Building Capacity from Within: Selective Segmentation and Police Reform in Latin America

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Abstract

Democracies in the Global South often face extreme violence alongside police forces that are ineffective, politicized, and abusive. We argue that governments can overcome dysfunctional policing through selective segmentation: creating new policing units with distinct recruitment, incentive, and oversight regimes that are insulated from dysfunctional parent institutions. We study this strategy in Ceará, Brazil, examining the Rapid and Visible Response Patrols (RAIO), a selective rapid-response squad. Using a staggered difference-in-differences design, we find that RAIO reduced homicides by 57 percent and robberies by 84 percent, with arrests falling alongside crime—a pattern consistent with deterrence as the primary operating mechanism. A survey of 2,000 residents with an image-based conjoint experiment shows that RAIO officers are viewed as more effective, less corrupt, and less abusive than conventional police. RAIO did not increase police killings and yielded electoral rewards for the incumbent governor, suggesting that selective segmentation can improve security while creating political incentives to sustain reform.

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INTRODUCTION

Democratic institutions are designed to constrain arbitrary violence, but in many developing countries these institutions coexist with the world's highest homicide rates and least effective police forces. For example, Latin America accounts for roughly one-third of global homicides, despite representing just 8 percent of the world's population, with notoriously high levels of police violence. Brazil reflects this contradiction on a massive scale: its 400,000 officers place it among the globe's largest police forces, yet the country suffers annually nearly 40,000 homicides and more than 6,000 police killings. This paradox is not unique to Latin America. Across many democracies in the Global South, formal institutional checks coexist with pervasive insecurity and violent policing. In South Africa, for example, the murder rate exceeds 40 per 100,000 inhabitants—nearly seven times the global average—while the police kill more than a thousand civilians each year. In the Philippines, successive democratically-elected administrations have relied on extrajudicial executions as de facto crime control policy. Across these diverse settings, law enforcement institutions are not only sources of abuse and corruption but are also profoundly ineffective at deterring and solving crimes.

These failures reflect deeper institutional weaknesses that extend beyond policing to the broader justice and security sector. In many violent democracies, prosecutors, judges, prison officials, and police face daunting structural constraints: criminal infiltration, politicized hierarchies, chronic under-resourcing, and weak internal accountability. Executives are called upon to deliver public goods like citizen security, yet the bureaucracies charged with deploying and controlling violence routinely lack both the capacity and the incentives to do so. While these challenges characterize coercive institutions most starkly, they reflect broader problems of bureaucratic reform under institutional weakness. Policing is the frontline manifestation of this dilemma, and the focus of this paper.

One increasingly common strategy to manage this challenge involves the creation of new, insulated, high-capacity units within or alongside existing coercive institutions, what we call selective segmentation. Rather than attempting to transform an entire bureaucratic apparatus that is politically captured or organizationally inert, executives may choose to build professional enclaves that operate under distinct recruitment, incentive, and accountability regimes. Segmentation allows governments to alter the composition, incentives, and oversight of a subset of agents while leaving the rest of the institutional landscape largely untouched. For policing, these new units can concentrate skilled personnel, impose stricter discipline, enhance visibility and monitoring, and reduce exposure to corrupt networks—thereby generating credible coercion and credible restraint at the same time.

Segmentation works through a set of interlocking mechanisms. First, selective recruitment and motivational differentiation change who polices and why: new units attract officers committed to rigorous professional norms rather than rent-seeking. Second, organizational insulation—via distinct command structures, higher pay, and disciplinary autonomy—protects enclaves from the corruption and political interference that afflict existing forces. Third, heightened visibility and external scrutiny strengthen accountability, ensuring that misconduct carries reputational and political costs for officers and executives alike. Fourth, segmentation introduces competitive emulation within the broader policing ecosystem: by setting new standards of performance and legitimacy, these units may trigger imitation, pressure, and eventual integration of new, better practices by other police divisions. Finally, the sheer concentration of skilled officers and rapid-response capacity enhances credible deterrence within the new force, recalibrating the enforcement calculus vis-a-vis criminals without resorting to indiscriminate repression.

This paper studies one such attempt to professionalize coercive capacity through selective segmentation: the creation of a new policing squad—the Rapid and Visible Response Patrols (RAIO)—in Ceará, Brazil. Rolled out amid soaring homicide rates, RAIO was built as a semi-autonomous motorcycle-based unit that embodied the logic of segmentation. Officers were drawn from existing forces, screened for disciplinary records, received an additional 280 hours of training, were paid roughly 30% more, and operated under strict real-time monitoring. RAIO therefore offers a clear test of whether segmentation can generate security gains while avoiding the abuses and political problems that so often plague traditional efforts to reform coercive institutions.

Our empirical analysis combines multiple sources of data and methods to isolate the causal effect of RAIO. Using a staggered difference-in-differences design that harnesses the phased roll-out of RAIO across Ceará's 184 municipalities, we estimate that the deployment of this new squad reduced homicides by roughly 57 percent and robberies by 84 percent, equivalent to 1.3 and 17 fewer incidents per municipality per month, respectively, when compared to not-yet-treated municipalities. Arrests fell modestly, suggesting deterrence rather than mass incarceration as the core mechanism contributing to crime reductions. To probe mechanisms related to credible deterrence, we fielded a survey of 2,000 residents in Fortaleza that included an image-based conjoint experiment. The results show that signals of rapid response increase perceived effectiveness, while the RAIO uniform—more than weaponry—drives perceptions of restraint and integrity, marking professional differentiation from the ordinary police. Focus groups with both RAIO officers and ordinary military police agents provide complementary and confirmatory qualitative evidence for mechanisms related to changes in officer quality, organizational insulation, and accountability.

Finally, we assess whether selective segmentation is politically feasible. We show that the expansion of RAIO generated electoral gains for the incumbent governor, indicating that voters rewarded effective crime reduction at the ballot box. Crime gains were also achieved without any increase in police killings, and the program's social benefits substantially exceeded its costs, suggesting that selective segmentation can deliver security gains while remaining fiscally and normatively sustainable.

Our paper makes several contributions to the study of bureaucratic change in weak institutional environments more generally, and to the study of police reform in particular. First, we introduce and test the concept of selective segmentation: the deliberate creation of new, semi-autonomous units that operate under distinct recruitment, incentive, and oversight regimes within otherwise weak bureaucracies. Existing work emphasizes internal reforms—such as retraining, accountability mechanisms, or leadership changes (Bailey and Dammert 2006; Goldsmith 1990). Yet these approaches often struggle when confronting entrenched interests, patronage networks, and deeply-rooted informal practices. We theorize selective segmentation as an alternative path and show empirically that it can improve performance, reducing homicides and robberies, while preserving accountability, as it did not increase arrests or police killings.

Second, we show empirically how institutional segmentation can enable reformers to bypass resistance within entrenched bureaucracies. Our theoretical framework identifies the conditions under which new, specialized agencies can complement or outperform legacy institutions, rather than merely duplicating their functions. Evidence from sectors beyond policing suggests that this logic travels. In Nigeria, the use of specialized courts—combined with selective case designation and assignment—reduced average case duration from eight to four years, even as corruption remained pervasive (World Justice Project 2023). By contrast, Ghana's revenue authority reform highlights the limits of formal autonomy alone: although the creation of a semi-autonomous tax agency improved collection, these gains were driven less by legal independence than by accompanying changes in recruitment, management practices, and organizational culture (Joshi and Ayee 2009). Together, these cases underscore that segmentation can generate incremental institutional change across policy domains, but that its success depends on complementary mechanisms—such as selective recruitment, organizational insulation, performance incentives, and external accountability—rather than autonomy in isolation.

Third, we contribute to research on the political economy of policing by showing that successful segmentation reshapes the political rewards of reform. Consistent with scholarship linking security improvements to electoral success (e.g. Levitt 1997; Ferejohn 1986), municipalities benefiting from RAIO deployments displayed stronger support for the incumbent governor who expanded the program. This implies that segmentation can align political incentives with effective governance, generating political returns that can help sustain these reforms over time.

Finally, our findings speak to broader debates on state-building and bureaucratic professionalization. Rather than assuming that institutional transformation must occur system-wide, we highlight how constrained governments can build capacity from within by creating credible, well-insulated pockets of effectiveness. In this sense, we draw inspiration from Geddes (1994), who shows that leaders can construct "bureaucratic enclaves" within otherwise dysfunctional states—spaces that perform well precisely because they are shielded from the surrounding institutional environment. We build on this insight and apply it to violent democracies: new policing squads can become protected domains within which credible coercion and credible restraint can coexist. Selective segmentation therefore offers a general framework for understanding how incremental reform and professionalization can emerge even in contexts of chronic institutional weakness.

Conceptual framework

The ability to enforce the law is a defining attribute of statehood (Weber 1946; Tilly 1985) and effective crime control is central to the practice of democratic governance. Citizens experience the state primarily through their interactions with the police (Lipsky 1980), and their sense of safety and justice depends on whether those interactions convey competence and fairness (Tyler 2006; Abril et al. 2023). Yet the same coercive power that enables the state to reduce crime can also erode citizens' rights and public trust. Accountability—through legal constraints, civilian oversight, and political responsibility—helps ensure that force is exercised on behalf of citizens rather than against them. The absence of accountability, on the other hand, transforms policing into a vehicle for social domination, partisan control, or private enrichment. The challenge, therefore, is not only to make police more effective at combating crime–itself a difficult task in weak democracies–but also to ensure that its coercive power remains lawful and accountable to the public.

Over the past several decades, scholars and policymakers have explored multiple strategies to resolve this tension. Existing approaches can be grouped into four broad logics: professionalization from within, demilitarization and community-oriented policing, militarization and coercive substitution, and segmented reform through new or parallel units. Each of these approaches is grounded in different assumptions about where dysfunction in policing institutions resides and how organizational change can occur and the empirical record for these strategies is mixed, revealing the enduring difficulties of reconciling effectiveness with accountability.

The first logic—professionalization from within—assumes that bureaucratic decay results from insufficient incentives, poor training, and weak oversight, rather than from more structural deficiencies or political capture. Reformers in this school target internal norms, promotion systems, and disciplinary mechanisms to produce more competent and rule-bound organizations (Bailey and Dammert 2006). Evidence from Chile's post-transition Carabineros, which adopted new training curricula and stricter internal oversight, and from Colombia's Policía Nacional, which expanded professional academies and incorporated human rights modules into its promotion system, suggests that professionalization can improve procedural compliance and managerial efficiency (e.g. Ungar 2011; González 2020). Yet in places where organized crime permeates state institutions and local economies (Dal Bó, Dal Bó and Di Tella 2006), and where patron-client networks link politicians, police, and traffickers, these professionalization efforts may leave patterns of violence and impunity relatively untouched. The underlying supposition that hierarchical retraining can realign incentives wholesale in highly criminalized environments has often proven to be overly optimistic.

A second logic—demilitarization and community policing (Blair et al. 2021)—locates failure not within bureaucratic structures but rather the broken social contract between police and citizens. By embedding officers more deeply in local neighborhoods and prioritizing unbiased and neutral treatment through procedural justice interventions, for example, these programs aim to replace fear-based compliance with elective cooperation (Tyler 2006; Mazerolle et al. 2013; Abril et al. 2023). The highest quality impact evaluations of community policing interventions, however, have shown null effects (Blair et al. 2021): when police lack credible coercive capacity, face criminal retaliation, or confront acute institutional instability, the promise of participatory engagement collapses. Procedural justice interventions have shown greater promise in changing citizen perceptions and increasing trust in the police, although in the developing world they have struggled to reduce crime (e.g. Abril et al. 2023). The demilitarization and community policing approach assumes that legitimacy can precede order—an assumption that may be unrealistic where citizens face credible threats of retaliation from criminal groups or corrupt officers, making cooperation with law enforcement a life or death decision.

Militarization and coercive substitution – commonly referred to as *mano dura* – is the third logic, and locates failure in the weakness or corruption of civilian law enforcement itself. When police are viewed as incapable or compromised, governments often turn to soldiers or heavily armed police units to impose order through overwhelming force (Flores-Macías 2018).

Such deployments offer visible action and short-term political credit (Holland 2013), especially amid acute insecurity. Yet the empirical record shows that they rarely produce sustained crime reductions and often generate new pathologies. Evidence from Mexico, Brazil, and Colombia points to limited effects on violence alongside increased human rights abuses and weakened civilian institutional legitimacy (Magaloni and Rodriguez 2020; Blair and Weintraub 2023). Militarization can also reshape security institutions themselves: soldiers lack training in civilian policing, while police units adopt combat-oriented practices that reinforce a warrior ethos. Over time, coercive substitution risks entrenching support for military policing even when ineffective, undermining reform and weakening democratic oversight of the use of force (Blair, Mendoza Mora and Weintraub 2025).

Creating new policing squads or parallel forces, a fourth and final approach, views the existing police as irredeemably compromised and aims to rebuild state coercive capacity through alternative organizational channels. Governments sometimes create highly specialized units with narrow mandates—such as Colombia's GAULA, which focuses exclusively on kidnapping and extortion, or counterterrorism task forces—designed to deliver technical expertise in tightly circumscribed domains. While effective at performing targeted tasks, these forces by design do not alter routine policing or broader patterns of state–citizen interactions. By contrast, the strategy that we analyze here involves segmented forces with far broader mandates, performing general law enforcement functions rather than tending to narrow tactical missions. These units are intended not merely to fill technical gaps but to help sidestep a host of organizational problems that characterize legacy institutions. The wager is that new, better-paid, and insulated units can exercise credible coercion with greater restraint than their pre-existing counterparts. While segmented institutions may offer a partial escape from bureaucratic inertia and criminal capture, we know little about when they build state capacity and legitimacy and when they simply reproduce the abuses, corruption, or politicization of the agencies they replace. We therefore build a theory of how segmented policing units can help resolve the tension between expanding coercive capacity and maintaining democratic accountability.

A THEORY OF SELECTIVE SEGMENTATION

When comprehensive reform is politically blocked, organizationally infeasible, or when leaders need quick wins, governments may turn to selective segmentation: creating small, insulated, and carefully screened sub-units within or alongside existing security institutions. The logic of building pockets of effectiveness within otherwise dysfunctional states has antecedents across multiple scholarly traditions. Geddes (1994) shows that leaders can construct "bureaucratic enclaves" that function effectively, despite hostile institutional environments, by shielding key agencies from patronage pressures and grounding them in meritocratic recruitment. Evans (1995) deepens this account, arguing that high-performing state agencies combine "embedded autonomy": simultaneously insulated from particularistic political pressures, yet embedded in the social and professional networks that generate information, legitimacy, and cooperation. Tendler (1997) shows why this combination works in practice—drawing on programs in Ceará itself—when frontline workers develop a strong professional mission reinforced by symbolic differentiation and community visibility, provided the central state actively protects them from local patronage networks. Finally, Honig (2018) adds that in complex environments formal monitoring and principal-agent accountability systems can undermine performance, crowding out frontline discretion and organizational autonomy that–along with mission-driven culture–are necessary for effective delivery. Together, these traditions suggest that selective segmentation might succeed when it combines insulation from political capture with embedded social accountability, mission-driven identity, and frontline discretion.

These insights motivate five mechanisms through which segmentation can reconcile policing effectiveness and accountability in violent democracies. First, we argue that segmentation can enhance credible deterrence. Concentrating skilled officers and resources into agile, high-visibility units increases the perceived probability of apprehension and the speed required to sanction criminals, even if total arrests remain modest. Classical deterrence theory (Becker 1968) and subsequent criminological work (Kleiman 2009) emphasize that the certainty and swiftness of punishment matter far more than either the volume of punishment produced or its harshness. This recalibration of the enforcement calculus can deter violent crime while avoiding the excesses of indiscriminate or large-scale repression. It also provides powerful incentives for politicians to create these squads in the first place, as the returns to crime reduction may produce quick improvements in public safety and perceptions of safety that are so important to elected officials.

Second, segmentation changes who polices and why. Positive selection and motivational differentiation jointly determine the internal composition and ethos of new policing units. Selective recruitment filters out officers most prone to corruption or abuse, improving the average quality within the new sub-unit relative to existing institutions. By suspending bureaucratic rigidities that normally govern hiring, promotion, and dismissal, reforms can purposefully select motivated and high-capacity officers. Because participation in these forces is usually voluntary and competitive, selection itself signals commitment to higher standards. Motivational differentiation deepens this effect: enhanced pay, career advancement, and symbolic prestige shift incentives toward professionalism and away from rent-seeking. This motivational filtering yields officers who internalize norms of restraint and responsiveness, features that are often absent in institutions marked by low morale, insufficient pay, and high levels of political interference.

Third, segmentation protects these enclaves through organizational insulation, shielding them from the perverse incentives that afflict legacy organizations. Insulation operates through several channels, including distinct command structures and disciplinary autonomy that allow units to impose meaningful sanctions on officers who commit abuses without political interference. It also involves securing separate intelligence flows to prevent information leakage to criminal networks or compromised commanders, as well as maintaining independent logistics and procurement processes. Because misconduct spreads through peer influence (Ouellet et al. 2019; Holz, Rivera and Ba 2023), isolating new units from existing social networks helps curb the diffusion of corruption, abuse, and poor policing practices.

Fourth, segmentation strengthens accountability by increasing visibility and enabling credible sanctioning. New units are typically created under intense public scrutiny and equipped with enhanced oversight tools—such as body-worn cameras, transparent disciplinary systems, or real-time monitoring—raising the likelihood that misconduct is detected and punished. Visibility generates both reputational and political constraints: executives may claim credit for improved security but also incur electoral or reputational costs if abuses occur within these units (Ferejohn 1986; Carreras and Visconti 2022). Segmentation further facilitates discipline through an internal "firing" margin, allowing officers who violate conduct rules to be removed from the sub-unit. In principal–agent terms, segmentation converts an opaque bureaucracy into a more monitorable and politically salient agent.

Finally, segmentation may induce competitive emulation, equivalent to a "race to the top." The creation of a selective, high-performing unit has the potential to trigger status and performance rivalries vis-a-vis competing forces. Ordinary police, facing constant comparisons with a new unit's professionalism, may fear reputational and material consequences such as budget reallocations, loss of prestige, or even demotion if they lag markedly behind their newly-established police counterparts. This dynamic can generate upward convergence, creating the possibility of gradual institutional improvements in *other* policing institutions, beyond the enclave itself.

Not all efforts to create new units succeed, of course. Some new policing squads quickly devolve into repressive or politicized forces. El Salvador's Grupos de Reacción Policial illustrate how elite branding without credible restraint through oversight may produce human rights violations (Puerta 2018). Our theory about professionalization by segmentation, however, specifies the scope conditions under which this strategy is most likely to succeed. Selectivity must be credible and enforced: screening and ongoing monitoring must truly exclude and remove corrupt or violent officers. The unit's mandate and size must be narrow enough to preserve cohesion and autonomy, while avoiding mission creep that could dilute core functions. Distinctiveness must be maintained both symbolically and operationally: if the unit becomes indistinguishable from ordinary forces, any deterrence and legitimacy gains that segmentation confers are likely to dissipate over time. And finally, political sponsorship must combine autonomy with oversight: executives must adequately fund and protect the unit's independence without walling it off from accountability mechanisms that might otherwise produce impunity. When these conditions are not met, segmentation may well degenerate into duplication (creating coordination problems), capture (leading to partisan policing), or outright abuse (resulting in widespread repression and eroded legitimacy).

Comparative experience underscores this delicate balancing act, and how often it can be upset. In Mexico, for instance, the rapid expansion and militarized subordination of the Federal Police under President Felipe Calderón—and later the creation of the country's National Guard—show how segmentation can slide into re-militarization, undermining civilian accountability and institutional coherence (Oñate and Ricart 2024). Similar dynamics emerged in Nigeria, where special tactical squads such as the SARS, initially designed to professionalize policing, devolved into predatory and violent behavior once oversight weakened (Okereke et al. 2021). Sometimes these units are dismantled prematurely due to political turnover, fiscal constraints, or backlash from entrenched interests. By contrast, forces that maintained highly selective recruitment within genuinely circumscribed mandates — such as Colombia's GAULA, focused exclusively on kidnapping and extortion — demonstrate that segmentation can, under constrained conditions, yield effective professionalization.

Brazil's own experience illustrates these dynamics. Rio de Janeiro's BOPE was designed from the outset as an elite tactical unit optimized for high-risk urban warfare. It developed a genuine operational reputation in that domain, but accountability was never part of its institutional DNA (Human Rights Watch 2009; Soares, Batista and Pimentel 2006).

Rio's Pacifying Police Units (UPP) attempted a more deliberate segmented reform through community policing enclaves in favelas and showed early promise. But the UPP suffered from several failures our theory anticipates: political incentives drove territorial overextension; the unit was composed of newly recruited officers selected and trained with very basic standards, and no accountability system insulated police officers from the corrupt command structures of the broader Military Police (Ferraz, Monteiro and Ottoni 2024). The program effectively collapsed after 2016, its officers increasingly indistinguishable from the ordinary forces they had been designed to replace. São Paulo's ROTA (Rondas Ostensivas Tobias de Aguiar), meanwhile, has maintained formal selectivity for decades but without comparable accountability mechanisms or insulation from politically-motivated deployment. Its command structure has historically been permeable to direction from state-level security secretaries, and its operational mandate has repeatedly expanded into mass repression functions during periods of unrest, producing sustained lethality rather than professional restraint (Human Rights Watch 2009; Caldeira 2000). Together, these cases underscore that insulation is necessary but not sufficient for successful segmentation. RAIO's design — with its stable sixteen-year command, distinct chain of authority, credible internal removal mechanism, and explicitly limited mandate — has, thus far, avoided each of these failure modes. Whether it can continue to do so as it scales remains an open and important question.

These examples also clarify the broader scope of our theory. Segmentation is most likely to occur and ultimately to succeed in violent democracies: states that combine electoral competition and formal civilian control with persistently high crime, low police legitimacy, and limited bureaucratic capacity. These are environments where executives face strong public pressure to deliver security but possess inadequate institutional tools to do so, and where wholesale reform tends to be blocked by entrenched interests within law enforcement agencies. Under these conditions, building insulated enclaves of professionalism is a politically feasible path for reform because it represents credible demonstration of state capacity that can be delivered within a single electoral cycle. Our theoretical argument is less applicable in consolidated democracies, where police bureaucracies are already professionalized and accountability mechanisms are strong, or in consolidated autocratic settings, where segmentation risks reinforcing repression rather than introducing restraint. In short, professionalization by segmentation is a strategy for flawed democracies trapped between violence and institutional weakness: strong enough to create new institutions, yet unable to successfully transform old ones.

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 forces such as BOPE, ROTA, and SWAT in Brazil and elsewhere, which primarily rely on cars. Motorcycle-based patrols can be especially effective in dense urban areas, as they provide access to narrow corridors inaccessible to larger vehicles and allow rapid entry and exit from high-crime locations, particularly in informal settlements and *favelas* (SSPDS-CE 2023). RAIO patrols are always comprised of four individuals and three motorcycles, with one officer positioned in the passenger seat, equipped with an assault rifle.[1] By making a significant show of force, the theory goes, RAIO should diminish the need to resort to its use.

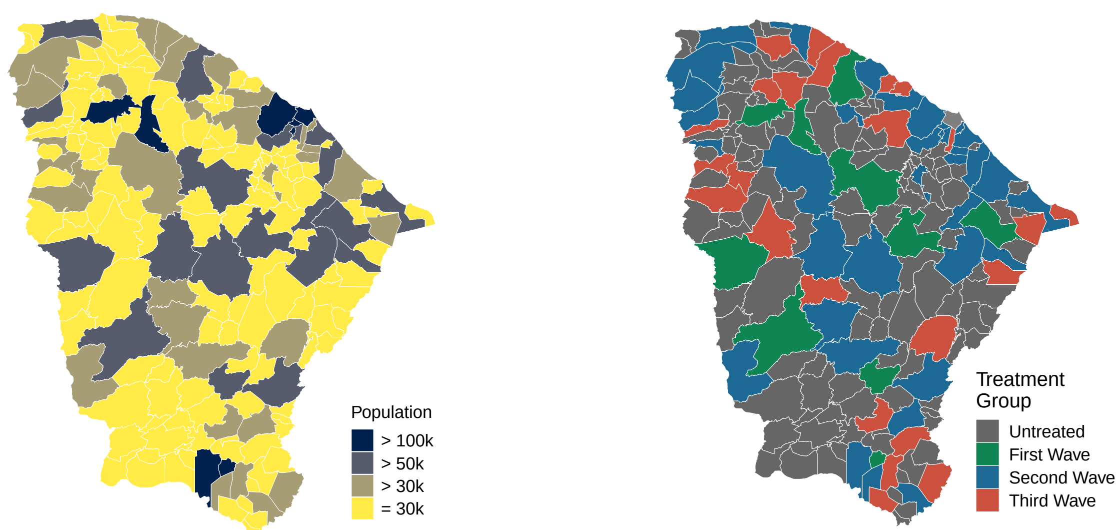
Deployed as a complement to other police squads, RAIO is often used to saturate an area when there is an emergency, or when specific neighborhoods experience sharp increases in violence. On regular days, RAIO commanders analyze crime patterns and plan patrols given maps indicating where homicides and property crime are geographically concentrated. Recruited exclusively from the military police, RAIO officers undergo a rigorous selection process, receive superior training, benefit from a more flexible work schedule, and enjoy better compensation when compared to their military police peers. (We discuss selection and incentives for RAIO officers in detail in the Mechanisms section.) RAIO has its own commander, who reports to the general commander of the Military Police. Unlike other Brazilian forces like

[1] They use a carbine, a slightly shorter and less powerful weapon than a rifle. For ease of reference, we refer to this as an assault rifle.

UPP or BOPE—which were embedded within broader militarized logics—and ROTA, which operates without comparable insulation from legacy structures, RAIO combines elite specialization with organizational separation, allowing it to alter internal incentives without relying on system-wide reform.

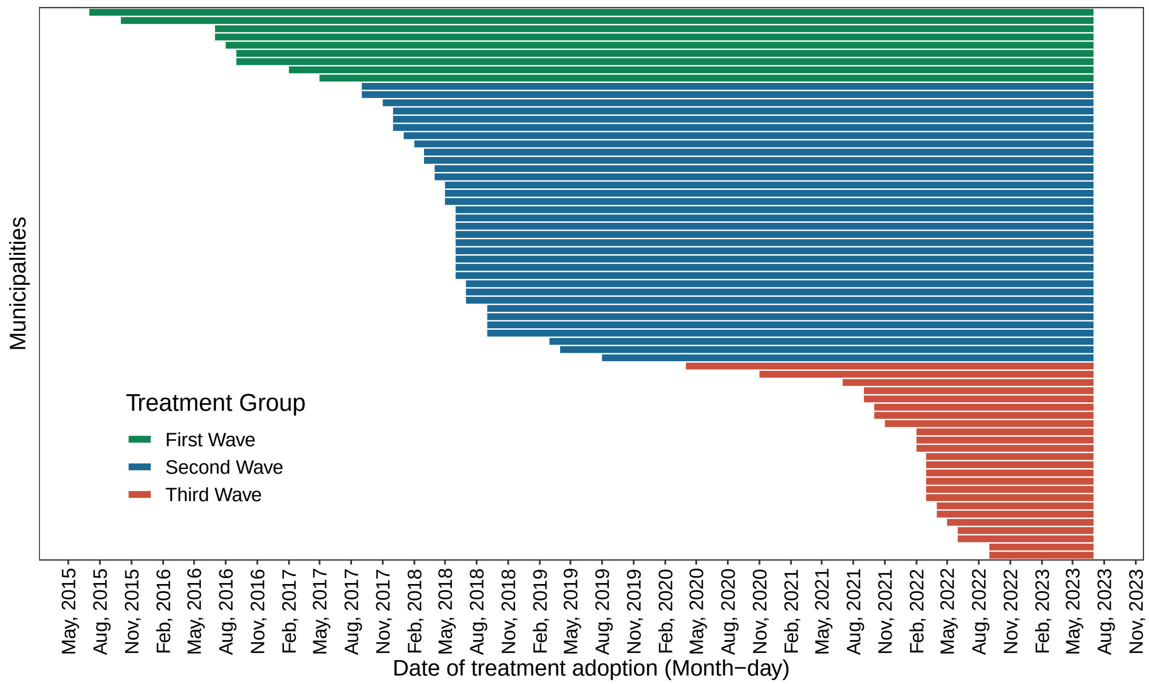
While RAIO initially operated exclusively in Fortaleza, Ceará's capital, the governor expanded the force to other municipalities in Ceará in 2015. The first phase of the expansion encompassed the state's largest municipalities, including Juazeiro do Norte, Sobral, and Quixadá (9 bases in total). The 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 bases), while a fourth phase has included all municipalities with more than 25,000 inhabitants (18 bases) (SSPDS-CE 2021). Figure 1 shows the temporal distribution of the RAIO expansion, while Figure 2 illustrates the geographic distribution of Ceara's population in the left panel (darker areas are more populous) and treatment phases in the right panel[2].

Figure 2: **Population and treatment rollout cycles**



[2]Table B.4 reports simple pre–post differences in crime outcomes across phases, showing sizable declines following the introduction of RAIO. These naive comparisons mirror the type of descriptive evidence available to policymakers at the time and help rationalize the sequential expansion of the program: initial improvements in larger municipalities (Cycle 1, above 100,000 inhabitants) were followed by rollout to mid-sized municipalities (Cycle 2, above 50,000) and subsequently to smaller localities (Cycle 3).

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



The demand for this force in Ceará was driven by an acute security crisis in the mid-2010s. Homicide rates began to increase in 2010, reaching 77 per 100,000 inhabitants in Fortaleza in 2014, the highest among all Brazilian state capitals. The state experienced a series of high-profile violent events in this period (Silva 2024). In 2015, the state prosecutor's office indicted 45 policemen for killing 11 citizens, which became known as the Messejana massacre. In 2016, a major prison crisis culminated in gang members placing a car bomb outside the state assembly to intimidate legislators considering a law to install mobile signal blockers in prisons.

Criminal dynamics shifted in 2016 with the emergence of the drug faction Guardians of the State (GDE), which challenged Comando Vermelho (CV) for territorial control and later allied with the Primeiro Comando da Capital (PCC), effectively importing Brazil's largest inter-faction rivalry into Ceará. Small groups of criminals gave way to sophisticated drug factions, which began to demarcate territories of control, actively recruit young people, and violently clash (Paiva 2019; 2022). Municipalities such as Caucaia and Maracanaú, contested by GDE and CV, exhibited homicide rates of approximately 100 per 100,000 residents in 2017. Amid mounting public safety challenges, the governor faced substantial pressure to act, making RAIO expansion a central element of his security strategy (Silva 2024).

Brazil may appear to be an unusual setting to study these questions, raising external validity concerns. Unlike many Latin American countries with more centralized policing institutions, Brazil's primary street-level force is militarized and organized at the state level. This architecture creates both unusual constraints and opportunities for reform. Militarization and scale heighten the potential harms from coercive capacity and can make comprehensive institutional overhaul politically and administratively difficult. Yet state-level control and large personnel pools may make selective segmentation more feasible: governments can create insulated sub-units with distinct recruitment, incentive, and oversight rules while leaving the broader organization formally intact. Importantly, these advantages may not travel to settings with highly centralized police hierarchies, limited personnel pools, or frequent leadership turnover that could undermine doctrinal enforcement. We therefore treat Ceará as a hard test for whether segmentation can deliver public safety gains without increasing lethal force, while emphasizing that the generalizable components of the strategy are primarily organizational (insulation, selective recruitment, internal accountability, and political shielding).

Data

Quantitative data

To study the effect of RAIO's expansion on crime, we collect data from the state's 184 municipalities from 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 in Ceará, including homicides, robbery, theft, sexual abuse, and drug possession. 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, which notes a 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 thresholds, 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. Because the rollout followed a population criterion, treated municipalities are larger and have higher GDP per capita and employment than control municipalities.

Table 1: **Descriptive statistics by treatment phase**

	Fortaleza (N=1)		Phase 1 (N=9)		Phase 2 (N=34)		Phase 3 (N=24)		Control (N=116)	
	2015	2020	2015	2020	2015	2020	2015	2020	2015	2020
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	823,674	754,360	17,749	16,947	10,662	10,277	2,740	2,633	1,143	1,064
Rate	31,788	28,260	12,744	11,945	13,441	12,589	8,124	7,516	7,185	6,461
Population	2,591,188	2,669,342	118,161	121,316	74,576	76,638	35,153	35,888	16,255	16,573
Per capita GDP	22,079	24,411	9,818	13,340	11,824	17,377	7,969	12,474	6,530	9,447
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

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 battalion (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.

Qualitative data

To complement our quantitative analyses and examine the mechanisms underlying selective segmentation, we conducted four focus groups with police personnel in Fortaleza in January 2026, including 16 RAIO officers and 16 conventional Military Police officers.[3] Participants were recruited through official channels, but discussions were conducted independently and under conditions of anonymity to encourage candid responses.[4] Each session lasted approximately 1.5 hours and explored recruitment and selection processes, internal disciplinary practices, operational routines, inter-unit relations, and perceptions of professional identity and accountability. The focus group script (translated into English) appears in Appendix **??**

Empirical strategy

We aim to identify changes in crime directly attributable to the implementation and rollout of the RAIO program. In an ideal setting, RAIO patrols would be randomly assigned across municipalities, generating exogenous variation in exposure to estimate causal effects on crime. Because random assignment was not feasible, we exploit the staggered rollout of RAIO squads across 184 municipalities in Ceará to estimate a difference-in-differences model and assess their impact on crime and police activity.[5] Our identification strategy compares before-and-after changes in crime outcomes between municipalities that received a RAIO squad in a given month and those that had not yet been treated.

Because RAIO draws personnel from the existing Military Police, its deployment reallocates high-performing officers towards RAIO within treated municipalities. Our estimates therefore capture the net effect of segmentation as implemented, rather than increasing total policing resources. Any observed reductions in crime are, therefore, unlikely to be mechani-

[3] Conventional police refers to *Policiamento Ordinário Geral* (POG), officers assigned to regional batallions who primary respond to emergency calls.

[4] Participants were nominated by their units to ensure variation in rank and experience among those who happened to be on duty on the day focus groups occurred. Participants' experience in the ordinary military police ranged from 1 to 32 years, while in RAIO groups the range was 2 to 15 years of service.

[5] In Appendix C. we present as robustness an alternative approach using a synthetic difference-in-differences Arkhangelsky et al. (2021).

cally driven by staffing increases alone.

The key identification assumption is that, absent the rollout, crime trends in municipalities with and without RAIO squads would have evolved in parallel. While this assumption is not directly testable, we assess its plausibility by examining pre-treatment trends. A related concern is reverse causality: deployments might respond to crime levels. This is unlikely, as the staggered rollout was determined by population thresholds rather than crime rates, and the precise timing of base openings depended on logistical factors—such as recruitment, training capacity, and infrastructure readiness—rather than contemporaneous crime. Finally, although municipalities above the threshold may anticipate eventual treatment, deployment decisions are centralized at the state level, limiting scope for anticipatory behavior.[6]

We estimate our main results using the estimator proposed by Callaway and Sant'Anna (2021) (hereafter CS), which yields consistent estimates in settings with staggered treatment adoption and heterogeneous treatment effects across municipalities (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).[7]

We estimate treatment effects using both never-treated and not-yet-treated municipalities as control units (Callaway and Sant'Anna 2021).[8] Our preferred specification relies on using

[6]Although the population threshold recommends a regression discontinuity design (RDD), we do not use it for several reasons: treatment is implemented in a phased rollout conditional on the running variable, restricting RDD estimates to individual phases that cannot be aggregated into an overall ATT; statistical power is severely limited for earlier phases; non-compliance restricts identification to an ITT rather than ATT; and within-phase estimates are especially vulnerable to geographic spillovers. CS is therefore our preferred design.

[7]We use the doubly robust CS estimator rather than alternative two-way fixed effects approaches (De Chaisemartin and d'Haultfoeuille 2020; Borusyak, Jaravel and Spiess 2021; Sun and Abraham 2021; Gardner 2022). The doubly robust estimator combines outcome regression and inverse probability weighting, requiring the correct specification of at least one of the two components (the outcome model or the propensity score model) to recover unbiased treatment effects (Sant'Anna and Zhao 2020).

[8]In Appendix A. we highlight the different assumptions required for each approach. Using not-yet-treated units combined with the no anticipation assumption means not allowing for heterogeneous pre-treatment trends across groups of municipalities. The no-anticipation assumption is plausible in our context given the specific features of the RAIO rollout, as discussed above. For robustness, we relax this assumption in Appendix B. by allowing for anticipation windows of one, three, and six months, following Callaway and Sant'Anna (2021). The results show that while the magnitude of some coefficients varies across specifications, our conclusions regarding the effect of RAIO on crime remain consistent.

not-yet-treated municipalities as our control units. This choice reflects the substantial heterogeneity between treated and never-treated municipalities: the former are considerably more populous and urbanized.

While the four deployment phases described above capture broad waves of municipal expansion, the CS estimator operates at a finer level of temporal resolution: it defines a group as the set of municipalities that first received a RAIO base in the same calendar month. Within each phase, bases opened across several distinct months, so a single phase encompasses multiple CS groups. In our application, g indexes the month of first treatment, and t indexes any calendar month in the sample period, before or after treatment. The CS estimator computes the average effect of treatment on the treated (ATT) for each group of municipalities first exposed to treatment in the period g, comparing them to municipalities that have not yet been treated by time t. Formally, this effect can be expressed as:

$$ATT(g,t) = \mathbb{E}\left[(Y_t(1) - Y_t(0)|G_g = 1\right], \tag{1}$$

where $Y_t(1)$ and $Y_t(0)$ denote potential outcomes with and without treatment, and $G_g = 1$ identifies the municipalities first treated in period g. This parameter represents the effect of RAIO at time t for municipalities first treated in group g, using not-yet-treated municipalities as the comparison group.

The estimator produces one $ATT(g,t)$ for each combination of group g and time t, allowing us to explore how the effect evolves both across groups and over time. To verify the validity of our identification strategy and test for pre-trends, we assess dynamic treatment effects by length of exposure, using the event-study aggregation proposed by Callaway and Sant'Anna (2021), which assigns appropriate weights to avoid the bias inherent to TWFE:

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

where τ denotes the total number of treatment periods, and $e = t - g$ measures time relative to treatment. This expression represents the average effect of the treatment e periods relative to adoption, averaged across all groups of municipalities that are ever observed in the sample for at least *e* periods after treatment. Negative values of *e* are used to assess parallel trends, while positive values capture post-treatment dynamics. Hence, we do not examine units treated for exactly *e* periods, but rather average the ATT for each group over its first *e* periods of exposure. Our event-study figures plot $\theta_{es}(e)$ across different values of e.

A final potential threat to identification is the presence of spillover effects in municipalities without RAIO squads. Staggered rollout designs assume that control units are not affected by the treatment assignment of other units, yet residents in non-treated municipalities may in fact experience shorter police response times due to proximity to RAIO bases, for example. To address these potential indirect effects, we construct a time-varying continuous measure of spatial exposure to treatment: for each municipality, we compute the minimum driving time between its centroid and the nearest RAIO base, which varies over time, including for municipalities that themselves never receive a base. We explore the sensitivity of our results to including this proximity measure as a control variable, allowing us to evaluate whether our baseline results are driven by indirect exposure.

RESULTS

Table 2 shows the average effect of RAIO on crime, using the CS and TWFE estimators.[9] We find substantively large and statistically significant decreases in monthly homicides, robberies, thefts, and arrests due to RAIO, which are robust to the specification of different control groups—both the not-yet-treated and never treated—when using CS. These are economically important decreases 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

[9] The Callaway and Sant'Anna (2021) estimator requires covariates that vary within units over time. Because key controls such as GDP and employment are only available annually, and repeating annual values across months would introduce perfect multicollinearity once municipality and time fixed effects are included, we report estimates without covariates, consistent with identification based on within-municipality changes in treatment timing.

reports per municipality).[10]

Appendix Table B.6 reports estimates that account for spatial spillovers. Treatment effects remain qualitatively similar to the baseline results, indicating that crime reductions are not driven solely by displacement or indirect exposure to nearby RAIO bases. For some offenses, treatment effect sizes increase, consistent with RAIO influencing the broader criminal environment beyond treated municipalities' boundaries.

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)	(2.298)	(3.370)	(0.744)
Police Killings	-0.078	-0.081	0.016	0.022	0.035	0.014
	(0.086)	(0.084)	(0.014)	(0.197)	(0.207)	(0.191)
Robberies	-17.349***	-17.207***	-7.605***	7.982	20.599	0.584
	(5.07)	(5.08)	(1.303)	(27.579)	(42.794)	(1.927)
Thefts	-5.173***	-5.061***	-3.007***	10.997	25.449	2.525
	(1.924)	(1.917)	(0.750)	(23.893)	(34.424)	(3.274)
Bank Robberies	0.012	0.000	-0.002	0.027	0.026	0.028
	(0.034)	(0.034)	(0.005)	(0.171)	(0.160)	(0.176)
Arrests	-3.113**	-2.989**	-2.813***	10.522	22.168	3.694
	(1.400)	(1.409)	(0.697)	(18.579)	(26.316)	(3.858)
Guns Seized	-0.706	-0.678	0.602***	1.927	3.772	0.844
	(0.590)	(0.584)	(0.215)	(3.631)	(5.156)	(1.510)
Drugs Seized	0.886	0.889	1.84	0.802	2.087	0.048
	(2.595)	(2.629)	(1.409)	(12.939)	(21.305)	(0.387)
Domestic Violence	1.976	1.907	1.416***	3.466	7.691	0.989
	(1.906)	(1.879)	(0.415)	(9.152)	(13.974)	(1.322)
Sexual offenses	0.293	0.291	0.115***	0.511	0.985	0.233
	(0.218)	(0.218)	(0.043)	(1.057)	(1.461)	(0.553)
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. Parenthetical values in the pre-treatment monthly average columns report standard errors of the mean. Significance levels are as follows: $p < 0.01$ ***; $p < 0.05$ **; $p < 0.10$ *.

We next estimate dynamic treatment effects using a weighted event-study specification.

[10]In Appendix B. we restrict the sample to municipalities with more than 20,000 inhabitants and, separately, to those with homicide rates above the national average. Under the hypothesis that RAIO battalions are more likely to be deployed in violent areas, or that population size is positively correlated with crime, this helps us assess whether our baseline results are sensitive to alternative control group specifications. The results reported in Appendix B. show that our main findings remain consistent given these changes to population and violence thresholds.

Pre-treatment estimates assess the plausibility of parallel trends, while post-treatment estimates capture the evolution of treatment effects relative to the control group. Figure 3 shows the effect of treatment exposure for homicides and robberies: we find little evidence for violations of the parallel trends assumption, with the exception of a few pre-treatment periods many years before program implementation begins.

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 lasts for more than four years. In other words, we find no evidence of rapid attenuation following deployment, with effects that remain stable over multiple years.

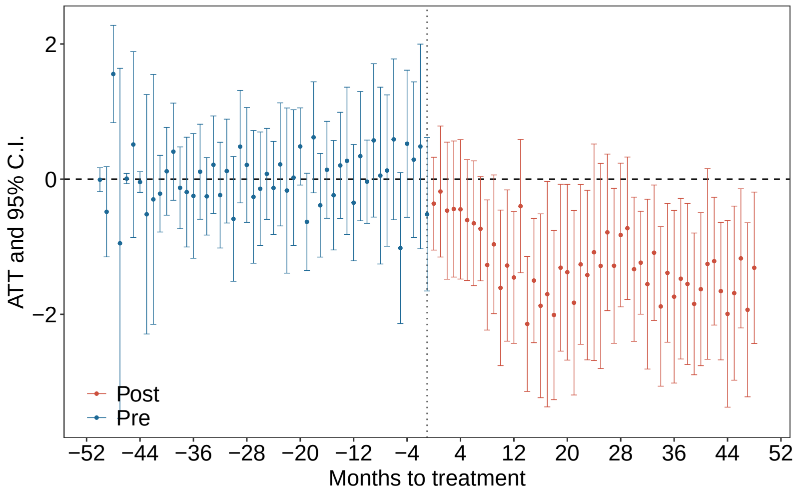
Furthermore, we explore how the ATT varies across the different cycles in which municipalities were first introduced to the RAIO program. Appendix Table B.5 presents the results and Appendix Figure B.1 the dynamic effect on homicides as a function of its cycle of treatment using the event study aggregation. Overall, this exercise suggests that the reductions in homicides, robberies, and thefts observed in the aggregate results are mainly driven by municipalities treated in cycle 2.[11]

Finally, in Figure 4 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 subsequently 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 criminals' incapacitation. In a similar vein, we find no consistent increases in

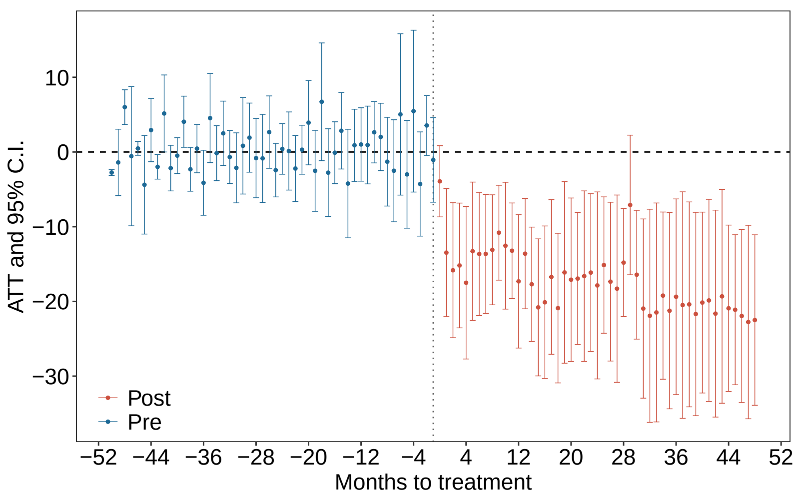
[11]In Appendix B.4., we present a more detailed discussion of our empirical strategy to estimate the effects of RAIO patrols across different subsamples, allowing us to identify heterogeneous effects by cycle. We further discuss potential mechanisms underlying the heterogeneity observed across cycles.

Figure 3: **Dynamic effects of RAIO patrols on crime**

(a) **Homicides**



(b) **Robberies**

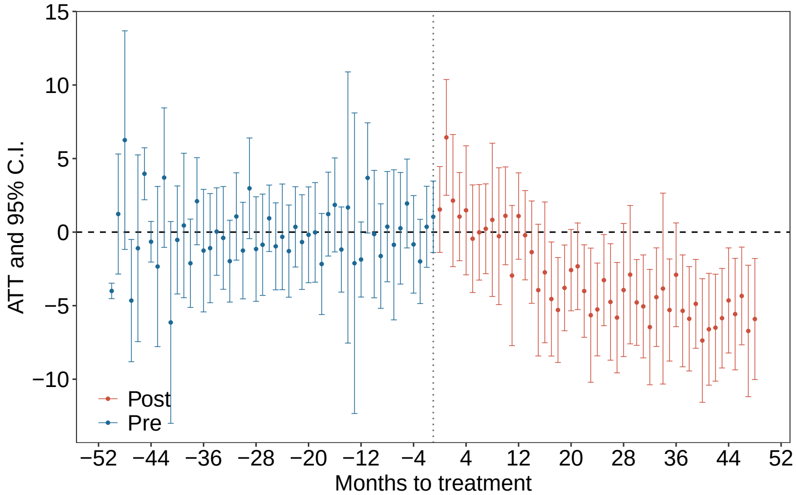


Note: This figure presents the average treatment effect on the treated by the length of exposure to treatment in (Callaway and Sant'Anna 2021). Table 2 shows the full set of the results upon which each figure is based. Period $t = -1$ represents one month prior to first RAIO deployment. Period $t = 1$ 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.

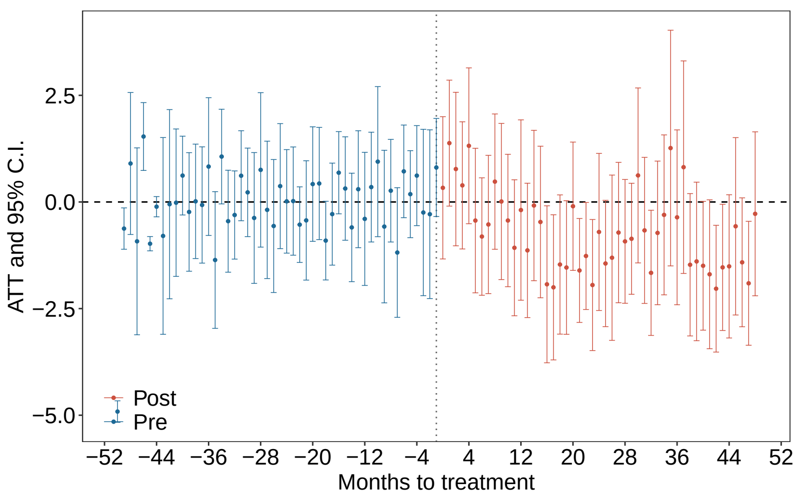
gun seizures in panel (d), meaning that criminals likely are not being prevented from committing crimes due to a scarcity of firearms. Taken together, these findings suggest that RAIO's crime reduction effects likely operate via credible deterrence of crime. We address this possibility in greater detail below using our survey data.

Figure 4: **Dynamic effects of RAIO patrols on police operations**

(a) **Arrests**



(b) **Guns seized**



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.

MECHANISMS

Our results raise a central question: how did a newly created policing unit achieve crime reductions? In this section, we combine experimental and descriptive data from our survey, alongside qualitative data from focus groups and interviews, to argue that selective segmentation allowed RAIO to be so successful.

CREDIBLE DETERRENCE

We administered a 2,000-person resident survey in Fortaleza between October and December 2023 to assess how residents perceive the RAIO relative to the ordinary military police.[12] We also embedded a conjoint experiment to identify which visible attributes of the RAIO—the motorcycle, the assault rifle, or the uniform—most strongly shape these perceptions. While credible deterrence ultimately operates through how potential offenders assess enforcement capacity and resolve, residents' perceptions capture the same observable signals—identity, firepower, and mobility—that criminal actors use to infer threat and response capability. Survey methodology and sampling are discussed in Appendix E.

In direct, non-experimental comparisons, respondents rate RAIO more favorably than the military police across perceived effectiveness, restraint, respect, and corruption, and express substantially higher confidence in RAIO's ability to reduce crime (Appendix Figure F.1). Residents report substantial exposure to both forces, with only modest differences in recent patrol and arrest visibility, suggesting that more favorable views of RAIO are unlikely to reflect novelty or lack of contact (Appendix Figure F.2).

To identify which features of RAIO drive these more favorable perceptions, we fielded a conjoint experiment, inspired by Flores-Macías and Zarkin (2021) and Blair, Mendoza Mora and Weintraub (2025). Respondents saw three sets of randomly-selected images consecutively, each featuring images of RAIO and military police officers that randomly varied along institutional identity (uniform), coercive capacity (weapon), and mobility (motorcycle), as shown in Figure 5. Our principal estimand is the Average Marginal Component Effect (AMCE), which captures the average causal effect of each attribute level—relative to a baseline—on respondents' evaluations of a police officer (Hainmueller, Hopkins and Yamamoto 2014; Bansak et al. 2023). We cluster standard errors by respondent.

Figure 6 presents the main conjoint results, while detailed results appear in Appendix F. and G. Dots with horizontal lines represent the level-specific AMCEs and 95% confidence

[12]Note that Fortaleza is not included in the sample used to estimate RAIO's effects on crime and policing activity.

Figure 5: **Conjoint survey images**

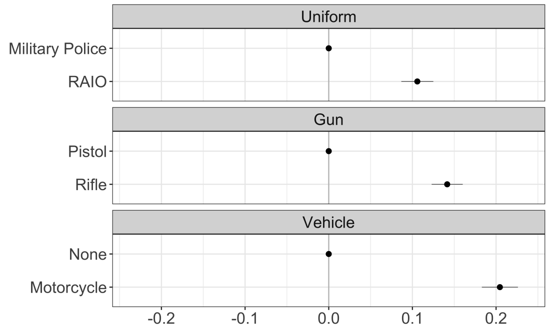
intervals. In line with our pre-registered hypotheses, we find that perceptions of policing are shaped by both institutional identity and visible capacity. Across outcomes related to deterrence and safety, the RAIO uniform substantially increases positive evaluations relative to the ordinary military police, even holding weapons and mobility constant. Signals of rapid response and firepower also matter: motorcycles and assault rifles strongly increase perceived crime-fighting ability and feelings of safety.

In contrast, perceptions of restraint and integrity are driven almost entirely by institutional identity: the RAIO uniform significantly reduces expectations of abuse and corruption, whereas heavier weaponry does not improve—and in some cases slightly worsens—these assessments. Put differently, capacity signals enhanced deterrence, but legitimacy and perceived accountability are tied to the institutional identity of RAIO, rather than its level of militarization. These patterns suggest that RAIO's deterrent effect operates not through indiscriminate militarization, but a combination of visible capacity and institutional legitimacy that plausibly increases the perceived certainty and swiftness of enforcement. RAIO focus group participants also emphasized differences in how the public perceives the two forces, noting that criminals reportedly prefer being arrested by RAIO.

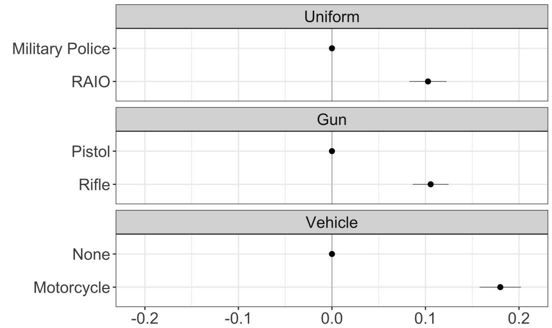
Two alternative interpretations of these findings deserve consideration. First, one might worry that positive resident views of RAIO reflect performative policing — that motorcycles

Figure 6: **Average Marginal Component Effects (AMCE)**

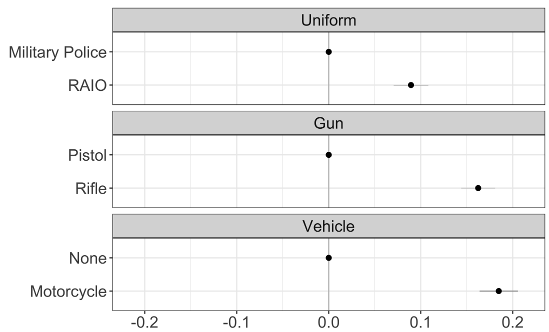
(a) **Combats crime**



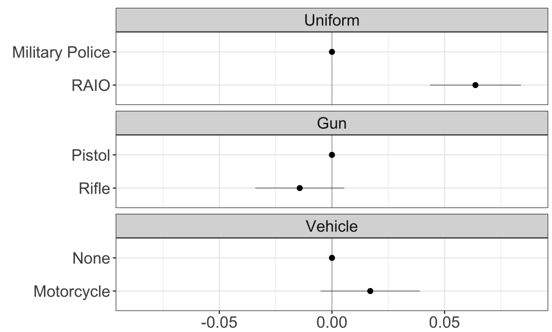
(b) **Makes them feel safer**



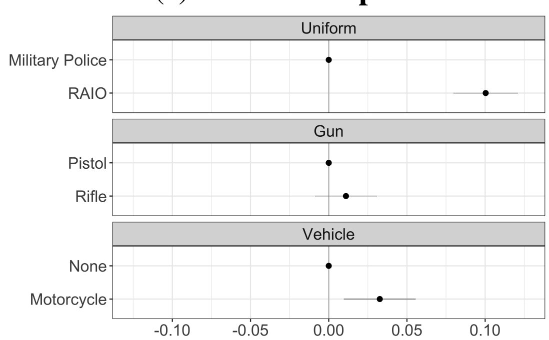
(c) **Uses force against criminals**



(d) **Does not commit abuse**



(e) **Is not corrupt**



Note: This figure shows the Average Marginal Component Effects (AMCE) for the conjoint experiment with 2,000 respondents. Tables G.1, G.2, G.3, G.4 and G.5 reports the full set of estimates for each of these plots. All estimates are based on the conditional logit model.

and distinctive uniforms shape perceptions without actually changing officer behavior or criminal calculations. Several features of our evidence militate against this interpretation. As noted above, residents report substantial prior contact with both forces, with only modest differences in patrol and arrest visibility, suggesting that familiarity rather than novelty drives their assessments (Appendix Figure F.2). The crime reduction effects we document persist over multiple years, which is difficult to reconcile with purely symbolic signaling. And the focus group evidence presented below reveals operational differences — intelligence-led briefings, distinct command accountability, peer-based conduct enforcement — that are inconsistent with a purely performative effect.

Second, one might ask whether the drop in arrests indicates not deterrence but improved community relations that enable more strategic patrolling: better intelligence leads to fewer low-value stops alongside lower crime overall. This mechanism is not implausible: RAIO engages in crime-pattern-based deployment, and communities may share information more openly with a force they perceive as trustworthy. But this would predict more targeted arrests alongside lower crime, whereas we observe both arrests and crime falling together. This joint pattern is a signature of deterrence: when potential offenders anticipate a higher probability of apprehension, they reduce offending. We therefore interpret community-based intelligence as one channel through which RAIO implements credible deterrence by improving deployment precision, rather than as an alternative to it.

RAIO's deterrence mechanism also differs from the "conditional repression" logic that Lessing (2018) identifies in Rio de Janeiro's UPP program, where the state conditioned the intensity of repression explicitly on criminal organizations' use of violence against civilians. While RAIO's conditionality concentrates patrols where and when violence spikes, it does not target named individuals or specific groups with explicit behavioral ultimatums. RAIO's success is therefore better characterized as credible general deterrence rather than as focused deterrence or conditional repression in the strict sense.

Who policies and why

Detailed information on recruitment, training, and internal governance suggests that Ceará's military police created a positively selected and highly trained subgroup of officers within RAIO. Like other elite units, RAIO draws from the ranks of the military police. Unlike other units, however, its selection process evolved from informal referrals into a formal, competitive system open to officers with at least one year of service. Selection combines operational screening (physical fitness and motorcycle proficiency) with extensive vetting of disciplinary records, mental health evaluations, and reputational assessments based on input from colleagues and supervisors. Applicants' names are publicly circulated within the force, allowing peers to flag prior misconduct even when not formally recorded. Finalists then complete a six-week training course focused on motorcycle operations, firearms, personal defense, and RAIO doctrine.

Focus group participants consistently described selection as the primary filter for entry into RAIO. As one officer put it, RAIO "receives only red apples," with training designed "to make them shine." Across groups, participants emphasized that entry requires substantial effort and sustained dedication, and that officers who do not initially succeed are often encouraged—formally or informally—to reapply in subsequent cohorts.

This selection and training pipeline contrasts sharply with conventional policing. In Brazil, officers are typically hired through public examinations, and initial training is often brief (roughly one month), including only three to four days of firearms practice. Senior officers in the conventional force emphasized that even this represents a substantial improvement over earlier cohorts, some of whom spent only half a day at the firing range during their initial training. One senior officer with 28 years of service reported accumulating just three days of shooting practice over his entire career.

Once assigned to RAIO, officers face a distinct incentive environment designed to sustain effort and professionalism. They receive a 30% wage premium and benefit from a more flexible work schedule than regular military police officers.[13] Performance is further rewarded through both pecuniary and symbolic incentives: officers earn time off for gun seizures and receive medals for exceptional performance.

In addition, RAIO officers appear to make more use of crime-analysis and intelligence products, consistent with an organizational culture oriented toward continual improvement.

[13]RAIO officers work two consecutive days of 8-hour shifts followed by two days off. By contrast, the standard Military Police schedule totals 44 hours per week and can be arranged in several demanding shift combinations, such as 12 hours on followed by 36 hours off, or sequences of 12 hours on, 24 hours off, another 24 hours on, and then 36 hours off.

In the focus groups, RAIO participants reported that patrol routines are guided by intelligence reports and that teams typically receive a pre-deployment briefing in which the supervising officer summarizes recent criminal conditions in the area, identifies key individuals linked to local criminal activity, and describes common crime tactics and routines. By contrast, conventional patrol officers reported having no routine briefings and receiving no systematic information about crime patterns in the areas they patrol.

ORGANIZATIONAL INSULATION

Focus groups with RAIO and non-RAIO officers provide evidence that RAIO operates as an insulated organizational unit rather than a tactical extension of ordinary patrol policing. Officers consistently emphasized three features of insulation: command autonomy, doctrinal coherence, and control over deployment.

First, RAIO operates under a distinct and unusually stable command structure. Both RAIO and conventional officers noted that RAIO reported to a separate chain of command, which has been led by the same colonel for 16 years. This continuity has facilitated the development of a coherent doctrine—covering rules of engagement, training protocols, patrol formations, and tactical procedures—that officers described as internally enforced and non-negotiable. RAIO officers emphasized that when deployed, they remain accountable to their own commanders, even when operating in support of other units. In one commonly cited incident, RAIO officers resisted a local commander's attempt to alter their patrol formation by contacting their central command, which subsequently intervened to enforce standard operating procedures.

Second, officers described insulation in operational terms. RAIO patrols follow internally determined deployment rules, operating exclusively in fixed formations (four officers patrolling with three motorcycles) and relying on centralized crime analysis to guide preventive patrols focused on homicides and property crime. While RAIO units are frequently requested to assist in high-risk situations—particularly during turf conflicts between criminal groups—officers stressed that such support does not place them under the operational authority of local military police commands. This separation was repeatedly described as critical for both effectiveness and officer safety.

Third, focus group discussions highlighted sharp contrasts in organizational support and morale between RAIO and conventional policing. RAIO officers emphasized mutual trust, close supervision, and the perception of institutional backing—both in terms of equipment and personal support. In contrast, non-RAIO officers consistently reported operating with limited training, scarce equipment, weak informational support, and little opportunity for preventive or intelligence-led patrols. Several RAIO officers noted that sensitive intelligence—such as information about armed groups—was routinely channeled directly to RAIO units through restricted communication networks, rather than shared within broader law enforcement institutions.

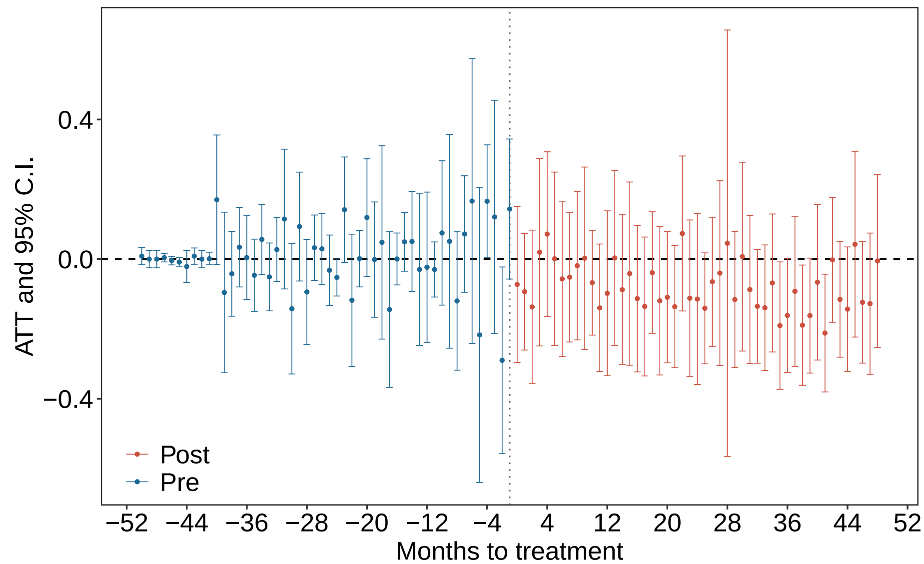
Taken together, these accounts suggest that RAIO's insulation is not merely symbolic or reputational, but organizational: rooted in distinct command authority, internally enforced doctrine, and control over deployment and information flows. While focus group evidence cannot establish the causal effects these factors generate, it helps clarify how selective segmentation operates in practice and why RAIO officers experience the unit as meaningfully separate from the institutional pathologies affecting the broader force.

ACCOUNTABILITY

A central concern with the creation of specialized policing units is that enhanced coercive capacity may weaken constraints on abuse. We begin to examine whether selective segmentation promotes accountability and restraint by assessing its effects on police violence. In Figure 7 we evaluate whether the deployment of RAIO increased police killings, perhaps the most reliable way to measure human rights abuses, as it is less likely to suffer from measurement error. We find that RAIO did not increase police killings: returning to our core difference-in-differences models, but using aggregated police killings as our dependent variable and not-yet-treated municipalities as our control group, we find no evidence that RAIO deployments increased killings of residents by the police.

Qualitative evidence from focus groups provides additional insight into how accountabil-

Figure 7: **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.

ity operates within selectively segmented units. Officers consistently described two internal accountability mechanisms within RAIO: credible removal from the squad and peer-based monitoring.

First, RAIO officers emphasized that membership in the unit is conditional rather than permanent. Because recruits are selected internally from the Military Police, those who violate unit standards can be removed and reassigned to conventional policing forces. In practice, this creates credible internal discipline that differs from formal dismissal from public employment, which is typically difficult to credibly threaten and enforce. While removals from RAIO were described as infrequent, officers reported approximately 15 cases in which RAIO members were removed from the unit, creating shared expectations about acceptable conduct.

Second, officers described peer-based monitoring as a central mechanism for enforcing restraint. Patrol teams are intentionally composed of officers with varying levels of experience, and senior officers are expected to supervise newer recruits during operations. In one example, officers described how newly assigned RAIO members mistreated civilians during a raid. According to participants, senior officers intervened immediately, reiterating that such behavior violated RAIO norms and doctrine. Officers repeatedly emphasized that enforcement of standards occurs informally and continuously through close interaction within small, stable teams. When asked how they respond to peer misconduct, RAIO officers reported that they either confront the colleague or inform a superior, whereas conventional military police officers described remaining silent or requesting reassignment away from the problematic officer.

Taken together, these accounts suggest that selective segmentation facilitates accountability not by eliminating misconduct risk, but by lowering the organizational costs of discipline and increasing the likelihood that deviations from unit norms are detected and sanctioned internally.

COMPETITIVE EMULATION

We find little evidence that selective segmentation generates a broader "race to the top" by inducing conventional Military Police units to emulate RAIO. In practice, RAIO operates as a tightly segmented unit—like other elite groups—so there is limited routine exchange of personnel, routines, or supervisory practices with conventional patrol units. In the focus groups, we discussed competitive emulation as mobility of personnel across units and the transfer of operational and managerial practices. Both appear constrained in the case of RAIO.

Conventional patrol officers described the idea of applying to an elite unit as ever-present, largely because elite units offer higher pay.[14] Yet actual application to RAIO was rare: only two of the sixteen conventional officers in our focus groups reported applying, and both described repeated setbacks (one application was deferred; another ended with failing the physical exam). When asked why they had not applied, one officer explained, "I don't want to make policing my whole life," underscoring the exceptional time and effort these positions demand. RAIO officers reinforced this point, arguing that rigorous selection is central to the unit's performance and that it cannot be straightforwardly scaled to the broader force because

[14] Many pointed to Casa Militar—the unit responsible for the governor's personal security—as the best-paid option, but noted that entry relies heavily on referrals and entails long additional and unpredictable hours. Several participants contrasted this with RAIO, emphasizing that RAIO's selection is unusually transparent relative to other elite assignments.

"not everyone has the required profile."

By contrast, the features that appear most distinctive —and potentially most transferable — relate to human-resource management and supervision. RAIO officers repeatedly emphasized that commanders know them personally and closely monitor daily activity; that the unit has some operational flexibility in scheduling; and that supervisors actively reinforce good practice.[15] When asked whether RAIO practices had been adopted in conventional policing, officers pointed primarily to the HR information system: RAIO managers input substantially more information on individual performance and activities, which, in turn, gives senior leadership a richer basis for promotion. Participants suggested that this system has attracted interest from other departments seeking to replicate RAIO-style personnel tracking. A final indication that RAIO became an internal reference point is organizational: Colonel Márcio, who founded and led RAIO for 16 years, later became Commander General for three years. The extent to which RAIO's personnel-management model diffused more broadly during his tenure is an important question for further research.

In summary, qualitative evidence from our focus groups suggests limited competitive emulation, with diffusion most plausibly occurring through personnel mobility — particularly if RAIO officers move to other units and carry over elements of RAIO's human-resources and supervisory practices. At the same time, segmentation may have an unintended downside: by providing politicians with a highly visible "showcase" unit, it can reduce pressure to undertake broader organizational reforms in conventional policing, since leaders can credibly signal performance improvements through RAIO alone. Consistent with this concern, conventional patrol officers often expressed demoralization, emphasizing a lack of recognition, poor working conditions, and inadequate facilities. The next section explores the political dividends of RAIO.

[15] One example of schedule flexibility—described as uncommon in conventional military policing—was: "If your shift runs until 4 a.m. because you made an arrest and you're scheduled to be on duty the next day, you're automatically dismissed. It's a safety issue. If you're too tired, it's too risky to drive a motorcycle."

How politically feasible is selective segmentation?

Electoral benefits

Effective public safety programs must be incentive-compatible for politicians to adopt and sustain them. In this section, we ask whether the roll-out of RAIO generated positive electoral consequences for the incumbent. Our setting is well suited to assess electoral incentives, as the RAIO program was expanded by a governor elected in 2014 who subsequently ran for reelection in 2018. Qualitative interviews suggest that electoral considerations shaped the timing of this expansion. Consistent with predictions about political business cycles in public safety provision (Levitt 1997; Guillamón, Bastida and Benito 2013), Figure 1 shows that 2018, the reelection year,featured the highest number of RAIO base inaugurations and the training of more than 1,000 officers, over three times the annual average in other years.

Using official electoral data, Table 3 presents a two period difference-in-differences analysis, where the treatment variable equals one for those municipalities that experienced the RAIO roll-out 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.

Estimates show that the RAIO expansion produced electoral benefits for Ceará's incumbent state governor. Column 2 in Table 3 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.[16] Columns 3 and 4 in Table 3 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 26.4% compared to the

[16] 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 votes for the incumbent.

Table 3: **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%, *.

sample mean. These results show that selective segmentation can generate tangible electoral returns, as the expansion of RAIO delivered measurable benefits to the incumbent governor at the ballot box.

COST-BENEFIT ANALYSIS

Selective segmentation generates political dividends, but do its social benefits outweigh its costs? We conduct a cost-benefit analysis that compares the operational costs of RAIO to the monetized social benefits of crime reductions attributable to the program (Jaitman et al. 2017). Using detailed expenditure data, we estimate that RAIO costs approximately US$1.2 million per municipality per year over the study period. Appendix I. makes explicit our core assumptions.

Our estimates from the prior section indicate that RAIO reduced robberies by 17.3 and homicides by 1.3 per month per municipality, equivalent to 207.6 robberies and 15.6 homicides annually. This implies an average cost of roughly US$5,900 to deter each robbery and US$78,500 to deter each homicide. To benchmark these figures, we draw on regional estimates for Latin America and the Caribbean (Perez-Vincent et al. 2024). Average social costs are estimated at approximately US$12,040 per robbery and US$178,570 per homicide. Even under highly conservative assumptions, the social costs of crime far exceed the per-incident costs of deterrence achieved through RAIO, suggesting that selective segmentation yields benefits that substantially outweigh its fiscal costs.

Conclusion

Policymakers in violent democracies face a persistent dilemma: citizens demand rapid improvements in public safety, yet the institutions responsible for providing order are often compromised by corruption, weak incentives, and political interference. As a result, system-wide reforms frequently stall, and governments turn to hardline security strategies that are exceptionally popular (Holland 2013; Blair, Weintraub and Zarkin 2022) but often fail to reduce crime, exacerbate abuses, and undermine support for the rule of law (Flores-Macías 2018; Flores-Macías and Zarkin 2021; Blair and Weintraub 2023). This paper studies an alternative response to this dilemma: selective segmentation, or the deliberate creation of insulated, high-capacity units within otherwise weak coercive institutions.

We study this strategy in Ceará, Brazil, where the state deployed a motorcycle-based policing unit designed to operate under distinct recruitment, incentive, and accountability regimes. Exploiting the staggered rollout of RAIO forces across municipalities, we show that the program produced large and sustained reductions in homicides and robberies. These gains did not coincide with increases in arrests or police killings, pointing to deterrence rather than mass incarceration or indiscriminate repression as the primary channel. Descriptive and experimental survey evidence further indicates that RAIO is perceived not only as more effective, but also as more restrained and less corrupt than the ordinary military police—an unusual combination in contexts where coercive capacity and legitimacy are often inversely related.

We show how segmentation operates through multiple, self-reinforcing mechanisms. Rapid-response capacity enhances credible deterrence by increasing the perceived certainty and swiftness of enforcement. Selective recruitment and incentive differentiation reshape who polices and why, concentrating motivated officers within a professional enclave. Organizational insulation—via distinct command structures, disciplinary autonomy, and separation from compromised networks—protects the unit from the pathologies afflicting the broader force. Together, these mechanisms have allowed RAIO to combine credible coercion with meaningful restraint.

While the dynamic estimates suggest that the effects of RAIO persist over the period we observe, this does not imply long-run success, nor that the findings will necessarily travel. The features that enable insulation, from selective recruitment and distinct command structures to strong internal norms—may depend on leadership continuity and organizational cohesion that may be difficult to sustain indefinitely. We therefore interpret our findings as evidence of impressive performance within the study window, and in the specific case of Ceará, rather than proof that segmented units are somehow immune to erosion over time anywhere.

Beyond policing, our findings speak to broader debates on bureaucratic reform and state-building under conditions of institutional weakness. Rather than assuming that transformation must occur uniformly across entire organizations, selective segmentation demonstrates how governments can build capacity incrementally by creating protected domains of effectiveness. In this sense, our results echo classic insights about bureaucratic enclaves (Geddes 1994), while extending them to violent democratic contexts where coercive institutions are both necessary and potentially dangerous. Segmentation need not promise miracles: it raises questions about scalability, integration, and long-term institutional spillovers. Still, our findings indicate that selective segmentation in Ceará was both politically feasible and cost-effective, pointing to its potential as a strategy for reconciling security, accountability, and democratic constraint.

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 produce unbiased effects of RAIO on crime if the following assumptions hold:

1. *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.

2. *Random sampling (or quasi-random treatment timing)*: Formally, treatment timing is assumed to be independent of potential outcomes, conditional on X, so that:

$$G_g \perp Y_t(0) \mid X.$$

   This assumption underlies the use of *not-yet-treated* municipalities as valid counterfactuals for the group first treated in period g. In our context, the RAIO expansion followed populational and administrative criteria largely unrelated to short-term changes in crime, which means that municipalities begin treatment at times that are quasi-random (since selection into treatment itself is not random).

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 based on a "Never-Treated" Group*: 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 "never-treated" group, would have followed parallel trajectories. More formally, the conditional parallel trends assumption states that for each g in G and $t \in \{2, \ldots, T\}$ such that $t \geq g - \delta$,

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

5. *Conditional parallel trends based on a "Not-Yet-Treated" Group*: For each g in G and each $(s,t) \in \{2,\ldots,T\} \times \{2,\ldots,T\}$ such that $t \geq g - \delta$ and $t + \delta \leq \bar{g}$,

$$\mathbb{E}[Y_t(0) - Y_{t-1}(0)|X, G_g = 1] = \mathbb{E}[Y_t(0) - Y_{t-1}(0)|X, D_s = 0, G_g = 0]$$

   This assumption imposes conditional parallel trends between group g and groups that are "not-yet-treated" by time $t + \delta$. Moreover, this assumption also assumes a large enough "not-yet-treated" group in any period, 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 once from the 184 municipalities of Ceará, 116 remained untreated in our period of analysis. Therefore, this assumption allows to use more groups as valid comparison units.

6. *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.

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)	(3.883)	(3.049)	(4.247)
Police Killings	0.024	0.009	-0.027	0.064	0.054	0.087
	(0.116)	(0.112)	(0.026)	(0.706)	(0.310)	(0.851)
Robberies	-18.606***	-18.145***	-10.100***	23.280	31.780	3.567
	(4.290)	(4.235)	(1.301)	(18.569)	(24.832)	(8.476)
Thefts	-4.114**	-3.936**	-2.915***	32.070	39.260	15.400
	(1.803)	(1.812)	(0.986)	(21.963)	(21.656)	(18.773)
Bank Robberies	0.057	0.044	0.067***	0.079	0.039	0.170
	(0.075)	(0.076)	(0.015)	(0.965)	(0.315)	(1.182)
Arrests	-0.721	-0.654	-0.598	30.680	34.20	22.53
	(1.870)	(1.912)	(0.805)	(19.290)	(17.824)	(19.309)
Guns Seized	-0.580	-0.501	0.809*	5.619	5.821	5.148
	(0.859)	(0.852)	(0.415)	(9.814)	(7.922)	(10.737)
Drugs Seized	-0.287	0.137	0.875	2.337	3.220	0.291
	(3.583)	(3.313)	(2.144)	(21.189)	(34.824)	(2.234)
Domestic Violence	1.319	1.309	0.434	10.110	11.868	6.033
	(1.094)	(1.016)	(0.523)	(8.577)	(8.526)	(8.380)
Sexual offenses	0.405	0.376	0.140	1.491	1.522	1.420
	(0.366)	(0.365)	(0.092)	(3.061)	(2.140)	(3.485)
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 inhabitants 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. Results assuming anticipation

In this section we provide robustness checks on our main DiD specification allowing for different months of anticipation.

Table B.2: Average treatment effects of the RAIO patrols - Crime Levels

	Methods and estimands				**Pre-treatment monthly averages**		
	Callaway and Sant'Anna (2021) (not yet treated)						
Dependent Variable	**ATT** ($\delta = 0$)	**ATT** ($\delta = 1$)	**ATT** ($\delta = 3$)	**ATT** ($\delta = 6$)	**Complete Sample**	**Treatment**	**Control**
Homicides	-1.252***	-2.046***	-2.389***	-1.388***	3.011	3.377	2.165
	(0.446)	(0.723)	(0.680)	(0.353)	(3.883)	(3.049)	(4.247)
Police Killings	-0.081	0.053	-0.073	0.050	0.064	0.054	0.087
	(0.084)	(0.050)	(0.084)	(0.053)	(0.706)	(0.310)	(0.851)
Robberies	-17.207***	-25.125***	-23.738***	-14.262***	23.280	31.780	3.567
	(5.08)	(7.211)	(5.684)	(2.732)	(18.569)	(24.832)	(8.476)
Thefts	-5.061***	0.170	-7.963**	-3.842	32.070	39.260	15.400
	(1.917)	(5.334)	(3.298)	(2.342)	(21.963)	(21.656)	(18.773)
Bank Robberies	0.000	-0.017	0.010	0.001	0.079	0.039	0.170
	(0.034)	(0.026)	(0.019)	(0.026)	(0.965)	(0.315)	(1.182)
Arrests	-2.989**	-9.550***	-14.297***	-13.163***	30.680	34.20	22.53
	(1.409)	(1.830)	(2.227)	(2.974)	(19.290)	(17.824)	(19.309)
Guns Seized	-0.678	-1.024**	-1.289	1.494***	5.619	5.821	5.148
	(0.584)	(0.410)	(1.558)	(0.539)	(9.814)	(7.922)	(10.737)
Drugs Seized	0.889	3.977	1.146	3.594**	2.337	3.220	0.291
	(2.629)	(2.494)	(5.205)	(1.424)	(21.189)	(34.824)	(2.234)
Domestic Violence	1.907	6.265	3.601	1.985	10.110	11.868	6.033
	(1.879)	(5.107)	(3.679)	(1.184)	(8.577)	(8.526)	(8.380)
Sexual offenses	0.291	0.090	0.396	0.330**	1.491	1.522	1.420
	(0.218)	(0.204)	(0.181)	(0.150)	(3.061)	(2.140)	(3.485)
Observations	18.300	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 with and without anticipation. The first column shows the dependent variable used among different offenses reported by SSPDS-CE monthly per municipality assuming non-anticipation. Our baseline sample considers 183 municipalities from January 2015 up to April 2023. Columns 1 to 4 show the ATT using the method proposed by Callaway and Sant'Anna (2021), using the not yet treated municipalities as control group respectively, while we vary the months of anticipation expressed by the term δ. Standard errors in parenthesis are clustered via bootstrap at the municipality level. Significance levels are as follows: 1%, ***; 5%, **; 10%, *.

B.3. Results assuming thresholds of population and violence

In this section we provide robustness checks on our main DiD specification using population and violence criteria to assess different specifications of control groups.

Table B.3: **Effects of RAIO patrols on crime**

	Methods and estimands		
		Callaway and Sant'Anna (2021) (not yet treated)	
Dependent Variable	**ATT (complete sample)**	**ATT (population >20.000)**	**ATT (homicide rate >23)**
Homicides	-1.252***	-1.932***	-2.013***
	(0.446)	(0.726)	(0.692)
Police Killings	-0.081	0.049	0.053
	(0.084)	(0.053)	(0.044)
Robberies	-17.207***	-24.517***	-24.741***
	(5.08)	(7.779)	(7.891)
Thefts	-5.061***	0.583	0.379
	(1.917)	(5.523)	(3.505)
Bank Robberies	0.000	-0.016	-0.012
	(0.034)	(0.027)	(0.026)
Arrests	-2.989**	-8.676***	-9.328***
	(1.409)	(1.742)	(1.749)
Guns Seized	-0.678	-0.922**	-1.039**
	(0.584)	(0.436)	(0.424)
Drugs Seized	0.889	3.900	4.103
	(2.629)	(2.496)	(2.759)
Domestic Violence	1.907	6.009	6.235
	(1.879)	(5.086)	(4.968)
Sexual offenses	0.291	0.050	0.088
	(0.218)	(0.200)	(0.192)
Observations	18.300	10.100	12.200
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 to 3 show the ATT using the method proposed by Callaway and Sant'Anna (2021), using not yet treated municipalities as control groups.While Column 1 includes all municipalities, Column 2 restricts the sample to those with more than 20,000 inhabitants, and Column 3 considers only municipalities with homicide rates above the national average of 23 per 100,000 inhabitants. Standard errors in parenthesis are clustered via bootstrap at the municipality level. Significance levels are as follows: $p < 0.01$ ***; $p < 0.05$ **; $p < 0.10$ *.

B.4. Results per cycle

Table B.4 presents pre- and post-intervention means for crime and police outcomes by cycle. Across most indicators, post-intervention averages are lower than baseline levels, with percentage changes generally larger in later cycles. As these are unconditional comparisons, they should be interpreted as descriptive evidence rather than causal effects.

To explore causal estimates, we use distinct samples for each cycle. For the first cycle, we use data from January 2015 to June 2017. In this case, the treatment group consists of municipalities that received a RAIO squad during this period, while the control group includes all municipalities that had not yet received one by June 2017. For the second cycle, we use data from July 2017 to November 2019. Here, municipalities treated in the second cycle belong to the treatment group, and those that did not receive a squad in either the first or second cycles serve as controls. Finally, for the third cycle, we use data from December 2019 to April 2023. In this case, municipalities treated in the third cycle comprise the treatment group, and those that never received a RAIO squad in any period (the never-treated) comprise the control group.

The overall effect is mainly driven by cycle 2. We interpret this as reflecting the fact that this cycle includes the largest number of municipalities receiving RAIO squads ($n = 34$), meaning that changes in this group account for a substantial share of the overall variation in crime outcomes. Another source of heterogeneity arises from differences in baseline crime levels across cycles: because the rollout followed a population-based criterion, municipalities treated earlier tend to be larger, and therefore exhibit higher average crime levels than those in the control group. For this reason, estimates for cycle 3 are noisier, as baseline crime levels in municipalities with fewer than 30,000 inhabitants are low, making it more difficult to detect meaningful changes over time or to validate the parallel trends assumption. These results should therefore be interpreted with caution.

Table B.4: **Pre and post intervention means**

Dependent Variable	Pre Intervention			Post Intervention			Delta		
	Mean cycle 1	***Mean cycle 2***	***Mean cycle 3***	***Mean cycle 1***	***Mean cycle 2***	***Mean cycle 3***	***Mean cycle 1***	***Mean cycle 2***	***Mean cycle 3***
Homicides	4.80	2.54	0.81	2.60	0.74	0.11	-45.8%	-70.9%	-86.4%
	(4.68)	(3.48)	(1.24)	(3.44)	(1.08)	(0.77)			
Robberies	39.15	29.02	2.58	20.05	1.86	0.08	-48.8%	-93.6%	-96.9%
	(48.53)	(50.48)	(4.20)	(43.24)	(4.43)	(0.95)			
Thefts	49.00	31.78	8.70	30.37	7.80	0.79	-38.0%	-75.5%	-90.9%
	(43.02)	(37.58)	(7.28)	(37.51)	(7.62)	(2.81)			
Bank Robberies	0.028	0.034	0.014	0.000	0.003	0.001	-100.0%	-91.2%	-92.9%
	(0.16)	(0.18)	(0.11)	(0.00)	(0.06)	(0.04)			
Arrests	46.46	25.81	8.83	15.22	5.72	0.78	-67.2%	-77.8%	-91.2%
	(38.44)	(25.27)	(7.26)	(16.02)	(3.87)	(2.77)			
Guns Seized	7.04	4.37	1.87	4.88	2.36	0.31	-30.7%	-46.0%	-83.4%
	(6.66)	(4.60)	(4.45)	(6.11)	(2.54)	(1.76)			
Drugs Seized	1.98	3.17	0.68	3.36	1.60	0.06	69.7%	-49.5%	-91.2%
	(5.73)	(29.42)	(2.76)	(15.97)	(12.91)	(1.52)			
Domestic Violence	18.68	8.80	2.32	13.38	4.13	0.63	-28.4%	-53.1%	-72.8%
	(20.02)	(14.79)	(2.17)	(17.67)	(3.19)	(2.05)			
Sexual offenses	1.57	1.21	0.49	1.46	0.68	0.09	-7.0%	-43.8%	-81.6%
	(1.64)	(1.58)	(0.95)	(1.74)	(0.97)	(0.63)			

Note: The table reports monthly mean outcomes by cycle before and after the intervention. Standard deviations are in parentheses. The post-intervention period corresponds to months following treatment exposure in each cycle, and Delta denotes the percentage change between pre- and post-intervention means.

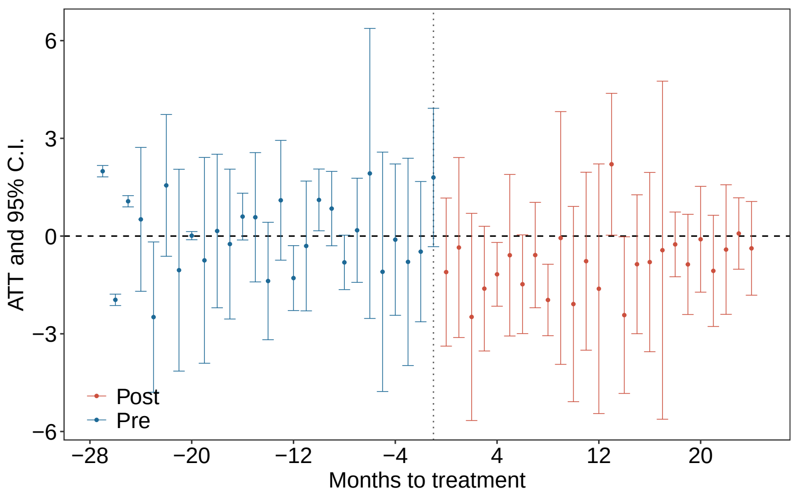
Table B.5: **Effects of RAIO patrols per cycle on crime**

Dependent Variable	*Callaway and Sant'Anna (2021) estimates*					
	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)	(4.68)	(0.453)	(3.48)	(2.739)	(1.24)
Robberies	-10.980	39.15	-16.070***	29.02	-2.790*	2.58
	(6.863)	(48.53)	(5.024)	(50.48)	(1.682)	(4.20)
Thefts	-2.370	49.00	-3.487**	31.78	-2.605	8.70
	(5.280)	(43.02)	(1.441)	(37.58)	(1.675)	(7.28)
Bank Robberies	0.030	0.028	0.000	0.034	0.002	0.014
	(0.021)	(0.16)	(0.042)	(0.18)	(0.006)	(0.11)
Arrests	-2.910	46.46	-0.659	25.81	-10.300*	8.83
	(4.334)	(38.44)	(1.283)	(25.27)	(6.005)	(7.26)
Guns Seized	-0.539	7.04	-0.487	4.37	-2.523***	1.87
	(1.708)	(6.66)	(0.628)	(4.60)	(0.394)	(4.45)
Drugs Seized	1.309	1.98	-2.979	3.17	1.426***	0.68
	(1.121)	(5.73)	(3.380)	(29.42)	(0.515)	(2.76)
Domestic Violence	5.427	18.68	1.191	8.80	-0.401	2.32
	(6.152)	(20.02)	(0.917)	(14.79)	(3.459)	(2.17)
Sexual offenses	0.565	1.57	0.131	1.21	0.547***	0.49
	(0.620)	(1.64)	(0.257)	(1.58)	(0.217)	(0.95)
Observations	5.307		6.222		6.771	
Time F.E.	✓		✓		✓	
Municipality F.E.	✓		✓		✓	
Controls	✗		✗		✗	

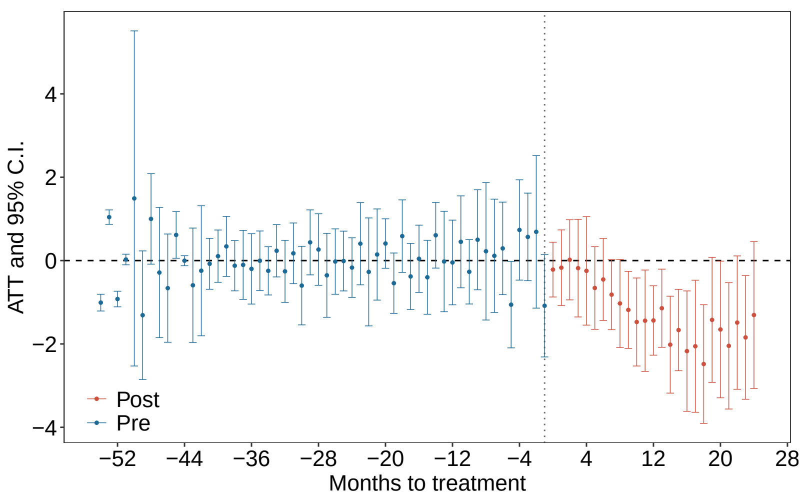
Note: This table presents the aggregated average treatment effects on the treated by cycle in Callaway and Sant'Anna (2021). The first column shows the dependent variable used among different offenses reported by SSPDS-CE monthly per municipality. Our baseline sample considers 183 municipalities from 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: $p < 0.01$ ***; $p < 0.05$ **; $p < 0.10$ *.

Figure B.1: **Heterogeneous treatment effects on homicides by RAIO roll-out cycle**

(a) First cycle



(b) Second 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 B.5 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.

B.5. Spatial spillovers

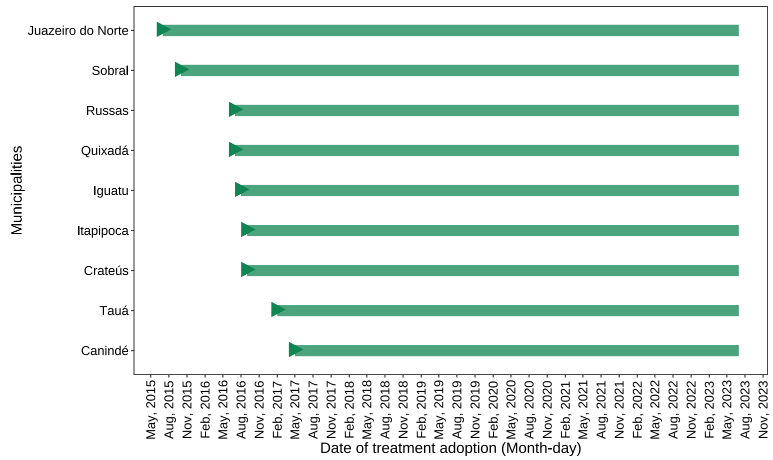
Table B.6: **Effects of RAIO patrols on crime accounting for spillovers**

	Methods and estimands			
	Callaway and Sant'Anna (2021) (not yet treated)		**TWFE**	
Dependent Variable	**ATT (baseline)**	**ATT (with spillovers)**	**ATT (baseline)**	**ATT (with spillovers)**
Homicides	-1.252***	-2.066***	-0.225	-0.212
	(0.446)	(0.738)	(0.164)	(0.184)
Police Killings	-0.081	0.051	0.016	0.016
	(0.084)	(0.050)	(0.014)	(0.014)
Robberies	-17.207***	-25.093***	-7.605***	-7.894***
	(5.08)	(7.223)	(1.303)	(1.401)
Thefts	-5.061***	0.172	-3.007***	-3.357***
	(1.917)	(5.331)	(0.750)	(0.841)
Bank Robberies	0.000	-0.021	-0.002	-0.003
	(0.034)	(0.027)	(0.005)	(0.006)
Arrests	-2.989**	-9.533***	-2.813***	-3.544***
	(1.409)	(1.849)	(0.697)	(0.76)
Guns Seized	-0.678	-0.998***	0.602***	0.587***
	(0.584)	(0.401)	(0.215)	(0.211)
Drugs Seized	0.889	3.607	1.84	2.289
	(2.629)	(2.527)	(1.409)	(1.614)
Domestic Violence	1.907	6.202	1.416***	1.324***
	(1.879)	(5.12)	(0.415)	(0.335)
Sexual offenses	0.291	0.093	0.115***	0.116***
	(0.218)	(0.201)	(0.043)	(0.044)
Observations	18.300	18.300	18.300	18.300
Time F.E.	✓	✓	✓	✓
Municipality F.E.	✓	✓	✓	✓
Spillovers	✗	✓	✗	✓
Controls	✗	✗	✗	✗

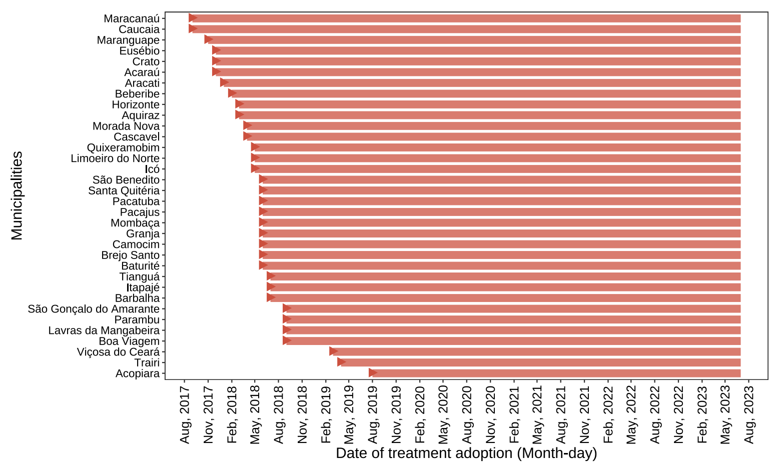
Note: This table presents the aggregated average treatment effects on the treated. The first column shows the dependent variable used among different offenses reported by SSPDS-CE monthly per municipality. Our baseline sample considers 183 municipalities from January 2015 up to April 2023. Columns 1 and 2 show the ATT using the method proposed by Callaway and Sant'Anna (2021), using not yet treated municipalities as control groups, respectively, while Columns 3 and 4 presents the standard Two Way Fixed Effects (TWFE) estimator. In Columns 2 and 4 we introduce our measurement of spatial spillover in the estimation. Standard errors in parenthesis are clustered via bootstrap at the municipality level. Significance levels are as follows: $p < 0.01$ ***; $p < 0.05$ **; $p < 0.10$ *.

Figure B.2: Staggered treatment adoption across treatment waves

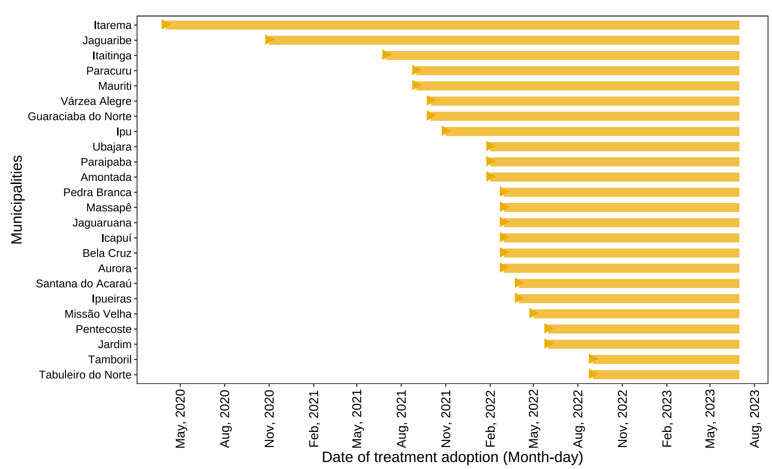
(a) First wave



(b) Second wave



(c) Third wave



C. Synthetic difference-in-differences and multi-period synthetic controls

C.1. Methodological overview

To complement our main identification strategy, we implement the Synthetic Difference-in-Differences (SDID) estimator proposed by Arkhangelsky et al. (2021). SDID combines features from both standard Difference-in-Differences (DiD) and Synthetic Controls (SC) approaches by constructing a weighted combination of untreated units and pre-treatment periods that best reproduces the treated units' pre-treatment trajectory.

Formally, SDID estimates unit weights (synthetic control weights) and time weights (synthetic trend weights) such that the weighted average outcome path of the control units closely matches that of treated units before treatment. This two-dimensional weighting structure reduces sensitivity to violations of the exact parallel trends assumption and improves robustness to heterogeneous treatment effects across units and over time.

C.2. Identification assumptions

Causal identification under SDID relies on two key assumptions:

1. **Approximate parallel trends after weighting.** Rather than requiring untreated municipalities to follow the same raw trend as treated ones, SDID requires that a weighted combination of untreated units, using estimated synthetic control weights, can reproduce the counterfactual evolution of treated units in the absence of treatment. The inclusion of time weights further improves this balancing.
2. **No interference between units (SUTVA).** Potential outcomes in each municipality must be unaffected by RAIO deployment in other municipalities. Given the potential for spatial spillovers in policing, this assumption is unlikely to hold perfectly in our context, and therefore we treat SDID as a robustness exercise rather than our primary identification strategy.

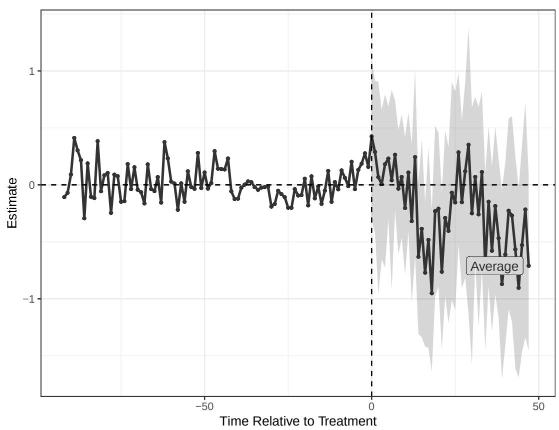
C.3. Multi-period treatment timing and the augmented synthetic control approach

The staggered implementation of RAIO squads motivates the use of an extension of SDID able to accommodate multiple treatment cohorts. We rely on the *multi-synthetic* approach implemented in the `augsynth` package (Ben-Michael, Feller and Rothstein 2022). This estimator allows for treatment to occur at different moments across units by constructing a separate synthetic control for each treated municipality or cohort.

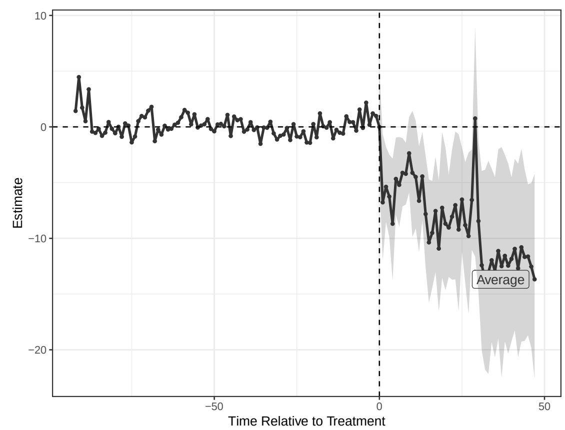
Table C.1 summarizes our SDID estimates, which are weighted averages of the synthetic controls constructed for each municipality that received a RAIO squad, in comparison to our baseline results. Furthermore, we present the graphic results of our estimates in Figures C.1 and C.2

Figure C.1: Synthetic difference-in-difference estimates

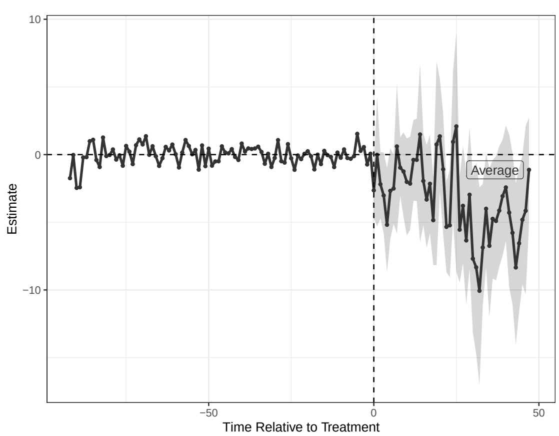
(a) Homicides



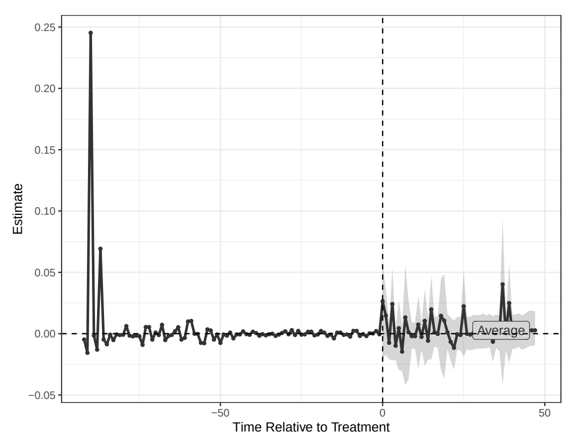
(b) Robberies



(c) Theft

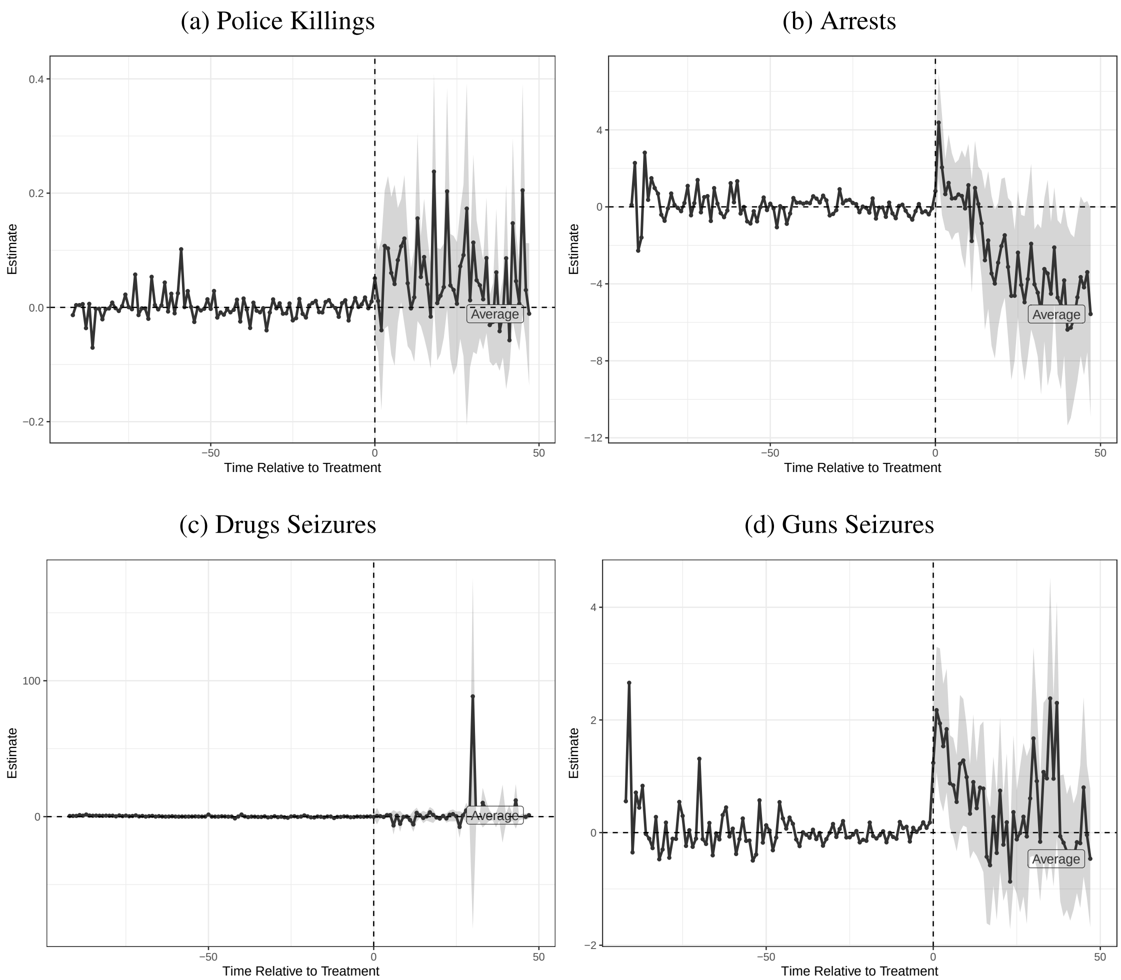


(d) Bank Robbery



Note: This figure presents the average treatment effect on the treated following (Arkhangelsky et al. 2021; Ben-Michael, Feller and Rothstein 2022). In the setting of staggered roll-out of RAIO squads, the estimates are weighted average of synthetic controls constructed for each treated municipality.

Figure C.2: Synthetic difference-in-difference estimates



Note: This figure presents the average treatment effect on the treated following (Arkhangelsky et al. 2021; Ben-Michael, Feller and Rothstein 2022). In the setting of staggered roll-out of RAIO squads, the estimates are weighted average of synthetic controls constructed for each treated municipality.

Table C.1: **Effects of RAIO patrols on crime using different estimation approaches**

	Methods and estimands		
	Callaway and Sant'Anna (2021) (not yet treated)	**TWFE**	**Arkhangelsky et al. (2021) Synthetic DiD**
Dependent Variable	**ATT**	**ATT**	**ATT**
Homicides	-1.252***	-0.225	-0.099
	(0.446)	(0.164)	(0.337)
Police Killings	-0.081	0.016	0.067**
	(0.084)	(0.014)	(0.029)
Robberies	-17.207***	-7.605***	-6.665***
	(5.08)	(1.303)	(2.090)
Thefts	-5.061***	-3.007***	-1.803*
	(1.917)	(0.750)	(0.952)
Bank Robberies	0.000	-0.002	0.008
	(0.034)	(0.005)	(0.007)
Arrests	-2.989**	-2.813***	1.635**
	(1.409)	(0.697)	(0.785)
Guns Seized	-0.678	0.602***	1.331***
	(0.584)	(0.215)	(0.331)
Drugs Seized	0.889	1.840	-0.78
	(2.629)	(1.409)	(1.149)
Domestic Violence	1.907	1.416***	1.173*
	(1.879)	(0.415)	(0.677)
Sexual offenses	0.291	0.115***	0.094
	(0.218)	(0.043)	(0.069)
Observations	18.300	18.300	18.300
Time F.E.	✓	✓	✓
Municipality F.E.	✓	✓	✓
Spillovers	✗	✗	✗
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 not yet treated municipalities as control groups, respectively, while Columns 3 and 4 presents the standard Two Way Fixed Effects (TWFE) estimator. In Columns 2 and 4 we introduce our measurement of spatial spillover in the estimation. Standard errors in parenthesis are clustered via bootstrap at the municipality level. Significance levels are as follows: $p < 0.01$ ***; $p < 0.05$ **; $p < 0.10$ *.

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

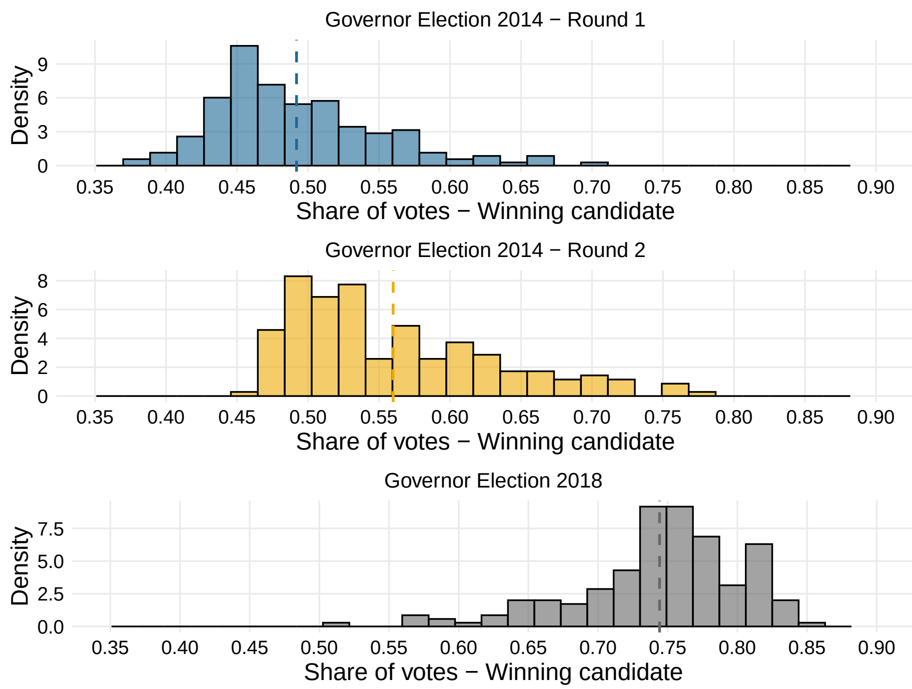


Figure D.2: Change in level means

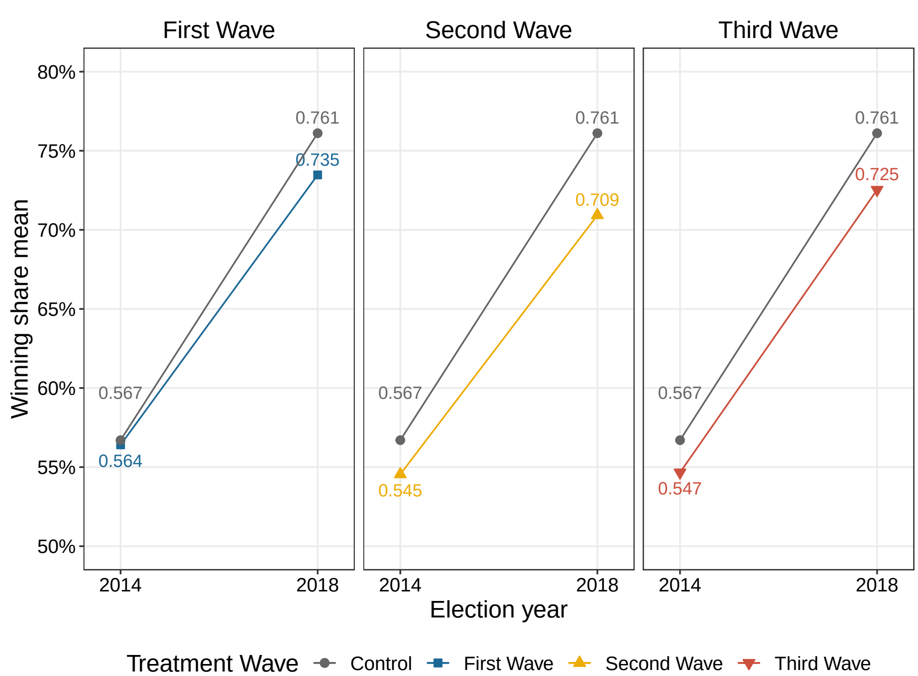


Table D.1: **Average treatment effects of RAIO on voting**

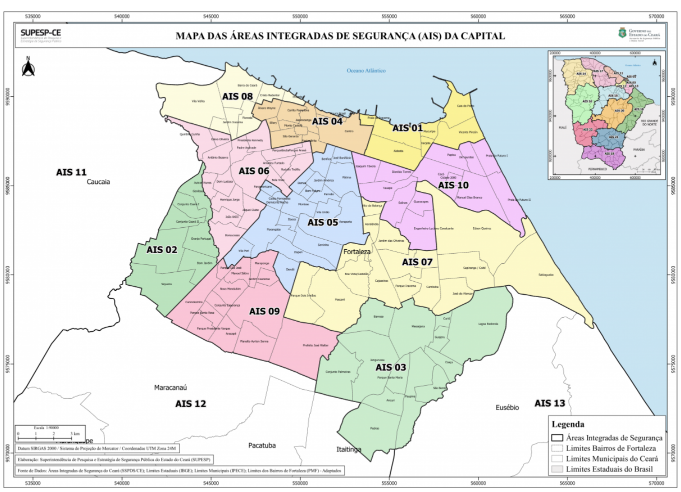
	Share of Votes		Total Votes	
	(1)	(2)	(3)	(4)
RAIO: Treatment	0.1897***	0.0781**	9,868.0***	4,213.5***
	(0.0130)	(0.0371)	(890.2)	(713.5)
Homicides		-0.0166		-777.7
		(0.0263)		(634.3)
Population		0.0003***		6.083***
		(0.0000)		(0.7235)
Robberies		0.0034		-52.13
		(0.0023)		(82.88)
Formal Jobs		0.0000		-0.0499
		(0.0000)		(0.4296)
Guns Seized		0.0184		627.6**
		(0.0128)		(275.2)
Drugs Seized		-0.0021		-17.63
		(0.0014)		(59.73)
Thefts		0.0089***		14.29
		(0.0028)		(62.13)
Sexual offenses		0.0242		442.4
		(0.0258)		(504.2)
Domestic violence		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. All covariates are measured prior to the intervention to avoid post-treatment bias and conditioning on variables potentially affected by treatment. Standard errors are clustered at the municipality level. Significance levels are as follows: 1%, ***; 5%, **; 10%, *.

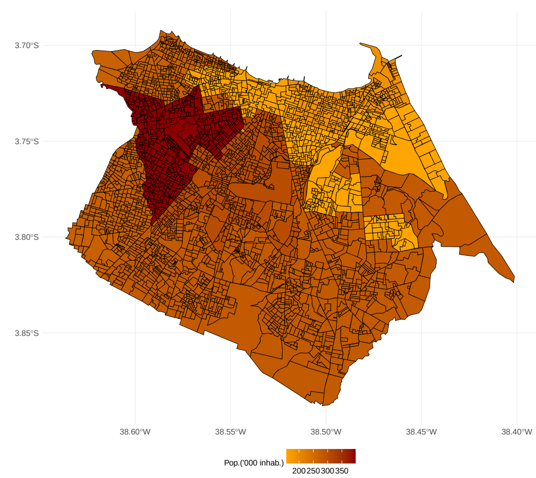
E. Survey methodology

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.

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**

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 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 between the census and the 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. Additional survey results

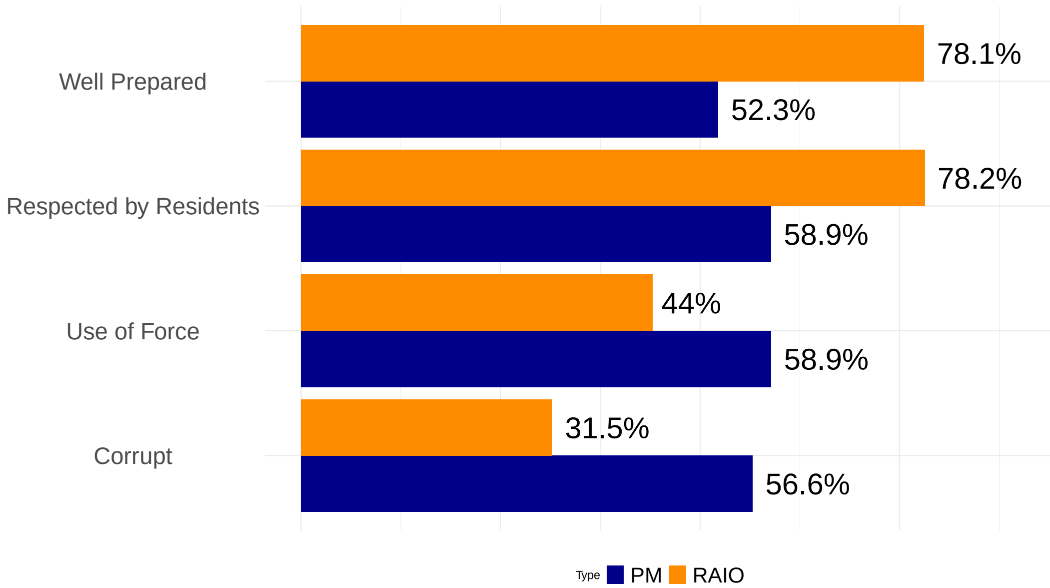
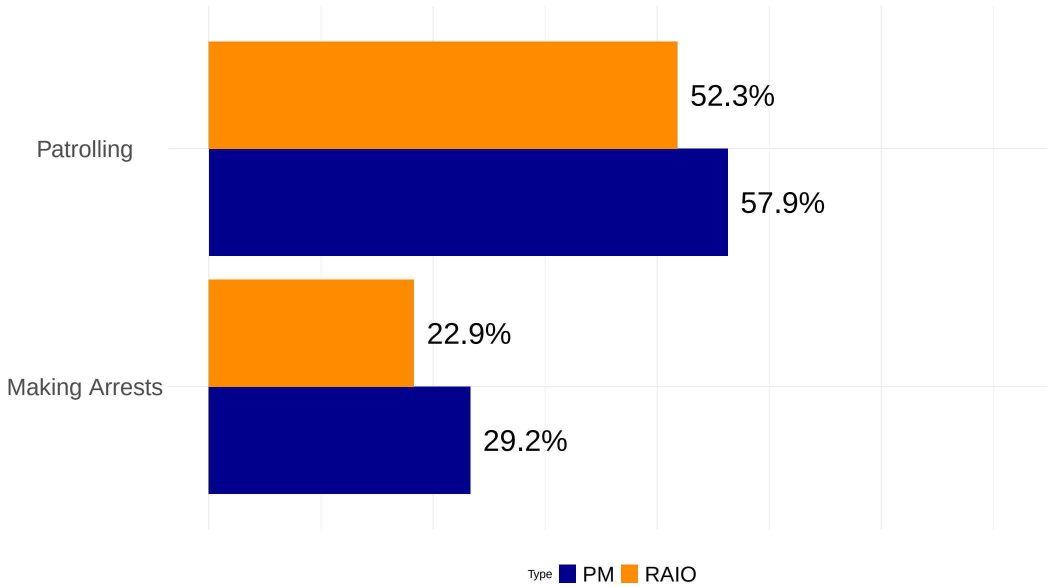


Figure F.1: **Comparative non-experimental evaluation of RAIO and ordinary military police**

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

Figure F.2: **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.

Here we present in narrative form results from the conjoint survey that we excluded from the manuscript for compactness.

We find that the RAIO uniform causes more positive assessments of effectiveness (panels a, b, and c of 6). 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 assault 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 assault 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 assault rifle and motorcycle.

In terms of perceptions of just treatment (panels d and e of 6), the RAIO uniform increases the probability 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 assault rifle and motorcycle: having been shown the assault 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 analysis 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, indicating broad-based preferences for RAIO. These subgroup results are available upon request.

G. TABLES FOR CONJOINT ESTIMATES

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

	Uniform		***Weapon***		***Vehicle***	
	Military Police	*RAIO*	*Pistol*	*Assault 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 G.2: Who makes you feel safer?

	Uniform		***Weapon***		***Vehicle***	
	Military Police	*RAIO*	*Pistol*	*Assault 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 G.3: Who is more likely to commit human rights abuses?

	Uniform		***Weapon***		***Vehicle***	
	Military Police	*RAIO*	*Pistol*	*Assault 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 G.4: Who is more likely to be corrupt?

	Uniform		***Weapon***		***Vehicle***	
	Military Police	*RAIO*	*Pistol*	*Assault 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 G.5: Who is more likely to use force against criminals?

	Uniform		***Weapon***		***Vehicle***	
	Military Police	*RAIO*	*Pistol*	*Assault 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 G.6: Standardized Index - All Questions (1)

	Uniform		***Weapon***		***Vehicle***	
	Military Police	*RAIO*	*Pistol*	*Assault 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 G.7: Standardized Index - All Questions (2)

	Uniform		***Weapon***		***Vehicle***	
	Military Police	*RAIO*	*Pistol*	*Assault 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.

H. SURVEY-RELATED 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.

I. Cost-benefit analysis assumptions

In order to conduct our cost-benefit analysis of RAIO, we use a detailed breakdown of program expenses provided by the State Security Secretariat to compute annual costs per municipality. Based on administrative financial records, the estimated cost of maintaining a RAIO base is approximately R$6,126,842 per municipality-year (US$1,225,368 using an exchange rate of 5 BRL/USD).

I.1. Cost Assumptions

Personnel expenditures account for the largest share of program costs. Each RAIO base is staffed by 23 officers (one captain, two lieutenants, four sergeants, eight corporals, and seven soldiers), and personnel costs include wages and benefits. Office expenditures are treated as fixed annual costs based on government financial statements. Additional costs include vehicles, training, weapons, protective equipment, and communication infrastructure. Vehicle expenditures reflect the allocation of motorcycles and patrol vehicles throughout the broader RAIO structure, while training and equipment costs are prorated across operational bases.

I.2. Crime reduction

As noted in the manuscript, 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 reduced annually. Under these estimates, the average cost per robbery deterred is approximately US$5,903, while the average cost per homicide reduced is US$78,549.

While we lack precise estimates of the economic cost of crime in Brazil, we rely on 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, including losses from stolen goods, damages, productivity declines, public spending on security and justice, and broader social impacts. Robberies represent 70% of reported crimes and account for approximately US$120.4 billion in annual costs. Dividing this figure by the estimated 10 million robberies committed annually yields an average cost of US$12,040 per robbery, about twice the estimated cost of deterring a robbery through RAIO. Homicides account for approximately 0.5% of regional GDP, or US$25 billion annually. With an estimated 140,000 homicides per year, the implied cost per homicide is roughly US$178,570. Even under our updated and more comprehensive cost estimates, the economic cost of a homicide remains more than twice as large as the estimated cost of preventing one through the RAIO program.

J. Focus group guiding scripts

Rank-and-file officers of the Ordinary Military Police or RAIO

Opening and Warm-Up (5 minutes)

To begin, please briefly introduce yourselves:

- Years of service;
- Current unit;
- A word or short phrase describing how you feel today about police work.

Warm-up question: When you tell someone you are a police officer, what is their reaction? (Moderator probe if necessary: pride, distrust, respect, fear, indifference?)

Block 1: Who Works and Why? (10 minutes)

Objective: Understand who works in policing and why.

- How would you describe a RAIO officer?
- Is a RAIO officer different from officers in the rest of the force? (Better trained? More committed? More hardworking? Less involved in misconduct?)
- What is your main motivation for working in the PMCE/RAIO?

Do you feel that your work has a positive impact? When is this most evident? When does it seem to make little difference?

Block 2: RAIO's influence on other units (20 minutes)

Objective: Understand whether RAIO's example has positive or negative spillovers.

- Within the PMCE, which units are most attractive? Why?

- What matters most when choosing a unit?
  - Type of activity?
  - Prestige?
  - Working conditions?
  - Risk?
  - Internal or external recognition?
- How do officers from other units perceive your group?
- Have you experienced situations that reinforced these perceptions?
- Have you seen colleagues change behavior to qualify for RAIO?
- What are the advantages of a unit like RAIO for the broader force? And the disadvantages?
- Have any RAIO practices spread to the rest of the force?

Block 3: accountability (15 minutes)

Objective: Understand perceptions of mission, supervision, and control mechanisms.

- What does "doing good police work" mean?
- What data or information do you use in daily work?
- How are patrol decisions made?
- How does supervision work?
- Is police work heavily monitored?
- Do you know officers removed from RAIO for misconduct?
- What mechanisms exist to identify and sanction misconduct?

Block 4: Institutional separation

- What everyday practices distinguish RAIO from the rest of the force?

Closing reflection (5–10 minutes)

- Is there anything important about police work or the PMCE that was not discussed but should have been?