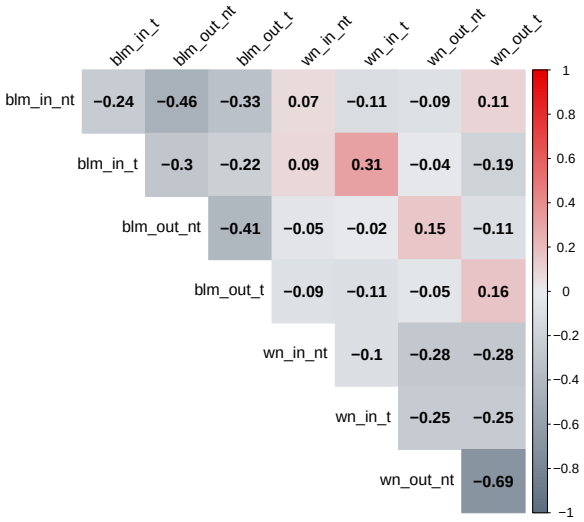
Figure B.3: Threat Perception of BLM and WN




*Notes*: Threat perception for Black Lives Matter and White Nationalists. N = 750. Survey question: "On a scale of 1 to 10, 1 being the LEAST threatening and 10 being the MOST threatening, how threatening do you perceive the following groups to be to U.S. society?"

### B.4.2 Correlations

Figure B.4: Correlations Across Group Identification and Threat Perception




*Notes*: "blm" = Black Lives Matter, "wn" = white nationalists, "out" = out-group, "in" = in-group, "nt" = non-threatening, "t" = threatening. The only correlation coefficient significant at the 0.1 level was the negative correlation between those who view white nationalists as a non-threatening out-group and those who view white nationalists as a threatening out-group.

### B.4.3 Modeling Group Identification and Threat Perception

In this section, I assess the main determinants of group identification and threat perception in my sample. Table B.9 shows the results for regressions predicting respondents' group identification (Models 1 and 2) and threat perception (Models 3 and 4). I include a range of demographic features in all models. In the models predicting threat perception, I add indicators for in-group identification, perceptions of the repression targets' use of violence, and perceptions of the illegality of protests for each group.[1]

Model 1 reveals that older respondents, non-Democrats, and conservatives were less likely to identify BLM as an in-group than were respondents who were younger, Democrats, and liberal. Black respondents were more likely to identify with BLM than were white respondents. In Model 2, we see that men, those with high income, the college educated, and conservatives were more likely to identify white nationalists as an in-group than women, low-income earners, those without a college education, and liberals. Also, older respondents, Black and Latino respondents, and those living in suburbs and towns were less likely to identify with white nationalists than younger respondents, white respondents, and those living in cities. Somewhat surprisingly, Republicans were significantly *less* likely to identify white nationalists as an in-group than were Democrats. One explanation for this pattern is that Democrats may be more likely to apply the *label* of white nationalist to individuals with racist views, while Republicans may be averse to the label or have a lower threshold for applying it to those with specific attitudes and behaviors. This puzzling correlation invites caution for the results in samples of in-group identification with white nationalists.

Model 3 shows the correlates for viewing Black Lives Matter as threatening. First, there is a strong correlation between perceptions of violence and illegality and threat perception. These findings suggest face validity for the measure of threat perception. Additionally, those who are moderate and conservative were more likely to view BLM as threatening and those who live in the Northeast were less likely to view BLM as threatening compared to those

[1]The survey questions asked respondents to rank their level of agreement with four statements on 7-point Likert scales ranging from "strongly agree" to "strongly disagree": "Individuals from [Black Lives Matter / white nationalists] often use violence at protests," and "Individuals from [Black Lives Matter / white nationalists] often protest illegally." I reverse the measures so higher values represent higher perceptions of violence and illegality. I ask these questions *after* the conjoint to avoid priming perceptions of violence and illegality. As such, these results should be treated as correlational.

in the South. Regarding perceptions of threat for white nationalists, again there is a strong positive relationship between perceptions of violence, perceptions of illegality, and threat perceptions. Interestingly, there is also an inverse relationship between perceptions of BLM illegality and perceptions that white nationalists are threatening. Finally, older respondents were more likely to view white nationalists as threatening, while those who identify with another gender were less so, though there are only 2 respondents in this category. Finally, it is interesting that in-group identification is not correlated with threat perception for either group, as might have been expected. This null finding suggests that in-group identity and threat perceptions, at least as measured in this experiment, are distinct concepts. The insignificant correlations in Figure B.4 further demonstrate that these group identifiers are distinct.

# Table B.9: Modeling Group Identification and Threat Perception

| | BLM In-Group | WN In-Group | BLM Threat | WN Threat |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| BLM In-Group | | | 0.19 | −0.31 |
| | | | (0.26) | (0.26) |
| BLM Violent | | | 0.50*** | −0.02 |
| | | | (0.11) | (0.11) |
| BLM Illegal | | | 0.26** | −0.35*** |
| | | | (0.11) | (0.11) |
| WN In-Group | | | 0.51 | 0.52 |
| | | | (0.33) | (0.33) |
| WN Violent | | | −0.06 | 0.47*** |
| | | | (0.11) | (0.11) |
| WN Illegal | | | −0.11 | 0.22** |
| | | | (0.11) | (0.11) |
| Male | −0.17 | 0.96*** | 0.00 | −0.37 |
| | (0.19) | (0.25) | (0.24) | (0.24) |
| Other Gender | 0.66 | −10.38 | −1.90 | −6.63*** |
| | (2.29) | (558.72) | (2.18) | (2.18) |
| High Income | 0.22 | 0.81*** | −0.22 | 0.38 |
| | (0.20) | (0.28) | (0.26) | (0.26) |
| College | 0.17 | 0.63** | −0.12 | −0.17 |
| | (0.20) | (0.28) | (0.26) | (0.25) |
| Middle Age | −1.06*** | −0.33 | −0.46 | 0.44 |
| | (0.21) | (0.26) | (0.28) | (0.28) |
| Old Age | −1.92*** | −2.29*** | 0.40 | 1.13*** |
| | (0.29) | (0.57) | (0.36) | (0.36) |
| Black | 1.44*** | −0.80* | −0.20 | −0.61 |
| | (0.32) | (0.42) | (0.40) | (0.40) |
| Latino | 0.01 | −0.77* | 0.41 | −0.06 |
| | (0.28) | (0.40) | (0.38) | (0.38) |
| Other Race | 0.22 | −1.96** | −0.26 | 0.13 |
| | (0.33) | (0.76) | (0.45) | (0.45) |
| City | 0.15 | 0.18 | −0.24 | 0.03 |
| | (0.26) | (0.33) | (0.34) | (0.34) |
| Suburb | −0.09 | −0.63* | 0.28 | 0.23 |
| | (0.25) | (0.35) | (0.32) | (0.32) |
| Town | −0.34 | −1.13** | 0.37 | 0.25 |
| | (0.31) | (0.48) | (0.39) | (0.39) |
| Republican | −0.69*** | −0.84*** | 0.20 | −0.02 |
| | (0.21) | (0.28) | (0.30) | (0.29) |
| Independent | −1.03*** | −0.67 | −0.03 | 0.42 |
| | (0.38) | (0.52) | (0.48) | (0.48) |
| Other Party | −1.49** | −1.57 | −0.02 | 0.29 |
| | (0.61) | (1.16) | (0.76) | (0.76) |
| Moderate | −0.56** | 0.35 | 0.69** | −0.27 |
| | (0.23) | (0.33) | (0.32) | (0.32) |
| Conservative | −0.68*** | 0.99*** | 1.09*** | −0.11 |
| | (0.25) | (0.32) | (0.35) | (0.35) |
| Other Ideology | −0.40 | 1.54* | 0.58 | −0.89 |
| | (0.62) | (0.84) | (0.81) | (0.81) |
| Northeast | 0.41 | −0.16 | −0.89** | −0.25 |
| | (0.26) | (0.35) | (0.35) | (0.34) |
| Midwest | −0.19 | −0.17 | −0.16 | −0.27 |
| | (0.25) | (0.33) | (0.31) | (0.31) |
| West | 0.15 | −0.05 | −0.25 | 0.42 |
| | (0.23) | (0.29) | (0.30) | (0.30) |
| Constant | 1.04*** | −2.18*** | 2.33*** | 4.64*** |
| | (0.34) | (0.48) | (0.63) | (0.63) |
| Observations | 728 | 728 | 728 | 728 |

*Notes*: Logit models (M1, M2) and linear models (M3, M4). Dependent variables are dummies for in-group identification (M1, M2) and continuous measure of threat perception from least (1) threatening to most (10) threatening (M3, M4). ***, **, * significant at .01, .05, .10, respectively. Omitted categories listed in Appendix Section B.2.

## B.5 Power Analysis

A major question when interpreting some of the null results in the main analysis is whether there is sufficient statistical power to detect a significant effect of the repression treatment if one existed. I use software from Schuessler and Freitag (2020) to verify whether or not the study is well-powered.[2] Table B.10 reports the results, setting values for power to .9 and alpha to 0.10, and recognizing that there are five levels in the repression treatment.

Table B.10:

Power

Analysis

| AMCE | Sample |
|---|---|
| 0.01 | 214054 |
| 0.02 | 53482 |
| 0.03 | 23746 |
| 0.04 | 13338 |
| 0.05 | 8522 |
| 0.06 | 5905 |
| 0.07 | 4327 |
| 0.08 | 3302 |
| 0.09 | 2601 |
| 0.10 | 2099 |
| 0.20 | 493 |

[2]Note that in a previous power analysis based on an M-Turk pilot study, I found that the smallest likely effect size was 0.10. In these results the effects are much smaller. This may be because almost all respondents had a high likelihood of punishing repression in my pilot study, inflating effect sizes. Such would be expected given that my M-Turk sample was much more liberal than the representative sample here.

## B.6 Robustness Checks

### B.6.1 Linear Models

Table B.11: Effect of Repression on Vote Share (Linear Models)

| | *In-Group* | | *Out-Group Non-Threatening* | | *Out-Group Threatening* | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | BLM | WN | BLM | WN | BLM | WN |
| Deny Permits to BLM | −0.05** | −0.01 | −0.02 | −0.01 | 0.03 | −0.04** |
| | (0.02) | (0.03) | (0.02) | (0.02) | (0.03) | (0.02) |
| Use Violence Against BLM | −0.06*** | −0.01 | −0.03 | −0.00 | 0.02 | −0.06*** |
| | (0.02) | (0.03) | (0.02) | (0.02) | (0.03) | (0.02) |
| Deny Permits to WN | −0.02 | −0.02 | −0.02 | −0.02 | −0.04 | −0.03 |
| | (0.02) | (0.03) | (0.02) | (0.02) | (0.03) | (0.02) |
| Use Violence Against WN | −0.05** | −0.03 | −0.01 | −0.02 | −0.03 | −0.03 |
| | (0.02) | (0.03) | (0.02) | (0.02) | (0.03) | (0.02) |
| Constant | 0.53*** | 0.51*** | 0.52*** | 0.51*** | 0.50*** | 0.53*** |
| | (0.01) | (0.02) | (0.01) | (0.01) | (0.02) | (0.01) |
| Observations | 6,120 | 2,800 | 5,440 | 6,140 | 3,440 | 6,060 |

*Notes*: Dependent variable is whether a candidate was selected. Standard errors clustered by respondent. Sample is BLM in-group (M1), white nationalists in-group (M2), BLM out-group / non-threatening (M3), white nationalists out-group / non-threatening (M4), BLM out-group / threatening (M5), white nationalists out-group / threatening (M6). ***, **, * significant at .01, .05, .10, respectively. Gray boxes represent coefficients used for hypothesis tests.

### B.6.2 Ordering Effects

Table B.12: Effect of Repression on Vote Share - Task 1

| | *In-Group* | | *Out-Group Non-Threatening* | | *Out-Group Threatening* | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | BLM | WN | BLM | WN | BLM | WN |
| Deny BLM Permits | −0.27 | −0.18 | −0.30 | 0.03 | 1.04*** | 0.06 |
| | (0.27) | (0.40) | (0.27) | (0.25) | (0.36) | (0.27) |
| Use Violence Against BLM | −0.36 | 0.54 | 0.07 | 0.10 | 0.68* | −0.26 |
| | (0.27) | (0.37) | (0.28) | (0.28) | (0.36) | (0.26) |
| Deny WN Permits | −0.45 | 0.10 | −0.24 | −0.42* | −0.23 | −0.42 |
| | (0.28) | (0.40) | (0.26) | (0.25) | (0.35) | (0.27) |
| Use Violence Against WN | −0.87*** | −0.91** | −0.32 | −0.43 | 0.11 | −0.33 |
| | (0.27) | (0.42) | (0.28) | (0.27) | (0.32) | (0.24) |
| Constant | 0.40** | 0.07 | 0.15 | 0.14 | −0.30 | 0.19 |
| | (0.18) | (0.25) | (0.17) | (0.17) | (0.22) | (0.16) |
| Observations | 612 | 280 | 544 | 614 | 344 | 606 |

*Notes*: Dependent variable is whether a candidate was selected. Standard errors clustered by respondent. Sample is BLM in-group (M1), white nationalists in-group (M2), BLM out-group / non-threatening (M3), white nationalists out-group / non-threatening (M4), BLM out-group / threatening (M5), white nationalists out-group / threatening (M6). ***, **, * significant at .01, .05, .10, respectively.

Table B.13: Effect of Repression on Vote Share - Tasks 1-2

| | *In-Group* | | *Out-Group Non-Threatening* | | *Out-Group Threatening* | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | BLM | WN | BLM | WN | BLM | WN |
| Deny BLM Permits | −0.16 | −0.15 | −0.18 | −0.03 | 0.49** | 0.06 |
| | (0.18) | (0.29) | (0.21) | (0.18) | (0.24) | (0.19) |
| Use Violence Against BLM | −0.39** | 0.30 | −0.18 | −0.15 | 0.58** | −0.22 |
| | (0.18) | (0.24) | (0.18) | (0.19) | (0.25) | (0.18) |
| Deny WN Permits | −0.16 | −0.00 | −0.07 | −0.11 | −0.13 | −0.19 |
| | (0.18) | (0.28) | (0.19) | (0.17) | (0.24) | (0.18) |
| Use Violence Against WN | −0.63*** | −0.31 | −0.11 | −0.29 | 0.09 | −0.26 |
| | (0.19) | (0.28) | (0.20) | (0.19) | (0.23) | (0.18) |
| Constant | 0.26** | 0.02 | 0.11 | 0.11 | −0.20 | 0.12 |
| | (0.12) | (0.17) | (0.12) | (0.11) | (0.14) | (0.11) |
| Observations | 1,224 | 560 | 1,088 | 1,228 | 688 | 1,212 |

*Notes*: Dependent variable is whether a candidate was selected. Standard errors clustered by respondent. Sample is BLM in-group (M1), white nationalists in-group (M2), BLM out-group / non-threatening (M3), white nationalists out-group / non-threatening (M4), BLM out-group / threatening (M5), white nationalists out-group / threatening (M6). ***, **, * significant at .01, .05, .10, respectively.

Table B.14: Effect of Repression on Vote Share - Tasks 1-3

| | *In-Group* | | *Out-Group Non-Threatening* | | *Out-Group Threatening* | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | BLM | WN | BLM | WN | BLM | WN |
| Deny BLM Permits | −0.23 | 0.04 | −0.14 | −0.08 | 0.24 | −0.16 |
| | (0.14) | (0.21) | (0.17) | (0.15) | (0.20) | (0.15) |
| Use Violence Against BLM | −0.39*** | 0.20 | −0.18 | −0.15 | 0.32 | −0.33** |
| | (0.15) | (0.20) | (0.16) | (0.16) | (0.20) | (0.15) |
| Deny WN Permits | −0.17 | −0.09 | −0.19 | −0.18 | −0.25 | −0.26* |
| | (0.15) | (0.25) | (0.16) | (0.15) | (0.20) | (0.14) |
| Use Violence Against WN | −0.26* | −0.09 | −0.22 | −0.24 | −0.18 | −0.28* |
| | (0.15) | (0.21) | (0.17) | (0.15) | (0.18) | (0.15) |
| Constant | 0.21** | −0.01 | 0.15 | 0.13 | −0.02 | 0.21** |
| | (0.09) | (0.13) | (0.11) | (0.10) | (0.12) | (0.09) |
| Observations | 1,836 | 840 | 1,632 | 1,842 | 1,032 | 1,818 |

*Notes*: Dependent variable is whether a candidate was selected. Standard errors clustered by respondent. Sample is BLM in-group (M1), white nationalists in-group (M2), BLM out-group / non-threatening (M3), white nationalists out-group / non-threatening (M4), BLM out-group / threatening (M5), white nationalists out-group / threatening (M6). ***, **, * significant at .01, .05, .10, respectively.

Table B.15: Effect of Repression on Vote Share - Tasks 1-4

| | *In-Group* | | *Out-Group Non-Threatening* | | *Out-Group Threatening* | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | BLM | WN | BLM | WN | BLM | WN |
| Deny BLM Permits | −0.17 | 0.14 | −0.06 | −0.00 | 0.23 | −0.18 |
| | (0.12) | (0.18) | (0.14) | (0.13) | (0.17) | (0.13) |
| Use Violence Against BLM | −0.21 | 0.34* | −0.04 | −0.07 | 0.28 | −0.18 |
| | (0.13) | (0.18) | (0.14) | (0.14) | (0.18) | (0.13) |
| Deny WN Permits | −0.06 | 0.16 | −0.08 | −0.12 | −0.23 | −0.24* |
| | (0.13) | (0.19) | (0.14) | (0.13) | (0.18) | (0.13) |
| Use Violence Against WN | −0.20 | −0.04 | −0.03 | −0.13 | −0.18 | −0.19 |
| | (0.12) | (0.17) | (0.14) | (0.12) | (0.16) | (0.13) |
| Constant | 0.13* | −0.12 | 0.04 | 0.07 | −0.01 | 0.16* |
| | (0.08) | (0.11) | (0.09) | (0.08) | (0.11) | (0.08) |
| Observations | 2,448 | 1,120 | 2,176 | 2,456 | 1,376 | 2,424 |

*Notes*: Dependent variable is whether a candidate was selected. Standard errors clustered by respondent. Sample is BLM in-group (M1), white nationalists in-group (M2), BLM out-group / non-threatening (M3), white nationalists out-group / non-threatening (M4), BLM out-group / threatening (M5), white nationalists out-group / threatening (M6). ***, **, * significant at .01, .05, .10, respectively.

Table B.16: Effect of Repression on Vote Share - Tasks 1-5

| | *In-Group* | | *Out-Group Non-Threatening* | | *Out-Group Threatening* | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | BLM | WN | BLM | WN | BLM | WN |
| Deny BLM Permits | −0.15 | 0.02 | −0.11 | −0.04 | 0.12 | −0.16 |
| | (0.11) | (0.16) | (0.13) | (0.12) | (0.15) | (0.12) |
| Use Violence Against BLM | −0.18 | 0.28* | −0.08 | −0.08 | 0.18 | −0.20* |
| | (0.12) | (0.16) | (0.12) | (0.13) | (0.16) | (0.11) |
| Deny WN Permits | −0.02 | 0.14 | −0.13 | −0.18 | −0.25 | −0.18 |
| | (0.11) | (0.17) | (0.12) | (0.12) | (0.16) | (0.11) |
| Use Violence Against WN | −0.08 | 0.05 | −0.07 | −0.13 | −0.14 | −0.13 |
| | (0.11) | (0.15) | (0.12) | (0.11) | (0.15) | (0.12) |
| Constant | 0.09 | −0.10 | 0.08 | 0.09 | 0.02 | 0.13* |
| | (0.07) | (0.10) | (0.08) | (0.08) | (0.09) | (0.07) |
| Observations | 3,060 | 1,400 | 2,720 | 3,070 | 1,720 | 3,030 |

*Notes*: Dependent variable is whether a candidate was selected. Standard errors clustered by respondent. Sample is BLM in-group (M1), white nationalists in-group (M2), BLM out-group / non-threatening (M3), white nationalists out-group / non-threatening (M4), BLM out-group / threatening (M5), white nationalists out-group / threatening (M6). ***, **, * significant at .01, .05, .10, respectively.

Table B.17: Effect of Repression on Vote Share - Tasks 1-6

| | *In-Group* | | *Out-Group Non-Threatening* | | *Out-Group Threatening* | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | BLM | WN | BLM | WN | BLM | WN |
| Deny BLM Permits | −0.17 | −0.05 | −0.14 | −0.03 | 0.07 | −0.21* |
| | (0.10) | (0.15) | (0.12) | (0.11) | (0.14) | (0.11) |
| Use Violence Against BLM | −0.23** | 0.15 | −0.05 | −0.01 | 0.14 | −0.26** |
| | (0.11) | (0.15) | (0.11) | (0.12) | (0.15) | (0.11) |
| Deny WN Permits | −0.06 | −0.05 | −0.08 | −0.11 | −0.28** | −0.17 |
| | (0.10) | (0.15) | (0.11) | (0.11) | (0.14) | (0.10) |
| Use Violence Against WN | −0.08 | −0.02 | −0.04 | −0.07 | −0.18 | −0.16 |
| | (0.10) | (0.14) | (0.11) | (0.10) | (0.14) | (0.11) |
| Constant | 0.11 | −0.01 | 0.06 | 0.04 | 0.05 | 0.16** |
| | (0.07) | (0.09) | (0.07) | (0.07) | (0.09) | (0.07) |
| Observations | 3,672 | 1,680 | 3,264 | 3,684 | 2,064 | 3,636 |

*Notes*: Dependent variable is whether a candidate was selected. Standard errors clustered by respondent. Sample is BLM in-group (M1), white nationalists in-group (M2), BLM out-group / non-threatening (M3), white nationalists out-group / non-threatening (M4), BLM out-group / threatening (M5), white nationalists out-group / threatening (M6). ***, **, * significant at .01, .05, .10, respectively.

Table B.18: Effect of Repression on Vote Share - Tasks 1-7

| | *In-Group* | | *Out-Group Non-Threatening* | | *Out-Group Threatening* | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | BLM | WN | BLM | WN | BLM | WN |
| Deny BLM Permits | −0.16 | −0.02 | −0.13 | 0.02 | 0.10 | −0.24** |
| | (0.10) | (0.14) | (0.10) | (0.10) | (0.13) | (0.10) |
| Use Violence Against BLM | −0.25** | 0.02 | −0.13 | 0.01 | 0.15 | −0.30*** |
| | (0.10) | (0.13) | (0.10) | (0.10) | (0.14) | (0.10) |
| Deny WN Permits | −0.06 | −0.05 | −0.09 | −0.08 | −0.24* | −0.19* |
| | (0.10) | (0.14) | (0.10) | (0.10) | (0.13) | (0.10) |
| Use Violence Against WN | −0.15 | −0.10 | −0.10 | −0.10 | −0.15 | −0.19* |
| | (0.10) | (0.13) | (0.10) | (0.10) | (0.13) | (0.10) |
| Constant | 0.13* | 0.03 | 0.09 | 0.03 | 0.03 | 0.18*** |
| | (0.06) | (0.09) | (0.06) | (0.06) | (0.09) | (0.07) |
| Observations | 4,284 | 1,960 | 3,808 | 4,298 | 2,408 | 4,242 |

*Notes*: Dependent variable is whether a candidate was selected. Standard errors clustered by respondent. Sample is BLM in-group (M1), white nationalists in-group (M2), BLM out-group / non-threatening (M3), white nationalists out-group / non-threatening (M4), BLM out-group / threatening (M5), white nationalists out-group / threatening (M6). ***, **, * significant at .01, .05, .10, respectively.

Table B.19: Effect of Repression on Vote Share - Tasks 1-8

| | *In-Group* | | *Out-Group Non-Threatening* | | *Out-Group Threatening* | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | BLM | WN | BLM | WN | BLM | WN |
| Deny BLM Permits | −0.17* | 0.00 | −0.12 | −0.00 | 0.09 | −0.24** |
| | (0.09) | (0.13) | (0.10) | (0.09) | (0.12) | (0.10) |
| Use Violence Against BLM | −0.21** | 0.02 | −0.13 | −0.01 | 0.12 | −0.26*** |
| | (0.10) | (0.12) | (0.09) | (0.10) | (0.13) | (0.09) |
| Deny WN Permits | −0.10 | −0.04 | −0.04 | −0.08 | −0.22* | −0.17* |
| | (0.09) | (0.13) | (0.09) | (0.09) | (0.12) | (0.09) |
| Use Violence Against WN | −0.18* | −0.12 | −0.07 | −0.08 | −0.12 | −0.18* |
| | (0.09) | (0.13) | (0.09) | (0.09) | (0.13) | (0.10) |
| Constant | 0.13** | 0.03 | 0.07 | 0.03 | 0.02 | 0.17*** |
| | (0.06) | (0.08) | (0.06) | (0.06) | (0.08) | (0.06) |
| Observations | 4,896 | 2,240 | 4,352 | 4,912 | 2,752 | 4,848 |

*Notes*: Dependent variable is whether a candidate was selected. Standard errors clustered by respondent. Sample is BLM in-group (M1), white nationalists in-group (M2), BLM out-group / non-threatening (M3), white nationalists out-group / non-threatening (M4), BLM out-group / threatening (M5), white nationalists out-group / threatening (M6). ***, **, * significant at .01, .05, .10, respectively.

Table B.20: Effect of Repression on Vote Share - Tasks 1-9

| | *In-Group* | | *Out-Group Non-Threatening* | | *Out-Group Threatening* | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | BLM | WN | BLM | WN | BLM | WN |
| Deny BLM Permits | −0.20** | −0.07 | −0.13 | −0.04 | 0.10 | −0.19** |
| | (0.09) | (0.12) | (0.09) | (0.09) | (0.11) | (0.09) |
| Use Violence Against BLM | −0.23** | −0.07 | −0.13 | 0.00 | 0.10 | −0.26*** |
| | (0.09) | (0.12) | (0.08) | (0.09) | (0.12) | (0.09) |
| Deny WN Permits | −0.06 | −0.08 | −0.07 | −0.08 | −0.15 | −0.09 |
| | (0.09) | (0.13) | (0.09) | (0.08) | (0.11) | (0.08) |
| Use Violence Against WN | −0.18** | −0.13 | −0.05 | −0.12 | −0.13 | −0.13 |
| | (0.08) | (0.12) | (0.09) | (0.08) | (0.12) | (0.09) |
| Constant | 0.14** | 0.07 | 0.08 | 0.05 | 0.02 | 0.14** |
| | (0.06) | (0.08) | (0.06) | (0.05) | (0.07) | (0.06) |
| Observations | 5,508 | 2,520 | 4,896 | 5,526 | 3,096 | 5,454 |

*Notes*: Dependent variable is whether a candidate was selected. Standard errors clustered by respondent. Sample is BLM in-group (M1), white nationalists in-group (M2), BLM out-group / non-threatening (M3), white nationalists out-group / non-threatening (M4), BLM out-group / threatening (M5), white nationalists out-group / threatening (M6). ***, **, * significant at .01, .05, .10, respectively.

### B.6.3 Alternative Thresholds for Threat Perception

Table B.21: Effect of Repression on Vote Share - Threatening > 6

| | *Out-Group Non-Threatening* | | *Out-Group Threatening* | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | BLM | WN | BLM | WN |
| Deny BLM Permits | −0.11 | 0.06 | 0.08 | −0.21*** |
| | (0.09) | (0.10) | (0.10) | (0.07) |
| Use Violence Against BLM | −0.06 | 0.08 | −0.00 | −0.26*** |
| | (0.09) | (0.09) | (0.10) | (0.08) |
| Deny WN Permits | −0.09 | −0.06 | −0.10 | −0.11 |
| | (0.09) | (0.09) | (0.10) | (0.07) |
| Use Violence Against WN | −0.07 | −0.05 | −0.08 | −0.16** |
| | (0.10) | (0.09) | (0.10) | (0.08) |
| Constant | 0.07 | −0.01 | 0.02 | 0.15*** |
| | (0.06) | (0.06) | (0.06) | (0.05) |
| Observations | 4,700 | 4,720 | 4,180 | 7,480 |

*Notes*: Dependent variable is whether a candidate was selected. Standard errors clustered by respondent. Sample is BLM in-group (M1), white nationalists in-group (M2), BLM out-group / non-threatening (M3), white nationalists out-group / non-threatening (M4), BLM out-group / threatening (M5), white nationalists out-group / threatening (M6). ***, **, * significant at .01, .05, .10, respectively.

Table B.22: Effect of Repression on Vote Share - Threatening > 8

| | *Out-Group Non-Threatening* | | *Out-Group Threatening* | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | BLM | WN | BLM | WN |
| Deny BLM Permits | −0.09 | −0.04 | 0.14 | −0.21** |
| | (0.08) | (0.07) | (0.12) | (0.10) |
| Use Violence Against BLM | −0.11 | −0.05 | 0.16 | −0.25** |
| | (0.08) | (0.07) | (0.13) | (0.10) |
| Deny WN Permits | −0.05 | −0.10 | −0.20 | −0.06 |
| | (0.08) | (0.07) | (0.13) | (0.10) |
| Use Violence Against WN | −0.09 | −0.08 | −0.04 | −0.18* |
| | (0.08) | (0.07) | (0.13) | (0.11) |
| Constant | 0.07 | 0.06 | −0.01 | 0.14** |
| | (0.05) | (0.05) | (0.08) | (0.06) |
| Observations | 6,280 | 7,760 | 2,600 | 4,440 |

*Notes*: Dependent variable is whether a candidate was selected. Standard errors clustered by respondent. Sample is BLM in-group (M1), white nationalists in-group (M2), BLM out-group / non-threatening (M3), white nationalists out-group / non-threatening (M4), BLM out-group / threatening (M5), white nationalists out-group / threatening (M6). ***, **, * significant at .01, .05, .10, respectively.

Table B.23: Effect of Repression on Vote Share - Threatening > 9

| | *Out-Group Non-Threatening* | | *Out-Group Threatening* | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | BLM | WN | BLM | WN |
| Deny BLM Permits | −0.03 | −0.08 | 0.03 | −0.15 |
| | (0.08) | (0.07) | (0.14) | (0.11) |
| Use Violence Against BLM | −0.10 | −0.07 | 0.20 | −0.27** |
| | (0.07) | (0.07) | (0.15) | (0.12) |
| Deny WN Permits | −0.07 | −0.08 | −0.17 | −0.11 |
| | (0.08) | (0.07) | (0.14) | (0.11) |
| Use Violence Against WN | −0.08 | −0.10 | −0.06 | −0.18 |
| | (0.08) | (0.07) | (0.15) | (0.13) |
| Constant | 0.06 | 0.07 | −0.00 | 0.14** |
| | (0.05) | (0.04) | (0.09) | (0.07) |
| Observations | 6,920 | 8,840 | 1,960 | 3,360 |

*Notes*: Dependent variable is whether a candidate was selected. Standard errors clustered by respondent. Sample is BLM in-group (M1), white nationalists in-group (M2), BLM out-group / non-threatening (M3), white nationalists out-group / non-threatening (M4), BLM out-group / threatening (M5), white nationalists out-group / threatening (M6). ***, **, * significant at .01, .05, .10, respectively.

Table B.24: Effect of Repression on Vote Share - Threatening = 10

| | *Out-Group Non-Threatening* | | *Out-Group Threatening* | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | BLM | WN | BLM | WN |
| Deny BLM Permits | −0.02 | −0.10* | 0.03 | −0.15 |
| | (0.07) | (0.06) | (0.14) | (0.11) |
| Use Violence Against BLM | −0.03 | −0.13** | 0.20 | −0.27** |
| | (0.07) | (0.06) | (0.15) | (0.12) |
| Deny WN Permits | −0.09 | −0.09 | −0.17 | −0.11 |
| | (0.07) | (0.06) | (0.14) | (0.11) |
| Use Violence Against WN | −0.07 | −0.12** | −0.06 | −0.18 |
| | (0.07) | (0.06) | (0.15) | (0.13) |
| Constant | 0.04 | 0.09** | −0.00 | 0.14** |
| | (0.04) | (0.04) | (0.09) | (0.07) |
| Observations | 8,880 | 12,200 | 1,960 | 3,360 |

*Notes*: Dependent variable is whether a candidate was selected. Standard errors clustered by respondent. Sample is BLM in-group (M1), white nationalists in-group (M2), BLM out-group / non-threatening (M3), white nationalists out-group / non-threatening (M4), BLM out-group / threatening (M5), white nationalists out-group / threatening (M6). ***, **, * significant at .01, .05, .10, respectively.

### B.6.4 Alternative Outcome Variable: Candidate Ranking

Table B.25: Effect of Repression on Vote Share - Rankings as Outcome

| | *In-Group* | | *Out-Group Non-Threatening* | | *Out-Group Threatening* | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | BLM | WN | BLM | WN | BLM | WN |
| Deny BLM Permits | $-0.43^{***}$ | $-0.06$ | $-0.12$ | $-0.03$ | $0.24$ | $-0.34^{**}$ |
| | (0.13) | (0.15) | (0.12) | (0.11) | (0.18) | (0.13) |
| Use Violence Against BLM | $-0.47^{***}$ | $-0.25^{*}$ | $-0.26^{**}$ | $-0.10$ | $-0.16$ | $-0.55^{***}$ |
| | (0.12) | (0.14) | (0.12) | (0.11) | (0.19) | (0.14) |
| Deny WN Permits | $-0.35^{***}$ | $-0.36^{**}$ | $0.06$ | $0.04$ | $-0.25$ | $-0.28^{**}$ |
| | (0.11) | (0.15) | (0.11) | (0.10) | (0.18) | (0.12) |
| Use Violence Against WN | $-0.28^{**}$ | $-0.27^{*}$ | $0.01$ | $-0.09$ | $-0.23$ | $-0.16$ |
| | (0.12) | (0.16) | (0.13) | (0.12) | (0.20) | (0.14) |
| Constant | $6.32^{***}$ | $7.34^{***}$ | $5.77^{***}$ | $5.67^{***}$ | $5.58^{***}$ | $5.56^{***}$ |
| | (0.13) | (0.16) | (0.12) | (0.11) | (0.18) | (0.13) |
| Observations | 6,094 | 2,789 | 5,415 | 6,121 | 3,440 | 6,039 |

*Notes*: Dependent variable is the candidate's ranking on a scale of 1 (definitely WOULD NOT vote for) to 10 (definitely WOULD vote for). Standard errors clustered by respondent. Sample is BLM in-group (M1), white nationalists in-group (M2), BLM out-group / non-threatening (M3), white nationalists out-group / non-threatening (M4), BLM out-group / threatening (M5), white nationalists out-group / threatening (M6). ***, **, * significant at .01, .05, .10, respectively. Linear models.

## B.7 Evaluating Causal Mechanisms

This section provides analysis to evaluate several of the theoretical claims that underline the main theory. Specifically, I analyze (1) whether voters who identify as an in-group with a repression target and/or consider one of the targets to be threatening are more likely to incorporate repression into their vote choice, (2) self-reported considerations in selecting candidates, and (3) the effect of group identification and threat perception in evaluating civil liberties - security trade-offs.

### B.7.1 Incorporation of Repression into Vote Choice

First, I analyze one of the causal mechanisms in my theory, which proposes that respondents who identified as an in-group with the repression target and those who perceived the repression target to be threatening would be especially likely to consider repression in their vote choice. I predicted that these individuals are more likely to consider repression

when selecting candidates than voters who perceive the repression target as an out-group and believe them to be non-threatening. The best evaluation of this dynamic is the conjoint analysis in the main paper, which mitigates social desirability bias by asking respondents to select candidates with randomized attributes. However, I also use several post-treatment questions to probe respondents' decision-making process.

I ask respondents which attributes they considered when selecting a candidate (they must mark the attributes they considered from a list of all candidate attributes). From this question I create the dummy variable *Repression Considered*, which is coded as "1" if the respondent marked repression and "0" otherwise. Second, I ask respondents how important each of the attributes was to their candidate selection, using a four-category scale that ranges from "not important" to "extremely important." I use the rankings of the importance of repression to create the variable *Repression Importance*, a four-point scale coded from "1" if repression is not important to "4" if repression is extremely important. Third, I ask respondents an open-ended question about whether there were any candidate attributes that made it impossible to support a candidate. *Repression Disqualifies* is a dummy coded "1" if the respondent mentioned that repression made it impossible to support a candidate.

Figure B.5 illustrates the number of respondents who marked that they consider a particular candidate attribute in their choice, while Figure B.6 shows the mean level of importance for each of the candidate attributes. As suggested in the conjoint results, the importance of repression ranks similarly with other salient issues for the 2020 election cycle abortion and COVID-19. In total, 37.6% (282/750) of respondents marked that they considered repression in their vote choice, and the mean level of importance was 2.97, closest to "very important." The last measure asked respondents to describe in their own works whether there were any conditions that disqualified a candidate from their vote. In total, 3.33% of respondents mentioned repression in their response. Some representative comments include the respondents who wrote that "people who support blm protesters getting tear gas are horrible" and "I could never vote for someone who approved of using weapons against protesters, or who would deny permits based on the group."

To further analyze the drivers of self-reported importance of repression in vote choice, I regress these measures on two categorical variables measuring group identification and threat

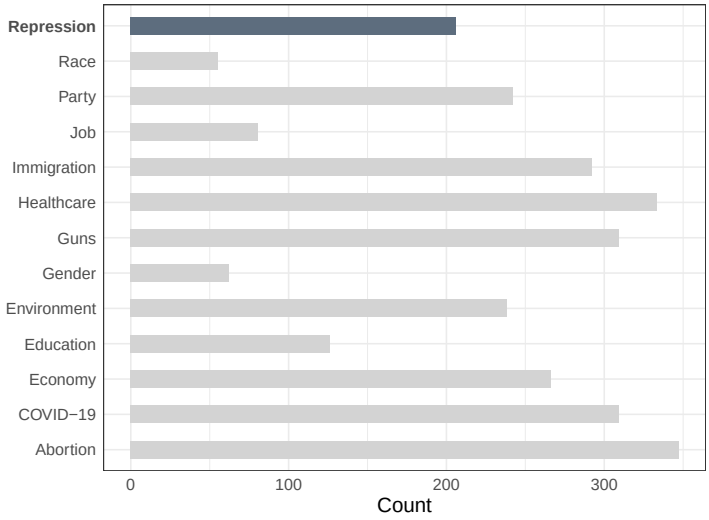
perceptions. The first measure codes respondents' orientation to Black Lives Matter in four categories: BLM in-group / BLM non-threatening, BLM in-group / BLM threatening, BLM out-group / BLM non-threatening, BLM out-group / BLM threatening. I create the same measure for white nationalists. I then use separate models to regress *Repression Considered*, *Repression Importance*, and *Repression Disqualifying* on these group identifiers, omitting the baseline category for respondents who viewed the repression target as both an out-group and non-threatening. I use a logistic link function for the dichotomous dependent variables and a linear link function for the continuous measure of repression importance. I also control for respondents' demographic features to mitigate omitted variable bias, given that the treatment conditions are not randomly assigned (Appendix Section B.2 details the coding for demographic variables and omitted categories). Table B.26 shows the results of this analysis.

I expected to find positive and statistically significant coefficients on each of the independent variables of interest. Compared to the baseline category of out-group identification and no threat perception, my theory suggested that in-group members and those who perceive the repression target to be threatening would be more likely to consider repression in their vote choice. There is some mixed support for this reasoning across the measures of consideration of repression. Considering first the measures in relation to Black Lives Matter, those who perceived BLM to be a non-threatening in-group were significantly more likely to consider repression and rank repression as having greater importance in their vote choice. There was an even stronger significant relationship between those who perceive BLM to be a threatening out-group and consideration of repression in vote choice. Regarding white nationalists, as I have already discussed the measure for in-group identification seems perhaps not to have captured the dynamics I expected. This may explain why those who identified white nationalists as an in-group were significantly *less* likely to consider repression than those who identified white nationalists as a non-threatening out-group. Alternatively, these results could support the null relationship for Hypothesis 1 based on my previous reasoning: because white nationalists have rarely been a target of government repression, this may explain why they are unlikely to consider repression in their vote choice. Finally, there is a weakly significant positive relationship between those who view white nationalists

as a threatening out-group and consideration of repression and likelihood that repression disqualified the candidate.

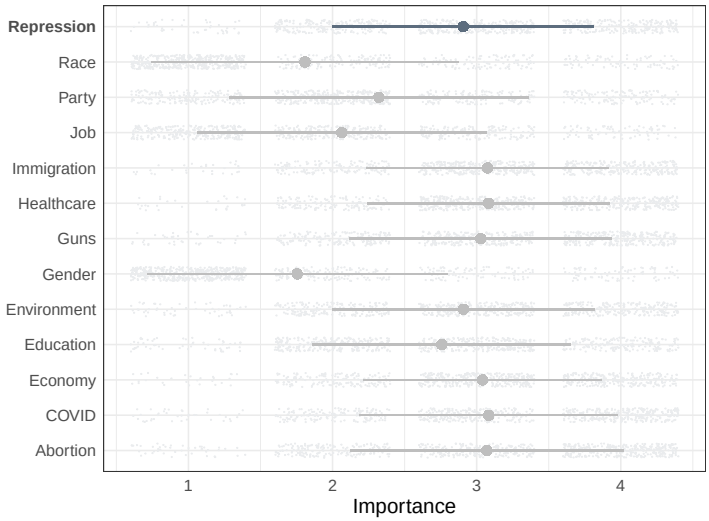
In all, there is some evidence across these models that, at least with relation to Black Lives Matter, in-group identification and threat perception increase the likelihood that a respondent will incorporate repression into their vote choice. For white nationalist orientation, however, there is not robust support for this mechanism.

Figure B.5: Self-Report of Consideration of Candidate Attributes in Vote Choice




*Notes*: Question text read "Which candidate attributes did you CONSIDER when choosing between the two candidates." Respondents were asked to mark all attributes that applied to their choice.

Figure B.6: Self-Report of Importance of Candidate Attributes in Vote Choice




*Notes*: Question text read "How important were each of the candidate attributes to your choice about who to vote for?" Respondents were asked to rank each candidate attribute on a four-point Likert scale from "not important" (1) to "extremely important" (4).

Table B.26: Self-Reported Consideration of Repression in Vote Choice

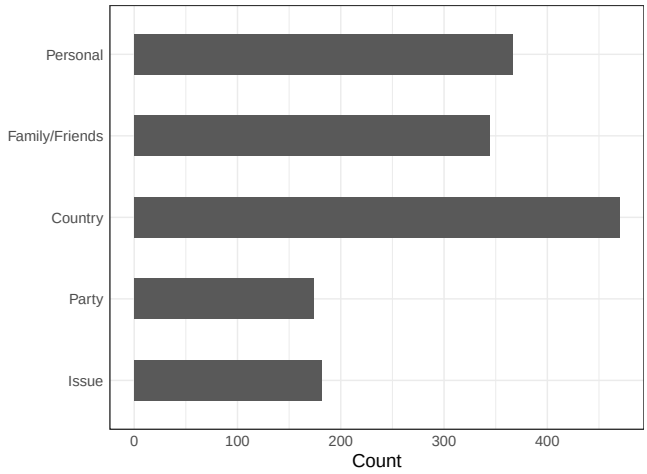
| | Consider Repression | Importance Repression | Repression Disqualifies |
|---|---|---|---|
| | (1) | (2) | (3) |
| BLM in-group/non-threat | 0.39* | 0.21** | 0.22 |
| | (0.22) | (0.09) | (0.55) |
| BLM in-group/threat | 0.44 | 0.41*** | 0.62 |
| | (0.29) | (0.11) | (0.91) |
| BLM out-group/threat | 0.76*** | 0.25*** | −0.69 |
| | (0.23) | (0.10) | (0.90) |
| WN in-group/non-threat | −0.85** | −0.09 | −0.57 |
| | (0.34) | (0.12) | (1.16) |
| WN in-group/threat | −0.90** | 0.28** | −15.89 |
| | (0.38) | (0.14) | (1,273.46) |
| WN out-group/threat | 0.35* | −0.09 | 1.04* |
| | (0.18) | (0.08) | (0.58) |
| Male | −0.12 | −0.07 | −0.27 |
| | (0.17) | (0.07) | (0.49) |
| Other Gender | −12.93 | 1.29** | −15.60 |
| | (378.15) | (0.63) | (7,412.96) |
| High Income | 0.05 | 0.02 | 0.00 |
| | (0.18) | (0.08) | (0.52) |
| College | −0.03 | 0.04 | −0.30 |
| | (0.18) | (0.07) | (0.51) |
| Middle Age | −0.09 | 0.09 | 0.19 |
| | (0.21) | (0.08) | (0.60) |
| Old Age | 0.33 | 0.13 | 0.86 |
| | (0.26) | (0.11) | (0.71) |
| Black | −0.34 | 0.00 | −0.29 |
| | (0.29) | (0.12) | (0.73) |
| Latino | −0.20 | 0.10 | −0.31 |
| | (0.28) | (0.11) | (0.76) |
| Other Race | 0.31 | 0.04 | −0.09 |
| | (0.32) | (0.13) | (0.76) |
| City | 0.30 | 0.20** | 1.51* |
| | (0.25) | (0.10) | (0.85) |
| Suburb | 0.21 | 0.31*** | 1.03 |
| | (0.23) | (0.09) | (0.84) |
| Town | 0.21 | −0.00 | 0.60 |
| | (0.28) | (0.11) | (0.98) |
| Republican | −0.55** | −0.17** | −1.40 |
| | (0.22) | (0.09) | (0.90) |
| Independent | −0.20 | −0.12 | 1.54** |
| | (0.35) | (0.14) | (0.68) |
| Other Party | −0.15 | 0.01 | 1.45 |
| | (0.55) | (0.22) | (1.10) |
| Moderate | −0.13 | −0.34*** | −1.31** |
| | (0.23) | (0.09) | (0.63) |
| Conservative | 0.14 | −0.01 | −0.69 |
| | (0.25) | (0.10) | (0.73) |
| Other Ideology | −0.22 | −0.61*** | −1.34 |
| | (0.61) | (0.24) | (1.48) |
| Northeast | 0.01 | 0.06 | −0.03 |
| | (0.25) | (0.10) | (0.63) |
| Midwest | −0.17 | −0.10 | −1.32 |
| | (0.23) | (0.09) | (0.87) |
| West | −0.22 | −0.06 | 0.07 |
| | (0.22) | (0.09) | (0.56) |
| Constant | −0.68* | 2.73*** | −4.07*** |
| | (0.37) | (0.15) | (1.17) |
| Observations | 728 | 727 | 728 |

*Notes*: Dependent variable is whether repression was considered (M1), the importance of repression (M2) and whether repression was disqualifying (M3). Logit link functions in M1/M3, linear in M2. Baseline categories for group identification: BLM or WN is a non-threatening out-group. ***, **, * significant at .01, .05, .10, respectively. Other baseline categories listed in Appendix Section B.2.

### B.7.2 Evidence of Group-Centric Voting

The second set of analyses considers questions that evaluate how respondents typically choose between candidates. In the first question, I ask respondents whether their main consideration when selecting candidates is (1) how a candidates' policies affect their own well-being, (2) how the candidates' policies affect the well-being of their friends and family, (3) how the candidates' policies affect the country as a whole, (4) the candidates' party, or (5) the candidates' position on a specific policy. I also ask respondents to rank these categories from most important to least important. My theory suggests that voters tend to make decisions based on how a particular policy affects their own well-being or the well-being of other in-group members. While not directly evaluating this dynamic, the descriptive evidence in Figures B.7 and B.8 suggest that a substantial number of voters do privilege the effect that policies have on in-group members when making their vote choice, even beyond considerations of party identification and specific issues.

Figure B.7: Self-Report of Factors Considered in Vote Choice

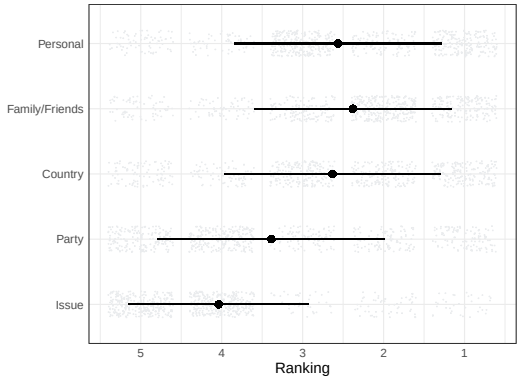



*Notes*: Question text reads "Which of the following statements reflect how you typically choose which candidate to support? Please mark all that apply." X-axis is the count of respondents who marked each of these categories. N = 750.

### B.7.3 Civil-Liberties / Security Trade-offs

In a third set of questions to evaluate causal mechanisms, I ask respondents about their attitudes toward the civil liberties - security trade-off. Specifically, I ask respondents to

Figure B.8: Self-Report of Ranking of Factors in Vote Choice




*Notes*: Question text reads "Now, please rank these statements from MOST IMPORTANT to LEAST IMPORTANT in how you typically choose which candidate to support." Respondents then re-ordered the attributes to reflect their preferred ranking, with 1 being the most important and 5 being the least important. N = 685.

identify on a 7-point Likert scale whether they strongly agree (1) or strongly disagree (7) that (1) "It is important that the government protect citizens from threatening groups, even if it means violating civil liberties," (2) "Sometimes the government must violate civil liberties in order to maintain security," and (3) "It is important that the rights of people with unpopular views be protected, including their right to protest." Figure B.9 shows descriptive statistics for the proportion of respondents marking each value of the Likert scales, with values on the left-hand side representing strong disagreement with each of the statements. Overall we see variation but coalescence around socially desirable responses, with the majority of respondents answering that it is important to protect citizens from threatening groups, sometimes the government must violate rights, and it is important that the rights of protesters be protected.

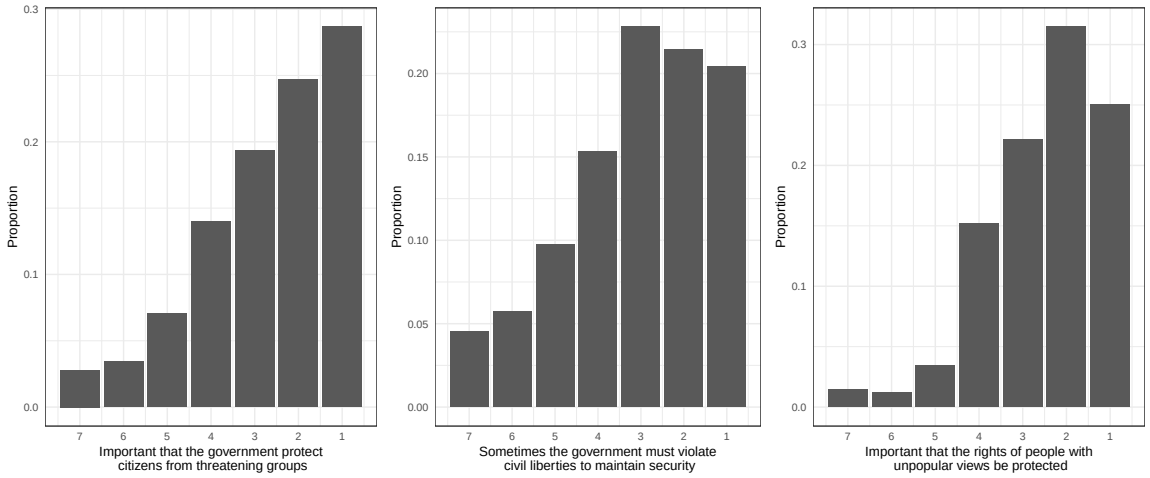
These questions push respondents to consider the relative importance of security versus civil liberties. My theory suggests that respondents who identify as an in-group with a repression target will place a higher value on civil liberties, but respondents who view one of the repression targets as threatening will place a higher value on security. To evaluate this possibility, I code three continuous dependent variables from the questions listed above, where "1" indicates strongly agree and "7" indicates strongly disagree: *Protect from Threats*,

*Maintain Security*, and *Protect Protesters*. I invert the score for *Protect Protesters* to create *Don't Protect Protesters* so that higher values on all three variables indicate relatively higher support for civil liberties vis-a-vis security. I then regress these measures on dummies for respondents' group identification, threat perception, and the standard battery of demographic controls (described in Appendix Section B.2). Across models, positive coefficients indicate relatively higher support for civil liberties relative to the omitted category and negative coefficients represent relatively higher support for security.

I expected that the coefficients for in-group identification would be positive and statistically significant: those who identify with one of the repression targets should place a relatively higher weight on civil liberties than security compared to out-group members. In contrast, I expected that the coefficients for threat perception would negative and statistically significant, signalling that those who view one of the repression targets as threatening will be more likely to support security over civil liberties. The results in Table B.27 do not reflect my expectations. First, there is not significant relationships between the measures of threat perception and the outcomes. Second, the results show that those who identify BLM and white nationalists as an in-group are significantly *less* likely to mark a response that privileges protection of civil liberties. Rather, respondents who are in-group members with repression targets are more likely to agree that the government should protect citizens from threats, even if it means violating civil liberties, and more likely to agree that sometimes the government must violate civil liberties in order to maintain security. One way to make sense of these relationships is that those who identified as an in-group with one repression target may feel threatened by the other target. As a result, they may have had the other group in mind when answering the questions, particularly if they were unlikely to view their own group as a threat. For instance, someone who identified white nationalists as an in-group may not see their own group as threatening, but they may be more likely to see BLM as threatening. In that case, they may answer the questions with BLM in mind and believe that it is important to protect from threats, even if it means violating the civil liberties of *another* group. The last finding is that those who view BLM as an in-group were more likely to agree that it is important that the rights of those with unpopular views be protected. Perhaps in the question that specifically measured protests from unpopular views, BLM

in-group members did respond to the question with their own group in mind.

Figure B.9: Relative Importance of Civil Liberties v. Security




*Notes*: X-axis represents Likert scale from strongly disagree (7) to strongly agree (1) with each statement. N = 750 across all graphs.

Table B.27: Predicting Importance of Civil Liberties v. Security

| | Protect from Threats | Maintain Security | ¬ Protect Protesters |
|---|---|---|---|
| | (1) | (2) | (3) |
| BLM In-Group | −0.40*** | −0.35** | 0.28*** |
| | (0.13) | (0.14) | (0.11) |
| WN In-Group | −0.87*** | −1.07*** | 0.17 |
| | (0.17) | (0.18) | (0.14) |
| BLM Threatening | −0.01 | −0.03 | 0.01 |
| | (0.02) | (0.02) | (0.01) |
| WN Threatening | −0.00 | −0.01 | 0.01 |
| | (0.02) | (0.02) | (0.02) |
| Male | −0.19 | 0.02 | 0.25** |
| | (0.12) | (0.13) | (0.10) |
| Other Gender | −1.33 | −1.15 | 0.88 |
| | (1.10) | (1.18) | (0.92) |
| High Income | −0.10 | −0.09 | 0.14 |
| | (0.13) | (0.14) | (0.11) |
| College | 0.03 | 0.13 | 0.21* |
| | (0.13) | (0.14) | (0.11) |
| Middle Age | 0.05 | −0.15 | 0.05 |
| | (0.14) | (0.15) | (0.12) |
| Old Age | 0.20 | −0.18 | 0.20 |
| | (0.18) | (0.19) | (0.15) |
| Black | −0.11 | 0.12 | 0.20 |
| | (0.20) | (0.22) | (0.17) |
| Latino | −0.04 | 0.04 | 0.28* |
| | (0.19) | (0.21) | (0.16) |
| Other Race | 0.06 | −0.19 | −0.17 |
| | (0.23) | (0.25) | (0.19) |
| City | 0.15 | 0.13 | −0.03 |
| | (0.17) | (0.18) | (0.14) |
| Suburb | 0.09 | 0.02 | 0.13 |
| | (0.16) | (0.17) | (0.14) |
| Town | 0.45** | 0.36* | −0.12 |
| | (0.20) | (0.21) | (0.16) |
| Republican | 0.35** | 0.39** | −0.21* |
| | (0.15) | (0.16) | (0.12) |
| Independent | 0.02 | −0.04 | −0.35* |
| | (0.24) | (0.26) | (0.20) |
| Other Party | −0.34 | −0.07 | −0.04 |
| | (0.38) | (0.41) | (0.32) |
| Moderate | −0.13 | −0.26 | −0.15 |
| | (0.16) | (0.17) | (0.14) |
| Conservative | −0.27 | −0.56*** | 0.23 |
| | (0.17) | (0.19) | (0.15) |
| Other Ideology | 0.28 | −0.32 | 0.10 |
| | (0.41) | (0.44) | (0.34) |
| Northeast | −0.01 | −0.04 | 0.12 |
| | (0.17) | (0.19) | (0.15) |
| Midwest | −0.03 | 0.04 | −0.13 |
| | (0.16) | (0.17) | (0.13) |
| West | 0.06 | 0.07 | 0.14 |
| | (0.15) | (0.16) | (0.13) |
| Constant | 3.03*** | 3.82*** | 4.81*** |
| | (0.28) | (0.30) | (0.23) |
| Observations | 728 | 728 | 728 |

*Notes*: Dependent variable is importance with protecting against threatening groups (M1), agreement that the government must sometimes violate civil liberties (M2) and the (reverse of) the importance of protecting those with unpopular views (M3). Linear models. ***, **, * significant at .01, .05, .10, respectively. Baseline categories for control variables listed in Appendix Section B.2.

## B.8 The Influence of Party Identification

This section examines (1) the relationship between party identification and the main theoretical concepts (group identification and threat perception), (2) the possibility of heterogeneous treatment effects across parties, and (3) whether individuals' baseline propensity to punish repression varies by party. I theorize that group identification and threat perception will be significant predictors for how individuals consider repression in their vote choice. Another factor which may also influence evaluations of repression is partisan identification, closely linked with political ideology. There is existing evidence that partisanship is a strong predictor of individuals' attitudes toward repression (Davis and Silver, 2004; Davis, 2007; Finkelstein et al., 2017; Jenkins-Smith and Herron, 2009; McFarland and Mathews, 2005). Recent research also indicates that political ideology shapes how individuals respond to repression at the ballot box. Aksoy (2018) finds that right-wing leaders are rewarded for counterterrorism activities – arguably a type of repression – in elections in Western Europe. Cordell (2021) finds that left-wing parties are punished for cooperating in the United State's torture programs through extraordinary rendition. In general, liberal parties tend to value rights protections and exhibit less tolerance of overt repression. In light of the influence of party identification, it is crucial to evaluate the extent to which partisanship may confound the results of the main hypothesis tests and/or provide an additional explanation for the relationship between repression and vote share.

I begin by assessing the extent to which in-group identification and threat perception are correlated with membership in particular parties. While the groups of interest in this study are unlikely to perfectly correlate with party identification, it is probable that Democrats are more likely to identify with Black Lives Matter and be threatened by white nationalists, while Republicans would be more likely to identify with white nationalists and be threatened by Black Lives Matter. The correlational evidence in Figure B.10 sheds light on these relationships. Republicans are more likely to view BLM as threatening, less likely to identify BLM as an in-group, less likely to view white nationalists as an in-group, and less likely to view white nationalists as threatening. Democrats are more likely to identify BLM as an in-group, less likely to view BLM as threatening, and more likely to view white nationalists

as threatening. Surprisingly, Democrats are also more likely to identify white nationalists as an in-group. As mentioned in the previous section, it may be that Democrats are more likely to apply the label of white nationalist to others. For the most part, the results of this correlation table reflect the regression results predicting in-group identification and threat perception in Table B.9. There, Republicans were less likely to identify with BLM and white nationalists than Democrats, but there was not a significant relationship between political party and threat perception. Overall, the correlational evidence across Figure B.10 and Table B.9 suggest that political party, group identification, and threat perception *are* correlated. At the same, these concepts are *not* synonymous. Therefore, one should not interpret the main results as reflecting purely the influence of political party.

I next consider whether political party is a confounding variable to the hypothesis tests. Holding political party constant, would we still expect to see the treatment effects in the main analysis? Or, would the relationship between in-group identification, threat perception, and incorporation of repression into vote choice disappear after incorporating the influence of political party? The results from the balance checks rule out the latter possibility. Treatment assignment is balanced for political party membership in all but three instances.[3] Further, even when controlling for political party and ideology in all models (Table B.6) and when these variables are imbalanced (Table B.7), the results for the hypothesis tests are robust. Partisanship can safely be ruled out as a confound.

Though in-group identification with Black Lives Matter is a strong and significant predictor of punishment of repression, it is still possible that those who belong to different parties have varying propensity to punish repression. Table B.28 evaluates this possibility by analyzing the influence of repression on vote share in three partisan sub-samples: Democrats, Republicans, and Non-Partisans. Among Democrats, both non-violent and violent repression of Black Lives Matter is associated with a reduced vote share, as well as violence against white nationalists. However, there is not a significant effect for denial of permits to white nationalists among Democrats. Among Republicans we see the reverse dynamics: only de-

[3]In the BLM in-group sample, Independents were more likely to receive the treatment where the candidate denies white nationalists permits; in the WN in-group sample, Republicans were more likely to receive the treatment where the candidate denies BLM permits; and in the sample of respondents who view BLM as a non-threatening out0group, Independents were more likely to receive the treatment where the candidate supported restricting permits for BLM protesters.

nial of permits to white nationalists is punished, and this is only at the .10 significance level. Among those who do not identify with one of the major parties, there is not a significant relationship between repression and vote share. These results do have commonalities with my hypothesis tests, as Democrats and Black Lives Matter in-group members seem to have similar patterns of punishing repression. Interestingly, the results for Democrats continue to hold when controlling for in-group identification, threat perception, and the other demographic covariates, though the significant coefficient for denial of permits to white nationalists among Republicans evaporates with the inclusion of these controls (results not shown). Overall, the results in Table B.28 suggest that Democrats generally have a higher baseline propensity to punish repression, but that this tendency varies across repression targets. These findings should be interpreted as providing an additional explanation for the relationship between repression and vote share, rather than negating the influence of Black Lives Matter in-group identification.

The above discussion rules out political party as a confounding variable and illuminates the role of party identification in linking repression to candidates' vote share. However, it is still worthwhile to consider whether the results of the hypothesis tests are primarily driven by members of one particular party. To evaluate this possibility, Tables B.29, B.30, and B.31 replicate the results in partisan sub-samples for Democrats, Republicans, and those not belonging to a major party, respectively. These results should be treated with caution given the smaller sample size across many of the models and the fact that the treatment is not block randomized along party lines. Still, these results provide insight into the influence of party on considerations of repression in vote choice.

Turning to Table B.29, which replicates the results among Democrats, the results across all the models are quite similar to the main hypothesis tests, with several distinctions: (1) Democrats who identify BLM as an in-group do not punish violence against white nationalists, (2) those who identify BLM and white nationalists as a non-threatening out-group punish denial of permits to BLM, and (3) those who view white nationalists as a non-threatening out-group punish violence against white nationalists. The main take-away from these results is that the propensity to punish repression increases in some sub-samples but decreases in others. It does not seem to be the case that Democrats across the board are more

willing to punish repression, and there are still variations across the different sub-categories.

Next, Table B.30 replicates the results among Republicans. The distinctions in these models are as follows: (1) there is no longer a significant negative effect for repression of Black Lives Matter among BLM in-group members, (2) those who identify with white nationalists punish repression of white nationalists in the form of denial of protest permits, (3) those who view white nationalists as a threatening out-group no longer punish repression of BLM, and (4) those who view white nationalists as a threatening out-group punish repression of white nationalists in the form of denial of protest permits. Overall, these results demonstrate that the bulk of the treatment effect for in-group punishment of repression for BLM members was driven by those who also identified as Democrats. On the other hand, these results yield some support for Hypothesis 1 in that white nationalist in-group members now punish repression against white nationalists. Part of this finding could be driven by the fact that the measure of in-group identification with white nationalists may be more effective at capturing true in-group identification in the Republican sub-sample. Finally, and in contrast to my predictions, there is some punishment of repression against white nationalists even among those who view the group as threatening. Perhaps even Republicans who view the group as threatening highly privilege respect for the rights of this group, though the explanation for this finding is somewhat uncertain.

Finally, Table B.31 replicates the results in the sample of respondents who do not identify with one of the major parties. These results are by nature difficult to interpret given that this sample is likely to be extremely heterogeneous, including both left-wing and right-wing individuals who are disillusioned with the parties as well as those who truly have no party identification or are moderates. These results should also be treated with caution given that the sample sizes are so small, encompassing responses from only 16 to 80 respondents depending on the model (recall that respondents complete ten conjoint tasks). Perhaps as a result of these dynamics, there are many differences to these results compared to the main hypothesis tests: (1) among BLM in-group members, there is no punishment of repression, (2) those who identify as an in-group with white nationalists reward repression in the form of violence against BLM and denial of permits to white nationalists, (3) those who view white nationalists as a non-threatening out-group reward repression in the form of denial of permits

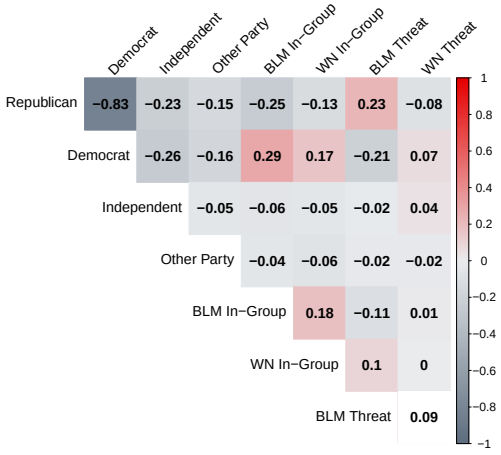
to BLM, and (4) those who identify white nationalists as a non-threatening out-group no longer punish repression against BLM.

What are the overall takeaways from the results broken down by sub-samples? Generally, it seems the primary significant results in the main hypothesis tests, in which BLM in-group members punish repression of BLM, are driven by Democratic respondents. At the same time, there *is* a unique effect of in-group identification, above and beyond the role of party identification, as demonstrated in the models with controls to adjust for imbalance along this variable. There is also some interesting heterogeneity across the parties, as the results suggest that Democrats are not necessarily universally more supportive of rights protections. In the in-group, partisan sub-samples, for instance, only Democrats who identified as an in-group with BLM punished repression of BLM, while only Republicans who identified as an in-group with BLM punish violent repression of white nationalists. Across the sub-samples, Democrats seemed to be more willing to punish repression of BLM, while Republicans seemed to be more willing to punish repression of white nationalists, suggesting that party identification does seem to indicate some group-level affiliation.

Ultimately, these results do not undermine the strong influence of in-group identification with BLM as shaping incorporation of repression into vote choice. However, they do round out our understanding of the role of party identification in showing that both in-group identity and party affiliation likely play a role in influencing how a particular individual evaluates repression at the polls.

### B.8.1 Correlations

Figure B.10: Correlations for Party Identification, Group Identification, and Threat Perception




*Notes*: Cells in white are not significant at the .05 level. N = 749 respondents. Variables for party and group identification are dummies coded "1" if the respondent belongs to the group of interest. Threat perception measured on a scale of 1 to 10 (least to most threatening).

### B.8.2 Treatment Effects by Political Party

Table B.28: Treatment Effects by Party Identification

| | (1) Democrats | (2) Republicans | (3) Other |
|---|---|---|---|
| Deny Permits to BLM | −0.22*** | 0.02 | 0.11 |
| | (0.08) | (0.08) | (0.17) |
| Use Violence Against BLM | −0.24*** | 0.00 | 0.04 |
| | (0.08) | (0.08) | (0.18) |
| Deny Permits to WN | −0.08 | −0.14* | 0.22 |
| | (0.07) | (0.08) | (0.18) |
| Use Violence Against WN | −0.15** | −0.11 | 0.04 |
| | (0.08) | (0.08) | (0.17) |
| Constant | 0.14*** | 0.05 | −0.08 |
| | (0.05) | (0.05) | (0.12) |
| Observations | 7,200 | 6,360 | 1,440 |

*Notes*: Dependent variable is whether or not a profile was selected. Baseline category: candidate supports everyone's right to protest. Standard errors clustered by respondent. ***, **, * significant at .01, .05, .10, respectively.

### B.8.3 Main Results in Partisan Sub-Samples

Table B.29: Effect of Repression on Vote Share Among Democrats

| | *In-Group* | | *Out-Group Non-Threatening* | | *Out-Group Threatening* | |
|---|---|---|---|---|---|---|
| | (1) BLM | (2) WN | (3) BLM | (4) WN | (5) BLM | (6) WN |
| Deny Permits to BLM | −0.23** | −0.04 | −0.31** | −0.23* | 0.19 | −0.32*** |
| | (0.10) | (0.15) | (0.12) | (0.13) | (0.27) | (0.12) |
| Use Violence Against BLM | −0.32*** | −0.00 | −0.20 | −0.21 | 0.09 | −0.42*** |
| | (0.11) | (0.14) | (0.12) | (0.13) | (0.27) | (0.13) |
| Deny Permits to WN | −0.05 | −0.16 | −0.10 | −0.19 | −0.27 | 0.05 |
| | (0.10) | (0.15) | (0.12) | (0.13) | (0.29) | (0.12) |
| Use Violence Against WN | −0.14 | −0.06 | −0.18 | −0.23* | −0.14 | −0.16 |
| | (0.10) | (0.14) | (0.12) | (0.12) | (0.30) | (0.13) |
| Constant | 0.15** | 0.05 | 0.15** | 0.17** | 0.02 | 0.18** |
| | (0.06) | (0.09) | (0.07) | (0.08) | (0.18) | (0.08) |
| Observations | 4,000 | 1,820 | 2,580 | 2,500 | 620 | 2,880 |

*Notes*: Dependent variable is whether a candidate was selected. Standard errors clustered by respondent. Sample is **DEMOCRATS** who identify as BLM in-group (M1), white nationalists in-group (M2), BLM out-group / non-threatening (M3), white nationalists out-group / non-threatening (M4), BLM out-group / threatening (M5), white nationalists out-group / threatening (M6). ***, **, * significant at .01, .05, .10, respectively. Gray boxes represent coefficients used for hypothesis tests.

Table B.30: Effect of Repression on Vote Share Among Republicans

| | *In-Group* | | *Out-Group Non-Threatening* | | *Out-Group Threatening* | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | BLM | WN | BLM | WN | BLM | WN |
| Deny Permits to BLM | −0.18 | −0.12 | 0.08 | 0.03 | 0.09 | 0.04 |
| | (0.14) | (0.20) | (0.15) | (0.12) | (0.13) | (0.12) |
| Use Violence Against BLM | −0.12 | −0.37 | 0.02 | 0.17 | 0.05 | −0.09 |
| | (0.15) | (0.23) | (0.13) | (0.12) | (0.13) | (0.12) |
| Deny Permits to WN | −0.23 | −0.10 | −0.08 | −0.03 | −0.15 | −0.30** |
| | (0.16) | (0.25) | (0.14) | (0.12) | (0.12) | (0.12) |
| Use Violence Against WN | −0.36** | −0.37** | 0.05 | −0.06 | −0.08 | −0.09 |
| | (0.15) | (0.19) | (0.13) | (0.11) | (0.13) | (0.14) |
| Constant | 0.18* | 0.20 | −0.01 | −0.02 | 0.02 | 0.09 |
| | (0.10) | (0.15) | (0.09) | (0.07) | (0.08) | (0.08) |
| Observations | 1,700 | 820 | 2,060 | 3,040 | 2,600 | 2,500 |

*Notes*: Dependent variable is whether a candidate was selected. Standard errors clustered by respondent. Sample is **REPUBLICANS** who identify as BLM in-group (M1), white nationalists in-group (M2), BLM out-group / non-threatening (M3), white nationalists out-group / non-threatening (M4), BLM out-group / threatening (M5), white nationalists out-group / threatening (M6). ***, **, * significant at .01, .05, .10, respectively. Gray boxes represent coefficients used for hypothesis tests.

Table B.31: Effect of Repression on Vote Share Among Non-Partisans

| | *In-Group* | | *Out-Group Non-Threatening* | | *Out-Group Threatening* | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | BLM | WN | BLM | WN | BLM | WN |
| Deny Permits to BLM | 0.16 | 0.56 | 0.10 | 0.41* | 0.06 | −0.31 |
| | (0.34) | (0.35) | (0.23) | (0.23) | (0.46) | (0.29) |
| Use Violence Against BLM | 0.11 | 0.85** | −0.17 | −0.02 | 0.65 | −0.08 |
| | (0.36) | (0.39) | (0.23) | (0.29) | (0.41) | (0.27) |
| Deny Permits to WN | 0.36 | 1.05*** | 0.13 | 0.32 | 0.24 | −0.06 |
| | (0.31) | (0.29) | (0.25) | (0.27) | (0.50) | (0.28) |
| Use Violence Against WN | 0.09 | 0.43 | 0.09 | 0.26 | −0.31 | −0.24 |
| | (0.24) | (0.29) | (0.26) | (0.24) | (0.46) | (0.29) |
| Constant | −0.14 | −0.56*** | −0.03 | −0.20 | −0.14 | 0.14 |
| | (0.21) | (0.17) | (0.16) | (0.17) | (0.29) | (0.19) |
| Observations | 420 | 160 | 800 | 600 | 220 | 680 |

*Notes*: Dependent variable is whether a candidate was selected. Standard errors clustered by respondent. Sample is **NON-PARTISANS** who identify as BLM in-group (M1), white nationalists in-group (M2), BLM out-group / non-threatening (M3), white nationalists out-group / non-threatening (M4), BLM out-group / threatening (M5), white nationalists out-group / threatening (M6). ***, **, * significant at .01, .05, .10, respectively. Gray boxes represent coefficients used for hypothesis tests.

### B.9 Survey Instrument

- Introductory Script: The purpose of this research study is to better understand individual attitudes about social and political issues. For that reason, we will be surveying individuals from the United States and asking them to complete a brief (approximately 15 minute) questionnaire. If you are willing to participate, our questionnaire will ask about your background (e.g., age, race, years of education) and opinions. Survey participants will receive compensation up to $4.50 in the form of cash payments, gift cards, loyalty points, etc. This is an entirely anonymous questionnaire: your responses will not be identifiable in any way and will remain confidential. Your participation is voluntary, and you may withdraw from this project at any time. This study is being conducted by Kelly Morrison, a researcher at the University of Pittsburgh. Please contact kellymorrison@pitt.edu if you have any questions.
- What is your gender? [male, female, other]
- Which of the following categories best describes your household income? [Less than $25,000, $25,000-$49,999, $50,000-$74,999, $75,000-$99,999, $100,000-$124,999, $125,000 or more]
- What is the highest degree or highest level of schooling you have completed? [Less than high school degree, High school degree or equivalent, Some college but no degree, Bachelor's degree, Graduate degree]
- What is your age? [18-34, 35-49, 50-64, 65+]
- Which race/ethnicity best describes you? (Please choose only one) [White, Black or African American, Hispanic/Latino, Other]
- In what type of community do you live? [Rural area, Large city, Suburb near a large city, Small city or town]
- In which state do you live? [Drop-down menu with all states and the District of Columbia]
- In which region do you live? [Midwest, Northeast, South, West]
- Generally speaking, do you think of yourself as a Republican, Democrat, or Independent? [Democrat, Republican, Independent, No preference, Other party, Don't know]

- In general, how would you describe your political views? [Very conservative, Conservative, Moderate, Liberal, Very liberal, Other]
- Please indicate whether you or someone close to you (a close friend or family member) would consider themselves a member of the following groups:
  - Republican Party [Yes, No]
  - Democratic Party [Yes, No]
  - Black Lives Matter [Yes, No]
  - Antifa [Yes, No]
  - Refugees [Yes, No]
  - Islamic extremists [Yes, No]
  - White nationalists [Yes, No]
  - Anti-mask advocates [Yes, No]
  - Animal rights activists [Yes, No]
  - Communists [Yes, No]
- On a scale of 1 to 10, 1 being the LEAST threatening and 10 being the MOST threatening, how threatening do you perceive the following groups to be to U.S. society?
  - Republican Party [1, 10]
  - Democratic Party [1, 10]
  - Black Lives Matter [1, 10]
  - Antifa [1, 10]
  - Refugees [1, 10]
  - Islamic extremists [1, 10]
  - White nationalists [1, 10]
  - Anti-mask advocates [1, 10]
  - Animal rights activists [1, 10]
  - Communists [1, 10]
- In the next section, you will consider a choice between candidates for the governor of your state. Some of these candidates will seem similar to actual candidates and others may seem unusual. That's okay. Just make the best choice about which candidate you would

prefer. When making your choice, presume that both candidates are equally qualified to hold office in terms of character, temperament, and other personal and moral qualities.

- CONJOINT TASKS 1-10. See Table 3.1 for all treatment attributes.
  - On a scale from 1 to 10, where 1 indicates that you definitely WOULD NOT vote for the candidate and 10 indicates that you definitely WOULD vote for the candidate, how would you rate each of the candidates below? [Candidate A [1,10]; Candidate B [1,10]]
  - If you had to choose, which candidate would you prefer to vote for? [Candidate A, Candidate B]
- Now, please share some information about how you made your choice to support one candidate or the other and how you typically decide between candidates in a real election.
- Which candidate attributes did you CONSIDER when choosing between the two candidates. (Please mark all attributes that apply) [Party, Race, Gender, Current Job, Position on Education, Position on the Economy, Position on Healthcare, Position on Guns, Position on the Environment, Position on COVID-19, Position on Immigration, Position on Protests, Position on Abortion]
- How important were each of the candidate attributes to your choice about who to vote for?
  - Party [Not important, Somewhat important, Very important, Extremely important]
  - Race [Not important, Somewhat important, Very important, Extremely important]
  - Gender [Not important, Somewhat important, Very important, Extremely important]
  - Current Job [Not important, Somewhat important, Very important, Extremely important]
  - Position on Education [Not important, Somewhat important, Very important, Extremely important]
  - Position on the Economy [Not important, Somewhat important, Very important, Extremely important]
  - Position on Healthcare [Not important, Somewhat important, Very important, Extremely important]

  - Position on Guns [Not important, Somewhat important, Very important, Extremely important]
  - Position on the Environment [Not important, Somewhat important, Very important, Extremely important]
  - Position on COVID-19 [Not important, Somewhat important, Very important, Extremely important]
  - Position on Immigration [Not important, Somewhat important, Very important, Extremely important]
  - Position on Protests [Not important, Somewhat important, Very important, Extremely important]
  - Position on Abortion [Not important, Somewhat important, Very important, Extremely important]
- Were there any candidate positions or qualities that made it impossible for you to support the candidate? If so, please describe these problematic positions and/or qualities below.
- Which of the following statements reflect how you typically choose which candidate to support? Please mark all that apply. [I consider how a candidates' policies will affect my personal well-being., I consider how a candidates' policies will affect the well-being of my family and friends., I consider how a candidates' policies will affect the country as a whole., I consider what party the candidate belongs to., I consider the candidate's position on one specific issue.]
- Now, please rank these statements from MOST IMPORTANT to LEAST IMPORTANT in how you typically choose which candidate to support. [I consider how a candidates' policies will affect my personal well-being., I consider how a candidates' policies will affect the well-being of my family and friends., I consider how a candidates' policies will affect the country as a whole., I consider what party the candidate belongs to., I consider the candidate's position on one specific issue.]
- Next, please share some information about civil liberties protections and protests in the United States.
- Please indicate your level of agreement with the following statement: It is important that the government protect citizens from threatening groups, even if it means violating civil

liberties. [Strongly agree, Agree, Somewhat agree, Neither agree or disagree, Somewhat disagree, Disagree, Strongly disagree]

- Please indicate your level of agreement with the following statement: Sometimes the government must violate civil liberties in order to maintain security. [Strongly agree, Agree, Somewhat agree, Neither agree or disagree, Somewhat disagree, Disagree, Strongly disagree]
- Please indicate your level of agreement with the following statement: It is important that the rights of people with unpopular views be protected, including their right to protest. [Strongly agree, Agree, Somewhat agree, Neither agree or disagree, Somewhat disagree, Disagree, Strongly disagree]
- Please indicate your level of agreement with the following statement: Individuals from the Black Lives Matter movement often use violence at protests. [Strongly agree, Agree, Somewhat agree, Neither agree or disagree, Somewhat disagree, Disagree, Strongly disagree]
- Please indicate your level of agreement with the following statement: Individuals from the Black Lives Movement often protest illegally. [Strongly agree, Agree, Somewhat agree, Neither agree or disagree, Somewhat disagree, Disagree, Strongly disagree]
- Please indicate your level of agreement with the following statement: White nationalists often use violence at protests. [Strongly agree, Agree, Somewhat agree, Neither agree or disagree, Somewhat disagree, Disagree, Strongly disagree]
- Please indicate your level of agreement with the following statement: White nationalists often protest illegally. [Strongly agree, Agree, Somewhat agree, Neither agree or disagree, Somewhat disagree, Disagree, Strongly disagree]
- Do you have any additional comments about this survey that you would like to share with the researcher? If so, please leave your comments below.

# Appendix C Paper 3 Additional Material

## C.1 Deviations from Pre-Registration

- In the pre-registered survey, the possible responses for the variable *Leader1* were Very supportive; Somewhat supportive; Slightly supportive; and Not at all supportive. I changed to the new Likert scale before data collection for ease of interpretation.
- In footnote 7 of the pre-registration I said that I would code the variable *Don't Support AI* as 1 if the respondents ranked the organization at or above a 2 on the feeling thermometer. This was a typo. I code the value as 1 if respondents ranked AI at or *below* a 2 on the feeling thermometer.
- Lucid over-sampled from my request of 750 respondents. While the pre-registered version of the survey specifies 750 respondents, the total sample included 758 respondents.
- For ease of presentation, I show results with two combined outcome measures, which are the sum of support for human rights across the three outcome measures and the sum of opposition to repressive leaders across the three outcome measures. In the pre-registered version of the study, I planned to present the results separately. However, the results were the same for each of the separate outcome measures, so I combined them to speed interpretation.
- The measure I planned to use for lack of support for human rights required that all respondents answer "bad" or "very bad" to the first question, "not at all important" to the second question, and that international organizations primarily work for foreign groups. After collecting the data, however, I found that only 15 respondents met all three of these criteria. To increase the size of this group, I present results with an alternative variable: respondents can also respond as "neutral" to the second question, and they need only meet two of the criteria, rather than three. Ultimately, the results are the same with both of these measures.
- I added "and oppose repressive leaders" to the hypotheses to better match the two outcome variables that I re-registered.

## C.2 Survey Instrument

- Intro: The purpose of this research study is to better understand individual attitudes about social and political issues. For that reason, I will be surveying individuals from the United States and asking them to complete a brief (approximately 10 minute) questionnaire. If you are willing to participate, the questionnaire will ask about your background (e.g., age, race, years of education) and opinions. Survey participants will receive compensation up to $1.00 in the form of cash payments, gift cards, loyalty points, etc. This is an entirely anonymous questionnaire: your responses will not be identifiable in any way and will remain confidential. Your participation is voluntary, and you may withdraw from this project at any time. This study is being conducted by Kelly Morrison, a researcher at the University of Pittsburgh. Please contact kellymorrison@pitt.edu if you have any questions.
- US_location: Participants should only proceed with the survey if they are currently located in the United States. Are you currently located in the United States? [Yes; No]
- Gender: What is your gender? [Male; Female; Other]
- Income: Which of the following categories best describes your household income? [Less than $25,000; $25,000 to $49,999; $50,000 to $74,999; $75,000 to $99,999; $100,000 to $124,999; $125,000 or more]
- Education: What is the highest degree or highest level of schooling you have completed? [Less than high school degree; High school degree or equivalent; Some college but no degree; Bachelor's degree; Graduate degree]
- Age: What is your age? [18-34; 35-49; 50-64; 65+]
- Race: Which race/ethnicity best describes you? (Please choose only one) [White; Black or African American; Hispanic/Latino; Other]
- Community: In what type of community do you live? [Rural area; Large city; Suburb near a large city; Small city or town]
- State: In which state do you live?
- Region: In which region do you live? [Midwest, Northeast, South, West]

- Party: Generally speaking, do you think of yourself as a Republican, Democrat, or Independent? [Democrat; Republican; Independent; No preference; Other party]
- Ideology: In general, how would you describe your political views? [Very conservative; Conservative; Moderate; Liberal; Very liberal; Other]
- Threat: On a scale of 1 to 10, 1 being the LEAST threatening and 10 being the MOST threatening, how threatening do you perceive the following groups to be to U.S. domestic security? [Republican Party; Democratic Party; Black Lives Matter; Antifa; Refugees; Islamic extremists; White nationalists; Animal rights activists; Communists; Illegal immigrants]
- AI_Familiar: Please rate your familiarity with the following organization: **Amnesty International**. [Very familiar; Somewhat familiar; Not very familiar; I have never heard of this organization]
- AI_Thermometer: Please use the feeling thermometer to express your feelings about **Amnesty International** on a scale of 0 to 10. Ratings above 5 degrees mean that you feel favorable and warm toward the group, ratings at 5 degrees mean that you don't feel particularly warm or cool toward the group, and ratings below 5 degrees mean that you don't feel favorable toward the group.
- IO1: What kind of influence are international human rights organizations having on the way things are going in the United States? [Very good, Good, Neutral, Bad; Very bad]
- IO2: How important do you think it is for international human rights organizations to hold governments accountable for human rights issues? [Very important, Somewhat important, Moderately important; Slightly important; Not at all important]
- IO3: Which of the following statements comes closest to your view: (1) International human rights organizations are primarily dedicated to protecting the rights of people in your country or (2) International human rights organizations are primarily dedicated to promoting the interests of foreign groups? [1; 2; Neither; Don't know]
- Vignette: Consider the following scenario. Black Lives Matter activists have been actively protesting in your state for the past several weekends against systemic racism and discrimination. The governor, a Republican, orders the police to use tear gas and rubber bullets against the protesters. He argues that the protests have begun to pose a threat

to local businesses and the security of the state. Following a recent escalation in violence as a result of these new policing tactics, the governor explains in an interview that the measures are necessary to protect citizens, the local economy, and the safety of the rest of the state. He hopes that the measures will deter future protests. [Treatment: *In reaction to these events, Amnesty International, an international advocacy organization, initiates a campaign to criticize the governor. They shame the governor for using violence, arguing that these actions represent a violation of the human rights of the protesters. The campaign features accounts from multiple victims, who explain how police violence left them with welts on their arms and legs and chemical irritants in their eyes. One activist recounted how she was struck by a flash grenade while distributing food to other protesters. She was transported via stretcher to a private car, which took her to the hospital because no ambulance would come to help. At the hospital, she suffered multiple cardiac arrests and was diagnosed with a concussion and whiplash. In the days since, she has had difficulty breathing while standing and becomes easily fatigued.*]

- HR1: Please indicate your level of agreement with the following statement. The use of tear gas and rubber bullets is an appropriate policing technique. [Strongly agree; Somewhat agree; Neither agree nor disagree; Somewhat disagree; Strongly disagree]
- HR2: How upset do you feel about the governor's use of tear gas and rubber bullets against protesters? [Not at all upset; Slightly upset; Somewhat upset; Very upset]
- HR3: How likely are you to participate in a campaign to ban the use of tear gas and rubber bullets by police officers? [Very unlikely; Somewhat unlikely; Neither likely not unlikely; Somewhat likely; Very likely]
- Leader1: How would you describe your support for the governor's reaction to these protests? [Very supportive; Somewhat supportive; Neither supportive nor unsupportive; Somewhat unsupportive; Very unsupportive]
- Leader2: Do you approve or disapprove of the way this governor is handling his job? [Strongly approve; Somewhat approve; Neither approve nor disapprove; Somewhat Disapprove; Strongly Disapprove]
- Leader3: How likely would you be to reelect this governor in the next election? [Very likely; Somewhat likely, Neither likely nor unlikely; Somewhat unlikely; Very unlikely]

- Conclusion: The discussion of police violence in this survey draws from a recent report from Amnesty International. To view the report, published in August 2020, click **here** or visit **amnestyusa.org** to learn more about human rights in the United States. (**Make sure to return to this page to complete the survey**). If you have any additional comments about the survey that you would like to share with the researcher, please leave them below.

### C.3 Coding Rules for Demographic Control Variables

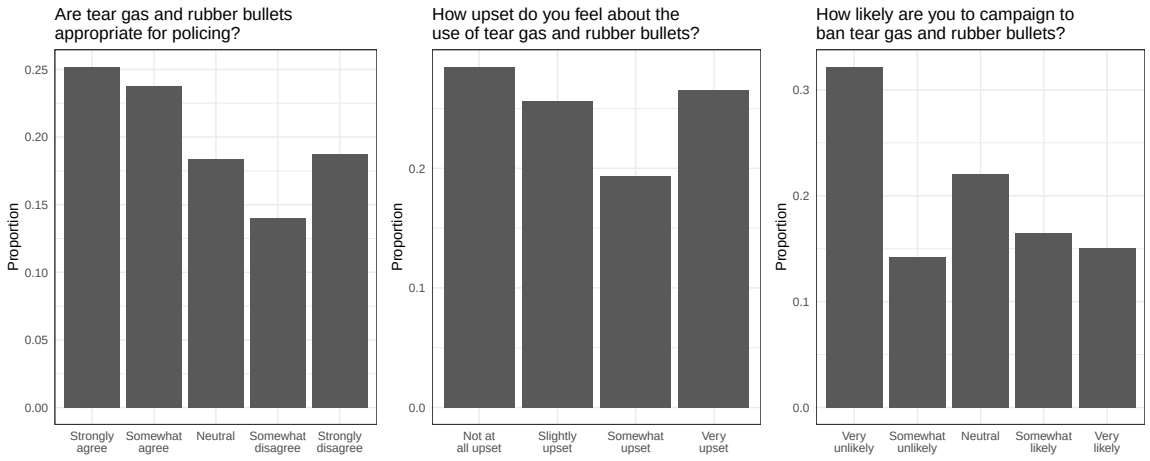
- *Gender*: male, other, female (omitted)
- *Income*: low (omitted: combines less than $30,000 and $30,000-$50,000), high (combines $50,000-$74,999 and $75,000 or more)
- *Education*: less than college (omitted: combines less than high school degree, high school degree or equivalent, some college but no degree), college (combines Bachelor's degree and Graduate degree)
- *Age*: young (omitted: 18-34), middle aged (combines 35-49 and 50-64), old (65+)
- *Race*: white (omitted), Black or African American, Hispanic/Latino, other
- *Community*: rural (omitted), large city, suburb near a large city, small city or town
- *Region*: Northeast (Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island, Vermont, New Jersey, New York, Pennsylvania), Midwest (Illinois, Indiana, Michigan, Ohio, Wisconsin, Iowa, Kansas, Minnesota, Missouri, Nebraska, North Dakota, South Dakota), South (omitted: Delaware, Florida, Georgia, Maryland, North Carolina, South Carolina, Virginia, District of Columbia, West Virginia, Alabama, Kentucky, Mississippi, Tennessee, Arkansas, Louisiana, Oklahoma, Texas), West (Arizona, Colorado, Idaho, Montana, Nevada, New Mexico, Utah, Wyoming, Alaska, California, Hawaii, Oregon, Washington)
- *Party ID*: Democrat (omitted), Republican, Independent, other (combines no preference, other party, and don't know)

- *Ideology*: liberal (omitted: combines very liberal and liberal), moderate, conservative (combines very conservative and conservative), other

## C.4 Descriptive Statistics

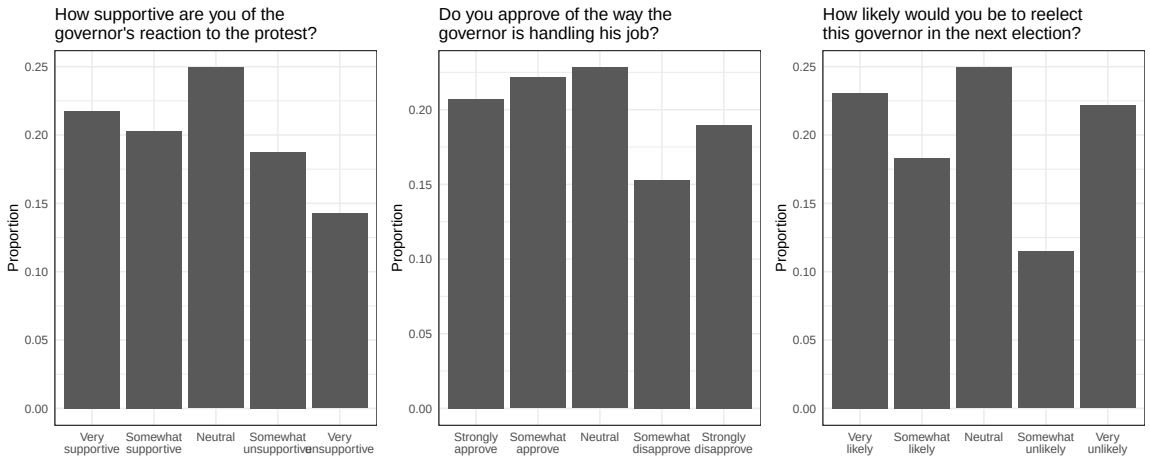
### C.4.1 Variation in Outcome Variables

Figure C.1: Variation in Support for Human Rights




*Notes*: Variation in the first set of outcome variables, measuring support for human rights. Higher values imply greater support for human rights and greater opposition to repression. N = 758.

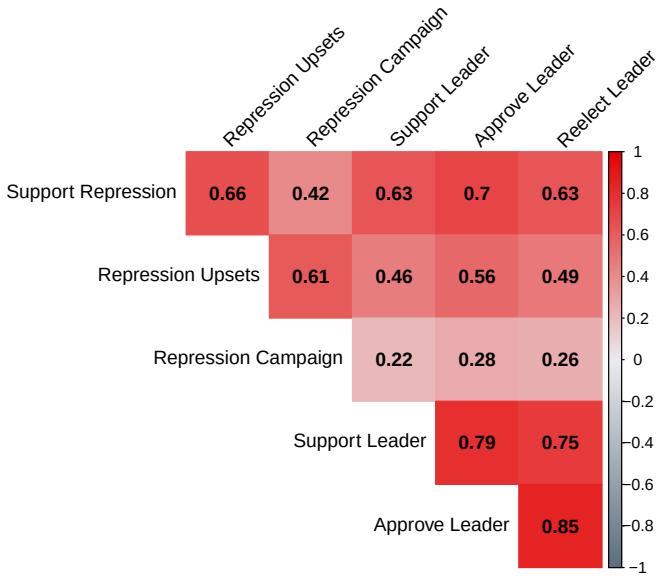
Figure C.2: Variation in Support for the Repressive Leader




*Notes*: Variation in the second set of outcome variables, measuring support for the leader who supported repression. Higher values imply *lower* levels of support for the leader (higher levels of support for human rights protections. N = 758.

### C.4.2 Correlation Across Outcome Variables

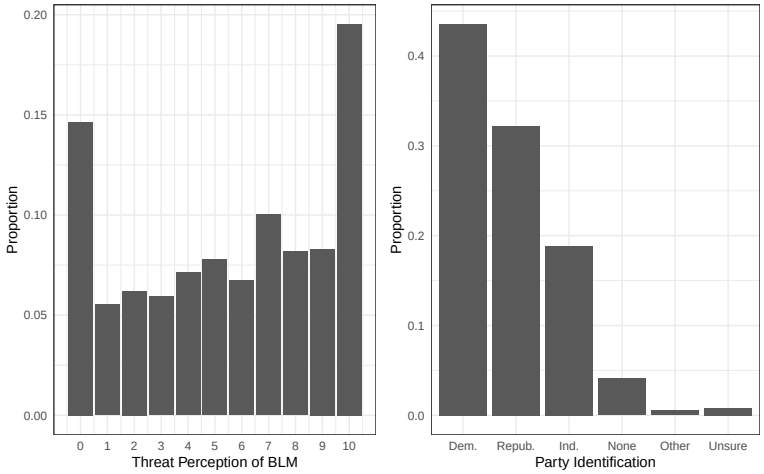
Figure C.3: Correlations Across Support for Human Rights and the Repressive Leader




*Notes*: Correlation coefficients across both sets of outcome variables. All coefficients were significant at the .95 level.

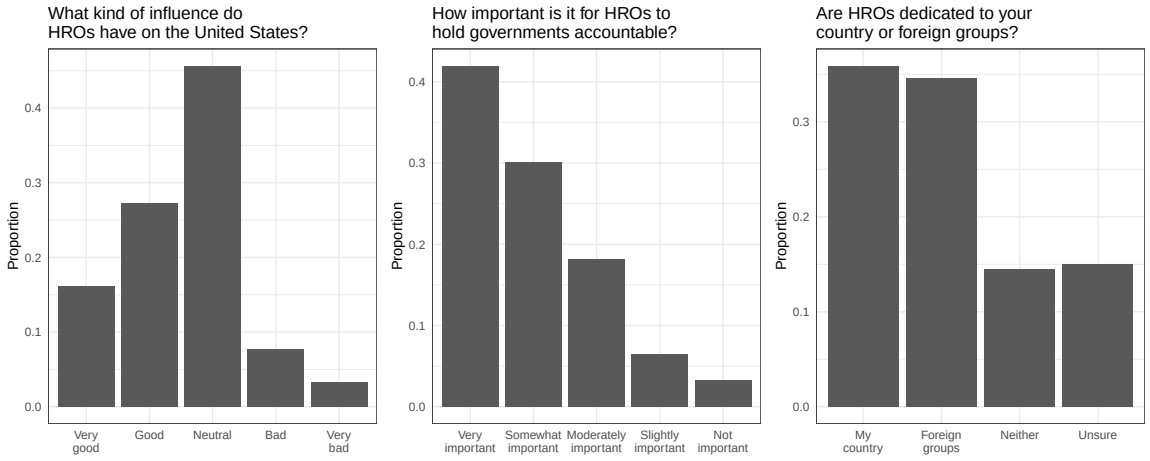
### C.4.3 Variation in Independent Variables

Figure C.4: Variation in Threat Perception and Party Identification




*Notes*: Variation in threat perception of Black Lives Matter and party identification. N = 758.

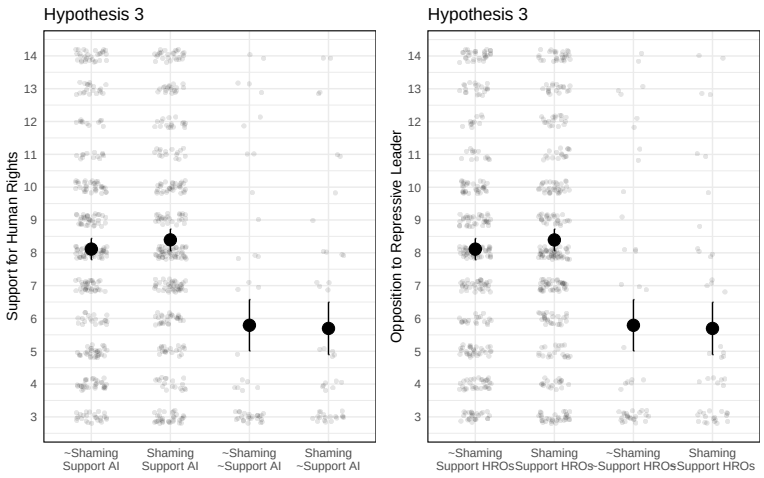
Figure C.5: Variation in Support for Human Rights Organizations




*Notes*: Variation in support for international organizations. N = 758.

## C.5 Alternative Measure of Support for HROs: Opposition to Amnesty International

Figure C.6: Effect of Shaming on Support for Human Rights, Alternative H3 Tests




*Notes*: The effect of shaming on support for human rights by support for Amnesty International. Panel 1 represents results for the first dependent variable (support for human rights) and panel 2 represents results for the second dependent variable (opposition to the repressive leader). 90% confidence intervals. N = 758.

## C.6 Balance Check

Table C.1: Balance Check

| | Shaming |
|---|---|
| BLM Threatening | 0.09 |
| | (0.17) |
| Republican | 0.01 |
| | (0.22) |
| Don't Support HROs | −0.21 |
| | (0.28) |
| Male | 0.15 |
| | (0.16) |
| Other Gender | 13.70 |
| | (535.41) |
| High Income | −0.11 |
| | (0.16) |
| College | 0.01 |
| | (0.16) |
| Middle Age | −0.15 |
| | (0.17) |
| Old Age | 0.31 |
| | (0.24) |
| Black | 0.10 |
| | (0.24) |
| Latino | 0.09 |
| | (0.30) |
| Other Race | −0.34 |
| | (0.30) |
| City | 0.24 |
| | (0.23) |
| Suburb | −0.03 |
| | (0.21) |
| Town | −0.12 |
| | (0.24) |
| Independent | −0.19 |
| | (0.22) |
| Other Party | −0.40 |
| | (0.37) |
| Moderate | 0.09 |
| | (0.21) |
| Conservative | −0.07 |
| | (0.24) |
| Other Ideology | 0.37 |
| | (0.56) |
| Northeast | 0.09 |
| | (0.21) |
| Midwest | −0.11 |
| | (0.21) |
| West | −0.05 |
| | (0.21) |
| Constant | 0.01 |
| | (0.28) |
| Observations | 758 |

*Notes*: Logit models. Dependent variable is shaming. N = 758. Omitted categories listed in Appendix Section C.3.

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