A New Path to Police Reform?
Effects of a New Police Squad in Ceará, Brazil

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Abstract

Because reforming ineffective law enforcement institutions is a formidable challenge, policymakers in violent societies often create new policing squads to reduce crime, curb abuses, and enhance legitimacy. We study the *Rondas e Ações Intensivas e Ostensivas* (RAIO) in Ceará, Brazil, a militarized, motorcycle-based police squad. Using difference-in-differences and regression discontinuity designs, we find that RAIO dramatically reduced homicides and property crimes, likely through deterrence rather than incapacitation, as arrests also declined. A citizen survey with 2,000 respondents and embedded experiments shows that RAIO is perceived as more effective, less corrupt, and less abusive than other forces. We demonstrate that RAIO's success did not increase human rights abuses, provided substantial social benefits relative to operational costs, and helped the incumbent governor electorally. We discuss the implications of our findings for institutional reform and public safety.

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Introduction

Reforming ineffective public institutions is a formidable challenge. Reform efforts encounter internal resistance, given that those in power typically are invested in preserving the status quo (North 1991). Bureaucratic inertia and a preference for stability over innovation in public sector institutions often allows even ineffective practices to persist (Olson 1982; Wilson 1989; Pierson 2000; Mahoney and Thelen 2009). Corruption can hinder or delay reform efforts, as patronage politics often involves filling bureaucracies with political allies instead of skilled public servants capable of addressing inefficient policies (Hellman 1998; Folke, Hirano and Snyder 2011; Huber and Ting 2021). Even when there is genuine interest in reform, rigid norms about hiring and firing within state institutions may make it difficult to dismiss underperforming employees in order to hire new talent capable of driving innovation (Finan, Olken and Pande 2017). These interlocking challenges span policy sectors and regions, extending from attempts to reform the education system in India (Devarajan 2011) to the healthcare sector in France (Wilsford 1994).

The difficulties of achieving institutional transformations are particularly pronounced within law enforcement institutions. Because the police can impose considerable political costs on politicians who seek to transform law enforcement institutions, the police are usually able to deter or undermine even critically-needed changes (González 2020). A unique, predominantly conservative culture is also more deeply ingrained in police agencies than other public sector institutions (Sierra-Arévalo 2024): characterized by solidarity and a strong adherence to traditional practices and norms, efforts to introduce new ideas and processes that could improve public safety may be stymied (Chan 1996; Loftus 2009; Westmarland and Conway 2017).

Policymakers have explored a range of options to reform law enforcement institutions, with the goals of making the police better able to reduce crime, less abusive of human rights, and more legitimate in the eyes of the communities they serve. While some have proposed wholesale transformations of policing institutions, radical reforms—such as the one that Camden, New Jersey pursued in 2013, when it dissolved its police department to start from scratch—are politically treacherous and rarely successful (González 2020). More incremental changes aim to address discrete issues, such as misconduct and accountability. Measures like body-worn cameras and the removal of officers with histories of abuse have shown varied results, with some successes but also unintended negative consequences (Ariel et al. 2020; Demir et al. 2020; Barbosa et al. 2021; Magaloni, Melo and Robles 2023; Fagundes, Monteiro and Souza 2024).[1] Training in procedural

[1] Magaloni, Melo and Robles (2023), for example, shows that body-worn cameras in Rio de Janeiro prompted police officers to

justice practices—which stress transparency, neutrality and consistency of interactions with citizens—has also shown promise (Abril et al. 2023).

Police reforms are especially difficult where citizens experience high levels of violence and powerful criminal organizations, making residents willing to sacrifice some degree of civil rights protections for the promise of greater security (Dammert and Bailey 2005; Arias and Goldstein 2010; Davis and Pereira 2003). Politicians in these environments may avoid deeply-needed reforms and instead rely on *mano dura* or "iron fist" approaches, including increasing criminal penalties for minor offenses, lowering the age of criminal prosecution, and deploying soldiers for domestic law enforcement (Muggah, Garzón and Suárez 2018). Despite the widespread popular support that these policies enjoy (Blair, Weintraub and Zarkin 2022; Holland 2013)—nearly 60% of Latin Americans support the military's participation in public safety (Zechmeister 2014)—evidence shows that using the military for policing is ineffective at best and, at worst, may increase crime and human rights abuses (Blair and Weintraub 2023; Durán-Martínez and Soifer 2021; Flores-Macías and Zarkin 2023; Magaloni and Rodriguez 2020).[2]

Some governments have taken a different route altogether, opting to create new policing squads to reduce crime and violence. These include the *Batalhão de Operações Policiais Especiais* (BOPE) in Brazil;[3] the *Grupo de Operaciones Especiales* (GOPES o GERI) in Mexico;[4] the Tactical Response Team in South Africa;[5] and the GAULA in Colombia.[6] Although these squads are often created to equip the police with specialized tactics for addressing specific problems, they frequently become models for broader police reform, as their specialized rules and methods prove more effective in combating crime. Governments can bypass institutional challenges and implement changes more quickly with new squads, avoiding drawn-out political conflicts within existing entities. These squads can operate with different rules or mandates compared to traditional forces, allowing governments to be more flexible and experimental, unburdened by the constraints of existing bureaucratic procedures. By selectively recruiting highly motivated officers and offering attractive

intentionally avoid interacting with civilians, as they feared punishment for negative consequences associated with those interactions.

[2] Military policing may also induce undemocratic attitudes and a preference for future military involvement in law enforcement and governance more broadly (Blair, Mendoza Mora and Weintraub forthcoming).

[3] BOPE, an elite unit of the Military Police in Rio de Janeiro, is known for its advanced weaponry and confrontations with heavily armed criminal groups in the city's *favelas* (Soares, Batista and Pimentel 2006).

[4] These are elite units that require new recruits to undergo rigorous selection processes and receive specialized training in urban combat, tactical driving, and crisis negotiation.

[5] This squad was created to deal with high-risk situations and violent crime, aiming to provide a rapid response to serious criminal incidents that were not capably resolved by the regular officers within the South African Police Service.

[6] This squad was established to attack specific kinds of crime, including extortion and kidnapping.

incentives, these squads can foster a more effective and professional policing culture. More rigorous training may not only mitigate misconduct, but also help build trust through improved accountability and citizen engagement. New policing units may also signal to the public that authorities are taking crime seriously. By creating a fresh institutional environment, new policing units have the potential to improve public safety.

Yet new policing squads may pose risks. If highly militarized, these squads may adopt aggressive tactics, increasing human rights abuses (Flores-Macías 2018). If they prove more effective than traditional law enforcement, over time the legitimacy and authority of the latter may suffer, much like when the police lose credibility following the deployment of soldiers for domestic law enforcement (Blair and Weintraub 2023; Blair, Weintraub and Zarkin 2022). These countervailing effects on public perceptions, combined with excitement regarding the deployment of new squads, can generate cross-agency frictions and foment rivalries, reducing overall law enforcement efficacy. Even if new squads and traditional police organizations are keen to work together, overlapping responsibilities between agencies can cause confusion, miscommunication, and duplication of effort. In the extreme, the creation of new policing agencies may increase the chances of widespread violence—especially in post-conflict settings—given that the fragmentation of policing institutions may exacerbate principal-agent problems, leading to interest divergence and shirking (Arriola et al. 2021).

In this paper, we study the creation of the *Rondas e Ações Intensivas e Ostensivas* (RAIO) in Ceará, Brazil, a new police squad designed to reduce crime and violence by deploying militarized, motorcycle-based patrols. Ceará, a state of nearly 10 million people, has experienced a sharp increase in violence over the past decade. In Fortaleza, the state capital, the homicide rate soared to 77 per 100,000 in 2014, the highest among Brazilian state capitals. RAIO was introduced, in part, to address this surge in violence. Members of RAIO were recruited from the ranks of the pre-existing military police forces, but only included officers with impeccable records of service. They were also better compensated: newly-recruited RAIO officers were offered, on average, 30% higher salaries than what they received when serving with the "ordinary" military police. Officers also received 280 hours of additional training and had their performance tracked meticulously by their supervisors.

We evaluate the effect of RAIO patrols on crime, and seek to uncover potential mechanisms driving these effects. Our difference-in-differences strategy exploits quasi-experimental variation in the roll-out of RAIO bases across Ceará's 184 municipalities. Because two-way linear fixed effects (TWFE) models produce biased estimators in the presence of heterogeneous and time-varying treatments (De Chaisemartin and d'Haultfoeuille 2020; Goodman-Bacon 2021; Borusyak, Jaravel and Spiess 2021; Sun and Abraham 2021; Athey and Imbens 2022), we use the doubly-robust estimator proposed by Callaway and Sant'Anna (2021). As an alternative empirical strategy, in Appendix C. we use a regression discontinuity design (RDD) that exploits a municipal population threshold used to determine program eligibility.[7]

The difference-in-differences results show meaningful reductions in homicides and violent property crimes attributable to RAIO, equivalent to a reduction of 1.3 homicides per month in each treated municipality—a 57% decrease when compared to not-yet treated units in the pre-treatment period—and 17.3 robberies per month in each treated municipality—a 84% decrease relative to not-yet treated units in the pre-treatment period. We also estimate a decrease of 3 arrests per month in each treated municipality, indicating that the crime reduction effects that we estimate are likely due to deterrence, rather than incapacitation of criminals (Becker 1968). We do not find any change in domestic abuse or sexual crimes. Our RDD results in Appendix C. show that RAIO patrols reduced other forms of violent crime, as well.

To understand what makes RAIO different from other police forces—specifically whether ordinary citizens simply fear RAIO, or instead perceive them to be more effective, better trained, and less likely to be corrupt—we conducted a survey of 2,000 residents in Fortaleza and especially dangerous districts within the city's broader metropolitan area. Following Flores-Macías and Zarkin (2021) and Blair, Mendoza Mora and Weintraub (forthcoming), we include an image-based conjoint experiment that provides respondents with images with randomly-varied attributes to identify which features (RAIO vs. ordinary military police uniform; high-caliber weapon vs. pistol; use of motorcycle vs. no motorcycle) affect attitudes. We find that while the motorcycle—signaling rapid response—is particularly important for increasing residents' perceptions of effectiveness, safety, and the likelihood of using force against criminals, the RAIO uniform is *far* more important in reducing perceptions of abuse and corruption.

Having established that the creation of this new policing squad reduces crime and violence, and that RAIO distinguished itself as an especially legitimate and trustworthy force, our discussion section first assesses how Ceará achieved this, focusing on both pecuniary and non-pecuniary incentives, and exceptionally high standards. It then shows that the incumbent governor responsible for the RAIO program who subse-

[7]Given that seven municipalities below the population threshold received the program during the second phase, we use a fuzzy RDD. We provide evidence that the sample is balanced on background characteristics, and in favor of the assumption of no sorting along the forcing variable. We relegate these results to an Appendix for compactness, and because these analyses may be underpowered.

quently ran for reelection benefited mightily from the expansion of the new RAIO squad. We then perform a back-of-the-envelope cost-benefit analysis and demonstrate that the social benefits generated by RAIO are far greater than the operational costs of implementing the program. Finally, we establish that RAIO's success at reducing crime did not come at the expense of increasing human rights abuses, at least as measured by police killings.

We make a number of contributions to the literature. Existing work on police reform often focuses on internal measures such as retraining, stronger accountability mechanisms, and changes in leadership (Bailey and Dammert 2006; Goldsmith 1990). However, we find that new policing squads under certain circumstances can represent viable alternatives to conventional reforms. While these deployments are often implemented with the assumption that they will enhance public safety, systematic evaluations of their impact are scarce (Moore, Trojanowicz and Kelling 1988; Sherman 1998). Our analysis suggests that, under certain conditions, the establishment of parallel institutions may be a effective strategy for achieving sustainable transformations of policing institutions. That we find limited downsides to the creation of the RAIO—e.g. no increase in arrests, which can be socially costly, and no evidence of increased police killings—gives us greater confidence that this approach could be effective elsewhere. At the same time, we acknowledge that RAIO's intense screening process for new members, better work conditions, and no tolerance policy for abuse are typically lacking in other police reform efforts, and it is not clear whether these strategies can be scaled up throughout the broader police force.[8]

Second, our study contributes to understanding how to circumvent obstacles that typically prevent reforms of public institutions. Previous studies have highlighted how efforts to reform state institutions encounter substantial resistance from within, making thoroughgoing reform difficult, if not impossible (Grindle 2004). Our findings illustrate how the government of Ceará deployed new, specialized policing units to operate alongside flawed, pre-existing police structures. Creating new bureaucracies may be a worthwhile policy option–within but also beyond the security sector–when traditional reform paths are blocked. The establishment of special transitional justice courts (e.g. Colombia's Special Jurisdiction for Peace) and anti-corruption commissions (e.g. Nigeria's Economic and Financial Crimes Commission) are motivated, in part, by similar concerns. We begin a conversation about the conditions under which these efforts are likely to be successful.

Finally, our research advances the understanding of the political effects of public safety interventions

[8]This includes the creation of Rio de Janeiro's Pacifying Police Units (UPP).

by demonstrating how successful crime reduction can have tangible electoral consequences. The literature on the political economy of public safety interventions suggests that reducing crime enhances the electoral prospects of incumbent governments (e.g. Levitt 1997). Our findings reveal that areas benefiting from the introduction of new policing squads exhibit increased support for the incumbent politician who implemented the policy. These strategies are not only effective but also are recognized as such by ordinary citizens, making them compatible with the electoral incentives of politicians, clearing a path for implementation.

As with any study focused on a single case, we cannot be sure how far our results will generalize beyond Ceará. Yet this Brazilian state shares a number of characteristics with other places in Latin America, and the RAIO patrols bear resemblance to other policing squads throughout the developing world. Similar to numerous cities in Latin America, Ceará's capital of Fortaleza suffers from severe social and economic divisions, as do the state's other municipalities. The history of the police in Fortaleza, and Ceará more broadly, has been marred by corruption, collusion with organized crime, and human rights violations. As a result, military policing is widely endorsed by the Brazilian public. Studying Ceará provides valuable insights into the dynamics of crime, violence, and policing in other cities across Latin America and the developing world, where experimentation with new policing strategies is common but has seldom undergone rigorous evaluation.

CONCEPTUAL FRAMEWORK

The police are unique among state agents given that they are authorized to regulate interpersonal relations through the use of physical force (Bayley 1990). In democratic societies, police experienced a major transformation over the twentieth century, evolving from an institution mainly entrusted with preserving order for elites—by suppressing collective unrest—to one tasked with responding to the diverse needs of the general public (Bailey 1985). Today governments and ordinary citizens turn to the police first to solve a myriad of problems (Dammert and Bailey 2005).

While crime-fighting is a central focus of contemporary policing, the police must also ensure equity, due process, just desserts, and parsimony (Thacher 2001). While citizens expect the police to wield the state's monopoly on force fairly, sparingly, and equitably (Rawls 1971), in practice law enforcement institutions frequently struggle to achieve these goals. Police forces often fail to protect citizens from crime and, at the extreme, act outside the bounds of the rule of law, engaging in extrajudicial killings and torture while remaining unaccountable to civilian authorities (Brinks 2007). Policing failures are particularly severe in Latin America, where scholars argue that the police are remnants of autocratic and illiberal "enclaves," even within democratic states (González 2020). Challenges include a lack of professionalization, inadequate training, insufficient specialization, limited resources, and poor effectiveness in crime prevention and investigation. Limited accountability fails to deter bad behavior, due to weak oversight mechanisms and widespread corruption (Davis 2006). These myriad deficiencies help explain why police forces suffer from high levels of societal distrust (Macaulay 2012; Ungar 2002).

In recent decades, reform strategies have sought to address many of these issues. Strategies include policies aimed at changing external entities alone (marginal reforms), modifying police practices (operational reforms), restructuring organizational systems and rules (structural reforms), and establishing external oversight mechanisms that alter internal practices (external control reforms) (González 2020). Achieving substantive change, however, is remarkably difficult. Many police forces remain aligned with the interests of political leaders who have little incentive to promote reforms (González and Zarkin 2024). Even when politicians face strong public pressure or are genuinely committed to reform, the structural power of the police limits available policy options. Police reforms are particularly challenging in contexts where citizens face high levels of drug-related violence and powerful criminal gangs. In such environments, residents often prioritize immediate security over long-term civil rights protections, creating pressure for aggressive and militarized policing strategies that may undermine reform efforts aimed at improving accountability and professionalism (Dammert and Bailey 2005; Arias and Goldstein 2010; Davis and Pereira 2003; Blair, Mendoza Mora and Weintraub forthcoming).

Given these constraints, holistic policing reforms have been jettisoned in the name of more incremental improvements to police services. Comprehensive reforms, such as those pursued in Camden, New Jersey (Vassallo 2001), and Honduras (Dye 2019) are outliers. More commonly-implemented strategies in the United States and the United Kingdom, for example, involve marginal improvements, using data to target, test, and track police deployments and their impacts (Weisburd and Braga 2006; Sherman 2013). Governments in the developing world have likewise sought to pursue changes while minimizing police resistance, with a specific focus on misconduct and accountability. Measures like body-worn cameras and the removal of officers with histories of abuse have shown varied results, with some successes but also unintended negative consequences (Ariel et al. 2020; Demir et al. 2020; Barbosa et al. 2021; Magaloni, Melo and Robles 2023; Fagundes, Monteiro and Souza 2024). Training in procedural justice practices—which stress transparency, neutrality and consistency of interactions with citizens—has also shown promise in part because it requires relatively minor organizational changes to implement (Abril et al. 2023).

Operational reforms, including the creation of specialized squads, have also become increasingly common. These new squads are usually created as pilots that are later scaled up, as was the case with the UPP in Rio de Janeiro, which began as small, separate units and gradually gained political support. The UPP eventually became a new police force in and of itself, designed to overcome the pathologies inherent to predecessor institutions (Ferraz, Monteiro and Ottoni 2024).

The introduction of new policing squads can be an effective strategy in crime reduction by addressing several key challenges inherent to existing policing institutions. One concerns attracting and purposefully selecting high-quality, motivated recruits (Linos 2018). When forming new police units, policing institutions have an opportunity to recruit the best and brightest from existing forces and circumvent many public sector barriers that would otherwise prevent them from recruiting whom they prefer during the selection process. This careful and deliberate selection may ensure that officers are better equipped, both mentally and physically, to handle the complexities of modern policing, which in turn can lead to more effective crime reduction. A second challenge lies in preventing police misconduct from taking root within law enforcement departments. Since misconduct often behaves like a social contagion—officers linked to partners with histories of misconduct are more likely to adopt similar behaviors (Quispe-Torreblanca and Stewart 2019; Holz, Rivera and Ba 2023)—establishing new forces could, at least in the short term, help disrupt these patterns.

Motivation also plays an important role in the potential success of new policing squads, given enthusiasm deficits common to police departments (Wilson 2009). If new squads are able to provide more attractive incentives to their recruits, they can be motivated to perform their duties with a higher level of professionalism and dedication. These incentives might include highly competitive salaries, opportunities for career advancement, public recognition for being part of a elite group, or tangible rewards for exemplary conduct, including time off. The prospect of receiving benefits for maintaining high standards of conduct and efficacy can significantly enhance the motivation of officers, leading to improved patrolling, more effective criminal investigations, and ultimately, crime reduction. Furthermore, by emphasizing the importance of respectful interactions with citizens and effective crime deterrence in ways that are untethered from past cycles of heated and contested interactions with civilians, these squads may be able to build from scratch a culture of excellence that contrasts with fraught police-civilian dynamics found in older institutions.

Finally, the introduction of new police squads can improve accountability. New units may be equipped with new and more robust mechanisms for deterring, investigating, and punishing bad behavior, such as corruption and human rights abuses, free from historical institutional weaknesses found in pre-existing departments. These mechanisms might include more rigorous oversight, transparent disciplinary procedures, and the use of technology to monitor interactions that officers have with the public. By creating an environment where officers are held to higher standards, and where immediate consequences can be exacted for misconduct, new squads can reduce the likelihood of abuses of power and thereby increase public trust in law enforcement. That trust is essential for effective policing, as it encourages community cooperation and deters criminal activity (Abril et al. 2023).

In summary, the introduction of new police squads can reduce crime by leveraging the careful selection of high-quality recruits, enhancing officer motivation through more attractive incentives, and implementing robust accountability measures to prevent misconduct. These factors together can create a policing environment that is more responsive and ultimately more successful in maintaining public safety.

Context and data

Context

We study the *Rondas e Ações Intensivas e Ostensivas* (RAIO) in Ceará, Brazil, a state of nearly 10 million inhabitants. In Brazil, state governments are the key providers of public security through two police institutions: the Military Police, which are the uniformed officers responsible for patrolling the streets and effecting arrests *in-flagrante delicto*, and the Civil Police, responsible for crime reporting and criminal investigation. Across all types of police institutions in Brazil (including federal police, federal police for highways, and police dedicated to the prison system), the Military Police is by far the largest, with about 400,000 sworn officers across 27 states. In Ceará, the Military Police numbered 22,427 total officers in 2023 (Fórum Brasileiro de Segurança Pública 2024). The Public Security Secretary and the Military Police Command of Ceará established the first rapid reaction police unit, now known as the *Batalhão de Rondas de Ações Intensivas e Ostensivas* (RAIO), in 2004. Created to be an elite squad within the military police, and starting with just 21 officers, its gradual expansion brought it to a total of 3,000 officers by 2023.

RAIO's primary objective is to conduct street patrols using heavy armaments and high-speed motorcycles. The use of motorcycles distinguishes RAIO from other highly militarized police wings such as BOPE, ROTA, and SWAT in Brazil and elsewhere that primarily use cars. The RAIO operate as a team, comprised of four individuals and three motorcycles, with one officer positioned in the passenger seat, equipped with a rifle.[9] By making a significant show of force, the theory goes, RAIO should diminish the need to resort to its use. Militarized patrols on motorcycles can potentially be more effective in densely populated urban environments, as they offer access to narrow corridors that may be impassable for larger vehicles (SSPDS-CE 2023). This feature is particularly valuable in informal settlements and *favelas* where the road network is either incomplete or non-existent. Furthermore, motorcycles allow for quick exits from dangerous situations, which may enhance officers' willingness to enter and patrol high-crime neighborhoods or blocks in the first place. Deployed as a complement to other police squads, RAIO is often used to saturate an area when there is an emergency, or when specific neighborhoods experience sharp increases in violence. On regular days, RAIO commanders analyze crime patterns and plan patrols given maps indicating where homicides and property crime are geographically concentrated. RAIO has its own commander that reports to the general commander of the Military Police.

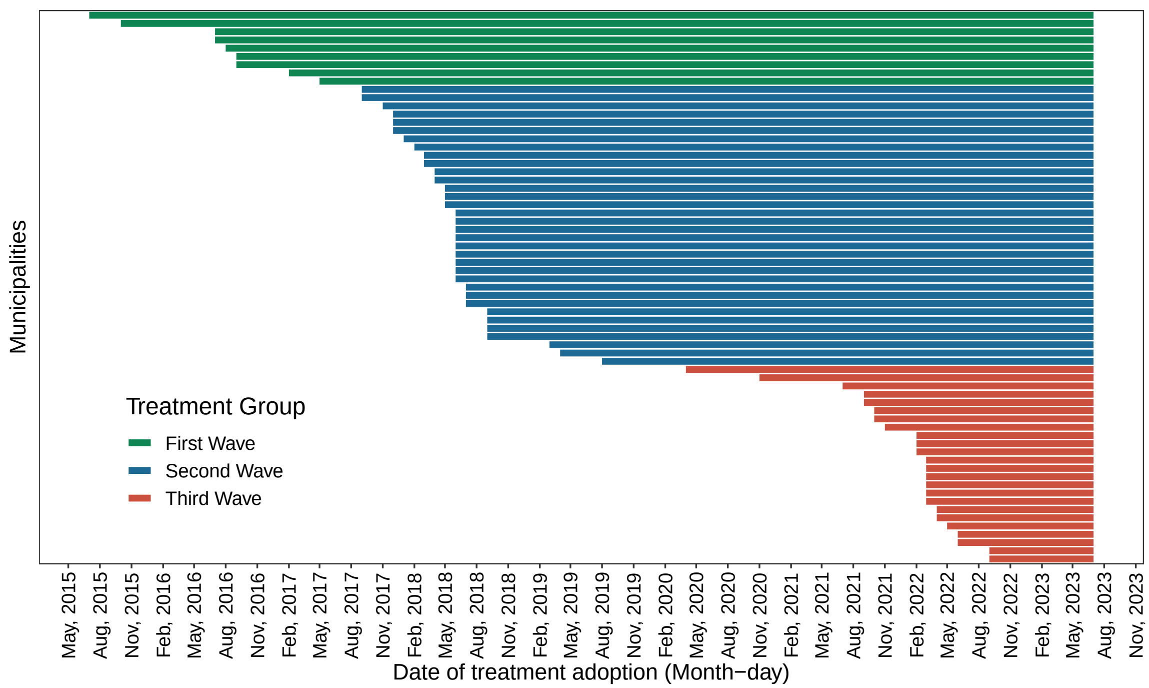
RAIO officers undergo a rigorous selection process, receive superior training, and enjoy better compensation when compared to their military police peers. Recruited exclusively from within the ranks of the military police, officers must maintain an impeccable record with the internal affairs office, as well as demonstrate exceptional physical fitness. Upon selection, they undergo a comprehensive 280-hour training program, during which they receive training in high-speed motorcycle operations and firearms proficiency. They receive a 30% higher salary when compared to other officers, and benefit from a more flexible schedule. (We discuss selection and incentives for RAIO officers in detail in the discussion section.)

While RAIO initially operated exclusively in Fortaleza, Ceará's capital the governor decided to expand the Battalion to other municipalities in Ceará in 2015. The first phase of the expansion encompassed the state's largest municipalities, including Juazeiro do Norte, Sobral, and Quixadá (9 bases in total). The second phase, which began in 2017, targeted remaining municipalities with populations exceeding 50,000 (34 bases). The third phase, initiated in 2020, focused on cities with populations ranging from 30,000 to 50,000 (24

[9] They use a carbine, a slightly shorter and less powerful weapon than a rifle. For ease of reference, we call this a rifle

bases), while a fourth phase was launched to include all municipalities with more than 25,000 inhabitants (18 bases). Once the program expansion ends, RAIO will be present in 66 of the state's 184 total municipalities (SSPDS-CE 2021). Figure 1 shows the temporal distribution of RAIO expansion, while Figure 2 illustrates the geographic distribution of treated cities in each roll-out phase.

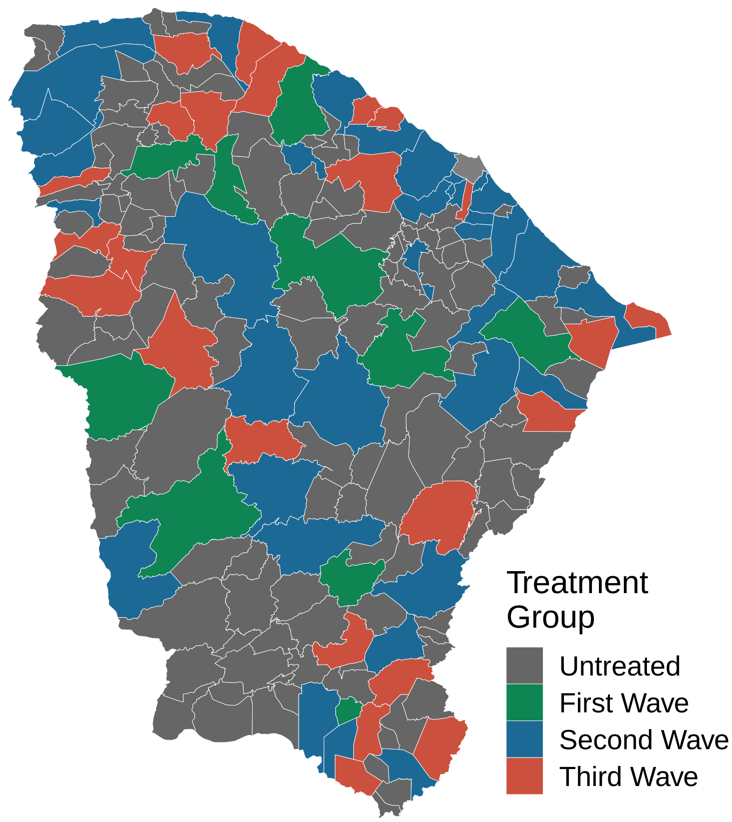
Figure 1: **Treatment adoption time per treated municipality**



The demand for rapid response patrolling in Ceará was driven by the security crisis facing the state in the mid-2010s. Homicide levels began to increase in 2010, reaching record levels of 77 per 100,000 inhabitants in Fortaleza in 2014, the highest rate among Brazilian state capitals. The state experienced high-profile violent events in this period (Silva 2024): in 2015, the state prosecutor's office indicted 45 policemen for killing 11 people, which became known as the Messejana massacre. In 2016, the city experienced a major prison crisis, which included gang members placing a car bomb in front of the state assembly to threaten legislators who were discussing a law that would have put mobile signal blockers around prisons in order to curtail the power of prison gangs.

Criminal dynamics also changed in 2016 when the drug faction "Guardians of the State" (GDE, given its initials in Portuguese) was created. The GDE began to compete for turf with Comando Vermelho, later allying itself with the Primeiro Comando da Capital (PCC) and therefore bringing the dispute between the two largest drug factions in Brazil to Ceará. Small groups of criminals gave way to sophisticated drug factions, which began to demarcate territories of control, actively recruit young people, and engage in violent

Figure 2: **Treatment rollout cycles**



disputes (Paiva 2019; 2022). Municipalities such as Caucaia and Maracanaú, contested by GDE and CV, exhibited homicide rates of approximately 100 per 100,000 people in 2017. Confronted with these public safety challenges, the governor faced substantial pressure to take action. RAIO expansion was one of the key security strategies he used to respond (Silva 2024).

Data

To study the effect of RAIO's expansion on crime within Ceará, we collect data from the state's 184 municipalities, provided to us by the Security Secretariat of Ceará (SSPDS-CE) and the Brazilian National Bureau of Statistics (IBGE). The SSPDS-CE provides detailed data on violent and property crime within Ceará, enabling us to examine the effect of the RAIO expansion on multiple crimes, including homicides, robbery, theft, sexual abuse, and drug possession. Additionally, the IBGE reports annual measures of socioeconomic variables at the municipality level, such as population size and gross domestic product, which serve as control variables.

For information on the phased roll-out of RAIO squads, we rely on data from the SSPDS-CE. We obtained a detailed calendar from the Secretariat, highlighting the start date for each RAIO base in Ceará. According to the Secretariat, decisions about where to expand RAIO bases were made according to municipal population size, rather than underlying crime patterns.

Table 1 presents the evolution of crime and socioeconomic indicators during RAIO's phased roll-out. Notably, Fortaleza experienced a significant decrease in violent deaths over this period. However, municipalities that received a RAIO base during phases one, two and three still exhibited alarmingly high levels of homicides and robberies, even though they were lower than those afflicting the state capital.

Table 1: **Descriptive statistics per phase**

		Descriptive variables									
		Fortaleza (N=1)		Phase 1 (N=9)		Phase 2 (N=34)		Phase 3 (N=24)		Control Group (N=116)	
		2015	2020	2015	2020	2015	2020	2015	2020	2015	2020
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Yearly estimates											
Homicides	Level	1,862	1,315	57.56	56.78	30.44	36.41	10.13	11.39	4.222	5.453
	Rate	71.86	49.26	44.62	38.93	36.69	38.12	28.47	32.23	23.79	32.33
Robberies	Level	30,139	25,159	588	492.6	381.4	293.5	109.0	77.3	30.04	21.99
	Rate	1,163.1	942.5	466.0	371.3	461.3	342.8	318.8	224.2	174.9	127.2
Formal Jobs	Level	9,884,088	9,052,320	212,992	203,373.33	127,950.71	123,334.24	32,886.26	31,601.22	13,721.231	12,778.564
	Rate	381,450	339,122	152,939.88	143,347.50	161,292.73	151,079.81	97,499.28	90,198.28	86,229.659	77,543.606
Population	Mean	2,591,188	2,669,342	11,8161.44	12,1316.67	74,576.26	76,638.85	35,153.73	35,888.26	16,255.452	16,573.634
Per capita GDP	Mean	22,079	24,411	9,818.99	13,340.19	11,824.62	17,377.09	7,969.41	12,474.80	6,530.001	9,447.122
Panel B. Monthly estimates											
Homicides	Level	155.2	109.6	4.796	4.731	2.537	3.034	0.810	0.949	0.351	0.454
	Rate	5.988	4.105	3.718	3.244	3.058	3.177	2.372	2.686	1.983	2.694
Robberies	Level	2,512	2,097	39.15	41.05	29.02	24.46	2.580	6.442	2.569	1.881
	Rate	96.93	78.54	38.83	30.94	38.45	28.57	26.57	18.69	14.96	10.88
Formal Jobs	Level	823,674	754,360	17,749.33	16,947.78	10,662.56	10,277.85	2,740.52	2,633.43	1,143.436	1,064.880
	Rate	31,788	28,260	12,744.99	11,945.63	13,441.06	12,589.98	8,124.94	7,516.52	7,185.805	6,461.967

Note: This table shows descriptive statistics for Fortaleza (state capital), municipalities treated in different phases of the RAIO expansion and those still lacking a RAIO batallion (control group). Homicides, robberies, and formal jobs are presented in level and annual rates per 100,000 inhabitants. Per capita GDP is the average per capita Gross Domestic Product, expressed in Brazilian Reais.

EMPIRICAL STRATEGY

We aim to identify changes in crime directly attributable to the implementation and rollout of the RAIO. In an ideal setting, we would randomly assign RAIO patrols to municipalities, producing exogenous changes in exposure, and then observe corresponding changes in crime. Because this was not possible, we leverage two features of the program to empirically evaluate its effect on crime and policing activities. We use its staggered rollout across 184 municipalities within Ceará to estimate a difference-in-differences model. In Appendix C we present an alternative identification strategy, an RDD, that exploits a population-based eligibility criterion to determine which municipalities received the program and when.

Given that RAIO bases were not randomly assigned to municipalities, naïve difference-in-means comparisons between treated and untreated municipalities will produce biased estimates of the program's impact on crime. We therefore use a difference-in-differences strategy that exploits the staggered rollout of RAIO bases across municipalities within Ceará (see Figure 1). Traditional two-way fixed effects (TWFE) models may produce biased average treatment effects on the treated (ATT) given staggered rollout (De Chaisemartin and d'Haultfoeuille 2020; Borusyak, Jaravel and Spiess 2021; Goodman-Bacon 2021; Sun and Abraham 2021; Callaway and Sant'Anna 2021; Athey and Imbens 2022).[10] We estimate our main results with Callaway and Sant'Anna (2021) (henceforth CS), providing robust and consistent estimates in the presence of differential treatment timing and heterogeneous effects by treated cohort.[11]

The CS estimator identifies the average treatment effect at time t for the group g when a municipality was first treated ($ATT_{dr}^{NYT}(g,t)$), using the *not-yet-treated* units as comparison units. The estimand of interest is defined as:

$$ATT_{dr}^{NYT}(g,t) = \mathbb{E}\left[\left(\frac{G_g}{\mathbb{E}[G_g]} - \frac{\frac{P_{g,t}(X)(1-D_t)(1-G_g)}{1-P_{g,t}(X)}}{\mathbb{E}\left[\frac{P_{g,t}(X)(1-D_t)(1-G_g)}{1-P_{gt}(X)}\right]}\right)(Y_t - Y_{g-1} - m_{g,t}^{NYT}(X))\right] \tag{1}$$

where g identifies treated units into different treatment cohorts, assigned according to the first time a unit enters the treatment group. CS estimates as many $t \times g$ ATTs as the data allows. The estimator in equation 1 identifies all ATTs per cohort g at times t using the *not-yet-treated* units as the control group. $P_{g,t}$ is the propensity score of being first treated on time t conditional on covariates X and either being treated in cohort g, ${}^{G_g}/_{\mathbb{E}[G_g]}$, or being *not-yet-treated* by time $(1-D_t)(1-G_g)$. In our case, G_g is an indicator variable equal to one if a municipality belongs to treatment cohort g, zero otherwise, and D_t is an indicator variable equal to one if the municipality is treated at time t, and zero otherwise. Y_t is the outcome of interest at time t, and Y_{g-1} the outcome at the time immediately before the first treatment period. Lastly, $m_{g,t}^{NYT}$ is the outcome

[10] When multiple treatment cohorts are considered with heterogeneous treatment effects per cohort, the TWFE model is a weighted average of all possible 2×2 difference-in-differences estimations in the sample (Goodman-Bacon 2021). Bias occurs with TWFE given that weights for valid comparison groups are contaminated by invalid comparison groups (e.g., treated units used as the control group).

[11] We use the doubly-robust CS estimator instead of other approaches (De Chaisemartin and d'Haultfoeuille 2020; Borusyak, Jaravel and Spiess 2021; Sun and Abraham 2021; Gardner 2022) for three reasons. First, it allows us to incorporate pre-treatment characteristics to comply with the conditional parallel trends assumption. Second, a doubly-robust estimator allows for more flexible modeling conditions, as it uses both outcome regression and inverse probability weighting (IPW). The CS doubly-robust estimator only requires that either the model for the outcome evolution of the comparison group or the propensity score model is correctly specified, but not necessarily both, to provide unbiased estimates of the treatment effect (Sant'Anna and Zhao 2020). Estimation with the doubly-robust estimator is therefore robust to misspecifications. Third, the doubly-robust approach allows us to estimate flexible variations of the ATT, including accommodating various treatment effect patterns across different subgroups or over time.

regression evolution for the *not-yet-treated* units from first treatment time g to time t conditional on covariates X.

The expression in equation 1 is used to highlight treatment effect heterogeneity across cohort g at different times t. Thus, $ATT_{dr}^{NYT}(g,t)$ is useful if the objective is to identify detailed causal effects for time t per treated cohort g, and across different treatment exposure $e = t - g$. In other situations, we may be interested in more aggregated causal parameters. CS allows for an event study aggregation of cohort $\times$ time ATTs, assigning appropriate weights to avoid the pitfalls of TWFEs. This aggregation is defined as:

$$\theta_{es}(e) = \sum_{g \in \mathcal{G}} \mathbb{1}\{g + e \leq \mathcal{T}\} P(G = g | G + e \leq \mathcal{T}) ATT(g, g + e) \tag{2}$$

Here the aggregated effect is a weighted average for all the $ATT_{dr}^{NYT}(g,t)$ effects. Therefore, $\theta_{es}(e)$, with $e = t - g$, is the average effect of participating in the treatment e periods before/after the treatment as adopted across all cohorts that are ever observed to participate in the treatment group for exactly e periods. The instantaneous treatment effect is represented by $e = 0$. Negative values of e represent pre-treatment periods and positive values post-treatment exposure effects of treatment.

To lend support to the parallel trends assumption, we test whether treated and control groups have different pre-treatment trends for our outcome. See Appendix A for a full discussion of identifying assumptions.[12]

RESULTS

Table 2 shows the average effect of the RAIO program on different types of crime, using the CS and TWFE estimators. We find evidence of decreases in monthly homicides, robberies, thefts, and arrests. These decreases are substantively large, significant at conventional levels, and robust to the specification of different control groups—both the not-yet-treated and never treated—when using the CS specification. These are economically important decreases: we find large reductions in homicides (1.3 monthly reports per municipality, or a 57.22% decrease when compared to not-yet treated units in the pre-treatment period), robberies (17.3 monthly reports per municipality, or an 84% decrease relative to not-yet treated units in the pre-treatment period), thefts (5.1 monthly reports per municipality) and arrests (3.11 monthly reports per municipality)

[12] Results are estimated using the `csdid2` command in Stata. For more information, please see `csdid2`. This command returns the same estimates as `csdid`, but is built into Mata, thereby increasing efficiency.

attributable to the RAIO rollout.

Table 2: **Effects of RAIO patrols on crime**

	Methods and estimands			Pre-treatment monthly averages		
	Callaway and Sant'Anna (2021)		TWFE			
Dependent Variable	**ATT (never treated)**	**ATT (not yet treated)**	**ATT**	**Complete Sample**	**Treatment**	**Control**
Homicides	-1.269***	-1.252***	-0.225	1.033	2.188	0.354
	(0.443)	(0.446)	(0.164)			
Police Killings	-0.078	-0.081	0.016	0.022	0.035	0.014
	(0.086)	(0.084)	(0.014)			
Robberies	-17.349***	-17.207***	-7.605***	7.982	20.599	0.584
	(5.07)	(5.08)	(1.303)			
Thefts	-5.173***	-5.061***	-3.007***	10.997	25.449	2.525
	(1.924)	(1.917)	(0.750)			
Bank Robberies	0.012	0.000	-0.002	0.027	0.026	0.028
	(0.034)	(0.034)	(0.005)			
Arrests	-3.113**	-2.989**	-2.813***	10.522	22.168	3.694
	(1.400)	(1.409)	(0.697)			
Guns Seized	-0.706	-0.678	0.602***	1.927	3.772	0.844
	(0.590)	(0.584)	(0.215)			
Drugs Seized	0.886	0.889	1.84	0.802	2.087	0.048
	(2.595)	(2.629)	(1.409)			
Domestic Violence	1.976	1.907	1.416***	3.466	7.691	0.989
	(1.906)	(1.879)	(0.415)			
Sexual offenses	0.293	0.291	0.115***	0.511	0.985	0.233
	(0.218)	(0.218)	(0.043)			
Observations	18.300	18.300	18.300			
Time F.E.	✓	✓	✓			
Municipality F.E.	✓	✓	✓			
Controls	✗	✗	✗			

Note: This table presents the aggregated average treatment effects on the treated. The first column shows the dependent variable used among different offenses reported by SSPDS-CE monthly per municipality. Our baseline sample considers 183 municipalities from January 2015 up to April 2023. Columns 1 and 2 show the ATT using the method proposed by Callaway and Sant'Anna (2021), using never treated and not yet treated municipalities as control groups, respectively, while Column 3 presents the standard Two Way Fixed Effects (TWFE) estimator. Standard errors in parenthesis are clustered via bootstrap at the municipality level. Significance levels are as follows: $p < 0.01$ ***; $p < 0.05$ **; $p < 0.10$ *.

We estimate how the effect of RAIO depends upon treatment exposure, using the weighted aggregation presented in equation 2, which takes the form of an event study. Estimates for pre-treatment periods allow us to assess the plausibility of the parallel trends assumption, while post-treatment periods provide evidence of the dynamic effect of the treated units compared to the control group. Figure 3 shows the effect of treatment exposure for homicides and robberies. We find no evidence for violations of the parallel trends assumption across any of our outcomes, with the exception of property crimes in a small number of pre-treatment periods years before program implementation.

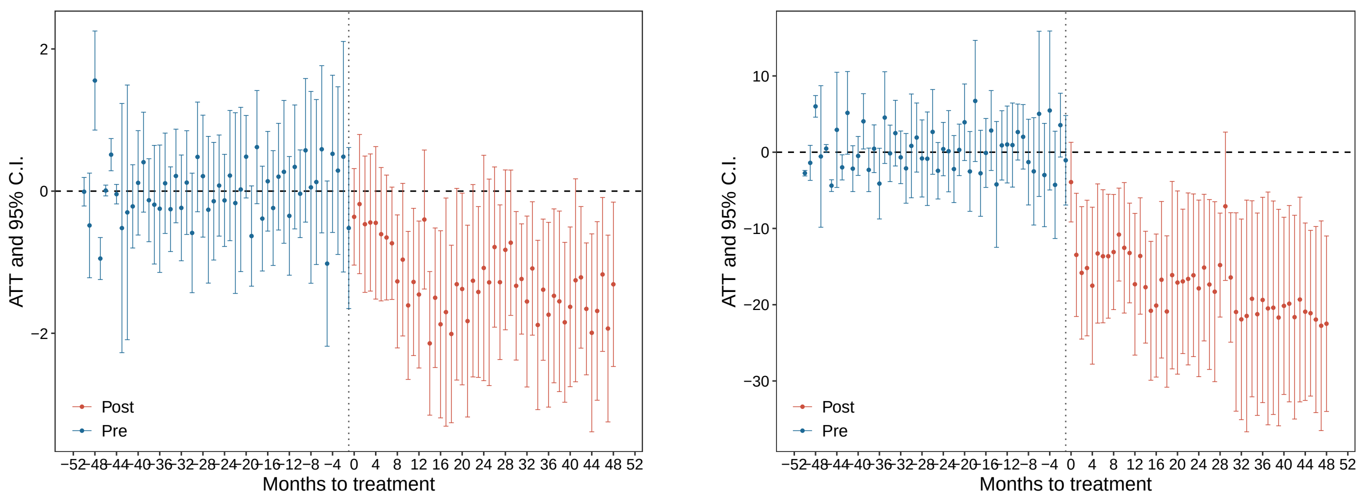
Figure 3a shows the dynamic effect of RAIO on homicides. We find a decrease in monthly reported homicides per municipality and an increasingly downward trend over time attributable to RAIO, with statistically significant reductions that extend beyond the first year of implementation. While there is no significant effect of RAIO patrolling on homicides overall during the first year of treatment (ATT = -0.438 and SE =

0.374 for $t = 13$), by the third year the program reduced monthly homicides per municipality by 1.891 (SE = 0.627 for $t = 36$), a sizable reduction. We likewise find evidence of a reduction in robberies immediately following the introduction of RAIO patrols. Figure 3b shows a marked reduction in robberies that is sustained for more than four years following the local rollout of RAIO.

Figure 3: **Effect of RAIO patrols on crime**

(a) Homicides

(b) Robberies



Note: This figure presents the average treatment effect on the treated by the length of exposure to treatment in (Callaway and Sant'Anna 2021). Table 2 shows the full set of the results upon which each figure is based. Period $t = -1$ represents one month prior to first RAIO deployment. Period $t = 1$ represents one month after first RAIO deployment. Thus, period $t = 0$ represents instantaneous treatment effect of RAIO on the treated municipality. In blue are the estimates on pre-treatment periods, and in red the estimates on post-treatment. Standard errors are clustered at the municipal level.

We explore how the ATT varies by the cycle within which municipalities were first introduced to treatment. For these estimates, we use a modified version of the estimator in equation 1, where we restrict our samples as follows. For the first cycle, we use information from January 2015 through June 2017. Here the not-yet-treated units and the never-treated units serve as the control group, while municipalities treated in the first cycle are our treatment group. For the second cycle, we use data from July 2017 to November 2019. For these models, never-treated municipalities and not-yet-treated municipalities serve as our control group, while treated municipalities in the second cycle become our treated group. Finally, for the third cycle, we use data from December 2019 to April 2023. For this third cycle group, never-treated and not-yet-treated municipalities serve as our control group, while treated municipalities in the third cycle serve as our treatment group. Finally, we aggregate this $ATT_{dr}^{NT}(g,t)$ estimates per cycle using the event study aggregation presented in equation 2. Table 3 presents the results.

Figure 4 presents the effect of RAIO on monthly homicides as a function of the number of months a municipality is exposed to RAIO patrols and its cycle of treatment. Panels 4b, 4c and 4d in Figure 4 present

Table 3: **Effects of RAIO patrols per cycle on crime**

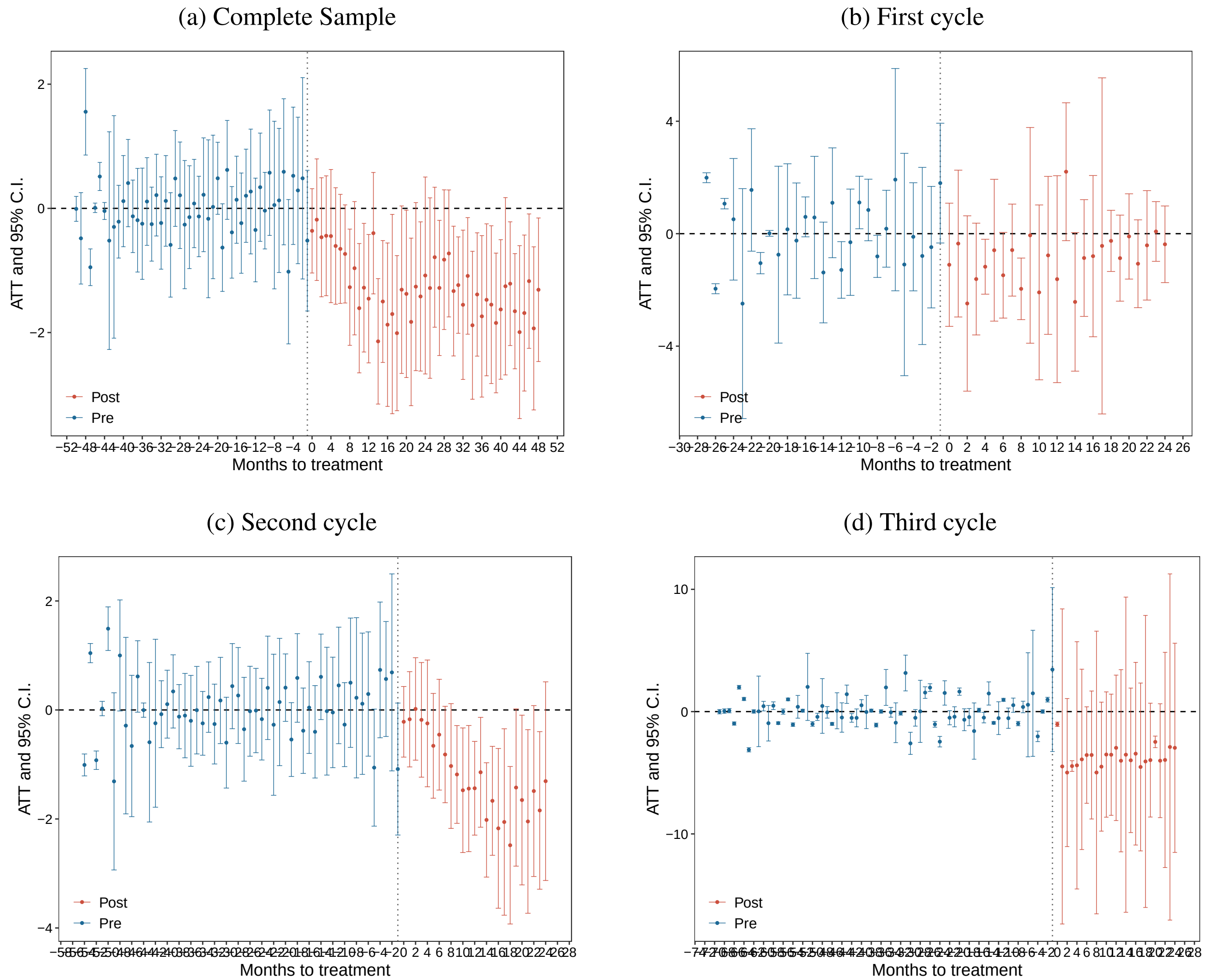
	Callaway and Sant'Anna (2021) estimates					
Dependent Variable	***ATT cycle 1***	***Mean cycle 1***	***ATT cycle 2***	***Mean cycle 2***	***ATT cycle 3***	***Mean cycle 3***
Homicides	-0.847	4.80	-1.223***	2.54	-3.746	0.81
	(0.733)		(0.453)		(2.739)	
Robberies	-10.980	39.15	-16.070***	29.02	-2.790*	2.58
	(6.863)		(5.024)		(1.682)	
Thefts	-2.370	49.00	-3.487**	31.78	-2.605	8.70
	(5.280)		(1.441)		(1.675)	
Bank Robberies	0.030	0.028	0.000	0.034	0.002	0.014
	(0.021)		(0.042)		(0.006)	
Arrests	-2.910	46.46	-0.659	25.81	-10.300*	8.83
	(4.334)		(1.283)		(6.005)	
Guns Seized	-0.539	7.04	-0.487	4.37	-2.523***	1.87
	(1.708)		(0.628)		(0.394)	
Drugs Seized	1.309	1.98	-2.979	3.17	1.426***	0.68
	(1.121)		(3.380)		(0.515)	
Domestic Violence	5.427	18.68	1.191	8.80	-0.401	2.32
	(6.152)		(0.917)		(3.459)	
Sexual offenses	0.565	1.57	0.131	1.21	0.547***	0.49
	(0.620)		(0.257)		(0.217)	
Observations	5.307		6.222		6.771	
Time F.E.	✓		✓		✓	
Municipality F.E.	✓		✓		✓	
Controls	✗		✗		✗	

Note: This table presents the aggregated average treatment effects on the treated by cycle in Callaway and Sant'Anna (2021). The first column shows the dependent variable used among different offenses reported by SSPDS-CE monthly per municipality. Our baseline sample considers 183 municipalities from January 2015 to April 2023, where cycles 1, 2, and 3 start in July 2015, September 2017, and April 2020, respectively. Columns 2, 4, and 6 report crime levels in the baseline period (2015). Standard errors are clustered via bootstrap at the municipality level. Significance levels are as follows: $p < 0.01$ ***; $p < 0.05$ **; $p < 0.10$ *.

estimates for the first, second, and third cycles, respectively, using the event study aggregation in equation 2. We observe differing effects of RAIO on monthly homicides across the different treatment cycles. While the first and third cycles do not seem to produce significant reductions in homicides—estimates for the first and third cycles show an ATT of -0.847 (SE=0.733) and -3.746 (SE=2.739), respectively—municipalities treated in the second cycle *do* experience fewer homicides, which corresponds to an estimated ATT of -1.223 (SE=0.453), or a reduction of 48.15% from the treatment baseline (mean = 2.54). Table 3 also indicates that the reduction for robberies and thefts are concentrated in the second cycle.

Finally, in Figure 5 we study two sets of core police activities—arrests and gun seizures—to better understand why RAIO may have reduced homicides and robberies. We find an immediate increase in arrests, which disappears after the first two months. The medium-term effects on arrests suggest a *decrease* of approximately three *in flagrante* arrests per month attributable to RAIO. This means that the crime reduction effects we identify above are not driven by the incapacitation of criminals. In a similar vein, we find no consistent increase in gun seizures in panel (d), meaning that criminals likely are not being prevented from

Figure 4: **Heterogeneous treatment effects on homicides by RAIO roll-out cycle**



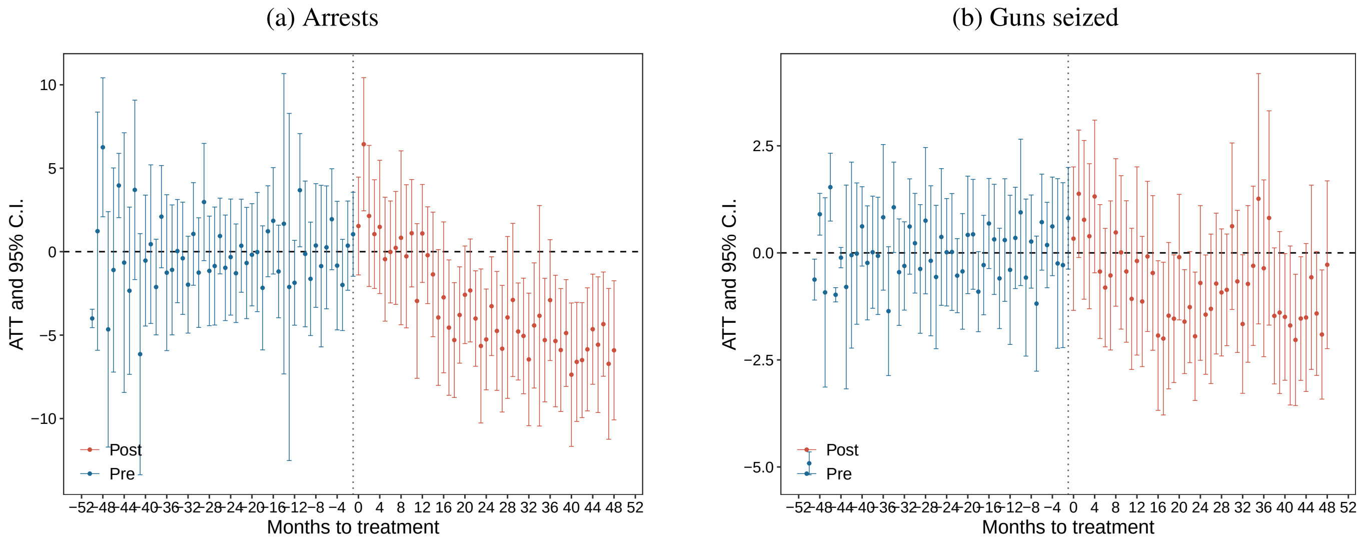
Note: This figure presents the average treatment effect on the treated by the length of exposure to treatment in Callaway and Sant'Anna (2021). Table 3 shows the full set of the results upon which each figure is based. Period $t = -1$ represents one month prior to first RAIO deployment. Period $t = 1$ represents one month after first RAIO deployment. Thus, period $t = 0$ represents instantaneous treatment effect of RAIO on the treated municipality. In blue are the estimates on pre-treatment periods, and in red the estimates on post-treatment. Standard errors are clustered at the municipal level.

committing crimes due to a scarcity of firearms. Taken together, these findings suggest that RAIO's crime reduction effects likely operate via deterrence of crime. We return to this possibility when discussing the survey results below.

WHAT DIFFERENTIATES RAIO FROM OTHER POLICE FORCES?

Our results indicate that RAIO is effective in reducing crime and violence, but what differentiates the RAIO from other police forces? One explanation centers on *equipment*: qualitative interviews with the police suggest that RAIO's high-powered motorcycles may provide an important advantage, while the use of high-caliber rifles may be especially effective at deterring criminals. At the same time, RAIO officers may simply

Figure 5: **Effect of RAIO patrols on police operations**



Note: This figure presents the average treatment effect on the treated by the length of exposure to treatment in Callaway and Sant'Anna (2021). Table 2 shows the full set of the results upon which each figure is based. Period $t = -1$ represents one month prior to first RAIO deployment. Period $t = 1$ represents one month after first RAIO deployment. Thus, period $t = 0$ represents instantaneous treatment effect of RAIO on the treated municipality. In blue are the estimates on pre-treatment periods, and in red the estimates on post-treatment. Standard errors are clustered at the municipal level.

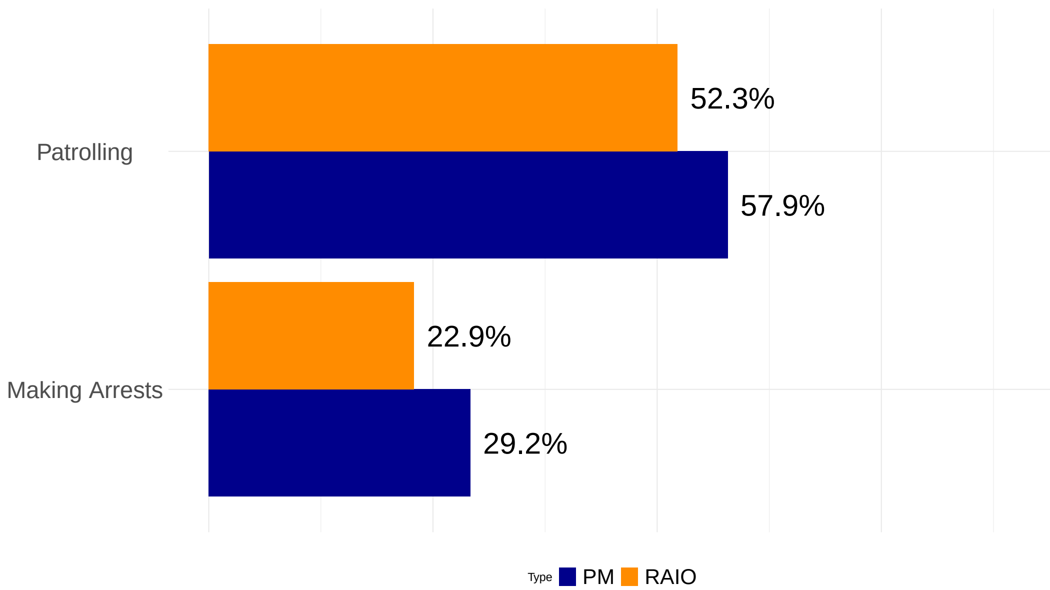
be better trained, more highly-motivated, and more engaged in their duties than their non-RAIO peers.

To shed light on what makes the RAIO different from other police forces, we administered a 2,000 person resident survey in Fortaleza between October and December 2023. We asked questions about how people perceived the RAIO in comparison with the ordinary military police. We also included an embedded survey experiment to test which RAIO attributes—the motorcycle, rifle, or uniform—were most valued by residents in shaping their perceptions about the RAIO. (Note that Fortaleza is not included in the sample on which we estimate RAIO's effect on crime and policing activities.) We discuss survey methodology and our sampling strategy in Appendix E..

We begin asking about the extent to which respondents are exposed to RAIO and their ordinary military police counterparts, given that any positive perceptions of RAIO may be a function of limited contact. Figure 6 shows that while residents are more likely to have seen the military police patrolling (57.9%) when compared to the RAIO (52.5%) in the prior month, these differences are not especially large. The same is true for making arrests (28.2% for the military police, versus 22.9% for RAIO).

Next we gauge the extent to which the RAIO benefit from improved perceptions relative to the military police and, if so, why. We ask respondents the extent to which they agreed with particular statements about each policing institution, including whether they: use excessive force; are well-prepared to deter crimes; are more respected by the population; and are corrupt. These statements are measured on a Likert scale from

Figure 6: **Frequency of residents' exposure to RAIO and ordinary military police**



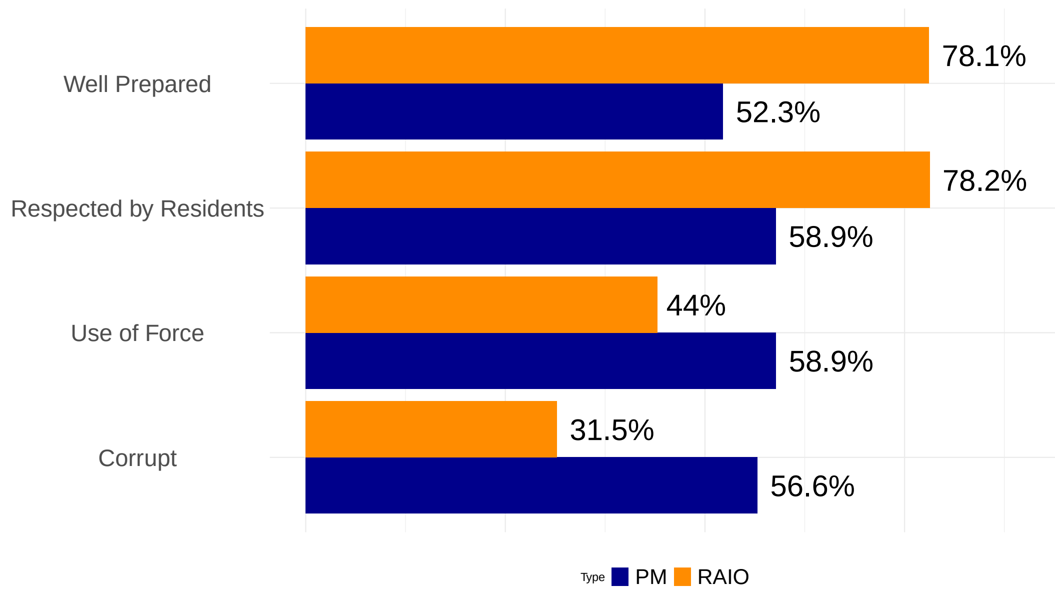
Note: Frequency is the percent of respondents reporting that they have seen each police force performing the given action either "frequently" or "very frequently" in the last month.

1-4 (from "not at all" to "completely"). Figure 7 shows that RAIO forces are perceived more effectively and positively across each of these four measures: RAIO police officers are seen as less likely to use excessive force, are more prepared to deter crimes, are perceived as being more respectful of residents, and are less likely to be corrupt. When asking specifically about the extent to which respondents believed that the RAIO / military police were effective in reducing the activities of criminal groups, 89% of respondents responded they were "partially" or "totally" in agreement with respect to the RAIO, while that number dropped to 72% for the military police.

Conjoint survey

To better identify what drives positive perceptions of the RAIO when compared to attitudes towards the military police, we include a conjoint experiment inspired by Flores-Macías and Zarkin (2021) and Blair, Mendoza Mora and Weintraub (forthcoming). The conjoint instructed participants to directly compare the RAIO and military police, rather than assessing them independently. Respondents saw three sets of randomly-selected images consecutively, each featuring either a RAIO or an ordinary military police officer carrying a rifle or a pistol, and either standing next to a motorcycle or next to no vehicle at all, as shown in sample images in Figure 8. Note that the individuals themselves are identical to one another, but we randomly vary

Figure 7: **Comparative evaluation of RAIO and ordinary military police**



Note: Frequency is the percent of respondents reporting that they are either partially or completely in agreement with statements.

their uniform, the gun they are holding, and whether they are standing next to a motorcycle (or no vehicle at all).

Figure 8: **Conjoint survey images**

The conjoint allows us to assess whether respondents' attitudes are driven by a preference for the RAIO (if so, residents will prefer the RAIO uniform regardless of the weapon shown), a preference for militarization (if so, residents will prefer the rifle over the pistol, regardless of who is holding it), a preference for rapid response (if so, residents will prefer the motorcycle rather than no vehicle), or a combination of all these factors.

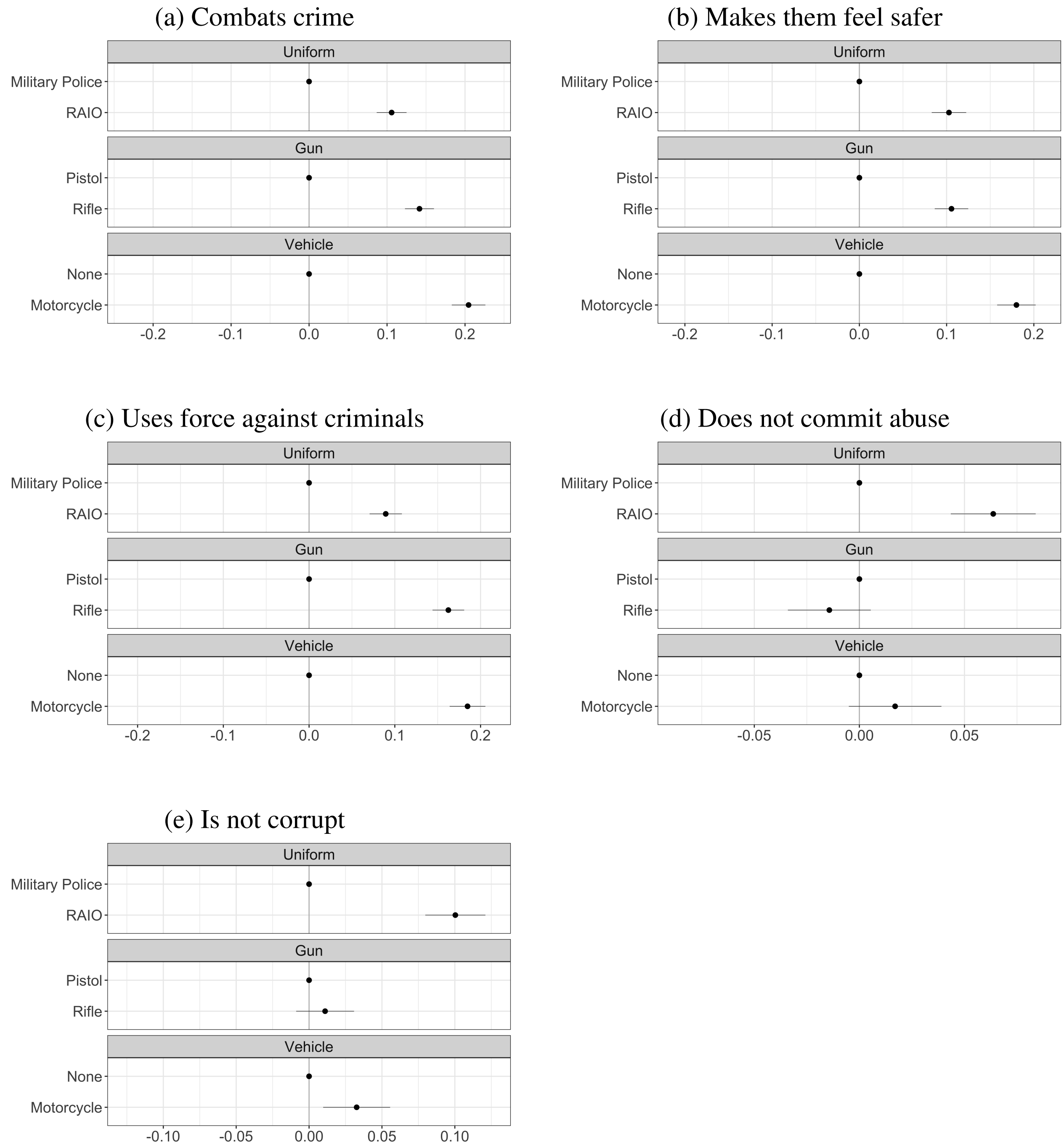
We asked each respondent to indicate—for each pair of images—which individual that they were shown would: (1) be more capable of combating crime; (2) make them feel safer; (3) commit abuses; (4) engage in corruption; (5) use force against criminals. In line with our pre-registered hypotheses, we report results for each individual item. The first and second items capture residents' perceptions of competence for both ordinary military police and the RAIO: how capable is each at performing two basic functions of policing? The third and fourth items capture residents' perceptions of just treatment, frequently associated with procedural justice: whether they abuse civilians and are corrupt. The final item is a bit less clear whether using force against criminals is in line with "ideal" policing practices. Our principal quantity of interest for the conjoint analysis is the Average Marginal Component Effect (AMCE). To calculate the AMCE, each outcome is regressed on several indicator variables, with each indicator representing an attribute level, and one level per attribute constituting the baseline (Hainmueller, Hopkins and Yamamoto 2014). The AMCE indicates the average causal effect of a characteristic on a police officer's evaluation among respondents when compared to the baseline attribute (Bansak et al. 2023). We cluster standard errors by respondent.

Figure 9 presents the main results from the conjoint survey. The dots with horizontal lines represent the level-specific AMCEs and 95% confidence intervals. In line with our pre-registered hypotheses, we find that civilian attitudes towards policing forces are shaped by institutional identity (what uniform respondents are shown), firepower (what gun they are shown), and perceived agility and maneuverability of the forces (whether a motorcycle is shown or not).

Overall, the RAIO uniform causes more positive assessments of effectiveness (panels a, b, and c). Compared to the ordinary military police uniform, the RAIO uniform increases the probability of a law enforcement agent being characterized as more effective at combating crime by 10.6 percentage points (95% CI = 8.67, 12.5). The motorcycle increases perceptions of crime fighting effectiveness by 20.5 percentage points (95% CI = 18.3, 22.6), as does the rifle, by 14.2 percentage points (95% CI = 12.3, 16). Likewise, compared to the ordinary military police uniform, the RAIO uniform increases the probability of feeling safe by 10.3 percentage points (95% CI = 8.29, 12.2). The rifle and motorcycle do the same, by similar magnitudes. The RAIO uniform increases the chance that citizens believe a law enforcement agent will use force more against criminals relative to the military police uniform baseline, as do the rifle and motorcycle.

In terms of perceptions of just treatment (panels d and e), the RAIO uniform increases the probability

Figure 9: **Average Marginal Component Effects (AMCE)**



Note: This figure shows the Average Marginal Component Effects (AMCE) for the conjoint experiment with 2.000 citizens of Ceará. Tables F.1, F.2, F.3, F.4 and F.5 reports the full set of estimates for each of these plots. All estimates are based on the conditional logit model.

that a law enforcement agent is characterized as less likely to commit abuse by 6.37 percentage points (95% CI = 4.35, 8.39), and less likely to be corrupt by 10 percentage points (95% CI = 7.97, 12.1). Here we see important differences with the rifle and motorcycle: having been shown the rifle does not improve perceptions about just and non-corrupt treatment, while the motorcycle only increases perceptions that a respondent judges that the person in the image is not corrupt. Regardless, these effects are much smaller than the positive effects that the RAIO uniform produces.

Subgroup analyses using marginal means to determine show that there are few systematic differences in appraisals of RAIO across sociodemographic groups (by age and gender); exposure to criminal groups; exposure to criminal governance; and self-reported exposure to violence.[13] indicating broad-based preferences for RAIO.

Discussion

A number of puzzles and related questions emerge from our empirical results. First, how was Ceará able to create a police squad that is perceived so competently, given that most police forces suffer from serious crises of legitimacy? Second, if new policing squads reduce crime, does this translate into positive electoral returns for politicians who implement them? Third, were the costs of deploying RAIO compensated by the social benefits associated with crime reductions? Finally, did crime reduction come at the expense of increased human rights abuses?

Selection, training and motivation

Our conjoint survey results indicate that residents of Fortaleza see the RAIO as a more competent and fair police force, not simply one that threatens them with force. Detailed information on how the police selects, trains and controls RAIO officers suggests that Ceará's military police was successful in creating a subgroup of better and more highly-motivated police officers. In this section we discuss qualitative data that supports mutually-reinforcing mechanisms of selection, training, and motivation to explain why RAIO officers may be views so much more positively than other police forces in the region.

RAIO members are selected from the ranks of military police officers. The military police opens a selection process with a limited number of vacancies and allows any officer with more than one year on the force to apply. The selection process begins with a physical exam, followed by a driver's test to determine whether the officer can drive a motorcycle. This is followed by studying data from the military police's internal affairs department regarding misconduct and citizen complaints about each officer; examining mental

[13]Results available upon request.

health records that attest to an officer's suitability for the position; and making contact with an applicant's former colleagues and superiors to learn more about the candidate's behavior and commitment to his professional duties. The names of these applicants are then published, which allows *any* police officer to tip off the selection committee about previous instances of malfeasance that officers may have exhibited, even if such instances went unregistered at the time. The final phase is a six-week training course on motorcycle driving, firearms, personal defense, and doctrine.

Once an officer begins to work for RAIO, he receives a 30% wage increase and benefits from a more flexible schedule compared to what he received within the military police: two consecutive days of 8-hour shifts, followed by two days off. The "ordinary" Military Police work schedule is 44 hours per week, that can be split in different shift combinations such as one that works 12 hours and rest for 36 hours or another that works for 12 hours, rest 24 hours, work for additional 24 hours and rest 36. RAIO also provides performance-based incentives based on a dashboard that tracks each officer's activity. Performance is ranked according to the number of guns and drugs seized: one gun seized, for example, provides an officer with a day off. Commanding officers also provide non-pecuniary rewards, including medals for outstanding performance. Finally, the Criminal Analysis Office of the Secretary of Security recognized that RAIO officers are among those who most frequently request and analyze data on crime patterns where they serve, with an eye on continual improvement.

Political returns to creating a new policing squad

The roll-out of successful public safety programs—those that reduce crime and improve citizens' perceptions of safety—may generate positive electoral consequences for incumbents (Holland 2013). Our setting is particularly suitable to test the electoral gains argument given that the RAIO program was expanded by a state governor who was elected in 2014 and then ran for reelection in 2018. Qualitative interviews suggest that electoral concerns were likely important drivers of the RAIO expansion. Indeed, in line with predictions about "political business cycles" related to the provision of public safety and the assignment of police officers (Levitt 1997; Guillamón, Bastida and Benito 2013), Figure 1 indicates that 2018—the year when the incumbent governor was running for re-election—was the year with the highest number of RAIO base inaugurations. In that year, the police also trained more than 1,000 police officers for RAIO, more than three times the average for other years.

Using official electoral data, Table 4 presents a two period difference-in-differences analysis, where the treatment variable equals one for those municipalities that experienced the roll-out of the RAIO between 2014 and 2018, and zero otherwise. In these analyses we evaluate the impact of RAIO on differences in vote shares for the incumbent state governor, Camilo Sobreira de Santana, in 2014 and 2018 (models 1 and 2), and total votes cast for him in 2014 and 2018 (models 3 and 4). While all models include municipality fixed effects, models 1 and 3 do not include baseline crime rates, while models 2 and 4 do so.

Table 4: **Average treatment effects of RAIO on voting**

	DV: Incumbent voting results			
	Share of Votes		Total Votes	
	(1)	(2)	(3)	(4)
RAIO: Treatment	0.1897***	0.0781**	9,868.0***	4,213.5***
	(0.0130)	(0.0371)	(890.2)	(713.5)
Observations	366	366	366	366
R2	0.34160	0.46718	0.9755	0.99058
FE: Municipality	✓	✓	✓	✓
Controls: pre-treatment crime rates	✗	✓	✗	✓
Pre-treatment DV control mean	56.7%	56.7%	15,965	15,965

Note: Results presented in the table correspond to the municipality level. In columns 1 and 2 we present estimates for the share of votes for Camilo Sobreira de Santana as the dependent variable in each municipality. Columns 3 and 4 present estimates for the total number of votes for Camilo Sobreira de Santana as the dependent variable in each municipality. In Table D.1 we report the estimates for all control variables used in the regressions. Standard errors are clustered at the municipality level. Significance levels are as follows: 1%, ***; 5%, **; 10%, *.

Estimates show that the RAIO expansion had electoral benefits for Ceará's incumbent state governor. Column 2 in Table 4 shows that, on average, his share of votes increased by 7.81 percentage points in treated municipalities versus control municipalities, an increase of 13.7% compared to the sample mean.[14] Columns 3 and 4 in Table 4 show that RAIO's expansion was associated with an increase in total votes for the incumbent. Column 4 shows that total votes increased by 4,213 compared to control municipalities, an increase of 26.39% compared to the sample mean. These results demonstrate that the creation of the new RAIO policing squad afforded the incumbent governor tangible benefits at the ballot box.

COST-BENEFIT ANALYSIS

We implement a simple cost-benefit analysis to monetize the social benefits of crime reductions attributable to RAIO (Jaitman et al. 2017), weighed against the operational costs of implementing the program. The costs of crime and violence can be conceptualized as the difference in well-being that could be achieved in a hypothetical scenario without crime and current well-being (Perez-Vincent et al. 2024).

[14] Figure D.1 shows the raw distribution of electoral results: the mean winning vote share in each municipality shifts from around 55% in 2014 to 75% in 2018, consistent with an increase in total votes received for the incumbent.

We use a detailed breakdown of RAIO expenses to calculate program costs per municipality year. Annualized costs are approximately US$38,000 per year per municipality over the four and a half year time horizon we study in this paper.[15] The results presented in a prior section demonstrated that the introduction of a RAIO squad led to a reduction of 17.3 robberies and 1.3 homicides per month per municipality, equivalent to 207.6 robberies and 15.6 homicides reduce each year per municipality. Using these figures, the average cost per robbery deterred is approximately US$183, while the average cost per homicide reduced is US$2,436. How do these costs compare to the social costs of crime?

While we lack specific data regarding the costs of crime for Brazil, we use regional benchmarks for Latin America and the Caribbean (Perez-Vincent et al. 2024). Crime and violence in the region account for an estimated 3.44% of GDP annually, or US$172 billion.[16] Robberies, which account for roughly 70% of all reported crimes, are responsible for an estimated US$120.4 billion of these annual costs. Dividing this total by the approximate 10 million robberies committed annually yields an average cost of US$12,040 per robbery, an order of magnitude greater than what it costs for RAIO to deter a robbery (US$183). Homicides, while thankfully less frequent, account for around 0.5% of the region's GDP, or US$25 billion annually. With an estimated 140,000 homicides per year in the region, the average cost per homicide is approximately US$178,570. The economic cost of a single homicide is, therefore, nearly 74 times greater than the cost of deterring that homicide with the RAIO program (US$2,436).

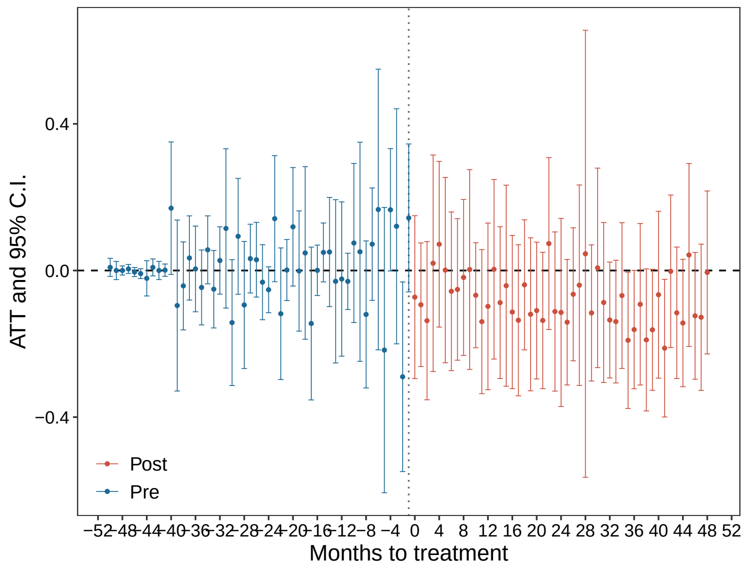
Effects on human rights abuses

If RAIO's success in reducing crime were to come at the expense of increased human rights abuses, we may have overstated its social benefits. In Figure 10 we assess whether the deployment of RAIO increased police killings, perhaps the most reliable way to measure human rights abuses. We find that it did not. Estimating the same difference-in-differences models, but using aggregated police killings as our dependent variable and not-yet-treated municipalities as our control group, we uncover no evidence that RAIO deployments increased killings of residents by the police.

[15] Our calculations involve the following assumptions. Each base has 24 officers and 12 motorcycles, on average. Only 50% of those motorcycles are active at a given time. We assume a 10-year planning horizon for vehicle replacement. Personnel costs, including retirement benefits, are distributed evenly over the 4.5 years, while motorcycle costs are annualized based on their estimated life cycle. Office expenses are treated as fixed annual costs. We use a discount rate of 11.25% for long-term costs estimation and an exchange rate of 6.29 BRL to 1 USD.

[16] This includes direct costs such as lost goods, damages, and productivity losses, as well as public sector spending on policing and justice, and broader societal impacts. See Perez-Vincent et al. (2024).

Figure 10: **Effect of RAIO patrols on police killings**



Note: This figure presents the average treatment effect of RAIO on policing killings using the Callaway and Sant'Anna (2021) estimation technique. Table 2 shows the full set of the results upon which each figure is based. Period $t = -1$ represents one month prior to first RAIO deployment. Period $t = 1$ represents one month after first RAIO deployment. Thus, period $t = 0$ represents instantaneous treatment effect of RAIO on the treated municipality. In blue are the estimates on pre-treatment periods, and in red the estimates on post-treatment. Standard errors are clustered at the municipal level.

Conclusion

Policymakers pursuing institutional reforms face a persistent dilemma: long-term change is often at odds with public demands for immediate results (Bayley 2001). Under intense pressure to do *something* in violent contexts, governments frequently resort to hardline security strategies, despite their limited impact on crime (Flores-Macías 2018; Blair and Weintraub 2023). In this paper, we study an alternative approach: establishing parallel police units that may help circumvent bureaucratic constraints, ensure stringent training requirements, and allow for new accountability measures to limit police abuse and corruption.

We evaluate the deployment of the RAIO, a motorcycle-based policing squad, across municipalities within a large Brazilian state. Officers of the RAIO are better-compensated and better trained than their ordinary military police counterparts. Our core difference-in-differences identification strategy and an auxiliary regression discontinuity design analysis demonstrate that RAIO produced important reductions in crime. Given that we find no sustained increases in arrests attributable to the RAIO, we believe that the crime reduction effects are likely due to improved deterrence. Our resident survey from Ceará's capital, Fortaleza, includes embedded survey experiments showing that the RAIO is not only perceived as more likely to combat crime and use force against criminals, but is also seen as more respectful of human rights and less corrupt than its ordinary military police counterpart.

While we are unable to separate out the effects of selection from those stemming from superior training or enhanced incentives, this paper suggests the need to rethink some core organizational issues within police forces suffering from legitimacy crises. As with other public officials, underpaid police officers may be more vulnerable to corruption (Van Rijckeghem and Weder 2001; Azfar and Nelson 2007; Klockars 2000). Poorly-trained officers may be unable to handle high-stress situations, and may more quickly resort to violence against citizens (Stoughton 2014). This paper suggests that police departments in violent contexts might experiment with new ways to motivate their officers in order to achieve both crime reduction and respect for human rights. Future studies should seek to isolate the causal effects of these individual components, to better understand the optimal mix of policies—at the recruitment, retention and termination phases—to maximize organizational efficiency, responsibly allocate public funds, and enhance citizen security.

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Appendix

A. Identifying assumptions for Callaway and Sant'Anna (2021) estimator

The Callaway and Sant'Anna (2021) estimators in equation 1 and equation 2 produce unbiased effects of RAIO on crime if the four following assumptions hold:

1. *Overlapping assumption*: This assumption states that a positive fraction of municipalities start treatment in period g, and that for all subsequent periods t, the propensity score is uniformly bounded away from one. In our sample, the estimated propensity score is bounded away from one for all observations.

2. *Treatment irreversibility*: This assumption holds that once a unit experiences treatment, it remains treated. Once the RAIO program starts in any treated municipality, it remains for all subsequent periods.

3. *Limited or no anticipation*: This assumptions restricts anticipation of the treatment to all treatment cohorts g. In other words, municipalities should not anticipate their treatment status δ periods prior to first being treated.

4. *Conditional parallel trends*: This assumption states that conditional on covariates, X, and in the absence of treatment, average outcomes for the group first treated in period g and the "not-yet-treated" group given by time $t+\delta$, where $\delta \geq 0$ is the anticipation horizon, would have followed parallel trajectories. More formally, the conditional parallel trends assumption states that for each g in G and $t \in \{2, \dots, T\}$ such that $t \geq g - \delta$,

$$[Y_t(0) - Y_{t-1})(0)|X, G_g = 1] = [Y_t(0) - Y_{t-1}(0)|X, C = 1]$$

   where G_g is an dummy variable equal to 1 if a municipality belongs to the group that was first treated in period g, and C is a dummy variable equal to 1 for "not-yet-treated" municipalities. Moreover, the conditional parallel trends assumption using the "not-yet-treated" group as the control group assumes a large enough "not-yet-treated" group is available in the data, and that units are "similar enough" to eventually treated units, such that they can be used as a valid comparison group. In our setting, we have a large "not-yet-treated" group available. A conventional practice to lend support to the parallel trends assumption is to test whether the treated and control groups have different pre-treatment effects on the outcome, visually demonstrating that had the treatment not been assigned, ATT(g, t) in post-treatment periods would not reject the null hypothesis, and then to do the same for pre-treatment periods.

B. Difference-in-differences additional estimates

B.1. Results using crime rates

In this section we provide robustness checks on our main DiD specification using crime rates as dependent variables.

Table B.1: Average treatment effects of the RAIO patrols - Crime Rates

	Methods and estimands			**Pre-treatment monthly averages**		
	Callaway and Sant'Anna (2021)		**TWFE**			
Dependent Variable	**ATT (never treated)**	**ATT (not yet treated)**	**ATT**	**Complete Sample**	**Treatment**	**Control**
Homicides	-1.126***	-1.061***	-0.781***	3.011	3.377	2.165
	(0.399)	(0.388)	0.202)			
Police Killings	0.024	0.009	-0.027	0.064	0.054	0.087
	(0.116)	(0.112)	(0.026)			
Robberies	-18.606***	-18.145***	-10.100***	23.280	31.780	3.567
	(4.290)	(4.235)	(1.301)			
Thefts	-4.114**	-3.936**	-2.915***	32.070	39.260	15.400
	(1.803)	(1.812)	(0.986)			
Bank Robberies	0.057	0.044	0.067***	0.079	0.039	0.170
	(0.075)	(0.076)	(0.015)			
Arrests	-0.721	-0.654	-0.598	30.680	34.20	22.53
	(1.870)	(1.912)	(0.805)			
Guns Seized	-0.580	-0.501	0.809*	5.619	5.821	5.148
	(0.859)	(0.852)	(0.415)			
Drugs Seized	-0.287	0.137	0.875	2.337	3.220	0.291
	(3.583)	(3.313)	(2.144)			
Domestic Violence	1.319	1.309	0.434	10.110	11.868	6.033
	(1.094)	(1.016)	(0.523)			
Sexual offenses	0.405	0.376	0.140	1.491	1.522	1.420
	(0.366)	(0.365)	(0.092)			
Observations	18.300	18.300	18.300			
Time F.E.	✓	✓	✓			
Municipality F.E.	✓	✓	✓			
Controls	✗	✗	✗			

Note: This table presents the aggregated average treatment effects on the treated. The first column shows the dependent variable used among different offenses reported per 100 thousand inhabithants by SSPDS-CE monthly per municipality. Our baseline sample considers 183 municipalities from January 2015 up to April 2023. Columns 1 and 2 show the ATT using the method proposed by Callaway and Sant'Anna (2021), using never treated and not yet treated municipalities as control group respectively, while Column 3 presents the standard Two Ways Fixed Effects estimator. Standard errors in parenthesis are clustered via bootstrap at the municipality level. Significance levels are as follows: 1%, ***; 5%, **; 10%, *.

B.2. Testing for Spatial Spillovers

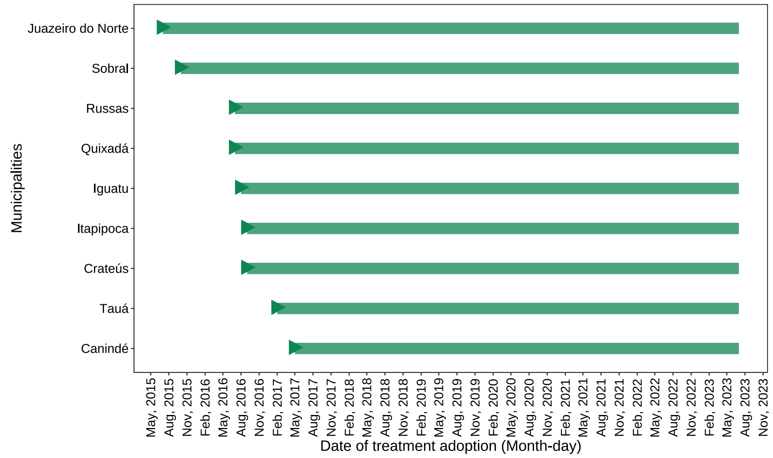
Table B.2: Average treatment effects of the RAIO patrols program on crime - testing spatial spillovers

	Post-treatment	
	Callaway and Sant'Anna (2021)	
Dependent Variable	**ATT Cycle 2 (full sample)**	**ATT Cycle 2 (w/o neighbors)**
Homicides	-1.7369	-1.7477
	(1.0857)	(1.0446)
Police Killings	-0.2062	-0.2084
	(0.1902)	(0.1991)
Robberies	-20.693**	-20.772**
	(8.923)	(9.0717)
Thefts	-6.707***	-6.984***
	(2.5679)	(2.5808)
Bank Robberies	0.0027	-0.009
	(0.032)	(0.0329)
Arrests	-3.770**	-4.001**
	(1.6511)	(1.6474)
Guns Seized	-0.9064	-0.884
	(0.8842)	(0.8979)
Drugs Seized	-7.4181	-6.8609
	(7.7576)	(7.8956)
Domestic Violence	-0.8331	-0.7501
	(2.5742)	(2.4238)
Sexual offenses	0.1987	0.237
	(0.2624)	(0.2983)
Observations	5.916	3.026
Time F.E.	✓	✓
Municipality F.E.	✓	✓
Controls	✗	✗

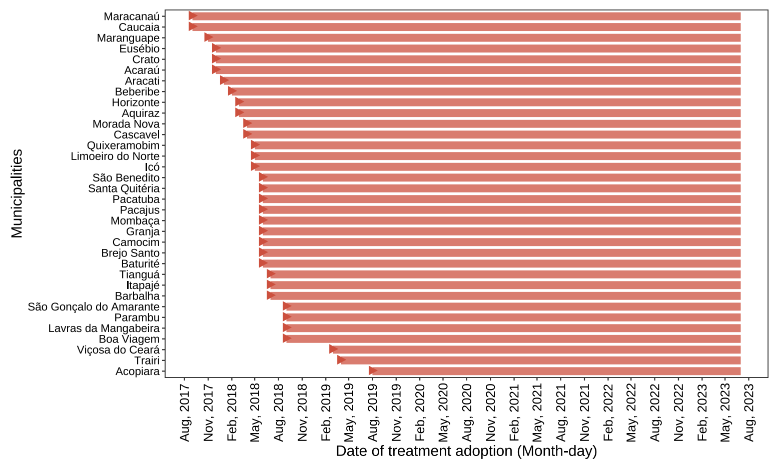
Note: This table presents the aggregated average treatment effects on the treated. The first column shows the dependent variable used among different offenses reported by SSPDS-CE monthly per municipality. For the Cycle 2, our sample considers 174 municipalities from September 2017 to March 2020. After excluding neighboring municipalities, our sample was reduced to 89 municipalities. Standard errors are clustered via bootstrap at the municipality level. Significance levels are as follows: 1%, ***; 5%, **; 10%, *.

Figure B.1: Staggered treatment adoption across treatment waves

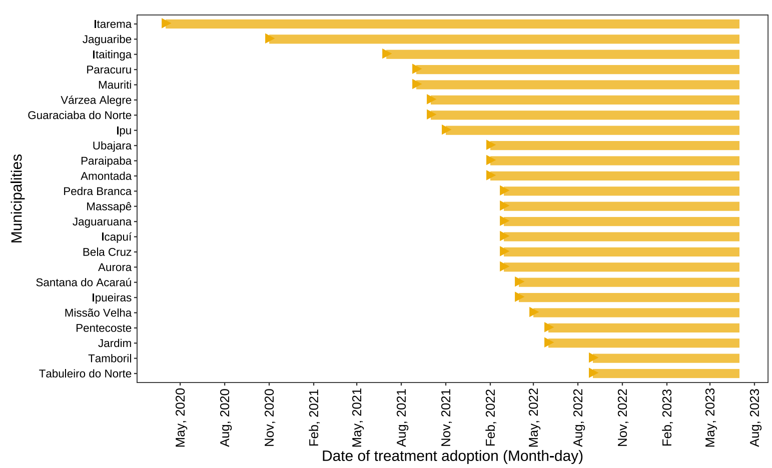
(a) First wave



(b) Second wave



(c) Third wave



Note:

C. Regression Discontinuity Design (RDD)

C.1. Description of RDD approach

The roll-out of RAIO harnessed population criteria to determine municipalities' eligibility for the program. In Phase 1, a group of municipalities with more than 100,000 inhabitants received a RAIO base, followed by the group with more than 50,000 inhabitants for Phase 2, followed by municipalities with more than 30,000 inhabitants for Phase 3. We harness these population threshold for program eligibility to implement a regression discontinuity design (Thistlethwaite and Campbell 1960; Hahn, Todd and Van der Klaauw 2001). We identify the specific day on which a municipality created a RAIO Batallion for each population threshold P_j ($P_1 : pop > 100.000$, $P_2 : 50.000 < pop < 100.000$ and $P_3 : 30.000 < pop < 50.000$). In Phase 1 there were only 9 municipalities assigned to treatment, making it difficult to identify causal effects due to lack of statistical power (Imbens and Lemieux 2008). We focus on Phases 2 and 3, where we have 32 and 24 municipalities assigned to treatment respectively, increasing the precision of our RDD estimates, although we acknowledge that this still remains a small sample and we may lack sufficient statistical power to identify effects.

According to the SSPD-CE, the population estimates used to define the roll-out order came from the Brazilian Geographic and Statistical Institute (*Instituto Brasileiro de Geografia e Estatistica*, IBGE). However, using data provided by IBGE, we identified some non-compliers: for example, Figure E.3 shows that seven municipalities below the 50,000 inhabitant threshold received a RAIO squad during phase 2. Given this partial non-compliance, we use a fuzzy regression discontinuity design: treatment assignment depended on population size in a probabilistic rather than deterministic way, allowing us to estimate the intention-to-treat (ITT) (Imbens and Angrist 1994; Angrist, Imbens and Rubin 1996).

We estimate the ITT parametrically as follows:

$$\text{RAIO}_i = \beta_t T_i + g(P_i) + \gamma_i + \epsilon_i \tag{1}$$

$$Y_i = \beta_y \hat{\text{RAIO}}_i + g(P_i) + \gamma_i + \mu_i \tag{2}$$

where T$_i$ is an indicator function that takes a value of 1 for municipalities i assigned to RAIO, and 0 otherwise; $g(P_i)$ represents a flexible function in the population size; γ_i are municipality fixed effects; and both error terms ϵ_i and μ_i are clustered at the municipality level. The coefficient β_t in equation 1 identifies the first-stage effect of being eligible for a RAIO squad on actual treatment assignment. The coefficient β_y in equation 2 identifies the reduced-form effect of being eligible to receive RAIO on crime outcomes.

The parameter β represents the ITT of RAIO's roll-out on crime and policing outcomes when two assumptions hold: (i) the sample is balanced on pre-determined characteristics between municipalities assigned and not assigned to RAIO roll-out; and (ii) municipalities are unable to manipulate population estimates to intentionally fall above the threshold to receive a RAIO base. Figure E.4 and E.6 display the t-statistics and standardized coefficients of β for phases 2 and 3, using gross domestic product, municipal budget, and number of formal jobs as dependent variables in Equation 2. We find that these baseline characteristics are well-balanced between treatment and control groups. Additionally, Figure E.5 and E.7 show that there is no bunching of population estimates close to both thresholds, lending credibility to the assumption of no sorting along the forcing variable (McCrary 2008).

In Figure 1, we illustrate the phased roll-out of RAIO starting in 2015, where municipalities received battalions on different dates during each phase. Specifically, in our RDD exercise, Phase 2 began in 2017 and concluded in 2019, with thirty-four municipalities receiving a RAIO squad during that period. In 2020, Phase 3 started, which lasted until 2022, assigning twenty-four municipalities to the roll-out.

To ensure comparability between treated and non-treated municipalities, we subset our sample to the period before non-assigned municipalities receive a RAIO base. This ensures that we compare municipalities that received the treatment with those that did not. Furthermore, our main specification for the RDD uses variation in crime outcomes as the dependent variable, allowing us to assess if crime levels changed after the roll-out of RAIO squads.

To account for varying lengths of exposure among municipalities assigned to phase 2, we calculate variation in monthly average pre- and post-roll-out completion for all crime outcomes. Thus, the variation in crime outcome for a municipality i assigned to RAIO phase 2 is calculated as:

$$\Delta y_i^{P2} = \frac{\left[\sum_{i=d_i}^{\text{Dec,19}} y_i/(\text{Dec,19} - d_i)\right]}{\left[\sum_{\text{Jan,15}}^{d_i-1} y_i/((d_i - 1) - \text{Jan,15})\right]}$$

Here, d_i is a variable indicating the month when municipality i assigned to phase 2 received the RAIO squad. In summary, Δy_i^{P2} captures the variation in monthly crime outcomes between the pre-and post-RAIO period from January 2015 to December 2019.

For the control group, we establish the start date of phase 2 (September 2017) as a baseline. The crime outcomes for these municipalities are measured as:

$$\Delta y_i^{C2} = \frac{\left[\sum_{i=\text{Sep},17}^{\text{Dec},19} y_i/(\text{Dec},19 - \text{Sep},17)\right]}{\left[\sum_{\text{Jan},15}^{\text{Aug},17} y_i/(\text{Aug},17 - \text{Jan, }15)\right]}$$

where y_i represents crime outcomes at the municipality level, and the date formats are standardized as ”Month, Year” (e.g., ”Dec,19” represents December 2019).

In phase 3, we exclude municipalities assigned to the RAIO expansion in phase 2 to address the issue of having municipalities already treated in the regression discontinuity design. Considering the start date of phase 3 (April 2020) as the baseline, the following expressions represent the variation pre and post RAIO roll-out phase 3 for the treated and control group respectively:

$$\Delta y_i^{P3} = \frac{\left[\sum_{i=d_i}^{\text{Dec},22} y_i/(\text{Dec},22 - d_i)\right]}{\left[\sum_{\text{Jan},15}^{d_i-1} y_i/((d_i - 1) - \text{Jan},15)\right]}$$

$$\Delta y_i^{C3} = \frac{\left[\sum_{i=\text{Apr},20}^{\text{Dec},22} y_i/(\text{Dec},22 - \text{Apr},20)\right]}{\left[\sum_{\text{Jan},15}^{\text{Mar},20} y_i/(\text{Mar},20 - \text{Jan, }15)\right]}$$

E.2. Results from the RDD

Our preferred specification uses changes in crime outcomes pre and post-roll-out as the dependent variable, rather than average levels following the roll-out.

Panel A in Table E.1 reports the RDD estimates on homicides, robberies, theft, sexual abuse, domestic violence, and guns seized per 100,000 inhabitants, using a linear polynomial specification. Panel B in Table E.1 reports results for the same outcomes using a quadratic polynomial. In column 1, we show that municipalities assigned to RAIO during the phase 2 roll-out reported a decrease in homicides of 5.2 percentage points per 100,000 inhabitants. This is consistent with the magnitude of the decreases we find in our difference-in-differences models. Column 4 demonstrates that the roll-out of RAIO squads increased gun seizures by 11.8 percentage points per 100,000 inhabitants.[1]

[1]Using a quadratic specification produces null results, likely because high-order polynomials in small samples heavily weigh

Although our RDD results are only significant at $p < .10$, likely due to the small sample size, they suggest that RAIO battalions likely generate effects through both deterrence and incapacitation, reducing homicides while also removing from circulation firearms used to commit crimes. For other crime outcomes, including robberies (column 2), thefts (column 3), sexual abuse (column 5), and domestic violence (column 6), we find no effect of RAIO. Figure E.1 shows the results described above: panel A shows a reduction in the rate of homicides, and panel D an increase in the rate of gun seizures. All estimates using optimal bandwidths follow Calonico, Cattaneo and Titiunik (2014) to minimize the mean squared error of the local polynomial RD point estimator.

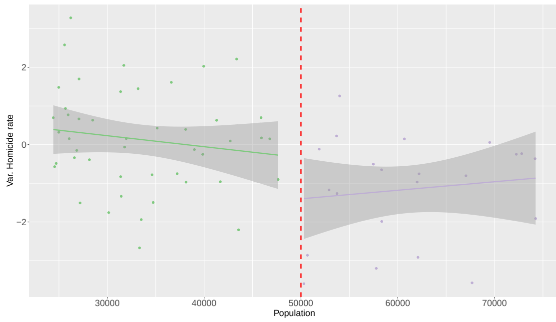
We perform the same exercise for the phase 3 roll-out: Figure E.2 and Table E.2 show the results of the RAIO roll-out for municipalities around the 30,000 inhabitant cutoff. While the estimates are even less precise for these exercises, and are not statistically significant at conventional levels, the RDD demonstrates similar patterns to what we find with differences-in-differences in that most crime reductions are driven by phase 2. Finally, we normalize the cutoffs of phases 2 and 3 to explore a stacked RDD, where we combine the shift in crime outcomes pre and post-RAIO roll-out from both phases to increase our number of observations and, therefore, our power within the RDD optimum bandwidth. Although this strategy yields more precise estimates, we do not find meaningful changes in crime, as Figure E.8 shows.

Despite the small sample size and large confidence intervals in our estimates, we interpret the RDD results as the local effect of the RAIO roll-out and complementary to our findings in the difference-in-differences model. The reduction in homicides found exclusively in Phase 2 seems consistent using both identification strategies. On the other hand, the large decrease in robberies in the difference-in-difference approach likely results from monthly reductions in municipalities far from the population threshold, which are excluded from the highly local effect estimated in the RDD.

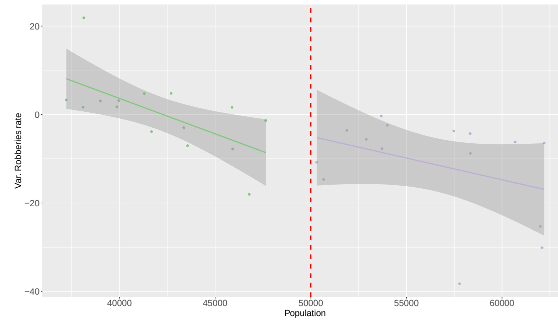
E.3. Validity of the RDD design

observations far from the threshold (Gelman and Imbens 2019).

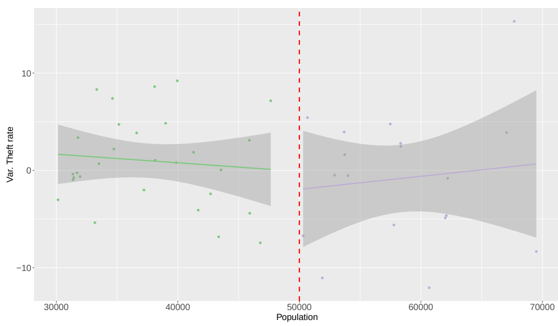
Figure E.1: **Fuzzy RDD estimates of the RAIO roll-out Phase 2**



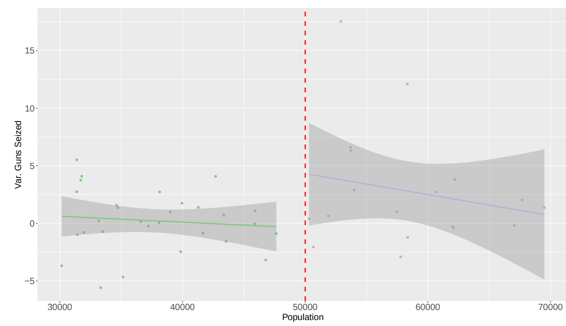
(a) Homicides



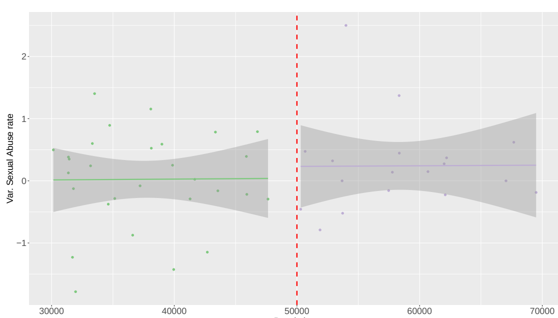
(b) Robberies



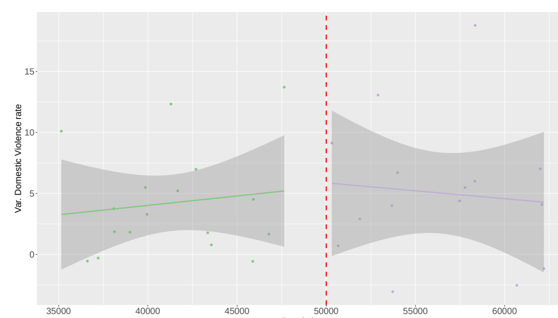
(c) Theft



(d) Guns seized



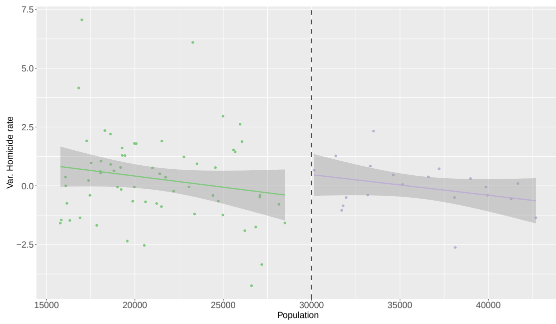
(e) Sexual Abuse



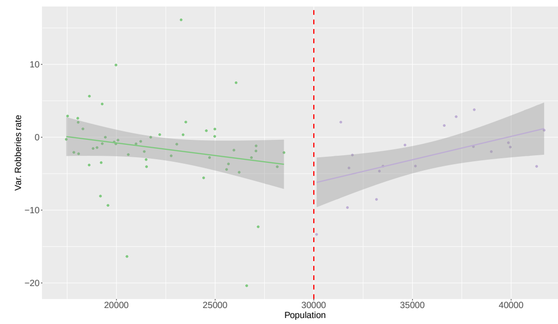
(f) Domestic Violence

Notes: The figure shows the effect of RAIO roll-out phase 2 on the rate of Homicides, Robberies, Theft, Guns seized, Sexual Abuse, and Domestic Violence per hundred thousand inhabitants graphically in Ceará Municipalities. Table E.1 shows the full set of the results upon which each figure is based. The outcomes are measured in the period pre and post-RAIO implementation in municipalities with more than 50 thousand inhabitants from 2014 to 2019. Plots were generated following Calonico, Cattaneo and Titiunik (2015). All estimates use a linear specification and a triangular kernel. Following Calonico, Cattaneo and Titiunik (2014), the optimal bandwidths were chosen to minimize the mean squared error of the local polynomial RD point estimator.

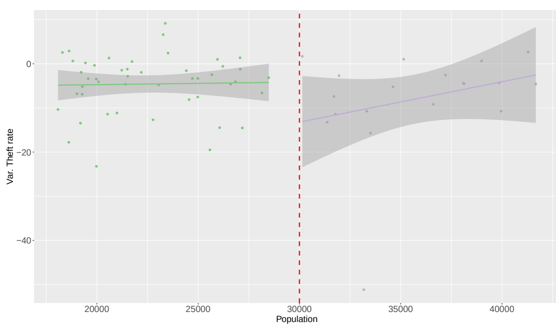
Figure E.2: **Fuzzy RDD estimates of the RAIO roll-out Phase 3**



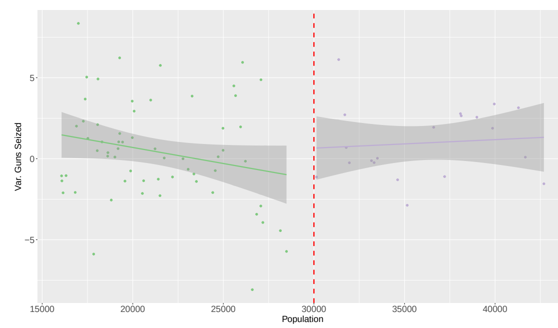
(a) Homicides



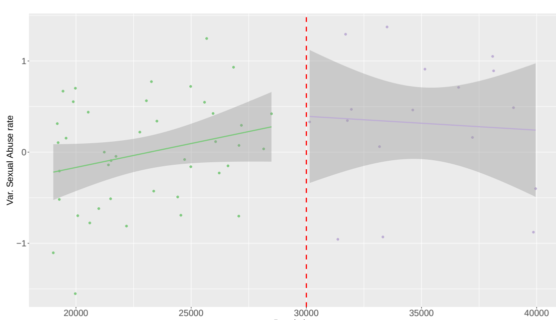
(b) Robberies



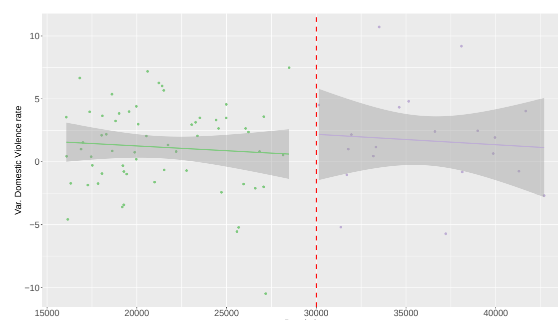
(c) Theft



(d) Guns seized



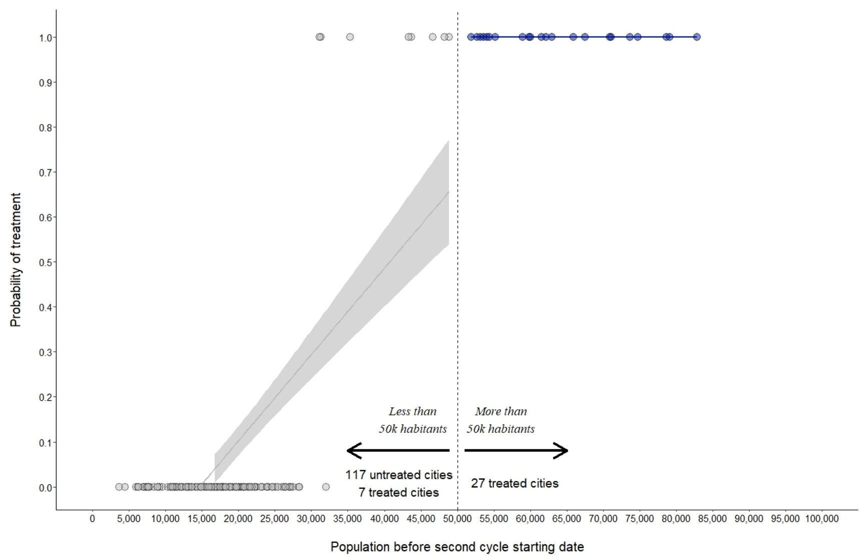
(e) Sexual Abuse



(f) Domestic Violence

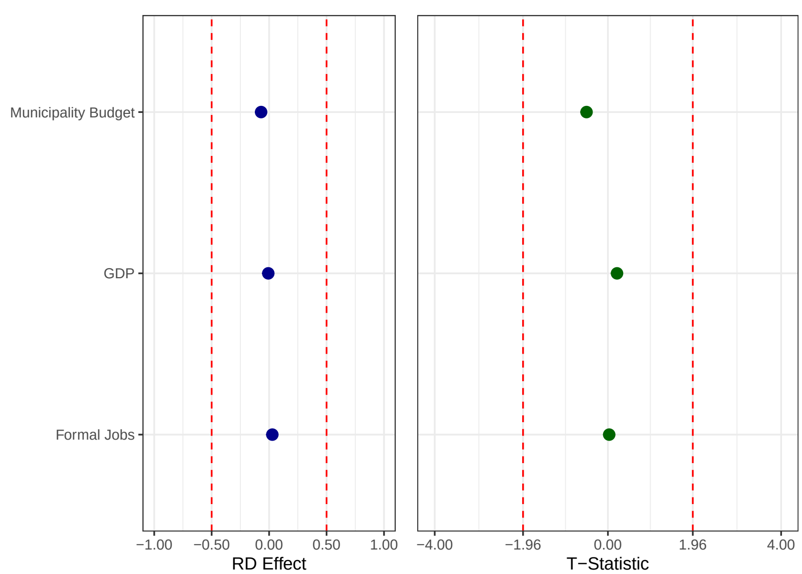
Notes: The figure shows the effect of RAIO roll-out phase 3 on the rate of Homicides, Robberies, Theft, Guns seized, Sexual Abuse, and Domestic Violence per hundred thousand inhabitants graphically in Ceará Municipalities. Table E.2 shows the full set of the results upon which each figure is based. The outcomes are measured in the period pre and post-RAIO implementation in municipalities with more than 50 thousand inhabitants from 2014 to 2019. Plots were generated following Calonico, Cattaneo and Titiunik (2015). All estimates use a linear specification and a triangular kernel. Following Calonico, Cattaneo and Titiunik (2014), the optimal bandwidths were chosen to minimize the mean squared error of the local polynomial RD point estimator.

Figure E.3: **RAIO phase 2 treatment assignment given population size**



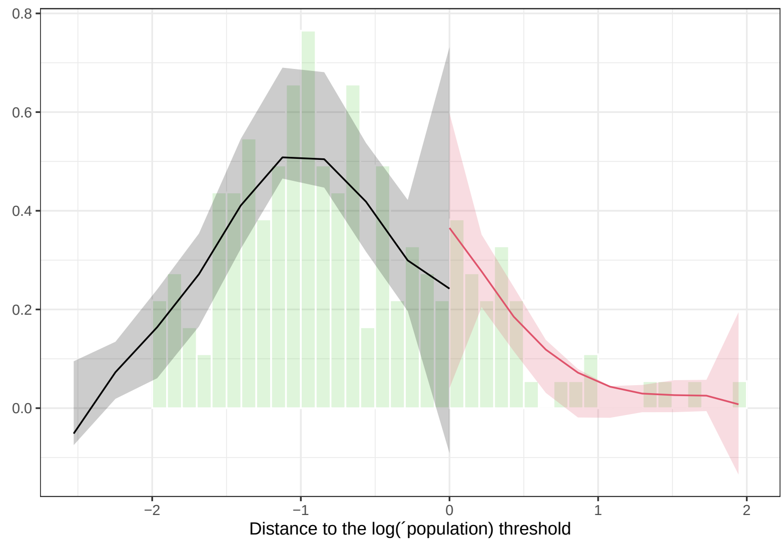
Note: This figure shows that seven municipalities that are below the threshold of 50.000 inhabitants were selected for receiving an RAIO squad in phase 2. To address the existence of non-compliers in our empirical strategy we employ a Fuzzy RDD design.

Figure E.4: **Baseline covariate balance around the RAIO phase 2 population threshold**



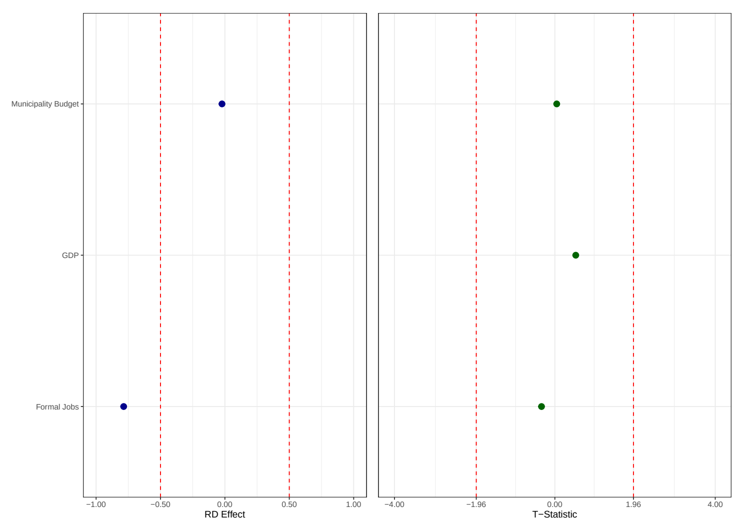
Notes: This figure shows the robust-bias corrected t-statistics and standardized coefficients from the baseline covariates' balance RD estimates. For each variable, we run an RD with linear polynomial and uniform kernel specification. Optimal bandwidth following Calonico, Cattaneo and Titiunik (2014) were chosen to minimize the mean squared error of the polynomial RD point estimator. In the t-statistics graph, we show 5% significance levels in red.

Figure E.5: **McCrary test for manipulation of the assignment variable - Phase 2**



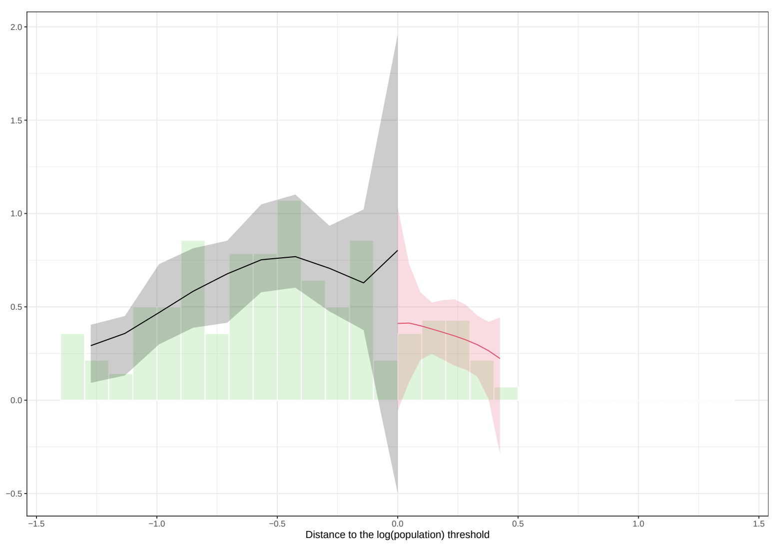
Notes: This figure displays the McCrary density test for the running variable around the RAIO Phase 2 cutoff. McCrrary test p-value = 0.16 (McCrary 2008).

Figure E.6: **Baseline covariate balance around the RAIO phase 3 population threshold**



Notes: This figure shows the robust-bias corrected t-statistics and standardized coefficients from the baseline covariates' balance RD estimates. For each variable, we run an RD with linear polynomial and uniform kernel specification. Optimal bandwidth following Calonico, Cattaneo and Titiunik (2014) were chosen to minimize the mean squared error of the polynomial RD point estimator. In the t-statistics graph, we show 5% significance levels in red.

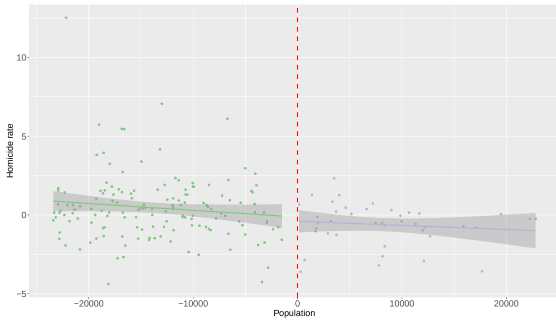
Figure E.7: **McCrary test for manipulation of the assignment variable - Phase 3**



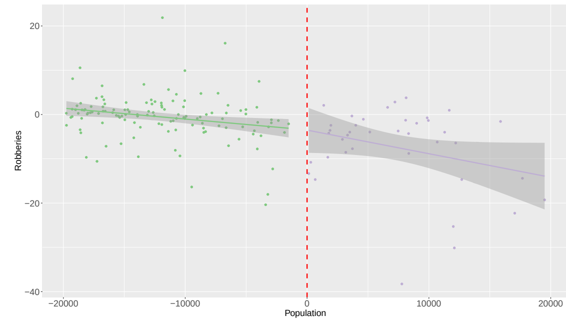
Notes: This figure displays the McCrary density test for the running variable around the RAIO Phase 3 cutoff. McCrrary test p-value = 0.75 (McCrary 2008).

E.4. Additional Results for the Regression Discontinuity Design

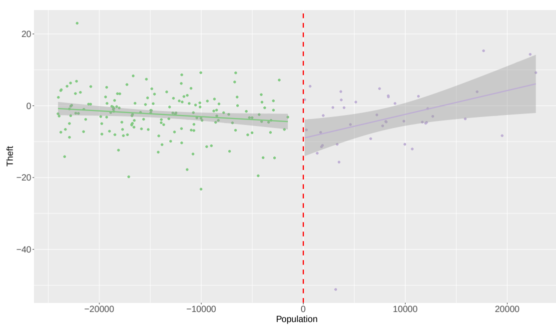
Figure E.8: **Fuzzy RDD estimates of the RAIO roll-out - Stacked RDD**



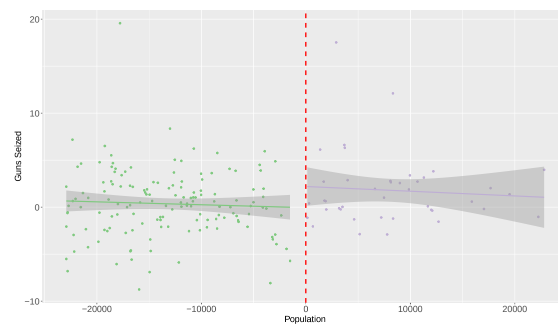
(a) Homicides



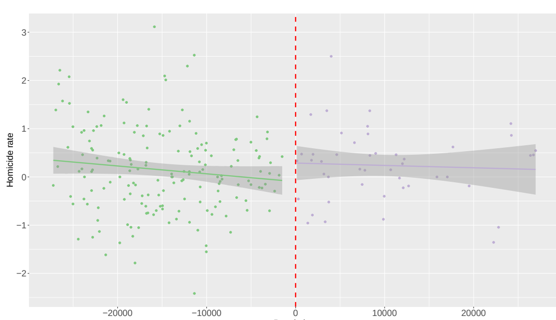
(b) Robberies



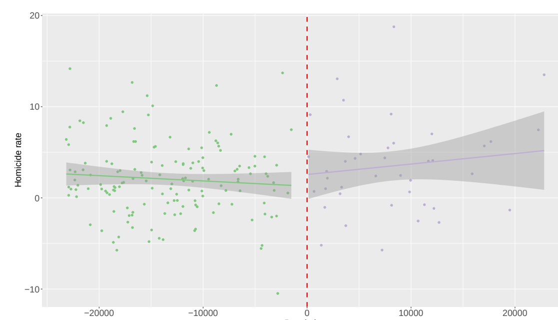
(c) Theft



(d) Guns seized



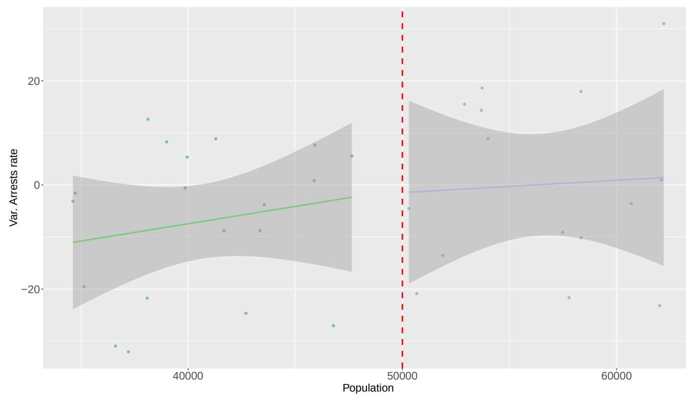
(e) Sexual Abuse



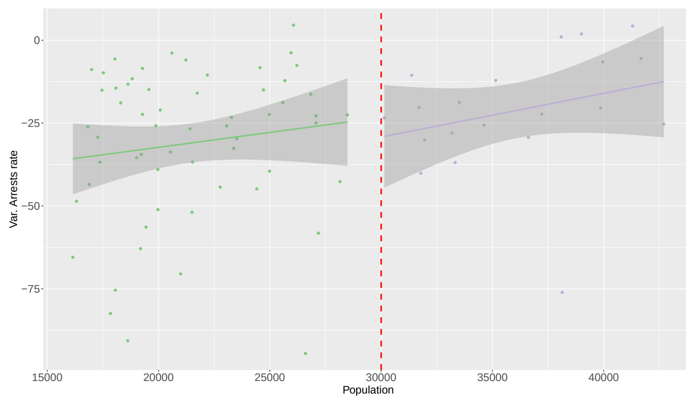
(f) Domestic Violence

Notes: The figure shows the effect of RAIO roll-out phase 2 and phase 3 on the rate of Homicides, Robberies, Theft, Guns seized, Sexual Abuse, and Domestic Violence per hundred thousand inhabitants graphically in Ceará Municipalities. Table **??** shows the full set of the results upon which each figure is based. The outcomes are measured pre and post-RAIO implementation in municipalities with more than 50 thousand inhabitants from 2014 to 2019. Plots were generated following Calonico, Cattaneo and Titiunik (2015). All estimates use a linear specification and a triangular kernel. Following Calonico, Cattaneo and Titiunik (2014), the optimal bandwidths were chosen to minimize the mean squared error of the local polynomial RD point estimator.

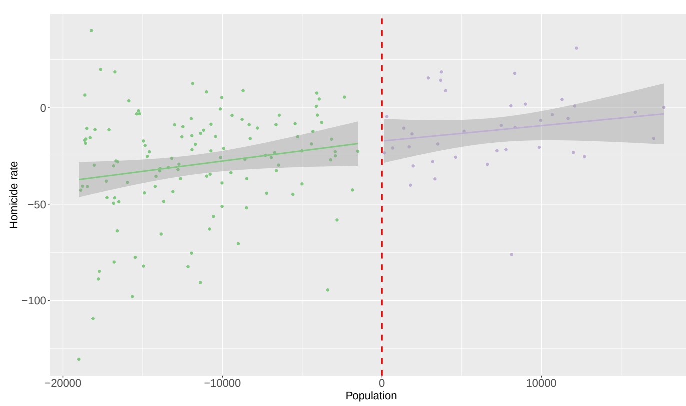
Figure E.9: **Fuzzy RDD estimates of the RAIO roll-out - Arrests**



(a) Arrests - Phase 2



(b) Arrests - Phase 3



(c) Arrests - Stack RDD

Notes: The figure shows the effect of RAIO roll-out phase 2 and phase 3 on the rate of Arrests per hundred thousand inhabitants graphically in Ceará Municipalities. The outcomes are measured pre and post-RAIO implementation in municipalities with more than 50 thousand inhabitants (Phase 2), more than 30 thousand inhabitants (Phase 3), and the combined results (Stack). Plots were generated following Calonico, Cattaneo and Titiunik (2015). All estimates use a linear specification and a triangular kernel. Following Calonico, Cattaneo and Titiunik (2014), the optimal bandwidths were chosen to minimize the mean squared error of the local polynomial RD point estimator.

Table E.1: **Fuzzy RDD estimates for RAIO roll-out Phase 2**

	Δ Homicides per 100k pop.	Δ Robberies per 100k pop.	Δ Theft per 100k pop.	Δ Guns seized per 100k pop.	Δ Sexual Abuse per 100k pop.	Δ Domestic Violence per 100k pop.	Δ Arrests per 100k pop.
Panel A: Linear Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RD Estimator	-5.231	-21.553	-7.254	11.850	0.679	-1.803	-2.404
Robust p-value	0.088*	0.627	0.287	0.097*	0.476	0.902	0.785
Robust conf. int.	[-14.891 , -0.283]	[-92.158 , 50.076]	[-28.566 , 6.123]	[0.172 , 35.679]	[-1.366 , 3.459]	[-623.595 , 536.370]	[-109.276 , 78.173]
CCT-optimal BW	19,117	13,165	20,401	20,855	20,057	15,250	16,487
Eff. number of obs.	64	29	73	74	70	44	54
Panel B: Quadratic Specification							
RD Estimator	5.943	-22.268	-1.917	-43.91	0.335	6.301	-2.179
Robust p-value	0.821	0.641	0.939	0.761	0.802	0.948	0.963
Robust conf. int.	[-15.554 , 20.511]	[-59.661 , 33.310]	[-38.338 , 34.907]	[-196.733 , 286.013]	[-2.987 , 4.061]	[-84.225 , 77.790]	[-71.304 , 75.414]
CCT-optimal BW	18,645	15,683	16,307	19,542	18,058	19,041	17,333
Eff. number of obs.	62	48	54	68	58	66	56

Note: The table reports RD estimates of the effect of RAIO phase 2 on the rate of Homicides, Robberies, Theft, Guns seized, Sexual Abuse, Domestic Violence, and Arrests per hundred thousand inhabitants in Ceará Municipalities around the threshold of 50 thousand inhabitants. Panel A shows the results for a first-degree polynomial estimation. Panel B shows the results for a second-degree polynomial estimation. Optimal bandwidths following Calonico, Cattaneo and Titiunik (2014) were chosen to minimize the mean squared error of the local polynomial RD point estimator. Following that same work, we report robust-bias corrected p-values and 90% CIs. Coefficients significantly different from zero at 99%(***), 95%(**) and 90%(*) confidence level.

Table E.2: **Fuzzy RDD estimates for RAIO roll-out Phase 3**

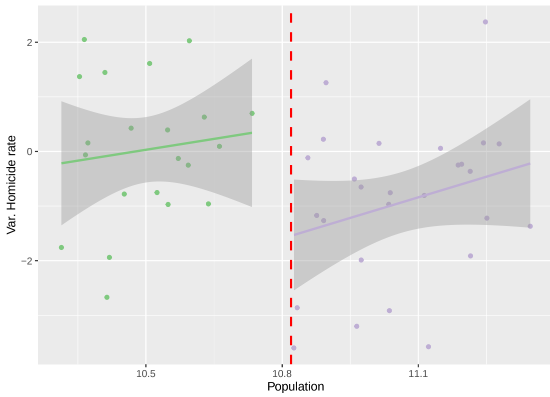
	Δ Homicides per 100k pop.	Δ Robberies per 100k pop.	Δ Theft per 100k pop.	Δ Guns seized per 100k pop.	Δ Sexual Abuse per 100k pop.	Δ Domestic Violence per 100k pop.	Δ Arrests per 100k pop.
Panel A: Linear Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RD Estimator	2.481	-6.524	2.215	9.217	-0.219	-5.969	0.038
Robust p-value	0.197	0.495	0.590	0.198	0.819	0.518	0.894
Robust conf. int.	[-0.031 , 4.992]	[-21.793 , 8.744]	[-18.284 , 22.713]	[-6.806 , 25.240]	[-1.572 , 1.134]	[-28.287 , 16.349]	[-58.851 , 58.226]
CCT-optimal BW	10,006	10,143	11,317	9,959	7,912	10,719	7,501
Eff. number of obs.	20	21	18	25	27	18	25
Panel B: Quadratic Specification							
RD Estimator	2.937	-11.384	8.390	12.379	0.127	25.676	-6.686
Robust p-value	0.600	0.237	0.532	0.115	0.805	0.286	0.478
Robust conf. int.	[-0.409 , 6.284]	[-29.776 ,7.008]	[-13.487 , 30.267]	[-3.919 , 28.677]	[-1.370 , 1.624]	[-25.331 , 76.684]	[-29.105 , 15.733]
CCT-optimal BW	14,533	12,585	11,923	14,144	11,032	13,880	14,020
Eff. number of obs.	20	21	27	18	18	25	15

Note: The table reports RD estimates of the effect of RAIO phase 3 on the rate of Homicides, Robberies, Theft, Guns seized, Sexual Abuse, Domestic Violence, and Arrests per hundred thousand inhabitants in Ceará Municipalities around the threshold of 30 thousand inhabitants. Panel A shows the results for a first-degree polynomial estimation. Panel B shows the results for a second-degree polynomial estimation. Optimal bandwidths following Calonico, Cattaneo and Titiunik (2014) were chosen to minimize the mean squared error of the local polynomial RD point estimator. Following that same work, we report robust-bias corrected p-values and 90% CIs. Coefficients significantly different from zero at 99%(***), 95%(**) and 90%(*) confidence level.

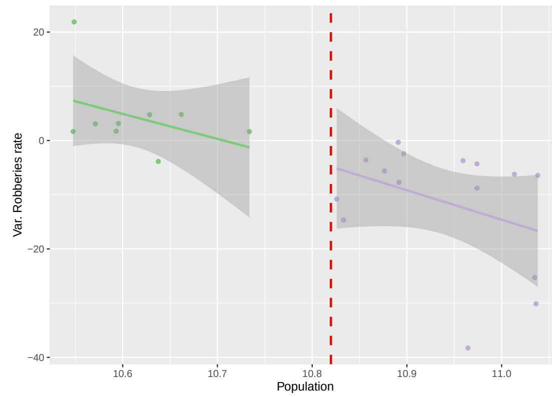
E.5. SHARP RDD

As mentioned in Appendix Section C., given imperfect compliance we estimate in a prior section the ITT—the effect of eligibility for RAIO's phase 2—using a fuzzy RDD. Here we use a sharp RDD, excluding non-compliant municipalities from the sample. Table 3 and Figure E.10 provide the results. Most of the estimated signs remain the same, although confidence intervals are wider due to fewer observations. The sharp RDD estimates demonstrate a statistically significant reduction in homicides, consistent with findings from the fuzzy RDD.

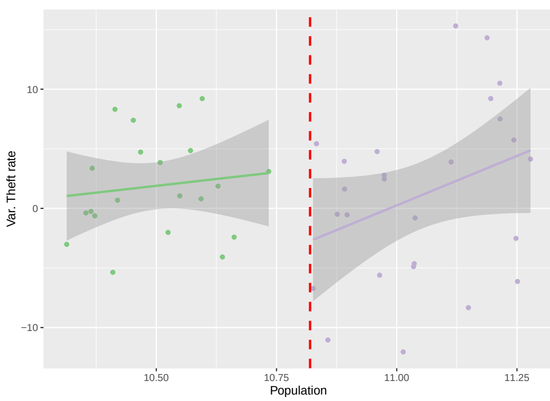
Figure E.10: Sharp RDD estimates of the RAIO roll-out Phase 3



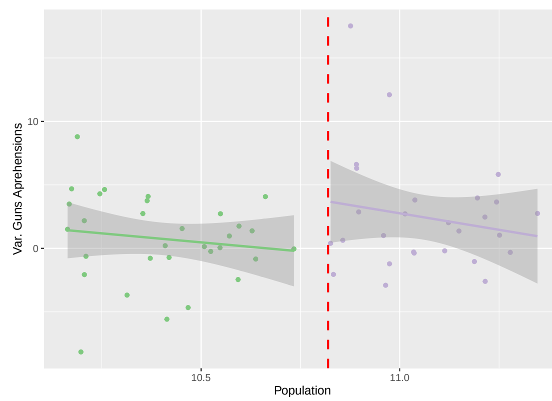
(a) Homicides



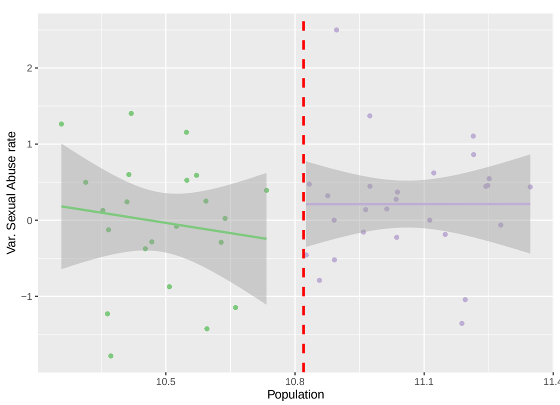
(b) Robberies



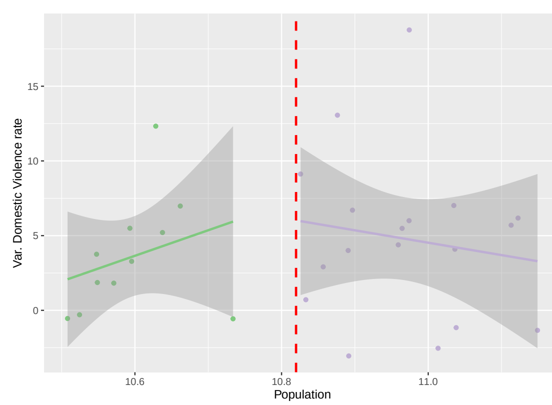
(c) Theft



(d) Guns seized



(e) Sexual Abuse



(f) Domestic Violence

Notes: The figure shows the effect of RAIO roll-out phase 2 on the rate of Homicides, Robberies, Theft, Guns seized, Sexual Abuse, and Domestic Violence per hundred thousand inhabitants graphically in Ceará using a Sharp RDD where we exclude the non-complier municipalities. Table 3 shows the full set of the results upon which each figure is based. The outcomes are measured pre and post-RAIO implementation in municipalities with more than 50 thousand inhabitants from 2014 to 2019. Plots were generated following Calonico, Cattaneo and Titiunik (2015). All estimates use a linear specification and a triangular kernel. Following Calonico, Cattaneo and Titiunik (2014), the optimal bandwidths were chosen to minimize the mean squared error of the local polynomial RD point estimator.

Table 3: Sharp RDD estimates of the RAIO roll-out Phase 2

	Δ Homicides per 100k pop.	Δ Robberies per 100k pop.	Δ Theft per 100k pop.	Δ Guns Aprehended per 100k pop.	Δ Sexual Abuse per 100k pop.	Δ Domestic Violence per 100k pop.	Δ Arrests per 100k pop.
Panel A: Linear Specification							
RD Estimator	-2.095	-6.551	-3.889	3.229	0.343	2.535	-2.632
Robust p-value	0.030**	0.366	0.279	0.245	0.613	0.767	0.754
Robust conf. int.	[-3.950 , -0.538]	[-10.627 , 3.087]	[-12.668 , 2.618]	[-1.467 , 8.540]	[-0.763 , 1.669]	[-13.859 , 19.934]	[-18.278 , 13.013]
CCT-optimal BW	0.545	0.281	0.525	0.665	0.565	0.349	0.359
Eff. number of obs.	48	24	47	58	49	29	30
Panel B: Quadratic Specification							
RD Estimator	-2.662	-8.248	-1.826	1.188	0.076	5.303	-3.904
Robust p-value	0.083*	0.198	0.945	0.948	0.936	0.594	0.859
Robust conf. int.	[-5.809 , -0.150]	[-18.564 , 2.274]	[-18.665 , 17.151]	[-7.277 , 7.874]	[-2.288 , 2.076]	[-13.384 , 26.200]	[-25.894 , 17.996]
CCT-optimal BW	0.545	0.380	0.438	0.590	0.598	0.607	0.539
Eff. number of obs.	48	33	41	50	50	50	48

Note: The table reports RD estimates of the effect of RAIO phase 2 on the rate of Homicides, Robberies, Theft, Guns seized, Sexual Abuse, Domestic Violence, and Arrests per hundred thousand inhabitants in Ceará Municipalities around the threshold of 50 thousand inhabitants. Panel A shows the results for a first-degree polynomial estimation. Panel B shows the results for a second-degree polynomial estimation. Optimal bandwidths following Calonico, Cattaneo and Titiunik (2014) were chosen to minimize the mean squared error of the local polynomial RD point estimator. Following that same work, we report robust-bias corrected p-values and 90% CIs. Coefficients significantly different from zero at 99%(***), 95%(**) and 90%(*) confidence level.

D. Electoral Data

In this section we present our results on the effect of RAIO squads on election outcomes.

Figure D.1: Share of votes obtained by the winning candidate across three election rounds in Ceará

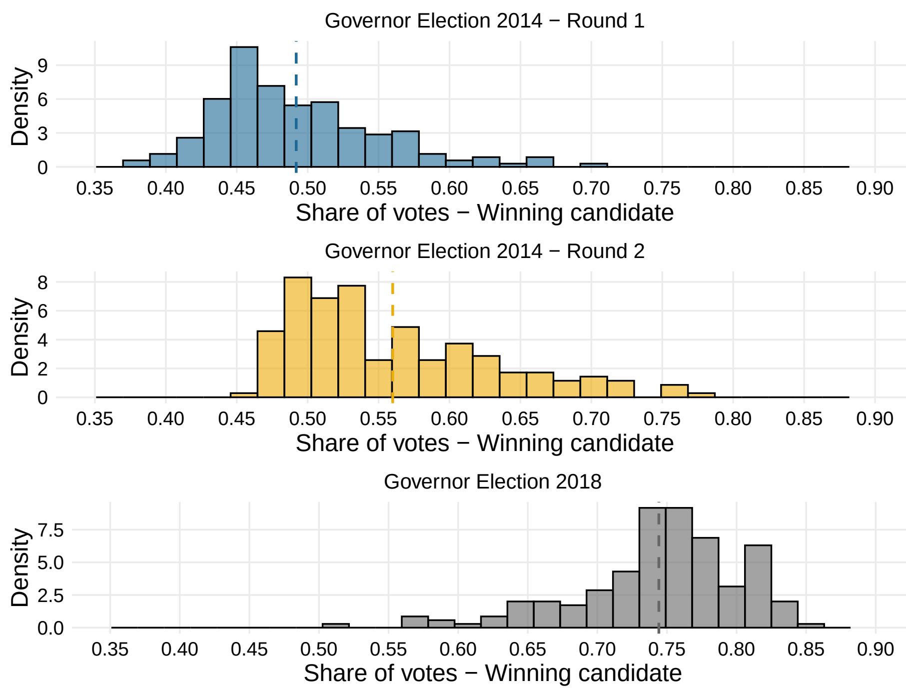


Figure D.2: Change in level means

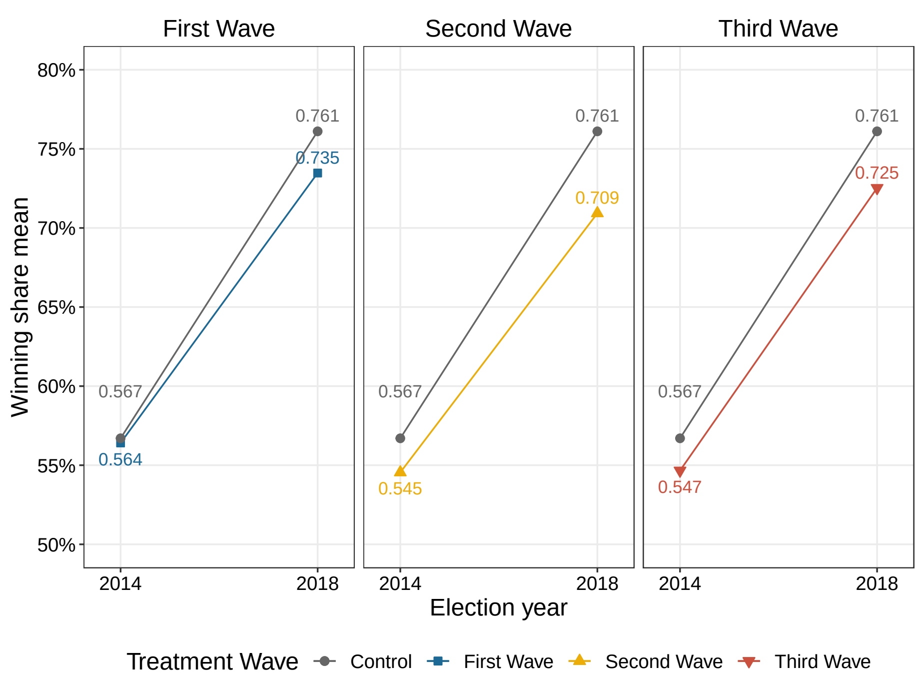


Table D.1: **Average Treatment Effects of RAIO on voting**

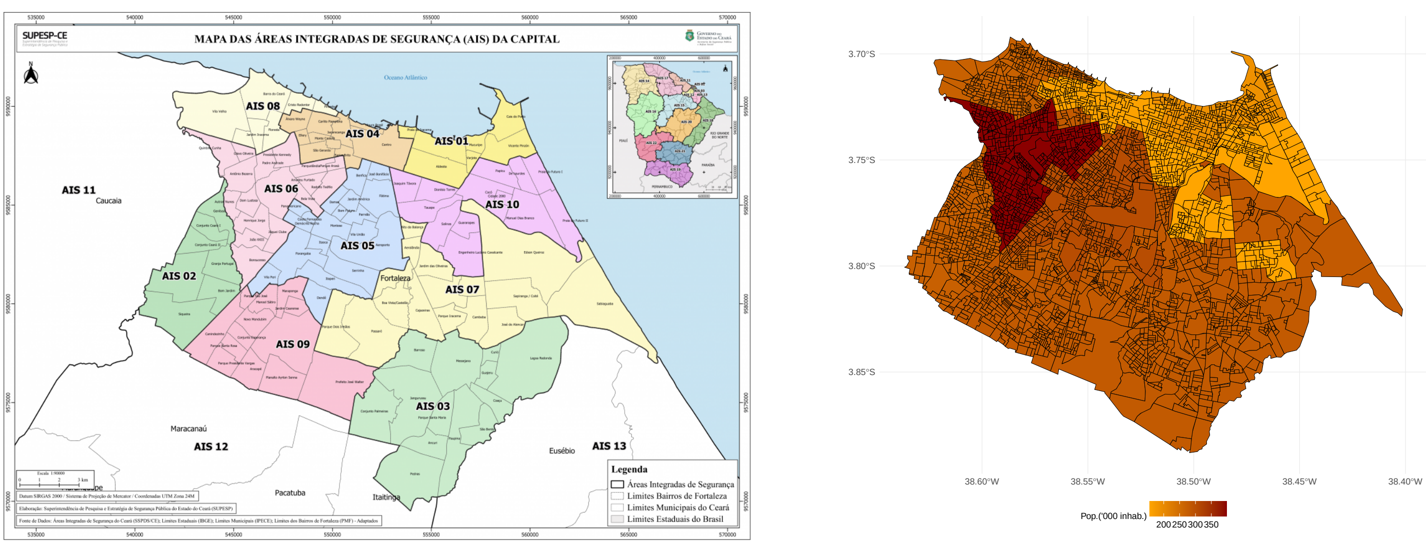
	Share of Votes		Total Votes	
	(1)	(2)	(3)	(4)
RAIO: Treatment	0.1897***	0.0781**	9,868.0***	4,213.5***
	(0.0130)	(0.0371)	(890.2)	(713.5)
homicides.pre		-0.0166		-777.7
		(0.0263)		(634.3)
population.pre		0.0003***		6.083***
		(0.0000)		(0.7235)
cvp.pre		0.0034		-52.13
		(0.0023)		(82.88)
jobs.pre		0.0000		-0.0499
		(0.0000)		(0.4296)
armas.pre		0.0184		627.6**
		(0.0128)		(275.2)
drogas.pre		-0.0021		-17.63
		(0.0014)		(59.73)
furto.pre		0.0089***		14.29
		(0.0028)		(62.13)
sexual_violence.pre		0.0242		442.4
		(0.0258)		(504.2)
dom_violence.pre		0.0097***		331.5***
		(0.0033)		(96.04)
Observations	366	366	366	366
R2	0.34160	0.46718	0.97755	0.99058
FE: Municipality	✓	✓	✓	✓

Note: Results presented in the table correspond to the municipality level. In columns 1 and 2 we present estimates for the share of votes for Camilo Sobreira de Santana in each municipality. Columns 3 and 4 present estimates for the total number of votes for Camilo Sobreira de Santana in each municipality. Standard errors are clustered at the municipality level. Significance levels are as follows: 1%, ***; 5%, **; 10%, *.

E. Survey Methodology

We conducted our resident survey in the metropolitan region of Ceará, Brazil. To do so, we hired a specialized research company, *Conectar*, to design the sampling strategy and conduct fieldwork. The sampling strategy sought to ensure that the survey would be representative at the metropolitan area level, which is divided into 13 Integrated Security Areas (AIS) as shown in Figure E.1. The survey firm used the 2010 Census data from the Brazilian Institute of Geography and Statistics (IBGE) to develop the sampling frame, as it offers the most accurate picture of the sociodemographic distribution of the population including detailed information on gender, age, and income.

The survey company defined a minimum sample size of 2,000 to achieve representativity while ensuring that even the smallest regions had a minimum of 100 surveys each. This approach was designed to offer a maximum estimated margin of error of 9.8 percentage points for smaller regions (with a 95% confidence interval), and an overall margin of error of 2.2 percentage points for the entire metropolitan region. Table E.1 below provides a summary of our ex-ante sampling strategy per AIS.



Integrated Security Areas (AIS) | Population Density by AIS

Figure E.1: Fortaleza Metropolitan Region

Enumerators assigned to a specific AIS were provided with a target number of interviews, determined by the population composition from the most recent census data for that area. Enumerators were required to achieve a sample that closely matched the census data regarding gender, education level, income, and employment composition within that AIS. Finally, within each household, one adult was selected for the

Table E.1: Sampling Frame - Fortaleza Metropolitan Area

AIS	Population (2010 Census)	AIS Pop. / Total Pop.	Proposed Sample	Margin of Error
1	190,223	5.5%	100	9.8
2	271,536	7.9%	180	7.3
3	282,323	8.2%	180	7.3
4	161,981	4.7%	100	9.8
5	303,428	8.8%	140	8.3
6	397,482	11.6%	180	7.3
7	281,685	8.2%	180	7.3
8	270,674	7.9%	130	8.6
9	281,749	8.2%	180	7.3
10	156,362	4.5%	100	9.8
11	368,918	10.7%	210	6.8
12	230,986	6.7%	160	7.7
13	243,430	7.1%	160	7.7
TOTAL	**3,440,777**	**100%**	**2,000**	**2.2**

interview, typically the individual who responded to the enumerator's request. We show in Table E.2 a comparison between some descriptive statistics from the Brazilian census and our survey sample.

Table E.2: Comparison Census and Survey Sample

	Fortaleza Metropolitan Region	
Variable	**Census**	**Sample**
% Employed (formal and informal sector)	39.18%	40.35%
Average Income (in minimum wages)	1.62	1.29
% Women	53.19%	55.05%
% Illiterate	8.57%	16.21%
% Elementary School (Incomplete)	29.00%	14.18%
% Elementary School (Complete)	16.50%	7.00%
% High School (Complete)	32.20%	48.59%
% Higher Education (Complete)	13.73%	13.71%

F. Tables for Conjoint Estimates

Table F.1: Who is more capable at combating crime?

	Uniform		*Weapon*		*Vehicle*	
	Military Police	*RAIO*	*Pistol*	*Rifle*	*None*	*Moto*
AMCE	-	0.106	-	0.142	-	0.205
Stnd. Error	-	0.010	-	0.009	-	0.011
Conf. Band 95%	-	[0.086, 0.125]	-	[0.123, 0.160]	-	[0.123, 0.160]
MM	0.448	0.55	0.429	0.573	0.401	0.604
Stnd. Error	0.005	0.005	0.005	0.004	0.005	0.005
Conf. Band 95%	[0.438, 0.458]	[0.540, 0.560]	[0.420, 0.439	[0.563, 0.583]	[0.412, 0.390	[0.593, 0.616]

Note: AMCE (Average Marginal Component Effects) represent the average marginal effect of each attribute level relative to a reference level, holding all other attributes constant. MM (Marginal Means) indicate the estimated mean utility of each attribute level relative to the overall mean utility. All estimates are based on the conditional logit model.

Table F.2: Who makes you feel safer?

	Uniform		*Weapon*		*Vehicle*	
	Military Police	*RAIO*	*Pistol*	*Rifle*	*None*	*Moto*
AMCE	-	0.103	-	0.106	-	0.180
Stnd. Error	-	0.010	-	0.009	-	0.011
Conf. Band 95%	-	[0.083, 0.122]	-	[0.086, 0.125]	-	[0.158, 0.202]
MM	0.449	0.549	0.447	0.555	0.413	0.592
Stnd. Error	0.005	0.005	0.004	0.005	0.005	0.005
Conf. Band 95%	[0.438, 0.458]	[0.538, 0.559]	[0.437, 0.457]	[0.544, 0.565]	[0.402, 0.424]	[0.580, 0.603]

Note: AMCE (Average Marginal Component Effects) represent the average marginal effect of each attribute level relative to a reference level, holding all other attributes constant. MM (Marginal Means) indicate the estimated mean utility of each attribute level relative to the overall mean utility. All estimates are based on the conditional logit model.

Table F.3: Who is more likely to commit human rights abuses?

	Uniform		*Weapon*		*Vehicle*	
	Military Police	*RAIO*	*Pistol*	*Rifle*	*None*	*Moto*
AMCE	-	0.064	-	-0.014	-	0.017
Stnd. Error	-	0.010	-	0.010	-	0.011
Conf. Band 95%	-	[0.043, 0.084]	-	[-0.034, 0.005]	-	[-0.005, 0.039]
MM	0.468	0.531	0.507	0.493	0.492	0.508
Stnd. Error	0.005	0.005	0.005	0.005	0.005	0.005
Conf. Band 95%	[0.457, 0.478]	[0.521, 0.541]	[0.497, 0.516]	[0.483, 0.503]	[0.482, 0.503]	[0.497, 0.519]

Note: AMCE (Average Marginal Component Effects) represent the average marginal effect of each attribute level relative to a reference level, holding all other attributes constant. MM (Marginal Means) indicate the estimated mean utility of each attribute level relative to the overall mean utility. All estimates are based on the conditional logit model.

Table F.4: Who is more likely to be corrupt?

	Uniform		***Weapon***		***Vehicle***	
	Military Police	*RAIO*	*Pistol*	*Rifle*	*None*	*Moto*
AMCE	-	0.100	-	0.011	-	0.032
Stnd. Error	-	0.010	-	0.010	-	0.011
Conf. Band 95%	-	[0.079, 0.121]	-	[-0.008, 0.030]	-	[0.009, 0.055]
MM	0.449	0.549	0.494	0.506	0.485	0.515
Stnd. Error	0.005	0.005	0.005	0.005	0.006	0.005
Conf. Band 95%	[0.439, 0.460]	[0.539, 0.559]	[0.484, 0.504]	[0.496, 0.517]	[0.474, 0.497]	[0.504, 0.527]

Note: AMCE (Average Marginal Component Effects) represent the average marginal effect of each attribute level relative to a reference level, holding all other attributes constant. MM (Marginal Means) indicate the estimated mean utility of each attribute level relative to the overall mean utility. All estimates are based on the conditional logit model.

Table F.5: Who is more likely to use force against criminals?

	Uniform		***Weapon***		***Vehicle***	
	Military Police	*RAIO*	*Pistol*	*Rifle*	*None*	*Moto*
AMCE	-	0.089	-	0.162	-	0.184
Stnd. Error	-	0.009	-	0.009	-	0.010
Conf. Band 95%	-	[0.070, 0.108]	-	[0.144, 0.181]	-	[0.164, 0.206]
MM	0.456	0.542	0.419	0.584	0.411	0.595
Stnd. Error	0.005	0.005	0.005	0.005	0.005	0.005
Conf. Band 95%	[0.446, 0.466]	[0.533, 0.552]	[0.410, 0.428]	[0.573, 0.594]	[0.400, 0.421]	[0.583, 0.601]

Note: AMCE (Average Marginal Component Effects) represent the average marginal effect of each attribute level relative to a reference level, holding all other attributes constant. MM (Marginal Means) indicate the estimated mean utility of each attribute level relative to the overall mean utility. All estimates are based on the conditional logit model.

Table F.6: Standardized Index - All Questions (1)

	Uniform		***Weapon***		***Vehicle***	
	Military Police	*RAIO*	*Pistol*	*Rifle*	*None*	*Moto*
AMCE	-	0.242	-	0.226	-	0.356
Stnd. Error	-	0.020	-	0.019	-	0.023
Conf. Band 95%	-	[0.202, 0.282]	-	[0.189, 0.264]	-	[0.310, 0.401]
MM	-0.120	0.115	-0.113	0.117	-0.170	0.181
Stnd. Error	0.011	0.012	0.010	0.011	0.013	0.013
Conf. Band 95%	[-0.143, -0.097]	[0.090, 0.138]	[-0.134, -0.091]	[0.094, 0.140]	[-0.195, -0.144]	[0.154, 0.207]

Note: AMCE (Average Marginal Component Effects) represent the average marginal effect of each attribute level relative to a reference level, holding all other attributes constant. MM (Marginal Means) indicate the estimated mean utility of each attribute level relative to the overall mean utility. All estimates are based on the conditional logit model.

Table F.7: Standardized Index - All Questions (2)

	Uniform		***Weapon***		***Vehicle***	
	Military Police	*RAIO*	*Pistol*	*Rifle*	*None*	*Moto*
AMCE	-	0.185	-	0.059	-	0.194
Stnd. Error	-	0.020	-	0.019	-	0.022
Conf. Band 95%	-	[0.145, 0.223]	-	[0.020, 0.096]	-	[0.150, 0.238]
MM	-0.092	0.087	-0.029	0.031	-0.092	0.098
Stnd. Error	0.012	0.012	0.011	0.012	0.013	0.013
Conf. Band 95%	[-0.116, -0.068]	[0.063, 0.112]	[-0.053, -0.006]	[0.006, 0.055]	[-0.118, -0.066]	[0.071, 0.124]

Note: AMCE (Average Marginal Component Effects) represent the average marginal effect of each attribute level relative to a reference level, holding all other attributes constant. MM (Marginal Means) indicate the estimated mean utility of each attribute level relative to the overall mean utility. All estimates are based on the conditional logit model.

G. Ethical concerns

Our survey on policing and security in Ceará, Brazil, was designed with careful attention to ethical concerns, particularly regarding potential risks to respondents. This project received approval from the Ethics Committee at REDACTED.

We outline below the specific ethical challenges we anticipated, the measures we took to mitigate harm, and how our design choices may have influenced participation and responses.

Psychological and emotional risks. Respondents may have experienced psychological distress when recalling past experiences of crime victimization or interactions with security forces. To minimize harm, we informed all participants that they could skip any question or end the survey at any time without facing any negative consequences. Additionally, our enumerators were trained to recognize signs of distress and provide respondents with information about available support resources where appropriate.

Risks of retaliation and confidentiality measures. Given the sensitivity of topics such as criminal governance, police abuse, and organized crime, respondents may have feared retribution for their answers. To mitigate this risk, we ensured that all surveys were conducted in private settings, out of earshot of others, and respondents were assured of strict confidentiality. Surveys were anonymous, with no personally identifiable information recorded, and data was stored securely using encrypted digital platforms. Furthermore, interviewers were trained to navigate sensitive topics in a neutral and non-leading manner to avoid placing respondents at additional risk.

Participant pool and inclusivity. Our sample included individuals from diverse socioeconomic backgrounds across multiple neighborhoods in Ceará. While participation was open to all adults within the targeted areas, we recognize that the inclusion of certain vulnerable populations—such as individuals from low-income communities disproportionately affected by crime and policing—posed additional ethical considerations. To address these, we ensured that participation was fully voluntary, provided clear explanations of the study's objectives, and took extra precautions to protect respondents' confidentiality, particularly when discussing their interactions with security forces and organized crime.

Potential differential harms and benefits. We recognize that research on security and policing can have uneven impacts on different groups. By shedding light on patterns of crime victimization and state responses, our study may contribute to policy discussions that could ultimately benefit affected communities. However, we were also mindful of the risks that research findings could be misused or lead to unintended consequences, such as increased policing in already over-policed communities. We sought to mitigate this risk by framing our research findings responsibly and engaging with local stakeholders to ensure that our results contribute to informed and ethical policy debates.