

# The Price of Order: How Violent Crime Weakens Democracy

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## Abstract

Security is the state's most fundamental function. When voters perceive that the state can no longer sustain order, they may prioritize protection over democratic rights, increasing support for authoritarian candidates. This paper tests this argument using a wave of violent bank heists that terrorized thousands of Brazilian municipalities between 2016 and 2020. Combining this original dataset with the 2018 election results, we exploit quasi-random variation in cross-municipal exposure. Results show that these attacks increased support for Jair Bolsonaro, a Presidential candidate who explicitly promoted extrajudicial actions against crime. Additional survey data confirms that the attacks also eroded institutional trust and increased tolerance for authoritarianism, revealing the broader risks that crime poses to democratic stability. Yet despite this national-level response, voters failed to punish incumbent governors—the officials directly responsible for security—showing that organized violence can reshape elections in ways not predicted by standard accountability models.

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The rise of politicians advancing authoritarian agendas in democracies raises an important question: why do voters support them in the first place? One potential explanation is that voters turn to these politicians when their perceptions of insecurity increase, which these candidates exploit by positioning themselves as the solution to crime, disorder, and chaos. In the narrative touted by authoritarian candidates, a security crisis requires more than just conventional politics or policy shifts, but a strong leader capable of restoring order – even if that takes extraordinary measures such as the use of excessive force, interference in local governments, or the disregard of civil rights.

Still, it is unclear whether these authoritarian appeals resonate among voters who feel unsafe, particularly because assessing the causal effects of insecurity on actual electoral outcomes is challenging: voters' perceptions of insecurity are almost always endogenous to the campaigns of the candidates running for office, and to the factors that determine the violence generated by non-state actors. Candidates who use fear to promote social divisions may intentionally exaggerate threats, while real threats, such as terrorist acts or drug-trafficking attacks, are often strategically targeted to produce political consequences. All these factors confound the substantive interpretation of the electoral effects of insecurity perceptions, as well as complicate their estimation.<sup>1</sup>

This article fills this important gap by examining whether exposure to overwhelming, salient, and violent criminal events makes authoritarian candidates more appealing to affected voters—and why. Our research design, combined with a novel dataset on violent crime, provides the leverage needed to overcome the challenge of identifying effects. We study thousands of violent bank heists that terrorized Brazilian municipalities between 2016 and 2020, and their influence on the 2018 national election. These heists consist of high-impact, overwhelmingly violent events perpetrated by roving bandits across small towns in the countryside. They are also fundamentally apolitical: we show that the choice of target was unrelated to the local political climate, the particular crime dynamics of the targeted communities, or other factors that are co-determinants of terrorism and cartel wars.

Our research design provides causal estimates of the effect of large heists under fairly benign assump-

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<sup>1</sup>Ordinary crimes arguably generate a different response from voters than organized violence. They do not necessarily instill a widespread sense of insecurity, as it is often not visible enough to the population, even when it is frequent. Moreover, commonly used proxies of crime, such as homicide rates, are highly correlated with local socio-economic characteristics.

tions. The comparison group of our empirical analysis comprises only municipalities that have been targeted by bandits close to the election day. Those who were attacked before the election comprise our treatment group, and those who were affected afterward are our control group. We only make comparisons within states, given that police forces are organized at that level, and so are our original data sources. Thus, our main identification assumption is that the timing of the attacks in a given municipality within a state, in relation to the election date, is exogenous to factors that might otherwise influence voting outcomes. We show that treatment and control groups are balanced on several covariates, and support the validity of our design with a series of robustness and placebo tests.

Now, why would voters affected by violent, coordinated attacks prefer an authoritarian candidate? We argue that public security differs fundamentally from other government services: safety is the state's most foundational responsibility (Hobbes, 1960; Olson, 1993; Tilly, 1985; Williams, 2009), and protection ranks above other citizens' priorities, including not only other public goods but also their own constitutional rights (Gratton and Lee, 2023; Mittiga, 2022). Events that make voters feel deeply unsafe, and therefore perceive that the state can no longer maintain order, may reshape electoral behavior in ways that standard models of democratic accountability are not equipped to explain. By making the restoration of order an overriding priority, insecurity leads citizens to trade liberty for safety and legitimizes political action that trespasses democratic norms.

The heists in our data have a singular characteristic that makes them ideal for the examination of the effects of violence that can convey a sense of institutional collapse. Contrary to everyday crimes, which succeed by evading law enforcement, these heists were planned to out-compete security forces through terror and armed confrontation. The gangs perpetrating these actions carried high-caliber weapons, used explosives, and employed different terror tactics such as building barricades with vehicles on fire. They often also used the local population in their escape plan, forcing passersby and bank employees to undress and act as human shields. We focus on events that targeted smaller municipalities—many with a single bank in town—where the attacks were salient in real time by large segments of the local population, and left both a generalized sense of powerlessness and a trail of material destruction.

We study the political implications of heists in the 2018 federal and state elections in Brazil. This elec-

tion provides a unique opportunity to measure the demand for authoritarian candidates. It was the first time since 1989 that a presidential race was not polarized between the Workers' Party (PT) on the Left and PSDB (Brazilian Social Democracy Party) on the Right. Instead, PT's main opponent and eventual election winner was Jair Bolsonaro, a former army captain who, up to that point, had a largely fringe career championing far-right views in Congress. These views include openly supporting the defunct military regime, advocating for extrajudicial police violence, and justifying the use of torture. In hindsight, Bolsonaro's authoritarian promise was fulfilled, as he is now serving prison time for planning a coup d'état.

Our estimation shows that voters respond strongly to heists. Where the bandits attacked, Bolsonaro's vote share in the first round of the election increased 3.4 percentage points—a 10% increase over the baseline. These votes were captured from candidates of the Left and the Right, and the electoral gains also extended to congressional candidates from Bolsonaro's party (PSL - Social Liberal Party). Interestingly, voters also had the option of a different candidate on the right, but without the authoritarian dimension: PSDB's second-time contender, Geraldo Alckmin. Yet, Alckmin's performance was not positively affected by the attacks, despite a favorable track record in public security from his four tenures as governor of Brazil's largest state, where the homicide rate declined significantly compared to the national trend.

We confirm our theoretical assertions regarding insecurity and the preference for authoritarian measures using survey data from two waves of the Latin American Public Opinion Project ([LAPOP, 2016-2019](#)). Combined with our bank heist data, these surveys allow us to examine citizens' views on safety and state institutions in some municipalities of our original sample. Despite sample size constraints, the results are widely consistent with the attacks triggering a generalized loss of confidence in the state: voters in locations that were attacked in the year preceding the survey were more likely to declare feeling unsafe, less likely to trust the police, and to say that they respect or are proud of the Brazilian political institutions. They are also less likely to believe that the Brazilian government protects the basic rights of its citizens. More importantly, they are more likely to explicitly accept a trade-off between democracy and safety by revealing that a military coup is justified when crime is too high.

Conventional accountability models cannot explain why the heists are irrelevant precisely at the level where incumbents have the most policy influence. Despite the strong electoral response in the federal

ances, we find no significant effects among gubernatorial candidates, even though governors have the most authority over public security (Laterzo, 2024). Incumbents—either candidates or parties—do not lose support after the attacks, and voters do not favor outsider candidates in their aftermath. These results are particularly striking in the Brazilian context, where voters are highly responsive to the performance of politicians in delivering public services in other areas (Boas, Hidalgo, and Toral, 2021; Bueno, 2023; Ferraz and Finan, 2011; Fujiwara, 2015; Schiumerini, 2025). What is more, none of the gubernatorial candidates in our sample had a clear law-and-order platform like Bolsonaro, and very few had a law enforcement or military background. Standard electoral competition models would suggest that these effects—and these types of candidates—should appear at the subnational level, where incumbents can directly influence policy.

This paper makes additional contributions. By providing causal evidence of the effect of violent and salient crime on actual political outcomes, our findings also fill a gap in a literature that has consistently detected in surveys a relationship between individual victimization and public security policies of authoritarian nature, both in Brazil (Albarracín and Tiscornia, 2024; Harig, 2022; Masullo, Krakowski, and Morisi, 2025) and in other contexts (Ley, 2018; Ventura, Ley, and Cantú, 2024; Visconti, 2020). Another important distinction of this study is that we examine the effect of events that are salient to entire communities, in contrast to the impact of individually experienced ordinary crime most often examined in this literature. Thus, it is not surprising that the events here shift voters' views on authoritarianism that go beyond the field of public security, reaching issues such as their tolerance for a military coup.

Our theory and results also bridge the work on the electoral impact of violence and the democratic backsliding literature. While we define our main dependent variable as support for authoritarian politicians, this characteristic is often bundled with others from populist leaders who have enjoyed electoral success in recent years (Mudde, 2007; Doyle, 2011; Inglehart and Norris, 2017; Hunter and Power, 2019). We provide a clear empirical test for the effect of crime on the demand for anti-establishment, populist, authoritarian politicians, confirming that exposure to violence leads voters to demand those who run on more general non-democratic political platforms.

The paper continues as follows: in the sequence, we lay our theoretical argument and its implications

that can be applied to the data, followed by a detailed background on the bank heists. We then present the context for the 2018 election, focusing on how public security concerns shaped the campaign. After presenting details on the data collection process, we explain our empirical strategy and discuss the results.

## POLITICS AND STRUCTURAL THREATS TO SECURITY

Providing order and security to its citizens is the primary function and legitimizing factor of the modern state, and citizens are willing to relinquish prized liberties and resources if that is the cost for feeling safe (Hobbes, 1960; Weber, 1978; Tilly, 1985). Without safety from expropriation, chaos, and violence, economic activity stalls (North, 1990), and other political demands become secondary (Huntington, 2006). With security, citizens' demands shift away to other areas, giving leaders incentives to provide public goods (Olson, 1993; De Mesquita et al., 2005). In democracies, this translates into politicians competing to distribute more and better policies and services to voters.

Yet, threats to the state's primary role in maintaining security are never fully eliminated. Even consolidated democracies face domestic and international challenges to their ability to keep citizens safe. Such threats have multiple sources that include mass immigration (Hangartner et al., 2019), racial resentment (Mendelberg, 2001), drug trafficking (Flores-Macías and Zarkin, 2021), terrorism (Mudde, 2007), and rapid economic decline (Autor, Dorn, and Hanson, 2013). The question we ask is, how do voters respond when they perceive the state is losing its capacity to provide security?

In democratic settings, it is natural to assume that voters will respond to insecurity as they would when incumbents underperform in other public policy areas. Retrospective voters would be less likely to reappoint incumbents, and forward-looking voters would understand that current leaders are less competent than anticipated, increasing the odds of a power turnover (Ashworth, 2012).

However, if security constitutes a core element of political legitimacy, it precedes electoral accountability. Rather than expecting better governance from a different candidate, voters might prioritize assurances that public authorities can prevent the encroachment of powerful non-state rivals who have the capacity to use violence to fulfill their goals. Above any other consideration, voters may be past the stage

of picking candidates under incomplete information. Instead, they may be demanding a different type of politician: one that is fully committed to placing security first, even at the expense of democratic rights.

This article argues that in contexts of organized violence, voters may interpret security threats not merely as policy failures but as indicators of a deeper erosion in the state's capacity to protect its citizens. At their core, regular electoral accountability and what we call structural threats to security lead to the same expectation: citizens exposed to violent crime will prioritize security concerns at the polls and favor candidates who explicitly propose solutions. However, the structural view of insecurity has crucial implications that cannot be directly derived from a policy-centered view of the issue. Exposure to overwhelming violence, such as that perpetrated by organized crime, terrorists, or domestic insurrectionists, may lead voters to support an authoritarian politician who puts order and security above other state actions – if such a candidate is on the ballot. The choice, then, is not simply a matter of replacing an incompetent incumbent or voting for a candidate who champions public security proposals, but to decide whether or not to sacrifice rights to regain order.

This concerning trade-off has long been recognized, and it is carefully articulated in [Mittiga \(2022\)](#). It stems from the hierarchy of priorities, in which the provision of safety is a necessary condition for the state's legitimacy, which “may necessitate recourse to emergency powers, which are often authoritarian in character and scope” ([Mittiga, 2022](#)). Recently, cases of voters turning to authoritarian candidates against perceived threats have become common ([Inglehart and Norris, 2017](#)). In the Philippines, Rodrigo R. Duterte explicitly declared during the 2016 campaign that he would encourage extrajudicial killings of criminals ([Thompson, 2016](#)). Similarly, Nayib Bukele in El Salvador campaigned on hardline policies against widespread *maras* and, once in power, implemented an unprecedented strategy that proved effective in reducing crime, albeit at the cost of repeated civil rights violations ([Meléndez-Sánchez and Vergara, 2024](#)).

However, campaigning on order is not limited to the developing world, nor necessarily backed by actual threats. Donald Trump littered all three of his national runs for United States President with messages of ungoverned cities and rampant crimes committed by *bad hombres* ([Reuters, 2024](#)). In 2025, as President, Trump deployed the National Guard in several cities to fight against “lawlessness” ([White](#)

House, 2025). Although in this paper we do not examine why these candidates emerge and why they choose a platform of security threats, these recent examples attest that authoritarian candidates actively try to create a sense of insecurity as a way to mobilize voters. In the empirical part of this study, we focus on Jair Bolsonaro in Brazil, an authoritarian figure whose mobilization tactics we describe in more detail in the next section.

The main difference between the campaigns of these strongmen and non-authoritarian types is not the focus on insecurity, or even the “tough-on-crime” approach. In fact, many mainstream candidates support punitive rather than preventive solutions to crime. The distinction lies in the framing: when strongmen treat crime as a major risk to national stability, they frame the problem as foundational rather than a policy failure. The message is that the state can no longer maintain its citizens’ safety, so traditional policy responses such as funding for police intelligence, changes in policing tactics, or even poverty alleviation approaches are insufficient. While doing so, they position themselves as restorers of order: if the state is to retain its legitimate monopoly on force, it has to act forcefully. The solutions derived from this framing are often drastic and justify actions such as the use of excessive force, federal intervention in policy domains controlled by subnational politicians, and a disregard for democratic norms, human rights, and even the constitution.

An additional implication of a structural framing of insecurity is that it primarily applies to national politics. The performance of candidates running for subnational office, which often holds the prerogative to deploy security agents and police forces, should not be severely affected by security threats within this framework. If increases in violence are not perceived as threats to the national order, subnational incumbents should be punished as classic models of accountability predict. However, in the case of structural threats to security, the stakes are above their jurisdiction. Empirically, this implies that we should not see an electoral response in subnational governments.

## **Bringing the Theory to the Data**

Research focused on discerning the impact of intentional violence on electoral results, particularly within the theoretical context presented above, must contend with at least two major challenges. First,

the empirical measure of crime needs to be based on events that are likely to elicit a sense of structural breakdown in security; that is, crime must be violent, destructive, and also salient, with visible effects and costly implications for entire communities. Ordinary crimes against individuals occur even in the world's safest countries and do not necessarily signal the erosion of state capacity.

Second, it is often hard to disentangle the effects of exposure to crime from other correlated factors, particularly because large-scale violence is often political in nature. Terrorist attacks, for example, are violent events carried out by armed groups that defy an established order. These are salient and visible by design (Powell, 2007), and also aimed to discredit the state when successful (Balcells and Torrats-Espinosa, 2018). However, since terrorism is inherently a political act, it is often targeted across time and space to affect political outcomes (Aksoy, 2014; Berrebi and Klor, 2008). Terrorist and rebel groups' attacks are typically aimed at promoting political agendas that deliberately confront the incumbent regime (Fortna, 2015) or target minorities (Siddiqui, 2022). Thus, it is empirically difficult to isolate the electoral effects of the attacks from those of the endogenous factors that determine their occurrence. Even when researchers manage to do so, the interpretation of the links between terrorism and elections is often muddled by voters' xenophobia, nationalist, ethnic, or separatist sentiments (Godefroidt, 2023), for example, and not only due to a perception of dwindling state ability to protect its citizens.

Research designs that rely on violent events perpetrated by organized crime might suffer from similar problems. Local organized crime activity, and consequently crime levels, are endogenous to both the political and socioeconomic environment of each region (Dell, 2015). The timing of organized crime violence is often also deliberate. Attacks taking place before elections are intended to influence voting outcomes, while those taking place after elections aim to influence policy (Daniele and Dipoppa, 2017; Alesina, Piccolo, and Pinotti, 2019). The struggle for territory control among criminal groups (Dal Bó, Dal Bó, and Di Tella, 2006) also has consequences for the levels of homicide, extortion, and general lawlessness (Yashar, 2018; Magaloni, Franco-Vivanco, and Melo, 2020). Worse yet, the local presence of drug-trafficking operations could signal the infiltration of these groups within the state (Albarracin, 2018; Trejo and Ley, 2020; Daniele and Dipoppa, 2017), which in itself will have direct electoral effects.

Even when crime is non-political by nature, one must contend with the fact that its incidence is highly

correlated with local inequality (Soares, 2004), unemployment (Britto, Pinotti, and Sampaio, 2022), public services delivery (Blattman et al., 2021), and social segregation (Novaes, 2024)– all factors that have nuanced electoral consequences. Moreover, as we mentioned, everyday crime does not represent structural threats to security. Much of the work based on observational studies focuses on the impact of homicides on political choices. Demonstrably, their gruesome nature affects not only murder victims, but their families and communities (Soares, 2006). However, despite high homicide rates, murders in Latin America concentrate among young men living in poorer areas of their cities (Novaes, 2024). Most of these crimes are not visible to non-victims and their social circle, and their impact on the general perceptions of insecurity is mediated by local conditions such as the presence of media (Marshall, 2023), which creates an additional source of endogeneity. Additionally, their localized profile within areas and social groups might not be conducive to being interpreted as a general view of state failure by communities.

Finally, while survey work that relies on variation in victimization across respondents has shown that personal exposure to crime has political implications (Ley, 2018; Ventura, Ley, and Cantú, 2024), this approach differs from ours at a conceptual level: our “treatment” is not the individual reaction or anger brought on by victimization (Bateson, 2012; García-Ponce, Young, and Zeitzoff, 2023), but the perception shared by communities that the state has failed its fundamental role.

**Bank Heists in Brazil as a Research Design.** To overcome these limitations, we study the phenomenon of *Novo Cangaço* in Brazil, which instilled fear and terror in thousands of Brazilian municipalities for a few years.<sup>2</sup> *Novo cangaço* gangs were roving bandits who invaded and immobilized entire small towns to rob banks. The *modus operandi* of these gangs was very similar across the country, with heists that were intentionally aimed to instill fear and terror in the local population and security forces. They were heavily armed with assault rifles that vastly surpassed the firing power available to local police forces, and in the vast majority of cases, they used explosives to break open vaults and ATMs. They also resorted to terror tactics such as kidnapping citizens and strapping them on top of vehicles, or keeping bank employees and clients undressed at gunpoint in the middle of the street. These bandits also relied on displays of overwhelming force to preemptively stop any police response: they would fire multiple

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<sup>2</sup>The name *Novo Cangaço* has “new” (in Portuguese) because the methods operated by criminals are similar to banditry groups (*cangaceiros*) operating in the early Twentieth century in the Brazilian Northeast semi-arid region (Aquino, 2020).

rounds against police stations upon entering the town, or set up barricades with burning cars to restrict movement around the banks.

Bank branches were often destroyed, leaving behind long-term struggles for local communities. Such was the case in Criciúma, a medium-sized city in the relatively safe state of Santa Catarina. In 2020, thirty criminals coordinated a massive robbery, stealing a bounty worth at least 20 million dollars. They were armed with automatic rifles and explosives, took many hostages, and gravely wounded a police officer. [Mariani, Ornelas, and Ricca \(2025\)](#), who first used bank heists to study the uptake of digital financial services, show that small cities with few or just one bank branch stayed without proper access to bank services for months (although they do not find a significant decline in economic activity). After assembling a unique dataset of such attacks in different Brazilian states over the 2016-2020 period, we have counted 1,649 similar heists, with varying degrees of violence, preparation, and success—in page 15 we describe our data in detail.

This type of criminal activity also fits the framework proposed in [Olson \(1993\)](#): here the state, or the “stationary bandit”, appears to be failing on its primary function of protecting citizens from roving bandits. Not surprisingly, the salience, visibility, and public impact of these heists are very different from ordinary crime. While cell-phone snatching, strong-arm car robberies, burglaries, and many other violent crimes certainly have an impact on political choices, they are smaller in scale, targeted at individuals, and often carried out when the police are not present ([Di Tella and Schargrodsky, 2004](#); [Blattman et al., 2021](#)). The heists here were brazen and planned to counter and even overwhelm police responses. Also, they were not aimed at specific individuals but designed to shock and awe entire communities into inaction.

While these events were as shocking as terrorist attacks, they lacked political targeting or motivation.<sup>3</sup> What is more, the gangs were never locally based,<sup>4</sup> they had no intention to control local crime or secure drug corridors, and selected their targets independently of local political dynamics ([Aquino, 2020](#)). In short, they are essentially events without any direct political aim, and to a large degree, exogenous to local conditions. This allows us to overcome the inherent endogeneity that permeates voters’ perceptions of

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<sup>3</sup>We test this conjecture later in our empirical strategy.

<sup>4</sup>These gangs would usually hide in rural properties, and attack municipalities within the same state, sometimes traveling hundreds of kilometers to their target. There are reported cases of gangs also operating across state borders.

insecurity and politics for a more precise estimation of the impact of violence on elections.

In the next section, we provide a background on the dynamics of crime and public security in Brazil around the federal and state elections of 2018. We show that public security was a salient issue on that year. We also explain how our empirical strategy is carefully designed to provide as-if random variation of attacks across municipalities in the same state, under fairly benign assumptions. Finally, we show empirical evidence to support the claim that these attacks were not targeted at municipalities based on political, economic, or demographic characteristics.

## CRIME, POLICE AND POLITICS IN BRAZIL

Organized crime is pervasive and politically salient in Brazil. During 2018, the year of the election we analyze, the homicide rate in the country was still climbing to reach its historic high. Unsurprisingly, voters surveyed in the period indicated that public security was one of their major priorities. In the seven LAPOP surveys between 2006 and 2019, 24% of Brazilian respondents stated that crime is the country's main problem, second only to "jobs and the economy" (26%) and ahead of health and education (20%).<sup>5</sup>

Brazilian citizens are acutely aware that the state's monopoly on violence cannot be taken for granted. The country has more residents living under criminal governance than any other in Latin America (Uribe et al., 2025). The city of Rio de Janeiro exemplifies the state's failure to maintain effective territorial rule, where paramilitary militias and drug trafficking organizations control a considerable portion of the territory (Cavguas, Bruce, and Meloni, 2023). In fact, the state of Rio de Janeiro was under federal intervention in 2018, with the local police placed under the command of an army general. But Rio is not an exception. Given the numerous documented cases of criminal governance in Brazil, it is understandable that citizens would be apprehensive about the erosion of the state's coercive power when a city comes under attack by *Novo Cangaço* gangs.

Federal interventions like the one Rio experienced are rare. The Brazilian public security system is decentralized, with states being mainly responsible for the service. Governors control both an investiga-

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<sup>5</sup>The question was: "In your opinion, what is the most severe problem that the country is facing?" Voters could pick one of several options. The following options were coded as security-related answers: Security, Crime, Violence, Gangs, Kidnappings, Armed conflict, and Drug traffic.

tive branch (*Polícia Civil*) and the military police (*Polícia Militar*), which is the main uniformed force responsible for patrolling and maintaining the public order. These forces together account for 83% of all police officers in the country. The Federal Police, with 4% of all officers, focuses on border issues, white-collar crime, and some dimensions of organized crime. Municipal guards sometimes complement the work of state police stationed locally, and *guarda* officers respond for 13% of all professionals.

## **Crime and the 2018 Presidential Election.**

Alongside Brazil's public security crisis in 2018, the presidential election was contested under unique circumstances: it took place two years after the impeachment of President Dilma Rousseff, which ended a 14-year presidential run for the Workers' Party (PT). Also, for the first time since reelection was allowed, the incumbent did not run (former Vice-President Michel Temer). Finally, former President Lula (2003-2010) was PT's preferred candidate and the leader in the polls until the courts made him ineligible less than two months before the Oct 7 election date.

The specific context opened way for the emergence of new political forces. For the first time since 1989, the presidential election was not a race between PT and the Center-Right PSDB (Party of the Brazilian Social Democracy). The first-round result was polarized between right-wing candidate Jair Bolsonaro from PSL (Social Liberal Party) with 46% of the vote, and PT's candidate Fernando Haddad with 29%.

Bolsonaro was also the first competitive candidate from the extreme Right since Brazil's democratization. Before his presidential bid, he had spent decades in Congress representing various small parties, exerting little influence over policy or legislation. A retired Army captain, he was better known for his open support of the defunct military regime and for his extreme positions on torture, minorities, human rights, and police violence—stances that were exceptionally rare among Brazilian politicians at the time and that he made central to his campaign. For instance, he was the only candidate to advocate lowering the age of criminal responsibility from 18 to 16, and to propose legal changes that would limit the investigation and prosecution of police officers involved in killings. He reiterated these views both in formal campaign materials and in interviews, such as on the country's most-watched news program (*Jornal Nacional*, Globo TV), where he stated that police officers who kill with "10 or 30 shots" should be celebrated

rather than prosecuted ([Globo, 2018](#)). In earlier statements, he also argued that one can only “fight violence with violence” ([Carta Capital, 2019](#)) and declared that, as an Army captain, his “specialty is to kill” ([Folha de São Paulo, 2017](#)).

Additionally, Bolsonaro’s campaign closely followed the strongman playbook on crime. Candidates in Brazil must publish campaign proposals, which are scrutinized by the press, and reflect the positions that they later discuss in debates and interviews.<sup>6</sup> Bolsonaro’s document over-emphasized the problem of violence with statements such as “Brazil is at war” (pg. 28). It directly linked the crisis to his broader campaign motif that he would protect the country against the threat of communism posed by PT and the left (pgs. 25-27). It also constantly emphasized the need for order and stability (e.g. pgs 5,10), something that was lost after the “Left corrupted Democracy” (pg. 10).<sup>7</sup> In short, his program treated crime as a symptom of a failing state, a byproduct of a Left-wing administration that focused more on human rights for criminals than on supporting police forces (pg. 32).

Interestingly, and crucial to test our argument, Bolsonaro was not the only candidate trying to own the issue of public security. One week before the election, vote intention polls still showed four candidates with two-digit percentages ([Datafolha, 2018](#)). Besides Bolsonaro and Haddad, the other competitive candidates were center-left *Ciro Gomes* (PDT, Democratic Labor Party), a former governor with high name recognition due to moderate success in past presidential candidacies (e.g. 12% of votes in 2002); and Center-Right *Geraldo Alckmin* (PSDB), a four-term governor of Brazil’s largest state, São Paulo.

Alckmin was the runner-up in the 2006 presidential race with 42% of the national vote, and his 2018 candidacy enjoyed heavy support from traditional political elites and the massive center-right party bloc. As a result, he was the candidate with the largest share of the mandatory campaign time on TV and radio (5min 32s inserts, compared to 2m32s inserts for Haddad and only 8s for Bolsonaro). He also actively tried to position himself as the tough-on-crime candidate. Similar to what Bolsonaro had done, he had an Army General coordinating his public security plan ([Folha de São Paulo, 2018a](#)). He also used more of his TV time to discuss public security ([Folha de São Paulo, 2018b](#)) than Bolsonaro did. He often

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<sup>6</sup>Candidate proposals are available at <https://divulgacandcontas.tse.jus.br/>. An extensive discussion by the press on the proposals for public security can also be found in [Veja \(2018\)](#).

<sup>7</sup>To support this argument, Bolsonaro constantly associated PT with other Left-wing non-democratic regimes in the region such as Cuba and Venezuela.

focused on his track record in the state of São Paulo, which experienced a sharp decline in homicide trends during his time as governor, in stark contrast to the upward trend in national numbers (see Figure A.2 in the Appendix). Alckmin was also the only other competitive candidate—in addition to Bolsonaro—to support more lax gun ownership laws.

However, in one crucial dimension, Bolsonaro was very different from Alckmin and the other contenders. Most candidates from both the Left and the Right offered extensive (and similar) policy proposals for public security.<sup>8</sup> Alckmin, for instance, was a quintessential establishment candidate, with a track record of respect for the institutional order. Bolsonaro, instead, framed the public security crisis as a fundamental failure of the state, at the national level, which could only be addressed with the extreme measures that would reestablish order.

The difference between Bolsonaro's and the other campaign approaches becomes more evident when analyzing individual voters' stated preferences. We do so using the LAPOP survey collected early in 2019—the very beginning of Bolsonaro's presidential term (LAPOP, 2016-2019). Figure 1 shows the responses to security-related questions, based on the respondent's candidate choice in the October 2018 election.<sup>9</sup>

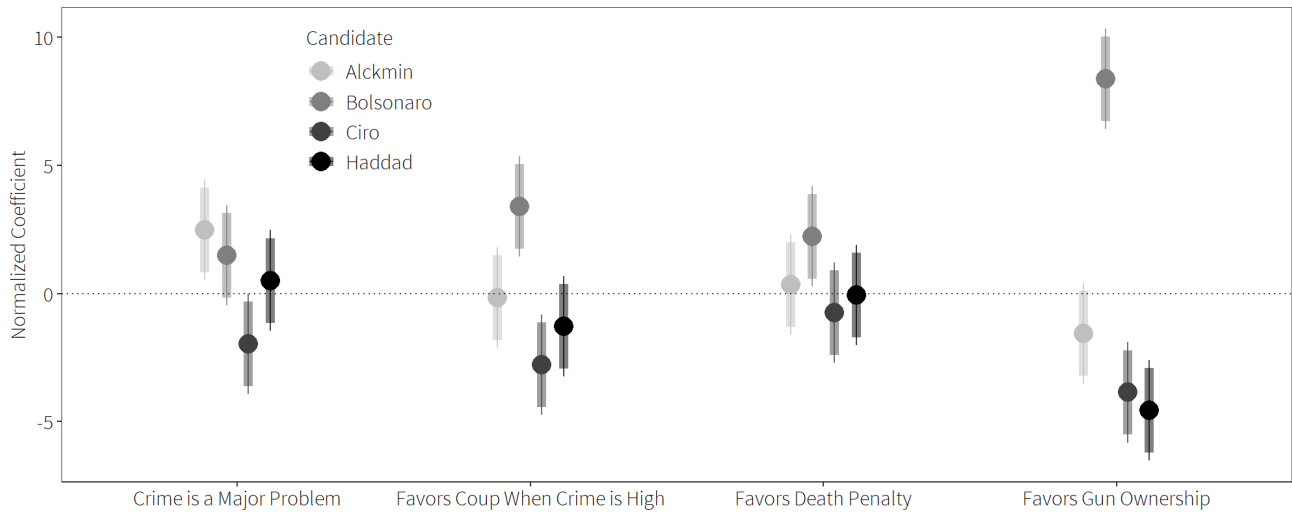
The survey suggests that Bolsonaro clearly captured the support of those favoring a tougher stance on crime. While both Bolsonaro and Alckmin voters expressed heightened concern with public security, Bolsonaro's supporters stood out for their willingness to sacrifice constitutional order during periods of elevated crime. They also placed greater value on the death penalty and showed overwhelming support for gun ownership.

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<sup>8</sup>For example, common proposals included increased funding to policing; better integration between different police forces; and expanded role of the federal government in areas such as in prison infrastructure, intelligence, technology, and in fighting organized crime.

<sup>9</sup>See also Rennó (2020) for a detailed analysis of Bolsonaro's electorate.

Figure 1: Vote choice and support for tough-on-crime measures



The x-axis describes the relevant survey question. Answers are binary (YES or NO). As is usual, coefficients are normalized by their standard deviation.

Not only did Bolsonaro win the election, but his party, until then minuscule, managed to elect fifty-two federal deputies. The coattail effect was not a coincidence: many tried to emulate Bolsonaro’s authoritarian platform during the campaign (Costa, 2018). We expect that PSL candidates also benefit from heists, and we present tests for this hypothesis in the empirical section.

## DATA: BANK ATTACKS

Crime statistics reporting in Brazil is decentralized. Data are collected and maintained by state governments, and, except for the state of Minas Gerais, they are not readily available online. We contacted the public security cabinets in all 26 Brazilian states, asking for access to municipal-level data on bank robberies from 2016 to 2020 through the Freedom of Information Law (*Lei de Acesso à Informação*, in Portuguese).

Sixteen out of twenty-six states provided bank heist information with sufficient level of detail. Most of the missing data is from states in the legal Amazon region that are remote and less populated: Amazonas, Acre, and Tocantins did not send any data. Although Roraima, Rondônia, Amapá, Mato Grosso, and

Mato Grosso do Sul sent a response, the data only contained very few events, not enough to provide any within-state variation across treatment and control groups near the election date.<sup>10</sup> Paraná was the only large state that did not provide any crime information at the municipal level, or before 2019. Whenever available, we also used data on robberies compiled by the unions of bank workers in each state. This data was available online for Ceará, Bahia, Paraíba, and Maranhão, and it was used to complement the government data (for Maranhão, it was the basis for the entire series, given that the state also did not respond to our request). With this, we increased our coverage to 17 states.

The data we use holds the precise date of the event and the municipality of occurrence. However, we must also identify bank attacks with the characteristic modus operandi of *Novo Cangaço*: events of such overwhelming violence and visibility. To do so, we classified as relevant events all heists that included either the use of explosives or heavily armed gangs.<sup>11</sup> Our data collection and processing could lead to measurement errors if we fail to correctly observe or classify an event. However, potential errors are unlikely to be associated with the timing of the presidential elections, and are likely common to both treatment and control observations within a state, which is our data collection unit (see more details on our empirical strategy in the next section).

We restrict our analysis to medium- and small-sized municipalities. There are two reasons for this decision. First, bank robberies in state capitals such as São Paulo or Rio de Janeiro, although extremely common, do not fit the profile of a shock to the sense of security of these cities. Attacks in these locations are only relevant and salient to a small percentage of local voters; and typically perpetrated by local (not roving) gangs that could seldom control the surrounding environment as they did in smaller towns, given the greater police presence. Second, most large cities suffered attacks both before and after that date, and as we explain in the empirical section, they would be dropped as they would be in the treatment and control sets. In practice, we restrict our data to municipalities with up to 50,000 voters in 2018, which includes 92% of all Brazilian municipalities.<sup>12</sup>

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<sup>10</sup>In Mato Grosso do Sul, for example, there was one robbery recorded in 2018 and none in 2019.

<sup>11</sup>In many states, either the government (e.g. Minas Gerais) or the labor union (e.g. Ceará) had extensive details on the events, including this information. For others, we used internet searches to confirm them. We did so for Piauí, Rio Grande do Sul, Rio de Janeiro, Espírito Santo, and Goiás.

<sup>12</sup>To show that our results are not dependent on the choice of cutoff, and that our intuition about city size is consistent with the data, we later show that the estimated effects become both higher in magnitude and more significant as we further

Table 1 summarizes our heist data. Overall, we processed a total of 1,649 events across 1,211 unique municipalities in 17 states between 2016 and 2020. The Table also shows a gradual decline in this type of crime in the period. While the heydays of *Novo Cangaço* occurred in the mid-2010s, state police forces, with the help of the federal police, eventually were able to catch up with the gangs through a series of successful intelligence operations, and to severely curb the practice by the end of 2020.<sup>13</sup>

Table 1: Data Summary

Year	Number of Attacks	Percentage with Explosives	Unique Municipalities	Percentage of the Country
2016	575	68.35	512	12.09
2017	501	72.26	452	10.67
2018	370	74.32	350	8.26
2019	141	56.03	135	3.19
2020	62	43.55	62	1.46

It only includes relevant events for 17 states, as described in the text, in municipalities with less than 50,000 voters (i.e. the denominator in column 4 also excludes the group of large cities).

Although the majority of relevant events include the use of explosives, there is anecdotal evidence that heists without them could have been even more terrifying. For example, in the state of Rio Grande do Sul, it was common for gangs to line up civilian hostages out in the street, often undressed, so the population and law enforcement would not approach the branch (Zero Hora, 2018). Figure A.3 (Appendix) has a few press photos showcasing typical *Novo Cangaço* heists.

The remaining data used in this article includes election results and candidate characteristics obtained from the Supreme Electoral Court (TSE), and demographic information for municipalities obtained from the Brazilian Institute of Geography and Statistics (IBGE).

reduce this population cutoff.

<sup>13</sup>A notorious example of the downfall of *Novo Cangaço* happened in the small city of Guararema, in the state of São Paulo (Globo, 2019). Armed with assault rifles, explosives, bulletproof vests, and vehicles, twenty-five criminals planned to rob two bank branches simultaneously in 2019, one of which was next to the police station. The attack, however, was foiled by the police, who had prior intelligence of the operation. In the ensuing confrontation, eleven suspects were killed.

## EMPIRICAL STRATEGY

This article aims to identify the electoral effects of bank attacks across Brazilian municipalities. Comparing locations that have been targeted or not would generate biased estimates since the same factors that increase the probability of a municipality being targeted, such as local wealth or security capabilities, might be correlated with electoral outcomes. One example in our context is that wealthier regions, which are also right-leaning, have been targeted more by *Novo Cangaço*.

Our empirical strategy addresses this source of bias by making comparisons only within the group of municipalities that were affected by bank raids around the election date, within the same state.<sup>14</sup> The identification assumption is relatively benign: it only requires that small differences in the timing of attacks around this date were not driven by factors that also influence the election outcomes. Thus, our treatment group is composed of cities that were raided before the election (October 7th, 2018), and the control group includes the locations that were targeted after this date. Accordingly, we estimate the effect of bank raids with the following equation.

$$y_{is} = \alpha + \beta T_{is} + \lambda_s + \theta_{is} + \epsilon_{is} \quad (1)$$

where the treatment  $T_{is}$  assumes value 1 when the attack in municipality  $i$  in state  $s$  happened before the election, and 0 otherwise. The treatment effect is given by  $\beta$ . We also include a vector of pre-determined covariates ( $\theta_{is}$ ) listed in the appendix (Tables A.1), and state fixed-effects ( $\lambda_s$ ), the latter to ensure that the variation used in the estimates is within states.

Our main specification only includes locations that were attacked six months before or after the election. There are a few reasons for this choice. First, our identification assumption is more likely to hold if treatment and control cities were raided within a relatively narrow window of time.<sup>15</sup> Accordingly, in the Appendix (Tables A.1), we show that treatment and control municipalities are balanced in several

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<sup>14</sup>We use within-state comparisons to ensure that both treatment and control municipalities (i) are under the responsibility of the same police forces; (ii) face the same pool of candidates and the same TV and Radio campaigns in gubernatorial and legislative elections; and (iii) are subject to the same potential measurement error in the data collection process.

<sup>15</sup>Treatment and control groups have municipalities that were attacked, on average, 87 and 84 days from the election, respectively. These averages are not statistically indistinguishable.

pre-treatment characteristics such as population size, past electoral choices, and economic development, among others.

Second, it is also more likely that raids removed from the electoral period are less salient to voters. Accordingly, we weight observations by their proximity to the election date using a linear kernel.<sup>16</sup> Again, in the Appendix, we show that the results remain robust to alternative specifications. Figure A.1 shows the results for different windows of time around the election date, ranging from 8 to 40 weeks (Figure A.1), and the panel C of Table A.6 shows the effects when observations have equal weights.

Finally, we also restrict the sample to municipalities that did not record any attack at least 2 years preceding the start of the sample period, to ensure that both treatment and control observations have a similar treatment history. Estimations for a sample without this restriction (Table A.7), and the estimation without covariates (Table A.5, panel B) are also in the Appendix.

## RESULTS AND DISCUSSION

Table 2 shows the effect of bank attacks on the first round of the 2018 presidential election. The first two columns show the percentage change in votes for the three relevant candidates in the race: Bolsonaro (PSL), Haddad (PT), and Alckmin (PSDB). The remaining two show the aggregate change for all the other candidates from Right and Left-wing parties, respectively.

In municipalities that suffered an attack before the election day, Bolsonaro had 3.4 percentage points (pp) more votes than in locations that suffered an attack after the election, a 10% effect. The coefficients for the other candidates are negative or near zero, and less precisely estimated. This includes Alckmin, the other candidate on the right, but with a non-authoritarian profile. This suggests that voters in distress picked Bolsonaro -the only clearly authoritarian candidate- over all other candidates in the entire ideological spectrum.

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<sup>16</sup>For example, attacks one day from the election are weighted one-hundred times more than attacks that happened one hundred days from the election.

Table 2: Effects on the Presidential Election of 2018

Vote % of...	Bolsonaro	Haddad	Alckmin	Other Right	Other Left
Effect	3.408*	-1.806+	-0.492	0.066	-1.176+
S.E.	(0.959)	(0.998)	(0.493)	(0.215)	(0.658)
Control Average	34.587	44.336	5.302	4.202	11.598

+p<0.1, \*p<0.05.

Total observations: 220. Standard errors (parenthesis) are heteroskedasticity robust. The coefficients for all covariates can be found in Table A.2 (appendix).

Other Right (in decreasing order of votes): NOVO, PATRIOTA, MDB, PODEMOS, DC, and PPL.

Other Left (in decreasing order of votes): PDT, REDE, PSOL, and PSTU.

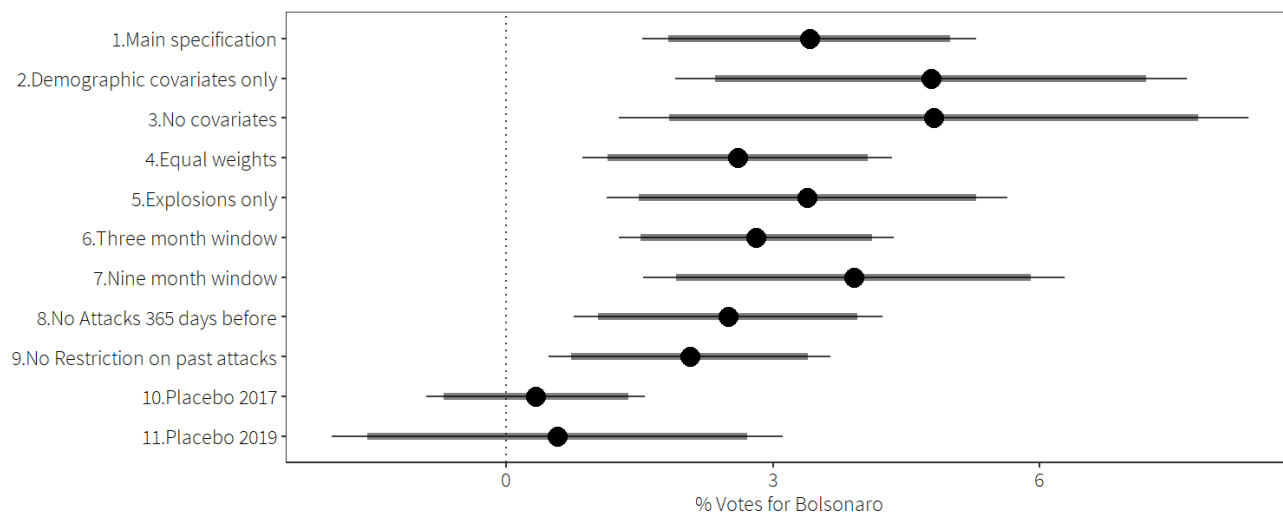
Figure 2 shows that these results are robust to several alternative specifications.<sup>17</sup> The estimates remain strong and significant after the exclusion of political covariates (estimate 2), or all covariates (estimate 3).<sup>18</sup> They are also similar under the alternative weighting procedure explained in page 19 (estimate 4), and for a sample that includes only events where explosives were used (estimate 5). Interestingly, when we consider only events that happened within a narrower 3-month time window around the election, the coefficient is slightly higher (estimates 6), becoming slightly lower as we expand that window of time to 9 months (estimates 7)—our main specification uses a 6-month time window. This is consistent with our previous argument that the salience of such events to voters' choices might decay over time.<sup>19</sup>

<sup>17</sup>The full estimates can be found in the appendix, Table A.5, Table A.7, and Table A.8.

<sup>18</sup>All municipal covariates used in the estimation were measured pre-treatment. They are fully described in Table A.1, which also shows that they are balanced across treatment and control groups. Demographic covariates: size of the local electorate; homicide rate; per capita GDP; share of urban population; indicator for metropolitan area; human development index; and municipal area. Political covariates: PT's presidential vote share in 2014; indicators for mayor's party being part of the state government coalition, being Left-wing, or for mayor being a female.

<sup>19</sup>Figure A.1 shows a sensitivity of the results to several other time windows around the election.

Figure 2: Effect on Bolsonaro's Votes for Alternative Statistical Specifications



The coefficients show the effect on the vote percentage for Bolsonaro. The full results for each regression can be found in the appendix, from Table A.5 to Table A.8. Thick (thin) bars show the 95% (90%) confidence intervals.

The y-axis describes how each specification deviates from the main. The main specification includes all municipalities attacked within a time window of 6 months around the election date, excluding the ones that have had another attack in the previous 2 years, for a total of 220 cities. Regressions include all covariates and weight observations by their distance, in time, to the election date, unless otherwise stated.

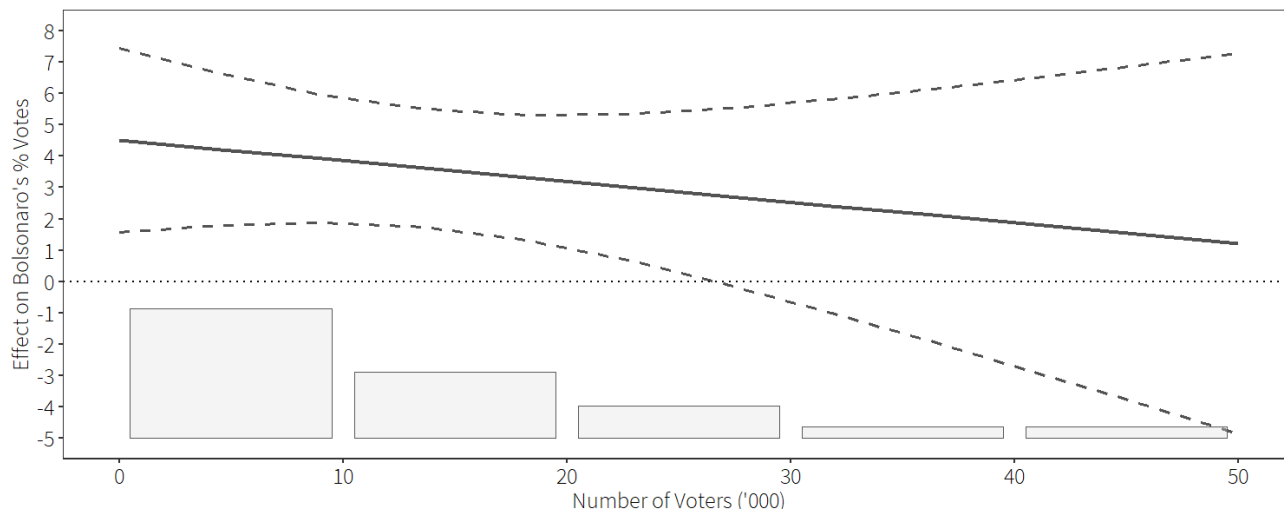
Estimates 8 and 9 further suggest that the salience of the events mediates the effect on voter behavior. Our main specification only includes municipalities that did not experience a bank attack two years before the start of the sample period (e.g. 6 months before the election). We do this to make sure that the trauma of an attack, which is the intended treatment intervention, is a novel occurrence to these communities. In line with these argument, if we relax this restriction and include locations that had previous *Novo Cangaço* attacks, either between one and two years before (estimate 8) or within the past year (estimate 9), the magnitude of the estimated effects is reduced but remains statistically significant.

Finally, we find no effects for placebo tests of the empirical strategy where we regress the election results from 2018 on bank raids from 2017 and 2019 (estimates 10 and 11).

The effect on Bolsonaro's votes is also slightly higher the smaller the municipality. This is shown by the plot in Figure 3 below. For municipalities between 0 and 10 thousand voters, which are the majority in our sample, the average effect is between 4 and 5pp. On the other hand, we observe a lower effect closer to 2pp for larger cities with 40 to 50 thousand voters. This is consistent with our intuition that in smaller

communities these attacks are more salient and costly to a larger share of the population.

Figure 3: Marginal Effect on Bolsonaro’s Votes, by Municipality Size



The line shows the marginal effect of bank attacks, by changes in the municipality’s number of voters. The confidence level is 95%. The bars show the distribution of interacting variable (number of voters). The full regression is shown in Table A.9 (appendix).

**Legislative Elections.** In Table 2, we further examine federal legislative elections. Parties in Brazil generally run non-programmatic campaigns for Congress, but there is reason to suspect a positive coattail effect on the performance of candidates allied to Bolsonaro, given that many of these candidates exhibited a clear affinity with Bolsonaro’s security agenda during the campaign (Costa, 2018). Despite weak institutional ties between Bolsonaro and his party in 2018, PSL, candidates for the national chamber attracted more voters in targeted municipalities, with 1.3pp more votes (a whopping 28% effect).<sup>20</sup>

We also examine the performance of “law-and-order” congressional candidates from any party, defined as the ones that use titles that refer to a background in security forces or the military, such as captain or colonel, in their ballot names (in line with Novaes (2024)). In Brazil, candidates adopt a “ballot name” to be used in both the campaign materials and in the voting machines. This name can be different from their legal one, so candidates often use it to signal expertise, background, ideology, religion, or compe-

<sup>20</sup>We do not observe a strong pattern for Senate candidates, although the coefficient is still positive in magnitude and the sample is fairly reduced. PSL was then a small party, and it did not present Senate candidates in many states. We emphasize that while the chamber elections are state-wide races with an open list and multiple candidates per party, there were only two positions available per state in the 2018 election for the Senate.

tencies to voters (Boas and Smith, 2019; Desai, Frey, and Tyson, 2025). Although we find positive effects for both the Senate and the House, they are only statistically significant for the Senate.

Overall, the evidence in Table 3 suggests congressional candidates that can credibly claim a background in public security might benefit from a higher salience of this policy area, but not as much as the ones that can associate themselves with the candidacy of Bolsonaro. This is consistent with the argument that under a severe public security crisis, voters demand action primarily from the highest political office in the country.

Table 3: Effects on the Congressional Election of 2018

Vote % of...	PSL CANDIDATES		LAW-AND-ORDER CANDIDATES	
	Senate	Chamber	Senate	Chamber
Effect	0.557	1.352*	0.858*	0.290
S.E.	(0.416)	(0.449)	(0.352)	(0.354)
Control Average	8.412	4.871	6.214	3.749
Observations	113	220	102	220

+p<0.1, \*p<0.05.

Standard errors (parenthesis) are heteroskedasticity robust. The coefficients for all covariates can be found in Table A.3 (appendix).

**Gubernatorial elections.** Within the framework of electoral accountability in democratic elections, voters punish or reward politicians for what they perceive as bad or good performance. For politicians responsible for public security, major heists can signal failure, potentially undermining their re-election prospects as voters in affected municipalities assign blame. In Brazil, where governors hold primary responsibility for public security, such accountability should manifest in gubernatorial elections, which coincide with national contests.

Table 4 shows that this is not the case. The first two columns have the effect of bank attacks on the votes for the incumbent candidate (first) or party (second) in their reelection attempts.<sup>21</sup> For both, the

<sup>21</sup>Not every incumbent governor ran for reelection in 2018. Thus, the data included in the estimation of the second column is a subset of the data used for the first. In one state, the incumbent party did not put forward a candidate.

estimate for the effect on the votes of the incumbent candidates are positive, inconsistent with models of retrospective voting, albeit close to zero and not statistically significant at the usual levels.

Table 4: Effects on the Gubernatorial Elections of 2018

Vote % of the...	Incumbent Candidate	Incumbent Party	PT or Allied Party	All Incumbents (Past 20Y)
Effect	0.283	1.313	1.338	0.138
S.E.	(1.703)	(1.886)	(1.597)	(2.040)
Control Average	40.720	40.595	36.748	56.196
Observations	183	204	220	204

+p<0.1, \*p<0.05.

Standard errors (parentheses) are heteroskedasticity robust. The dependent variables are shown in the column headers and described in the text. The coefficients for all covariates can be found in Table A.4 (appendix).

In the third column, we examine whether there is an effect on the votes of the candidate directly supported by the PT party in these gubernatorial races.<sup>22</sup> Given that PT held the presidency from 2003 until Dilma Rousseff's impeachment in 2016, this column aims to test whether the voters shift the blame and punish the gubernatorial candidate associated with the recent, long-lasting Left-wing federal administration after these crimes. Again, the estimate is statistically insignificant and positive in magnitude.

In the fourth column, we expand the definition of the incumbent party. We show the effect on the votes of every party that elected a governor in that state in the previous 20 years (i.e., since the 1998 election). For example, although the incumbent in the state of Bahia was PT in 2018, both PT and Right-wing DEM elected governors in the previous 20 years, so we aggregate the 2018 votes for the candidates of both parties. Here are checking whether voters try to punish a long-term set of incumbents. Again, although the effect is negative, it is statistically insignificant and less than 0.5pp.

Taken together with the presidential results, these findings indicate that voters are highly responsive to large heists, but not in the way expected at the direct level of policy accountability. We find these results consistent with the argument that voters might see state police as insufficient to tackle the security

<sup>22</sup>PT ran their own candidates in all states in the sample but Maranhão, Paraíba, and Pernambuco, where it supported PCdoB, and PSB twice; respectively.

problem, seeking an overhaul that only the federal government can achieve.

## **Additional Survey Evidence Linking Bank Attacks to Views on the State**

The results above establish a causal link between violent crime and electoral support for an authoritarian politician. We interpret them within a theoretical framework where voters affected by this type of violence rank protection and public security above other public goods, including democracy. As citizens become more willing to trade off human rights and democratic norms for safety, they are more likely to support candidates who offer extralegal responses to the problem of crime.

In this section, we present supporting evidence for this argument. We use data from two waves of the Latin American Public Opinion Project ([LAPOP, 2016-2019](#)) collected in Brazil during the period coinciding with our sample: the first between April and May of 2017, the second between January and March of 2019.<sup>23</sup> This survey only has data from a few small municipalities in the country. Altogether, considering only municipalities with less than 150,000 voters, there were 9 municipalities in the survey that suffered a bank attack in the year before the data collection (our treatment group), and 31 that did not (control), for a total of 826 survey respondents.<sup>24</sup> We include questions that are related to our theoretical expectations and which appear on both survey waves we analyze.

We use this data to regress the treatment condition, i.e., bank attacks in the previous 12 months, on several survey responses in these locations.<sup>25</sup> We first focus on questions directly related to the arguments that violent crime leads voters to lose confidence in the state's ability to protect them, and to trade off democracy for safety. The results are shown in Table 5.

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<sup>23</sup>These data were supplied by LAPOP Project at Vanderbilt University, which takes no responsibility for any interpretation of the data.

<sup>24</sup>We raised our municipality size threshold for this exercise to 150,000 given that, under the lower threshold of 50,000, we would only have 6 attacked and 15 not attacked municipalities. We only considered municipalities in the 17 states for which we had bank robbery data.

<sup>25</sup>More precisely, we regress the response to each question on a binary variable that indicates whether or not the municipality was attacked. We cluster all errors at the municipality level to account for potential correlation between individual responses in the same location. As in our main empirical exercise, we also control for the pre-existing covariates described in Table A.1, and include state-survey wave fixed effects. Both the questions and the coding of the responses are described in detail in Appendix B.

Table 5: Bank Attacks and Views on Brazilian Political Institutions

Survey Question	Effect of a Bank Attack	Standard Error	Baseline (Control Avg.)	Valid Responses
Feel Safe	-0.224*	(0.082)	2.631	821
Trust the Police	-0.337*	(0.124)	4.412	823
Respect for Institutions	-0.320*	(0.151)	4.234	817
System Protects Basic Rights	-0.374*	(0.102)	2.973	819
Proud of Institutions	-0.436*	(0.157)	2.947	822
Military Coup is Acceptable	0.091*	(0.043)	0.348	369
Military Coup (Corruption)	0.017	(0.066)	0.347	397
Crime is Number One Problem	-0.010	(0.022)	0.126	801

+p<0.1, \*p<0.05.

Includes responses from 9 municipalities that were attacked (Treatment group), and 31 municipalities that were not (Control group). Standard errors (parentheses) are clustered at the municipality level. Regressions include the covariates in Table A.13, and state-survey fixed effects. The dependent variables and their scaling are described in the text and Appendix B. Full regression results are shown in Table A.10 (appendix).

In affected locations, voters were more likely to report that they feel unsafe, and less likely to trust the police (first two rows). These results indicate that heists do indeed make residents feel unsafe and unprotected. Closer to our argument, survey respondents were also significantly less likely to declare respect for the country's political institutions, to think that the political system in Brazil protects the basic rights of its citizens, or to indicate that they feel proud of the country's political system. In sum, survey evidence confirms that violent heists lead to a generalized loss of trust in the state.

More importantly, results directly demonstrate that respondents are willing to trade off democratic values for safety. Respondents in affected municipalities are 26% more likely to agree that a military coup is acceptable when crime is extremely high. While 35% of voters in locations that were not attacked said that a coup was an acceptable response to high crime, the share reaches 44% in attacked cities. Interestingly, there is no effect on voters' willingness to accept a coup in cases of high corruption – which is consistent with our argument that the fundamental issue following these events is state protection.<sup>26</sup> Finally, the

<sup>26</sup>The number of individual responses to the coup questions is much smaller given that half of the survey respondents received the crime-related question, and the other half received the corruption-related question.

attacks did not have any effect on the voters’ perception that crime is the major problem in the country. All questions are described in detail in Appendix B.

We also run two placebo tests for this empirical exercise. Table 6 shows the lack of noticeable effect of bank attacks on questions that are unrelated to public security: treatment did not change voters’ trust in the electoral process, the media, or their opinions on issues that are often related to Right-wing conservative platforms such as abortion, climate change, and the economy.

Table 6: Bank Attacks and Views on Other Topics

Survey Question	Effect of a Bank Attack	Standard Error	Baseline (Control Avg.)	Valid Responses
Trust Elections	-0.141	(0.182)	3.298	818
Trust Media	-0.142	(0.203)	4.513	821
Supports Abortion	0.019	(0.059)	0.647	810
State of the Economy	0.073	(0.060)	1.651	816
Climate Change is a Problem	0.037	(0.062)	3.653	818

+p<0.1, \*p<0.05.

Includes responses from 9 municipalities that were attacked (Treatment group), and 31 municipalities that were not (Control group). Standard errors (parentheses) are clustered at the municipality level. Regressions include the covariates in Table A.13, and state-survey fixed effects. The dependent variables and their scaling are described in the text and Appendix B. Full regression results are shown in Table A.11 (appendix).

In the Appendix (Table A.12) we also run an alternative specification where, for the same sample, we regress the responses in Table 5 on a binary variable that indicates whether or not the individual reported being a victim of crime before the survey (i.e. the treatment becomes individual victimization instead of the collective crime generated by the bank attack). As expected, the results show that victimized individuals are more likely to feel unsafe, less likely to trust the police, and less likely to say that Brazil protects the basic rights of its citizens. More importantly, we neither see a generalized loss in respect and pride for the state nor an increased willingness to accept a coup as a response to high crime.

Finally, we acknowledge that this empirical exercise is less reliable than the research design used in the estimation of the electoral effects of bank attacks. In addition to the reduced sample of municipalities in the survey, here non-attacked locations act a counterfactual for treated municipalities. Nevertheless,

we ease the concerns with potential unobservable variable bias on this estimation by showing, in the Appendix, Table [A.13](#), that the 9 treated municipalities and the 31 control locations are balanced for all municipal covariates measured in the survey.

## CONCLUSION

We show that in Brazil, exposure to organized violence, which in our case is literally brought by roving bandits, caused voters to reward Bolsonaro, even over other candidates that intensively campaigned with a focus on public security. By showing how crime can lead to a heightened perception of institutional crisis, these findings help explain the appeal of authoritarian candidates within democratic regimes. Consequently, they also justify the electoral strategy often used by these candidates to amplify economic, criminal, and security threats, and to prey on voters' fears.

By contrast, we find no effects of crime at the subnational level, where incumbents actually hold levers to influence policing. Ultimately, these results suggest that the canonical models of electoral accountability—which assume retrospective voting or that voters respond to traditional policy platforms—are insufficient for analyzing political outcomes when the presumption of the state's monopoly of violence is under duress. When citizens perceive the state as failing in its most basic function of providing security, they will be willing to tradeoff their constitutional rights for security by supporting authoritarian candidates.

Finally, a direct implication of these results is that security threats may lead voters to disregard other qualities of the candidates they choose, electing leaders who lack competence in other policy areas. This implies that the electoral dynamic uncovered by this article might have significant implications to the quality of democracy in areas that go way beyond the issue of public security. For example, improved coordination between state police and federal forces brought the Novo Cangaço phenomenon to a halt in Brazil. In 2021, when Bolsonaro was already President, the Federal Highway Police (PRF) tortured and killed an individual for information about a gang hideout. When storming a ranch, the PRF further killed 25 suspects and the caretaker of a neighboring property ([Abreu, 2024](#)). The Bolsonaro clan immediately stepped in to praise the operation, as Senator Flávio Bolsonaro, the President's son, posted

on social media: “No police officers dead. Congratulations [PRF] and [Military Police of Minas Gerais]. Don’t worry – only crooks will complain. #GreatDay”. Curiously, the same PRF was involved in an electoral manipulation scandal in the 2022 presidential race, as it prevented buses of Lula’s likely supporters from reaching polling stations ([CNN Brasil, 2024](#)).

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The Price of Order:  
How Violent Crime Weakens Democracy

Appendix for Online Publication

# TABLE OF CONTENTS

<b>A</b>	<b>Other Tables and Figures</b>	<b>1</b>
<b>B</b>	<b>Questions from the LAPOP Survey</b>	<b>20</b>

## LIST OF TABLES

A.1	Balance of Pre-Treatment Covariates: Bank Raids around the 2018 Election . . . . .	1
A.2	Main Results with All Coefficients . . . . .	2
A.3	Legislative Results with All Coefficients . . . . .	3
A.4	Gubernatorial Results with All Coefficients . . . . .	4
A.5	ROBUSTNESS to COVARIATES: Effects on the Presidential Elections of 2018 . . . . .	5
A.6	EQUAL WEIGHTS: Effects on the Presidential Elections of 2018 . . . . .	6
A.7	OTHER ROBUSTNESS: Effects on the Presidential Elections of 2018 . . . . .	7
A.8	Placebo Effects on the Presidential Elections of 2018 . . . . .	8
A.8	Placebo Effects on the Presidential Elections of 2018 (continued) . . . . .	9
A.9	Marginal Effect of Bank Attacks, by population . . . . .	10
A.10	Voter's Institutional Perceptions with All Coefficients . . . . .	11
A.10	Voter's Institutional Perceptions with All Coefficients (continued) . . . . .	12
A.11	Voter's Perceptions on Unrelated Issues with All Coefficients . . . . .	13
A.12	Voter's Institutional Perceptions after Individual Victimization . . . . .	14
A.12	Voter's Institutional Perceptions after Individual Victimization . . . . .	15
A.13	Balance of Municipal Characteristics for the Survey Exercise . . . . .	16
A.14	Questions Directly Related to Institutional Trust and Security . . . . .	20
A.15	Questions Unrelated to Institutional Trust and Security . . . . .	21

## LIST OF FIGURES

A.1	Effect of Bank Raids on the Presidential Election, by Time Window . . . . .	17
A.2	Rate of Homicides by Firearms (Brazil vs. São Paulo) . . . . .	18
A.3	Pictures . . . . .	19

## A OTHER TABLES AND FIGURES

Table A.1: Balance of Pre-Treatment Covariates: Bank Raids around the 2018 Election

	Coefficient	S.E.	Control Average	Observations
Electorate	-0.179	(0.113)	10.891	220
Homicide Rate	1.040	(2.466)	19.544	220
GDP	0.079	(0.076)	75.357	220
Urbanization	-0.018	(0.028)	0.625	220
HDI	-0.001	(0.007)	0.659	220
Area	0.082	(0.140)	0.522	220
2014 Presidential Vote	-0.025	(0.019)	0.596	220
Mayor   State Coalition	0.056	(0.057)	0.126	220
Mayor   Left	-0.079	(0.064)	0.243	220
Mayor   Female	0.019	(0.041)	0.069	220

+p<0.1, \*p<0.05.

Standard errors (parenthesis) are heteroskedasticity robust. All regressions include state fixed-effects.

**Electorate:** in thousand voters, by TSE for the 2018 election. Coefficient estimated in log(variable).

**Homicide Rate:** per thousand inhabitants, by IBGE, average of 2013-2017.

**Per Capita GDP:** in thousand R\$ per person, average of 2016 and 2017, by IBGE. Coefficient estimated in log(variable).

**Urbanization:** Share of the urban population, calculated by IBGE based on the 2010 Census.

**Human Development (HDI):** Index between 0 and 1, calculated by IBGE based on the 2010 Census.

**Area:** Municipal area in km<sup>2</sup>. Coefficient estimated in log(variable).

**PT Votes 2014:** Share between 0 and 1, calculated by based on the second round of the 2014 presidential election, a race between PT and PSDB parties, by TSE.

**Mayor, State Coalition:** binary variable that assumes value 1 if the mayor elected in 2016 belongs to the same party as the governor elected in 2014, by TSE.

**Mayor, Left:** binary variable that assumes value 1 if the mayor elected in 2016 belongs to a one of the left-wing parties in Brazil (PT, PCdoB, PSB, PDT, PSOL, PMN, or PPS), by TSE.

**Mayor, Female:** binary variable that assumes value 1 if the mayor elected in 2016 was female, by TSE.

Table A.2: Main Results with All Coefficients

	Bolsonaro	Haddad	Alckmin	Other Right	Other Left
Effect	3.408*	-1.806+	-0.492	0.066	-1.176+
	(0.959)	(0.998)	(0.493)	(0.215)	(0.658)
Electorate	0.840	-0.488	-0.410	0.005	0.045
	(0.846)	(0.826)	(0.443)	(0.156)	(0.443)
Homicide Rate	0.015	-0.008	0.007	0.008	-0.021
	(0.029)	(0.028)	(0.012)	(0.006)	(0.014)
GDP	2.355	-0.983	-0.983	0.834*	-1.211*
	(1.742)	(1.238)	(0.877)	(0.247)	(0.532)
Urbanization	0.639	-3.052	0.315	0.927	1.204
	(3.203)	(3.406)	(1.402)	(0.603)	(1.959)
HDI	53.861*	-70.462*	-8.310	6.396+	18.405+
	(16.609)	(17.412)	(7.515)	(3.591)	(10.287)
Area	0.203	-0.238	0.378	-0.119	-0.219
	(0.628)	(0.609)	(0.285)	(0.121)	(0.399)
2014 Presidential Vote	-65.045*	79.450*	-9.786*	-4.324*	-0.302
	(4.412)	(4.804)	(2.460)	(1.010)	(3.280)
Mayor   State Coalition	-2.116+	1.492	1.246+	0.401	-1.011+
	(1.076)	(1.144)	(0.640)	(0.346)	(0.611)
Mayor   Left	0.811	1.687	-1.527*	-0.369	-0.600
	(1.087)	(1.374)	(0.613)	(0.247)	(1.010)
Mayor   Female	-0.599	0.282	0.654	-0.340	-0.003
	(1.784)	(1.421)	(1.053)	(0.329)	(0.919)
Control Average	34.587	44.336	5.302	4.202	11.598
Observations	220	220	220	220	220

+p<0.1, \*p<0.05. Standard errors (parenthesis) are heteroskedasticity robust.

The estimation corresponds to equation 1. The main specification has the following characteristics: observations are weighted by their proximity to the election date, with a triangular kernel (bank raids closer to the election have higher weights); it uses all events 6 months before and after the October election date; and excludes municipalities that had a bank raid in the previous 2 years before the start date for the sample.

Table A.3: Legislative Results with All Coefficients

Vote % of...	PSL CANDIDATES		LAW-AND-ORDER CANDIDATES	
	Senate	Chamber	Senate	Chamber
Effect	0.557 (0.416)	1.352* (0.449)	0.858* (0.352)	0.290 (0.354)
Electorate	0.437 (0.421)	2.432* (0.407)	0.430 (0.414)	1.425* (0.273)
Homicide Rate	0.007 (0.016)	-0.015 (0.011)	0.030 (0.019)	-0.019* (0.010)
GDP	0.051 (0.616)	1.835* (0.472)	0.349 (0.414)	0.825+ (0.433)
Urbanization	1.476 (1.625)	1.353 (1.177)	-0.096 (1.238)	-0.565 (1.311)
HDI	-5.690 (7.712)	-17.303* (5.877)	-0.356 (6.642)	3.143 (6.294)
Area	0.125 (0.243)	-0.507* (0.217)	0.160 (0.229)	-0.685* (0.219)
2014 Presidential Vote	-9.292* (2.853)	-12.621* (2.262)	-2.578 (2.388)	-8.663* (2.281)
Mayor   State Coalition	-0.590 (0.721)	-1.002* (0.429)	-1.173* (0.547)	-0.138 (0.482)
Mayor   Left	0.156 (0.690)	0.303 (0.411)	0.699 (0.456)	0.223 (0.421)
Mayor   Female	-0.917 (0.746)	-0.341 (0.592)	-2.193* (0.875)	0.370 (0.643)

+p<0.1, \*p<0.05. Standard errors (parenthesis) are heteroskedasticity robust.

The estimation corresponds to equation 1. The main specification has the following characteristics: observations are weighted by their proximity to the election date, with a triangular kernel (bank raids closer to the election have higher weights); it uses all events 6 months before and after the October election date; and excludes municipalities that had a bank raid in the previous 2 years before the start date for the sample.

Table A.4: Gubernatorial Results with All Coefficients

	Bolsonaro	Haddad	Alckmin	Other Right	Other Left
Effect	0.283 ( 1.703)	1.313 ( 1.886)	1.338 ( 1.597)	0.138 ( 2.040)	0.291 (0.212)
Electorate	0.687 ( 1.503)	0.690 ( 1.535)	0.191 ( 1.268)	-0.230 ( 1.363)	0.267 (0.201)
Homicide Rate	-0.083+ ( 0.049)	-0.075 ( 0.052)	0.062 ( 0.042)	-0.053 ( 0.053)	-0.014* (0.006)
GDP	-4.725* ( 2.129)	-4.313* ( 2.183)	-2.651 ( 1.763)	-1.297 ( 2.169)	0.150 (0.309)
Urbanization	-9.986 ( 6.241)	-10.843 ( 6.944)	-2.409 ( 5.928)	-9.855 ( 6.190)	1.306 (0.918)
HDI	3.197 (27.971)	8.833 (31.897)	-3.822 (28.589)	-29.037 (29.592)	-5.194 (4.921)
Area	-1.511 ( 1.028)	-1.573 ( 1.088)	-0.696 ( 0.882)	-2.888* ( 1.120)	-0.065 (0.140)
2014 Presidential Vote	12.642 (10.152)	14.819 (10.246)	57.935* ( 6.915)	13.272 ( 8.453)	-0.492 (0.675)
Mayor   State Coalition	3.974+ ( 2.271)	4.241+ ( 2.292)	-1.499 ( 1.905)	5.636* ( 1.902)	0.259 (0.323)
Mayor   Left	-0.272 ( 2.227)	0.520 ( 2.487)	3.477+ ( 1.981)	1.693 ( 2.352)	0.117 (0.154)
Mayor   Female	-1.580 ( 2.499)	-2.483 ( 2.622)	-3.810+ ( 2.164)	-4.908+ ( 2.861)	-0.113 (0.390)

+p<0.1, \*p<0.05. Standard errors (parenthesis) are heteroskedasticity robust.

The estimation corresponds to equation 1. The main specification has the following characteristics: observations are weighted by their proximity to the election date, with a triangular kernel (bank raids closer to the election have higher weights); it uses all events 6 months before and after the October election date; and excludes municipalities that had a bank raid in the previous 2 years before the start date for the sample.

Table A.5: ROBUSTNESS to COVARIATES: Effects on the Presidential Elections of 2018

	Bolsonaro	Haddad	Alckmin	Other Right	Other Left
<b>Panel A: DEMOGRAPHIC COVARIATES ONLY</b>					
Effect	4.774*	-3.780*	-0.028	0.224	-1.189+
	(1.468)	(1.664)	(0.534)	(0.226)	(0.622)
Electorate	0.250	0.057	-0.353	0.008	0.029
	(1.158)	(1.315)	(0.476)	(0.157)	(0.420)
Homicide Rate	0.058	-0.056	0.009	0.010	-0.021
	(0.036)	(0.045)	(0.010)	(0.006)	(0.015)
GDP	3.530	-2.510	-0.775	0.899*	-1.133*
	(2.201)	(2.424)	(0.963)	(0.294)	(0.507)
Urbanization	1.501	-4.356	0.646	1.070	1.173
	(5.382)	(5.766)	(1.464)	(0.683)	(1.911)
HDI	139.242*	-173.717*	2.845	11.219*	20.290+
	(27.881)	(28.588)	(6.897)	(3.573)	(11.203)
Area	-0.601	0.655	0.300	-0.174	-0.176
	(0.909)	(0.949)	(0.316)	(0.131)	(0.386)
<b>Panel B: NO COVARIATES</b>					
Effect	4.807*	-3.682+	-0.007	0.255	-1.369*
	(1.805)	(2.142)	(0.492)	(0.269)	(0.642)

+p<0.1, \*p<0.05. Standard errors (parenthesis) are heteroskedasticity robust.

All panels show the estimation of equation 1. The main specification has the following characteristics: observations are weighted by their proximity to the election date, with a triangular kernel (bank raids closer to the election have higher weights); it uses all events 6 months before and after the October election date; and excludes municipalities that had a bank raid in the previous 2 years before the start date for the sample. **Panel A** shows the results with the inclusion of demographic covariates only. **Panel B** shows the results without the inclusion of the pre-determined covariates.

Table A.6: EQUAL WEIGHTS: Effects on the Presidential Elections of 2018

	Bolsonaro	Haddad	Alckmin	Other Right	Other Left
Effect	2.600*	-1.251	-0.653	0.130	-0.827
	(0.887)	(0.920)	(0.428)	(0.189)	(0.600)
Electorate	1.165	-1.388*	-0.367	0.148	0.434
	(0.715)	(0.703)	(0.385)	(0.136)	(0.402)
Homicide Rate	0.030	0.002	0.006	0.001	-0.039*
	(0.025)	(0.026)	(0.011)	(0.006)	(0.016)
GDP	2.901+	-1.389	-0.823	0.806*	-1.480*
	(1.493)	(1.234)	(0.684)	(0.232)	(0.543)
Urbanization	-0.748	-1.016	-0.112	0.068	1.814
	(2.849)	(2.887)	(1.217)	(0.535)	(1.742)
HDI	49.517*	-67.230*	-8.378	9.661*	16.360+
	(14.630)	(15.752)	(6.182)	(3.089)	(9.047)
Area	-0.355	0.226	0.263	-0.226*	0.095
	(0.580)	(0.559)	(0.265)	(0.107)	(0.364)
2014 Presidential Vote	-64.175*	77.930*	-10.958*	-3.681*	0.878
	(3.856)	(4.174)	(2.219)	(0.900)	(2.623)
Mayor   State Coalition	-1.755+	0.846	1.210*	0.587*	-0.884
	(0.929)	(0.949)	(0.493)	(0.274)	(0.548)
Mayor   Left	0.397	0.620	-1.057*	-0.481*	0.524
	(0.992)	(1.114)	(0.508)	(0.211)	(0.835)
Mayor   Female	-0.329	0.869	0.461	-0.366	-0.639
	(1.347)	(1.153)	(0.798)	(0.311)	(1.048)

+p<0.1, \*p<0.05.

Standard errors (parenthesis) are heteroskedasticity robust.

All panels show the estimation of equation 1. The specification has the following characteristics: all observations are weighted equally, independent of the time between the event and the election; it uses all events 6 months before and after the October election date; and excludes municipalities that had a bank raid in the previous 2 years before the start date for the sample.

Table A.7: OTHER ROBUSTNESS: Effects on the Presidential Elections of 2018

Specification	A	B	C	D	E
Effect	3.385*	2.812*	3.908*	2.491*	2.062*
	(1.150)	(0.788)	(1.209)	(0.886)	(0.809)
Electorate	0.655	0.945	0.297	0.572	0.461
	(1.014)	(0.701)	(1.093)	(0.794)	(0.719)
Homicide Rate	0.068+	0.022	-0.008	0.025	0.025
	(0.036)	(0.023)	(0.038)	(0.032)	(0.030)
GDP	1.203	2.453	1.357	1.489	2.077
	(1.960)	(1.544)	(2.040)	(1.471)	(1.383)
Urbanization	2.005	0.233	4.491	1.845	1.130
	(4.038)	(2.672)	(4.754)	(3.286)	(2.881)
HDI	47.991*	48.662*	48.744*	51.352*	52.734*
	(22.775)	(13.091)	(22.635)	(17.507)	(15.538)
Area	-0.108	-0.155	0.363	0.477	0.231
	(0.761)	(0.529)	(0.787)	(0.518)	(0.486)
2014 Presidential Vote	-63.954*	-65.764*	-65.973*	-68.833*	-69.084*
	(5.704)	(3.859)	(6.832)	(4.217)	(3.855)
Mayor   State Coalition	-3.239*	-1.762*	-4.234*	-1.276	-1.215
	(1.601)	(0.847)	(1.693)	(1.132)	(0.963)
Mayor   Left	1.307	0.480	1.077	1.039	0.617
	(1.265)	(0.868)	(1.387)	(0.951)	(0.817)
Mayor   Female	1.470	-0.436	2.104	0.223	0.024
	(1.853)	(1.265)	(3.058)	(1.685)	(1.493)

+p<0.1, \*p<0.05.

Standard errors (parenthesis) are heteroskedasticity robust.

All panels show the estimation of equation 1. Column A only includes heists with explosions. Column B uses all events 3 months before and after the October election date. Column C uses all events 9 months before and after the October election date. Column D only excludes cities with previous heists 1 year before the sample's start. Column E includes cities with previous heists.

Table A.8: Placebo Effects on the Presidential Elections of 2018

	Bolsonaro	Haddad	Alckmin	Other Right	Other Left
<b>Panel A: Placebo in 2016</b>					
Effect	0.334 (0.627)	-0.680 (0.700)	0.507 (0.373)	0.090 (0.147)	-0.250 (0.520)
Electorate	1.423* (0.603)	-1.383* (0.666)	-0.027 (0.370)	0.072 (0.136)	-0.084 (0.438)
Homicide Rate	-0.002 (0.023)	0.029 (0.028)	0.012 (0.009)	0.005 (0.006)	-0.044+ (0.023)
GDP	3.530* (0.728)	-1.337+ (0.807)	-1.621* (0.540)	0.706* (0.213)	-1.278+ (0.708)
Urbanization	3.182 (2.049)	-2.445 (2.240)	-4.014* (1.224)	0.506 (0.476)	2.784+ (1.675)
HDI	45.986* (11.361)	-76.453* (12.021)	0.276 (4.689)	6.894* (2.905)	23.169* (9.091)
Area	-0.766+ (0.447)	0.661 (0.532)	0.137 (0.212)	-0.141 (0.101)	0.107 (0.369)
2014 Presidential Vote	-63.910* (3.278)	77.512* (3.453)	-11.333* (1.922)	-2.118* (0.795)	-0.164 (2.096)
Mayor   State Coalition	0.355 (0.772)	-0.381 (0.880)	0.661 (0.429)	0.134 (0.163)	-0.766 (0.691)
Mayor   Left	0.130 (0.740)	-0.105 (0.861)	-0.488 (0.308)	-0.225 (0.160)	0.683 (0.684)
Mayor   Female	-0.910 (0.645)	1.656+ (0.939)	0.208 (0.417)	0.156 (0.203)	-1.111 (0.778)

+p<0.1, \*p<0.05.

Standard errors (parenthesis) are heteroskedasticity robust.

All panels show the estimation of equation 1 with all covariates. Observations are weighted by their proximity to the election date, with a triangular kernel (bank raids closer to the election have higher weights). The estimation uses all events 6 months before and after the October election date, and excludes municipalities that had a bank raid in the previous 2 years before the start date for the sample. **Panel A** shows the results using 2018 votes around the placebo election date of Oct 07, 2016. **Panel B** shows the results using 2018 votes around the placebo election date of Oct 07, 2017.

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Table A.8: Placebo Effects on the Presidential Elections of 2018 (continued)

	Bolsonaro	Haddad	Alckmin	Other Right	Other Left
<b>Panel B: Placebo in 2017</b>					
Effect	0.109 (0.762)	-0.868 (0.736)	-0.114 (0.264)	0.246+ (0.126)	0.629 (0.477)
Electorate	0.770+ (0.411)	-1.165* (0.376)	-0.174 (0.199)	0.262* (0.077)	0.305 (0.238)
Homicide Rate	0.027* (0.013)	0.008 (0.014)	0.002 (0.005)	0.002 (0.003)	-0.040* (0.010)
GDP	2.645* (0.711)	-1.452* (0.659)	-0.671* (0.339)	0.639* (0.135)	-1.151* (0.393)
Urbanization	0.775 (1.477)	-1.117 (1.598)	-1.604* (0.614)	0.026 (0.271)	1.934+ (1.026)
HDI	51.823* (7.135)	-73.853* (7.631)	-8.965* (3.009)	8.252* (1.484)	22.598* (5.386)
Area	-0.259 (0.299)	0.313 (0.295)	0.226+ (0.131)	-0.252* (0.052)	-0.029 (0.218)
2014 Presidential Vote	-65.994* (2.311)	80.518* (2.374)	-11.217* (1.088)	-3.358* (0.470)	0.028 (1.526)
Mayor   State Coalition	-0.332 (0.530)	0.406 (0.614)	0.710* (0.257)	0.215+ (0.125)	-0.998* (0.455)
Mayor   Left	0.001 (0.469)	-0.013 (0.565)	-0.612* (0.194)	-0.204* (0.102)	0.826+ (0.441)
Mayor   Female	-0.291 (0.525)	0.634 (0.596)	0.396 (0.303)	0.075 (0.117)	-0.816+ (0.476)

+p<0.1, \*p<0.05.

Standard errors (parenthesis) are heteroskedasticity robust.

All panels show the estimation of equation 1 with all covariates. Observations are weighted by their proximity to the election date, with a triangular kernel (bank raids closer to the election have higher weights). The estimation uses all events 6 months before and after the October election date, and excludes municipalities that had a bank raid in the previous 2 years before the start date for the sample. **Panel A** shows the results using 2018 votes around the placebo election date of Oct 07, 2016. **Panel B** shows the results using 2018 votes around the placebo election date of Oct 07, 2017.

Table A.9: Marginal Effect of Bank Attacks, by population

	Coefficient	S.E.	P-value
Effect	4.270*	( 1.517)	0.005
Electorate	0.109+	( 0.065)	0.098
Homicide Rate	0.013	( 0.029)	0.650
GDP	2.314	( 1.697)	0.174
Urbanization	0.765	( 3.114)	0.806
HDI	50.152*	(16.312)	0.002
Area	0.230	( 0.600)	0.703
2014 Presidential Vote	-65.857*	( 4.418)	0.000
Mayor   State Coalition	-2.214*	( 1.105)	0.046
Mayor   Left	0.916	( 1.114)	0.412
Mayor   Female	-0.814	( 1.676)	0.628
Effect x Electorate	-0.065	( 0.084)	0.440

+p<0.1, \*p<0.05.

Standard errors (parenthesis) are heteroskedasticity robust. The regression shows the estimation of equation 1, with the treatment dummy also interacted with the size of the local electorate. For this specific regression, the electorate is expressed in thousand voters, and not in log(variable).

Table A.10: Voter's Institutional Perceptions with All Coefficients

Dep. Variable	Feel Safe	Trust the Police	Respect Institutions	System Protects Basic Rights
Effect	-0.224* (0.082)	-0.337* (0.124)	-0.320* (0.151)	-0.374* (0.102)
Electorate	-0.015 (0.072)	-0.151+ (0.084)	-0.229+ (0.130)	-0.117 (0.086)
Homicide Rate	-0.004+ (0.002)	0.002 (0.003)	-0.001 (0.004)	0.000 (0.003)
GDP	0.022 (0.112)	-0.189* (0.090)	-0.320* (0.151)	-0.211* (0.092)
Urbanization	0.314 (0.478)	0.817 (0.560)	-0.948 (0.768)	0.148 (0.551)
HDI	-2.015 (2.459)	3.069 (2.611)	1.911 (2.909)	0.169 (2.376)
Area	0.059* (0.024)	0.125* (0.047)	0.054 (0.046)	0.075* (0.032)
2014 Presidential Vote	0.220 (0.417)	0.141 (0.459)	-0.436 (0.508)	-0.835 (0.562)
Mayor   State Coalition	0.195 (0.133)	0.013 (0.133)	-0.100 (0.201)	-0.260+ (0.138)
Mayor   Left	-0.076 (0.162)	0.336+ (0.178)	0.163 (0.247)	0.210 (0.266)
Mayor   Female	0.240* (0.107)	-0.096 (0.172)	-0.051 (0.246)	-0.037 (0.298)

+p<0.1, \*p<0.05.

Includes responses from 9 municipalities that were attacked (Treatment group), and 31 municipalities that were not (Control group). Standard errors (parentheses) are clustered at the municipality level. Regressions include state-survey fixed effects. The dependent variables and their scaling are described in the text and Appendix B.

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Table A.10: Voter's Institutional Perceptions with All Coefficients (continued)

Dep. Variable	Proud of Institutions	Military Coup is Acceptable	Military Coup (Corruption)	Crime is Number One Problem
Effect	-0.436* (0.157)	0.091* (0.043)	0.017 (0.066)	-0.010 (0.022)
Electorate	-0.039 (0.103)	-0.006 (0.036)	0.070* (0.035)	0.018 (0.023)
Homicide Rate	-0.005 (0.004)	0.001 (0.001)	0.002 (0.002)	0.002* (0.001)
GDP	0.022 (0.114)	-0.077 (0.048)	-0.025 (0.048)	0.056 (0.035)
Urbanization	0.209 (1.013)	-0.152 (0.250)	-0.625+ (0.324)	-0.005 (0.181)
HDI	-2.529 (3.066)	1.481 (1.096)	1.026 (1.610)	-0.961 (0.921)
Area	0.196* (0.044)	0.002 (0.024)	0.026 (0.020)	-0.021* (0.009)
2014 Presidential Vote	-0.605 (0.460)	0.194 (0.172)	-0.207 (0.277)	-0.139 (0.128)
Mayor   State Coalition	-0.511* (0.113)	-0.029 (0.057)	-0.108 (0.066)	-0.018 (0.034)
Mayor   Left	0.554 (0.413)	0.043 (0.093)	0.033 (0.094)	-0.061 (0.042)
Mayor   Female	-0.268 (0.270)	0.107 (0.100)	-0.121 (0.095)	-0.030 (0.025)

+p<0.1, \*p<0.05.

Includes responses from 9 municipalities that were attacked (Treatment group), and 31 municipalities that were not (Control group). Standard errors (parentheses) are clustered at the municipality level. Regressions include state-survey fixed effects. The dependent variables and their scaling are described in the text and Appendix B.

Table A.11: Voter's Perceptions on Unrelated Issues with All Coefficients

Dep. Variable	Trust Elections	Trust Media	Supports Abortion	State of the Economy	Climate Change is a Problem
Effect	-0.141 (0.182)	-0.142 (0.203)	0.019 (0.059)	0.073 (0.060)	0.037 (0.062)
Electorate	0.009 (0.116)	-0.208* (0.103)	0.045+ (0.023)	-0.041 (0.031)	-0.013 (0.052)
Homicide Rate	0.000 (0.004)	-0.003 (0.004)	-0.001 (0.001)	0.005* (0.001)	0.001 (0.002)
GDP	-0.066 (0.113)	-0.173 (0.151)	0.113* (0.041)	-0.060 (0.038)	-0.053 (0.064)
Urbanization	-0.264 (0.608)	0.046 (0.713)	-0.540* (0.259)	-0.600* (0.162)	-0.536 (0.342)
HDI	-2.500 (3.045)	2.623 (3.560)	-0.806 (1.224)	1.585 (1.172)	3.780* (1.560)
Area	-0.064 (0.039)	0.076 (0.048)	-0.012 (0.012)	-0.021+ (0.011)	-0.017 (0.023)
2014 Presidential Vote	-0.508 (0.504)	-1.494* (0.416)	0.217 (0.209)	-0.248 (0.221)	0.713* (0.336)
Mayor   State Coalition	-0.087 (0.177)	-0.006 (0.208)	-0.034 (0.044)	0.068 (0.055)	0.030 (0.077)
Mayor   Left	0.701* (0.316)	0.283 (0.201)	0.204* (0.061)	-0.117 (0.095)	0.033 (0.097)
Mayor   Female	-0.133 (0.232)	-0.250 (0.280)	0.022 (0.063)	0.114+ (0.066)	-0.075 (0.146)

+p<0.1, \*p<0.05.

Includes responses from 9 municipalities that were attacked (Treatment group), and 31 municipalities that were not (Control group). Standard errors (parentheses) are clustered at the municipality level. Regressions include state-survey fixed effects. The dependent variables and their scaling are described in the text and Appendix B.

Table A.12: Voter's Institutional Perceptions after Individual Victimization

Dep. Variable	Feel Safe	Trust the Police	Respect Institutions	System Protects Basic Rights
Effect	-0.349* (0.115)	-0.635* (0.227)	-0.248 (0.173)	-0.049 (0.185)
Electorate	-0.037 (0.075)	-0.162+ (0.090)	-0.271* (0.123)	-0.186* (0.089)
Homicide Rate	-0.004 (0.003)	0.003 (0.004)	0.000 (0.004)	0.001 (0.003)
GDP	0.042 (0.119)	-0.157 (0.095)	-0.304* (0.146)	-0.199+ (0.103)
Urbanization	0.268 (0.513)	0.715 (0.513)	-0.976 (0.756)	0.211 (0.595)
HDI	-1.300 (2.421)	3.925 (2.637)	2.967 (2.600)	1.354 (2.568)
Area	0.047 (0.029)	0.116* (0.052)	0.041 (0.043)	0.066+ (0.035)
2014 Presidential Vote	0.323 (0.422)	0.234 (0.471)	-0.353 (0.455)	-0.852 (0.542)
Mayor   State Coalition	0.209 (0.137)	0.033 (0.147)	-0.066 (0.203)	-0.191 (0.138)
Mayor   Left	-0.067 (0.173)	0.342+ (0.182)	0.172 (0.256)	0.204 (0.274)
Mayor   Female	0.209* (0.103)	-0.170 (0.156)	-0.092 (0.222)	-0.076 (0.291)

+p<0.1, \*p<0.05.

Includes responses from the 40 municipalities with less than 150,000 voters in surveys. Treatment here is self-reported victimization at the individual level. Standard errors (parentheses) are clustered at the municipality level. Regressions include state-survey fixed effects. The dependent variables and their scaling are described in the text and Appendix B.

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Table A.12: Voter's Institutional Perceptions after Individual Victimization

Dep. Variable	Proud of Institutions	Military Coup is Acceptable	Military Coup (Corruption)	Crime is Number One Problem
Effect	-0.506* (0.184)	-0.075 (0.079)	0.056 (0.062)	-0.003 (0.029)
Electorate	-0.076 (0.103)	0.008 (0.037)	0.071* (0.029)	0.018 (0.023)
Homicide Rate	-0.004 (0.005)	0.001 (0.001)	0.002 (0.002)	0.002* (0.001)
GDP	0.052 (0.130)	-0.084+ (0.046)	-0.029 (0.048)	0.056 (0.035)
Urbanization	0.155 (1.034)	-0.173 (0.250)	-0.637+ (0.325)	-0.006 (0.185)
HDI	-1.326 (3.234)	1.192 (1.095)	0.972 (1.518)	-0.936 (0.901)
Area	0.183* (0.044)	0.001 (0.024)	0.026 (0.020)	-0.021* (0.009)
2014 Presidential Vote	-0.521 (0.525)	0.242 (0.182)	-0.221 (0.280)	-0.140 (0.130)
Mayor   State Coalition	-0.471* (0.117)	-0.043 (0.058)	-0.111+ (0.064)	-0.018 (0.034)
Mayor   Left	0.560 (0.419)	0.028 (0.098)	0.035 (0.094)	-0.060 (0.042)
Mayor   Female	-0.346 (0.268)	0.121 (0.098)	-0.118 (0.094)	-0.031 (0.026)

+p<0.1, \*p<0.05.

Includes responses from the 40 municipalities with less than 150,000 voters in surveys. Treatment here is self-reported victimization at the individual level. Standard errors (parentheses) are clustered at the municipality level. Regressions include state-survey fixed effects. The dependent variables and their scaling are described in the text and Appendix B.

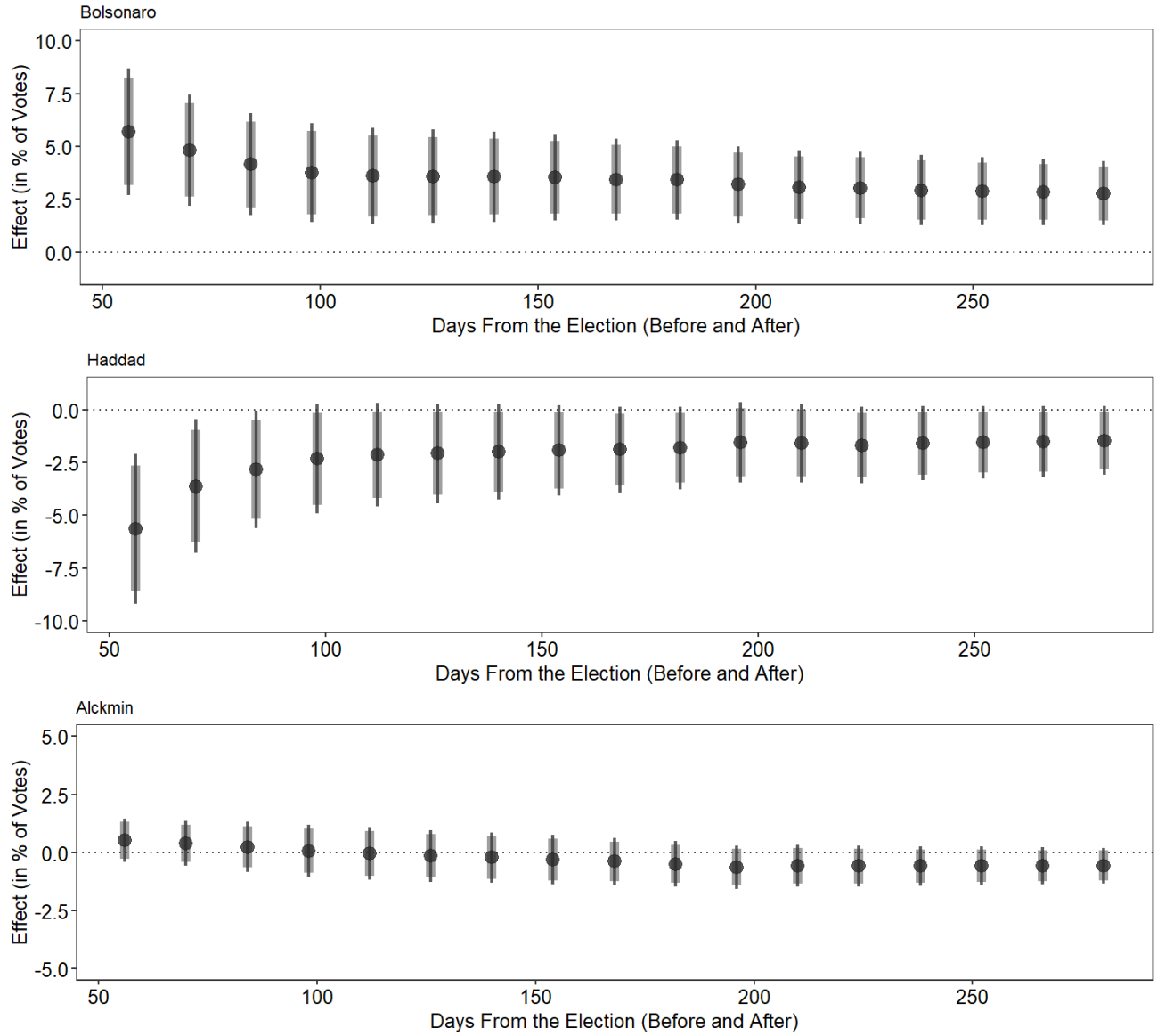
Table A.13: Balance of Municipal Characteristics for the Survey Exercise

	Coefficient	S.E.	Control Average	Observations (Respondents)
Electorate	0.426	(0.329)	24.592	826
Homicide Rate	3.479	(9.619)	25.171	826
GDP	-0.233	(0.226)	1.697	826
Urbanization	-0.001	(0.037)	0.820	826
HDI	-0.009	(0.012)	0.707	826
Area	0.190	(0.374)	0.506	826
2014 Presidential Vote	0.028	(0.047)	0.475	826
Mayor   State Coalition	0.039	(0.104)	0.123	826
Mayor   Left	-0.056	(0.116)	0.355	826
Mayor   Female	-0.089	(0.110)	0.107	826

+p<0.1, \*p<0.05.

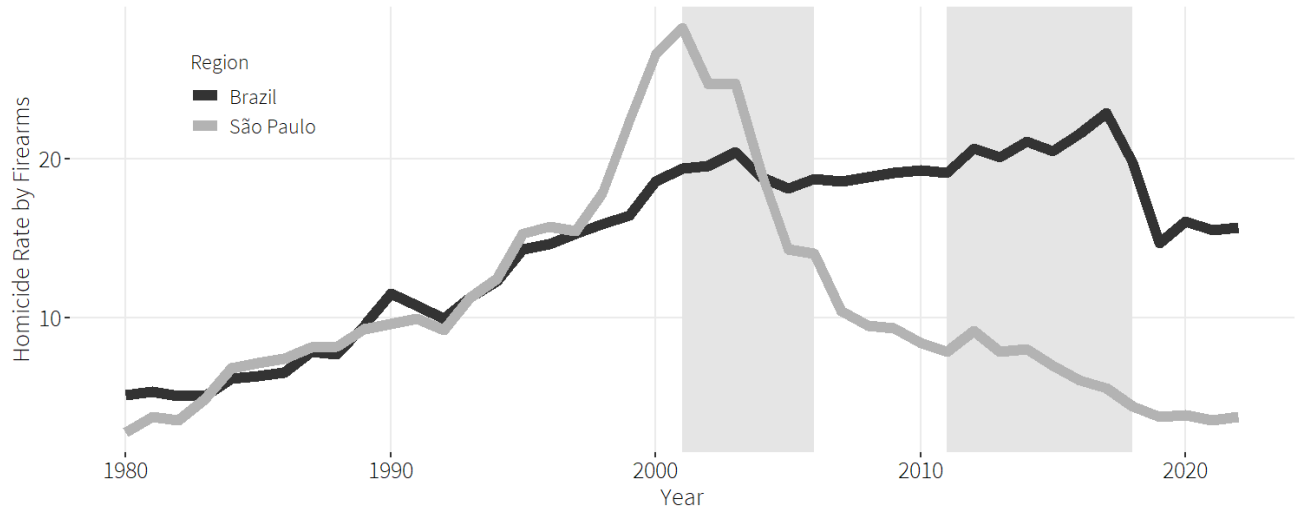
All variables are measured at the municipality level. The Table includes data from 9 municipalities that were attacked (Treatment group), and 31 municipalities that were not (Control group). Standard errors (parentheses) are clustered at the municipality level, and all regressions include State-Survey Wave fixed effects. The construction of the variables is described in Table [A.1](#).

Figure A.1: Effect of Bank Raids on the Presidential Election, by Time Window



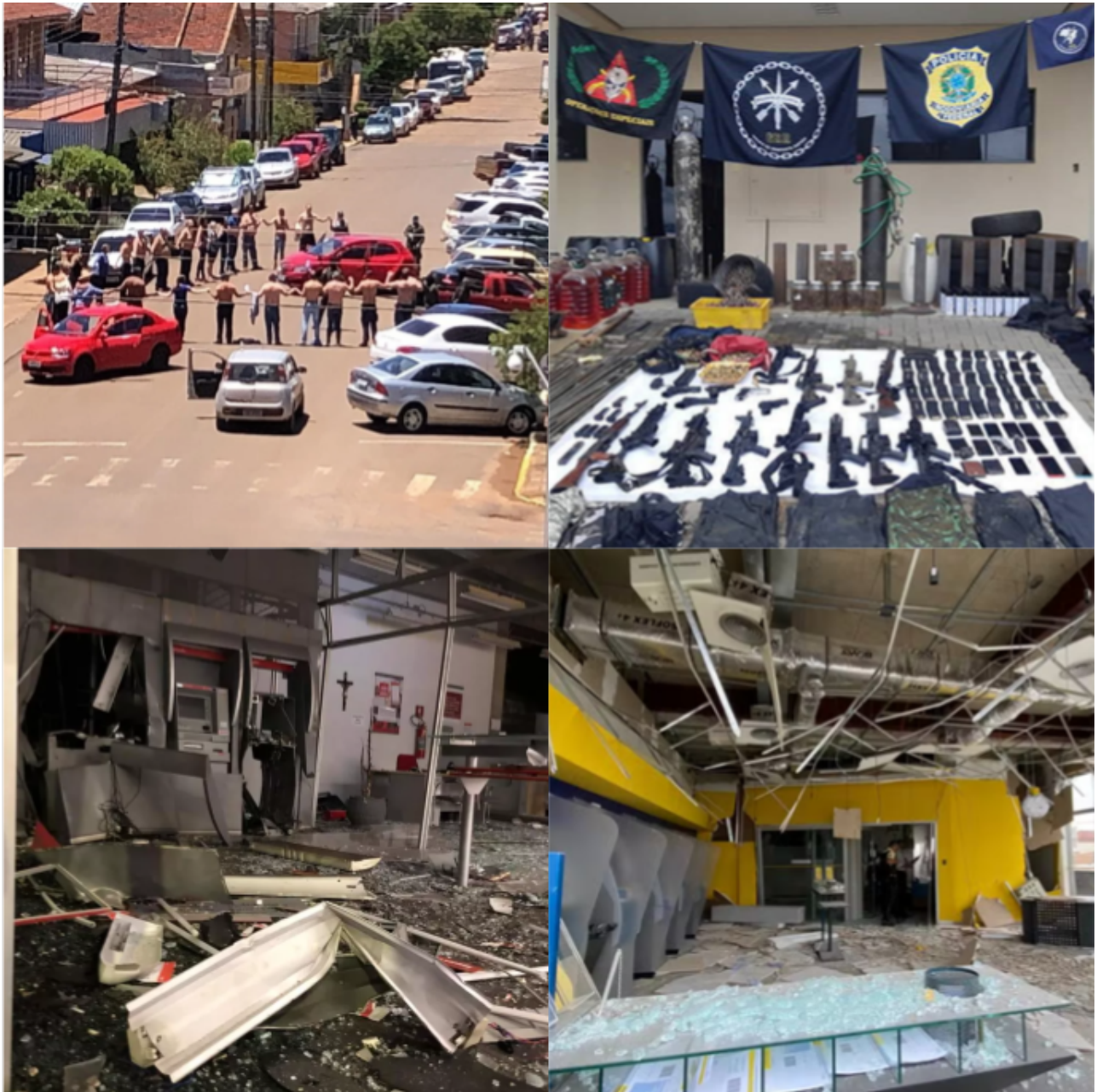
The thin (thick) bar has the 95% (90%) confidence intervals. The dots correspond to the treatment effect estimated with equation 1. Each estimate includes only bank raids that happened, before or after the election (Oct 7, 2018), within the number of days indicated in the x-axis.

Figure A.2: Rate of Homicides by Firearms (Brazil vs. São Paulo)



Annual averages per 1,000 population. The periods shaded in gray correspond to Geraldo Alckmin's tenures as São Paulo's state governor.

Figure A.3: Pictures



Top Left: Attack with hostages in Ibirairas (RS), December, 2018. Source: [gauchazh.clicrbs.com.br](http://gauchazh.clicrbs.com.br)

Bottom Left: Destroyed branch in São Miguel Arcanjo (SP), September, 2018. Source: [g1.globo.com](http://g1.globo.com).

Top Right: Weapons from an arrest in Varginha (MG), November, 2021. Source: [folha.com.br](http://folha.com.br).

Bottom Right: Destroyed branch in São Pedro (SP), April, 2024. Source: [g1.globo.com](http://g1.globo.com).

## B QUESTIONS FROM THE LAPOP SURVEY

Table A.14: Questions Directly Related to Institutional Trust and Security

Variable	Scale	Question	Code
Respect for Institutions	1 to 7	To what extent do you have respect for the Brazilian political institutions?	B2
System Protects Basic Rights	1 to 7	To what extent do you think that the citizens basic rights are protected by the political system?	B3
Proud of Institutions	1 to 7	To what extent are you proud to live under the Brazilian political system?	B4
Feels Safe	1 to 4	Talking about your neighborhood in the context of the possibility of becoming victim of a robbery, do you feel? (1) very insecure (2) a little insecure (3) somewhat safe (4) very safe	AOJ11
Military Coup is Acceptable	binary	When there is a lot of crime, would it be justified for the military to take power through a coup? (1) yes (0) no	JC10
Trust the Police	1 to 7	To what extent do you trust the (military) police?	B18
Crime is Number One Problem	binary	In your opinion, what is the most severe problem that the country is facing? Voters could pick one of several options, the following were coded as security-related: Security, Crime, Violence, Gangs, Kidnappings, Armed conflict, and Drug trafficking	A4

Table A.15: Questions Unrelated to Institutional Trust and Security

Variable	Scale	Question	Code
Trust Elections	1 to 7	To what extent do you trust the Brazilian electoral system?	B47a
Trust Media	1 to 7	To what extent do you trust the media?	B37
Abortion is Justified	binary	Do you believe that the interruption of a pregnancy, i.e. an abortion, is justified when the mother's health is at risk ? (0) No (1) Yes	W14A
Economy is Better	1 to 3	Do you believe that the current conomic situation is better, the same, or worse than twelve months ago? (1) Worse (2) Same (3) Better	SOCT2
Climate Change is a Problem	1 to 4	If nothing is done to reduce future climate change, how serious do you think the problem is for Brazil? (1) Not serious at all (2) A little serious (3) Somewhat serious (4) Very serious	ENV2B