# Peace Agreements as Triggers of Violence: Evidence from the 2016 Peace Referendum in Colombia*

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**Abstract**

Partial peace agreements with a subset of non-state actors can disrupt existing power dynamics within a conflict, potentially triggering new episodes of violence. Using a regression discontinuity approach, the analysis reveals a statistically significant increase in violence perpetrated by non-state armed groups, other than the FARC, in municipalities that rejected the peace agreement between the Colombian government and the FARC insurgency in the 2016 peace referendum. I interpret the peace agreement as an economic shock that altered the incentives for violence among non-state armed groups that did not participate in the peace process. Consistent with this interpretation, I find that the increase in violence is more pronounced in areas characterized by significant coca cultivation and gold mining activities.

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# 1 Introduction

The PA-X database reports 2,055 peace agreements associated with 175 peace processes.[1] Among these, 443 are ceasefire agreements within 166 peace processes, representing 95% of all peace processes. After cross-referencing with the Uppsala Conflict Data Program (UCDP), I calculated that 48 conflicts reignited after the signing of a ceasefire agreement, indicating a failure rate of about 29% for these peace processes. A closer inspection reveals that these 166 peace processes span 49 countries, with 29 of these countries facing multi-actor armed conflicts—where state forces combat more than one non-state armed actor. This means that 60% of the countries that return to conflict after arranging peace agreements are those dealing with multi-actor armed conflicts. This suggests that the presence of multi-actor non-state armed conflicts is a significant predictor of failed peace processes.

What factors contribute to the resurgence of violence in post-conflict societies? Peace agreements are pivotal in resolving armed conflicts and fostering long-term stability. Partial peace agreements involving only some non-state armed actors can disrupt existing power dynamics and potentially trigger new violence.[2] These disruptions often stem from the reallocation of resources and territorial control, creating economic shocks that alter the incentives for violence among groups excluded from the peace process.

This study focuses on the peace agreement between the Colombian government and the FARC insurgency, one of the longest-running and most complex conflicts in Latin America. The peace process, which culminated in a national referendum, aimed to end decades of violence and integrate FARC combatants into civilian life. However, the referendum results, particularly the rejection of the peace agreement in specific municipalities, had unintended consequences on selective violence dynamics.

I argue that the peace agreement served as an economic shock, altering the incentives of non-state armed groups excluded from the process. Organizations like the ELN, paramilitary factions, and criminal networks seized the opportunity to expand their influence in territories formerly dominated by the FARC. This transition spurred selective violence, especially in areas offering significant potential for territorial con-

[1] The PA-X Peace Agreement Database.

[2] A partial peace agreement is a type of accord that is negotiated and signed between a government and one or more, but not all, of the non-state armed actors involved in a conflict. Unlike comprehensive peace agreements, which aim to address and resolve all aspects of the conflict by including all significant parties, partial peace agreements deal with only a subset of the warring factions

trol, substantial coca cultivation or gold mining, limited civilian capacity to resist, and a weak state monopoly on violence.

I use a regression discontinuity design with a discrete score variable to study the theoretical argument. The analysis reveals a statistically significant increase in violence in municipalities that rejected the peace agreement. Point estimates show an average increase of 211% in one-sided attacks perpetrated by non-state armed groups during the sample period. Additionally, The effects are particularly evident during the window when the international community, the FARC, and the Colombian government were occupied with the disarmament process, creating an opening for other armed groups to seize strategic territories. The study explores the impact of historical violence and electoral preferences on the referendum results, revealing no significant correlation. The results remain robust under the standard identification assumptions of a regression discontinuity design. They are also robust to various methodological choices, such as using unequal bandwidths, incorporating quadratic polynomials, and applying different kernel functions.

By rejecting the peace agreement, many communities signaled their strong preference for harshly punishing armed actors, effectively guiding non-state armed groups—excluded from the peace process—toward territories they deemed susceptible in the wake of the FARC's departure. In municipalities that rejected the peace agreement, the interplay of economic incentives, weak civilian resistance, and limited state capacity produces a perfect environment for selective violence. Not only do these areas often contain valuable resources such as coca cultivation and gold mines—enticing targets for armed groups seeking to secure or expand their revenue streams—but they also suffer from insufficient local organization or social capital to push back against incursions. Compounding these vulnerabilities, inadequate military presence from the state further emboldens non-signatory armed actors, enabling them to intimidate or eliminate potential opposition with relative ease. Consequently, the combination of resource wealth, minimal resistance, and weak state enforcement intensifies the likelihood and severity of selective violence in these municipalities.

This paper contributes to the broader understanding of the unintended consequences of partial peace agreements and the economic motivations behind post-agreement violence. There is substantial evidence demonstrating that economic shocks can lead to increased violence. For instance, Miguel et al. (2004a) finds that negative economic shocks, like adverse weather affecting agricultural yields, significantly increase the likelihood of civil conflict in sub-Saharan Africa. Similarly, Bazzi and Blattman (2014) conclude that while economic shocks might not initiate new conflicts, they can significantly influence ongoing ones by contributing to their persistence or increasing their intensity. Dube and Vargas (2013) examines the opportunity cost and rapacity effects that link conflict with income fluctuations, as proposed by Dal Bó and Dal Bó (2011). Dube and Naidu (2015) finds that US military aid increases paramilitary attacks, especially during election years and in politically competitive municipalities, but has no impact on guerrilla attacks. In this paper, I demonstrate that peace agreements can act as economic shocks, potentially triggering increased selective violence against the civilian population.

The paper also contributes to the literature on post-electoral violence by illustrating how political shifts, such as the outcomes of peace referendums, can escalate armed conflicts. Democracy, while often seen as a mechanism for peaceful conflict resolution and political stability, can paradoxically generate political violence under certain conditions. Competitive elections can exacerbate existing societal tensions, especially in divided societies. Chacón et al. (2011) argues that as the size of a group increases, so does its likelihood of winning an election or an armed conflict. Therefore, if all groups have a chance of electoral success, they also have a high likelihood of prevailing in a fight. Collier and Rohner (2008) suggests that democracy decreases rebellion in wealthy countries while increasing it in poorer ones. In the Colombian case, Fergusson et al. (2021) shows that when left-wing parties, previously excluded, narrowly win local executive office, there is a corresponding increase in violent events by right-wing paramilitaries. This paper highlights the unintended consequences of expressing preferences about peace and war during a peace referendum on post-electoral violence. It demonstrates how the outcomes of such referendums can influence the actions of armed groups and escalate violence.

Finally, this paper contributes to the existing literature on the consequences of the peace agreement signed between the Colombian government and the FARC insurgency in 2016. There is research on the effects of the peace agreement on the production of coca crops (Prem et al., 2023), fertility choices (Guerra-Cujar et al., 2024), human capital (Prem et al., 2020a), firm creation (Bernal et al., 2024), selective violence (Prem et al., 2022), deforestation (Prem et al., 2020b), and landmine victimization (Perilla et al., 2024).

## 2 The Unintended Consequences of Peace Agreements

Peace agreements can fail for a variety of reasons, reflecting the complex and dynamic nature of post-conflict environments. A major challenge is the lack of trust among parties to the agreement, as historical grievances and deep-seated mistrust can hinder cooperation and undermine compliance (Hartzell and Hoddie, 2003; Walter, 2002). Spoilers—individuals or groups who profit from continued violence—often seize on these vulnerabilities to subvert the peace process (Reiter, 2016; Stedman, 1997). Political and economic incentives may also diverge from peace objectives, leading key actors to pursue personal or group interests over broader stability (Collier et al., 2008; Weinstein, 2006). Excluding significant stakeholders from negotiations can further sow dissatisfaction and foster resistance (Nilsson, 2012; Paffenholz, 2014). Meanwhile, socio-economic pressures such as poverty, unemployment, and weak state institutions create an environment in which violence can easily resurface (Cederman et al., 2011; Collier and Hoeffler, 2004; Prem et al., 2022).

Much of the literature on failed peace processes and the unintended consequences of peace agreements focuses on scenarios where a state negotiates with a single actor, often overlooking contexts in which a central government confronts multiple uprisings by actors with varied interests. In such settings, the interplay among non-state armed groups can heavily shape a peace process's outcome, as these groups are entangled in overlapping alliances, rivalries, and struggles for resources or territorial control. A peace agreement between a government and one armed faction can prompt multiple reactions among those who remain outside the deal: while non-signatory groups may perceive the accord as a threat to their influence—driving them to sabotage or "spoil" its implementation—they may also see it as an opportunity to expand their own power by seizing territory or resources relinquished by the signatory actor. This dynamic is illustrated in Colombia, where, following the peace agreement with the FARC, organizations such as the National Liberation Army (ELN) and various criminal networks occupied areas previously under FARC control, intensifying violence and reasserting their own agendas in the process (Prem et al., 2022).

In conflicts fueled by economic incentives, controlling valuable resources—whether legitimate commodities like minerals or oil, or illicit markets such as drug trafficking—often drives many armed groups (Dube and Vargas, 2013). In this context, partial peace agreements can function as an economic shock for any factions left out of the deal. Because armed groups involved in illicit economies thrive on instability and weak governance, the resulting power vacuums allow non-signatory actors to fight for and seize valuable resources previously held by the demobilized group. Consequently, while partial peace agreements aim to reduce violence, they may inadvertently intensify competition over economic assets, empower non-signatory groups, and increase the very instability they seek to address (Prem et al., 2021).

The expansion of territorial control by non-signatory actors can spark violence against civilians, but this outcome is by no means guaranteed. Several mechanisms either constrain or motivate the use of force. First, although non-signatory groups may seek to consolidate power in newly vacated areas, violence tends to be selective rather than indiscriminate; these groups weigh local attitudes toward the peace agreement before deciding where to exert violence. Second, territorial expansion is often limited to locations adjacent to municipalities experiencing a power vacuum, largely for logistical reasons. Third, non-signatory actors prioritize acquiring control over territories that offer valuable resources—such as illicit crops, minerals, or other revenue-generating assets—to sustain their operations. Fourth, the civilian population can constrain violence by resisting or negotiating with armed actors, altering the cost–benefit calculus of using violence. Finally, a robust state presence, especially when the government maintains an effective monopoly on legitimate violence, can deter territorial expansion by non-signatory groups and lessen the likelihood of civilian victimization.

I contend that in multi-actor armed conflicts fueled by greed, partial peace agreements can have adverse effects. When one group demobilizes, the illicit resources that were once their domain become available to the remaining armed groups. This redistribution of resources can incite violence among the non-signatory actors as they fight for control, ultimately escalating conflict. In contexts where multi-actor armed conflicts are driven by economic interests, partial peace agreements can be interpreted as economic shocks, which can have destabilizing effects similar to those caused by abrupt changes in income. There is ample evidence demonstrating that income shocks are significant drivers of violence (Bazzi and Blattman, 2014; Dal Bó and Dal Bó, 2011; Dube and Vargas, 2013; Elliott and Kreutz, 2019; Gawande et al., 2017; Le Billon, 2005; Miguel et al., 2004b; Miguel and Satyanath, 2011; Morelli and Rohner, 2015; Nwokolo, 2021; Ross, 2004).

The expansion of territorial control by non-signatory actors often induces violence, but as Kalyvas (2006) argues, this violence is neither random nor indiscriminate—it is deployed strategically to consolidate authority and deter opposition. In newly contested areas, armed groups rely on violence to gain local compliance, secure cooperation from civilian populations, and dissuade potential rivals or defectors. By carefully calibrating their use of force, non-signatory actors can establish de facto governance and extract resources, all while limiting open resistance (Arjona, 2016). The strategic use of violence by non-signatory armed groups is closely tied to local attitudes toward peace agreements. Informed by polls, electoral outcomes, or other signals reflecting public opinion, these groups selectively deploy force to maximize their control while minimizing resistance.

Armed groups are not immune to local political preferences, and electoral results often serve as a crucial indicator of where a population stands. By monitoring voting patterns and outcomes, non-state armed groups gain insights into the public's support. According to Steele (2017), armed groups carefully monitor local election results and interpret these outcomes as signals of political loyalty or opposition. Paramilitary groups in Colombia leveraged electoral data to decide which communities to target for displacement, primarily to consolidate political and social control. Steele (2017) highlights that displacement is not random. It is used deliberately to either remove an uncooperative population or to prevent potential collaboration with rival groups.

Communities that reject peace agreements may do so because they favor a more punitive response to former insurgents, believing that leniency compromises security. In the eyes of new or dissident armed groups, these communities represent a potential obstacle: they are less likely to tolerate another illegal force stepping in and may even collaborate with government crackdowns. By signaling their opposition to a softer approach through public opinion or voting behavior, these communities convey a willingness to support forceful state action against non-state actors. Tellez (2019a) shows that the design of specific provisions in a peace agreement can significantly sway public support for ending armed conflict. Citizens' approval often hinges on how ex-combatants are punished: harsher penalties for perpetrators generally increase backing for an accord, whereas allowing former fighters immediate access to political processes—such as congressional seats or the right to form a political party—can spark pronounced disapproval among certain segments of the population. Montoya and Tellez (2020) also shows that some communities radicalize after severe violence, demanding harsher measures or retribution.

Many people who demonstrate strong preferences against a peace agreement do so precisely because they seek stricter retribution for ex-combatants, revealing not only skepticism toward lenient terms but also a reluctance to collaborate with any other non-state armed groups looking to fill the power vacuum left by the demobilized armed group. Consequently, non-state armed groups anticipate stronger resistance and a heightened risk of exposure if they attempt to operate in such areas, making selective violence a more likely tactic to deter cooperation with the government and to assert control despite local hostility. As a result, non-state armed groups will tend to employ more selective violence in communities that reject peace agreements, aiming to quell resistance and maintain their territorial ambitions.

In extremely resource-rich or strategically valuable territories—such as those hosting steady income from illicit crops, lucrative mineral deposits, or critical transportation corridors—armed groups face powerful incentives to gain and hold control at any cost, as argued by Arjona (2016). When the financial or strategic stakes are high, rebels or paramilitaries may commit substantial resources to repression, overwhelming local opposition and disregarding institutional constraints. By establishing strict, centralized governance, they can secure vital assets (coca crops, gold mines, smuggling routes, etc.) and protect the flow of revenue critical to their survival or expansion. In these contexts, the potential gains from seizing and maintaining territorial control far outweigh the costs of intensified violence, reinforcing the logic that competition over valuable territories is a primary driver of conflict dynamics and civilian victimization. As a result, non-state armed groups are more willing to employ selective violence against civilians in economically valuable areas to suppress dissent and consolidate their control.

While non-state armed groups often resort to selective violence in pursuit of territorial and economic control, local factors can significantly mitigate or deter such aggression. Kaplan (2017) shows evidence that community-level organizing, including social networks of mutual defense, can raise the costs of violence for armed actors. Such collective actions—from public protests, social activism, and demonstrations to voter mobilization—signal that communities will actively resist or publicize abuses. Robust local institutions, such as village councils, religious networks, or peasant associations are key to a community's ability to resist or mitigate violence from armed groups. Likewise, Arjona (2016) shows that the ways civilians organize and negotiate governance structures within conflict zones can constrain rebel behavior by setting norms and expectations around acceptable conduct. Civilians are not passive. Their preferences, social structures, and capacity for collective organization influence whether rebels can impose a "rebelocracy" or must settle for an "aliocracy." Through these mechanisms, civilians can assert a measure of autonomy and deterrence, thereby reducing the likelihood and severity of selective violence by non-state armed groups. Conversely, where the civilian capacity to resist is weaker, selective violence by non-state actors tends to escalate.

A robust state presence is essential for reducing civilian victimization in post-conflict settings, particularly when development programs are integrated with security policies. As Collier (2009) argues, effective security forces bolster investor confidence and enable the restoration of public services, laying a foundation for economic recovery. North et al. (2012) also underscore that transitioning from fragile "limited access orders" to durable "open access orders" depends on the state's capacity to contain violence and promote inclusive governance. By strengthening these institutional foundations and ensuring that security and development initiatives go hand in hand, post-conflict states can make progress toward the ideal of a legitimate monopoly on force, thereby lowering the likelihood of renewed violence. Ultimately, where state capacity—especially in terms of a credible military presence—is lacking, non-state armed groups face fewer deterrents to using selective violence to maintain or expand their influence.

While partial peace agreements aim to reduce conflict, they can inadvertently alter incentives in ways that encourage the escalation of violence. To test my theoretical argument that partial peace agreements can act as economic shocks, leading to increased violence among non-signatory armed groups, I will employ a quasi-natural experiment in Colombia, utilizing the peace agreement between the FARC insurgency and the Colombian government in 2016 as a case study.

## 3 The Colombian Context

The confrontation between the Colombian government and leftist guerrilla groups began in the 1960s with the formation of the Revolutionary Armed Forces of Colombia (FARC) and the National Liberation Army (ELN). These guerrilla groups, inspired by Marxist-Leninist ideology, sought to address social inequalities and redistribute land to the peasantry. Over time, the conflict evolved, drawing in other actors, including paramilitary groups and criminal organizations, leading to a multifaceted and protracted war.

As the conflict in Colombia progressed, non-state armed groups increasingly engaged in drug trafficking and illegal mining to fund their operations. Initially motivated by ideological goals, these groups gradually found that involvement in the lucrative drug trade and control over illegal mining operations provided substantial financial resources (Abadie et al., 2015; Saab and Taylor, 2009). The FARC, in particular, became deeply entrenched in the cocaine trade, overseeing coca cultivation, production, and distribution networks. By the early 2000s, it was estimated that a significant portion of the FARC's income—up to 60%—was derived from drug trafficking, while illegal mining also emerged as a critical revenue stream, contributing to their financial resources (Brittain, 2010; Mejía, 2016). This shift towards economic-driven activities marked a significant evolution in the nature of the conflict, blending ideological motivations with profit-oriented criminal enterprises (Mejia and Restrepo, 2013).

In 2011, the Colombian government initiated secret conversations with the FARC insurgency, aiming to negotiate an eventual demobilization of the guerrilla group. These discussions were a strategic move to build trust and set the stage for more formal peace negotiations. In 2012, the Colombian government and the FARC insurgency made a formal announcement in Havana, Cuba, signaling the formal opening of peace negotiations. This announcement marked a significant milestone in the peace process, outlining key points of discussion, including land reform, political participation, disarmament, and the rights of victims. The peace talks aimed to address the root causes of the conflict and create conditions for lasting peace and stability in Colombia. After several years of intense negotiations, the FARC announced a formal ceasefire in July 2016. This ceasefire paved the way for the finalization of the peace agreement.

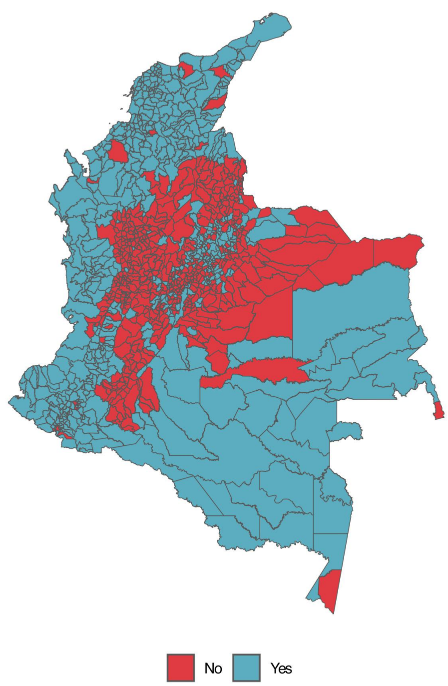
In an effort to legitimize the peace agreement with the FARC insurgency and ensure broad national support, the Colombian government decided to hold a national referendum.[3] This decision reflected the government's commitment to seek the approval of the Colombian people for the terms negotiated with the FARC. The referendum, held on October 2, 2016, posed a simple question to voters: whether they accepted or rejected the peace accord. Despite widespread anticipation and international support for the peace process, the referendum resulted in a narrow rejection of the agreement, with 50.2% voting against it (Branton et al., 2019).

Figure 1 depicts the geographic distribution of the peace referendum results and reveals a stark contrast in voting patterns across Colombia. It shows that the peace

[3]The peace referendum was sanctioned by both the Constitutional Court and Congress, with specific criteria to ensure its validity. For the referendum to be valid, two conditions had to be met: first, the percentage of votes in favor of the peace deal needed to constitute at least 13% of the total electorate; second, the number of supporting votes had to exceed the number of votes rejecting the peace deal. These requirements were put in place to confirm that the peace agreement had substantial support from the Colombian population.

agreement was predominantly supported by municipalities located at the periphery of the country. These areas, characterized by their remoteness, had borne the brunt of the armed conflict. As a result, they are municipalities with low levels of development, high poverty rates, significant presence of coca crops, and historically high levels of violence (Tellez, 2019b). Following the unexpected rejection of the peace agreement in the October 2016 referendum, the Colombian government and the FARC insurgency promptly initiated a second round of negotiations to address the concerns and issues raised by the referendum's outcome. In December 2016, the Colombian government and the FARC formally signed the final peace agreement.

Figure 1. Outcome of the peace referendum in October 2016

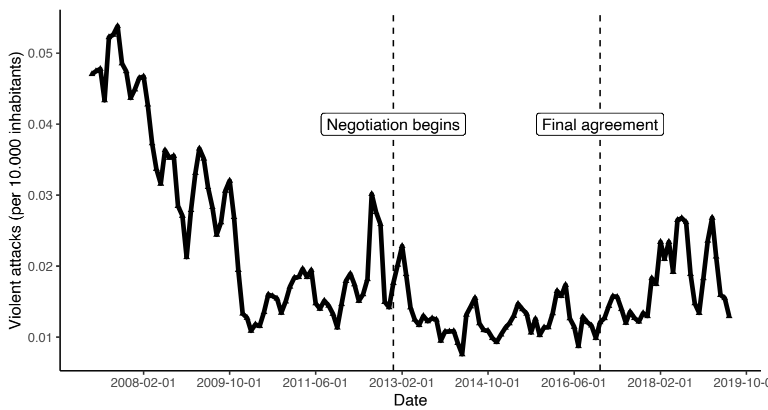



The map presents the distribution of the outcome of the referendum across Colombian municipalities in October 2016 and does not include the islands of San Andrés, Providencia, and Santa Catalina.

While the FARC insurgency and the Colombian government were engaged in peace negotiations, other non-state armed groups, such as the ELN, Clan del Golfo, and various criminal organizations, continued to operate. These groups maintained their involvement in illicit activities, particularly drug trafficking, which remained a significant source of revenue and power. These armed actors exploited the power vacuum left by the FARC in certain regions, expanding its influence and control over drug production and distribution networks (Prem et al., 2022).

Figure 2 illustrates the evolution of attacks perpetrated by non-state actors, normalized by population, from 2007 to 2019. It marks key milestones, including the beginning of the peace negotiations in 2012 and the signing of the final peace agreement in December 2016. The data shows a general downward trend in violence leading up to the negotiations, reflecting broader efforts to reduce conflict. However, a sharp reduction in violence is particularly noticeable starting in 2012, coinciding with the announcement of ceasefires by the FARC as part of the peace process. This period of decreased violence highlights the immediate impact of the peace talks on reducing hostilities. Despite this progress, the figure also shows a concerning trend following the signing of the peace agreement. From late 2016 onwards, there is a notable increase in violent events, with levels of violence eventually rising to resemble those observed in 2009.

Figure 2. Violent attacks perpetrated by non-state armed groups (2007-2019)




The figure presents the evolution of violent attacks perpetrated by non-state armed actors between 2007-2019 in Colombia. It highlights the date of the beginning and end of peace negotiations between the Colombian government and the FARC insurgency.

My argument is that the peace agreement between the Colombian government and the FARC insurgency created an economic shock within the context of the armed conflict. Before the peace negotiations (2007-2011), the FARC was active in 109 municipalities, 29 of which had coca crops, representing 30% of all coca-producing municipalities. These areas covered 37,015 hectares, or 38% of the total coca crop area in Colombia, which was 96,085 hectares in 2015. After the FARC's demobilization, other non-state armed groups likely took over this production. In 2015, each hectare produced an average of 6.8 kg of cocaine hydrochloride, priced at US$1,732 per kilo (UNODC, 2016). Thus, the FARC-controlled municipalities could produce approximately 251,702 kg of cocaine hydrochloride, with a market value of around US$436 million. The unintended economic effects of the partial peace agreement are evident in this economic shift, which the redistribution of such a resource likely fueled increased competition and violence among the remaining armed groups.

The increase in violence following the FARC's demobilization is unlikely to be widespread but will instead concentrate in specific areas. As argued in Section 2, non-state armed groups will use the peace referendum results as an indicator of the costs associated with controlling former FARC territories (Steele, 2017). The peace referendum outcome reveals which communities strongly favor a more punitive approach to former insurgents (Tellez, 2019a; Montoya and Tellez, 2020). In areas that voted against the agreement, these groups anticipate local support for harsher retaliation and greater hostility toward armed actors, therefore:

**Hypothesis 1.** *Non-state armed groups excluded from the peace negotiations will be more likely to employ selective violence in communities that rejected the peace agreement.*

My empirical analysis reveals heterogeneous effects of selective violence, underscoring its strategic character. Specifically, municipalities exposed to non-state armed groups become targets as these actors seek to consolidate territorial control (Prem et al., 2022). Selective violence also converges on areas offering key resources—from economic assets to logistical advantages—that armed groups desire (Arjona, 2016). Moreover, it intensifies where civilian resistance is weak, allowing perpetrators to operate with minimal backlash (Kaplan, 2017; Arjona, 2016). Finally, municipalities where the state fails to hold a monopoly on violence experience more frequent targeted attacks, since the absence of a robust security apparatus enables non-state actors to exert dominance through selective violence (Prem et al., 2022). The following hypotheses outline the expected relationships between these factors and the incidence of selective violence in municipalities that have already rejected the peace agreement:

**Hypothesis 2.** *Municipalities exposed to the influence of non-state armed groups that did not participate in the 2016 peace agreement, where those groups aim to solidify territorial control, will experience higher levels of selective violence.*

**Hypothesis 3.** *Municipalities that provide key resources (e.g., economic or logistical* *assets) to non-state armed groups excluded from the 2016 peace agreement will be targeted by selective violence as part of a strategic resource-capture strategy.*

**Hypothesis 4.** *Municipalities with low levels of civilian resistance are more vulnerable to selective violence carried out by non-state armed groups that did not participate in the 2016 peace agreement.*

**Hypothesis 5.** *Municipalities where the state does not hold a monopoly on violence will experience increased selective violence by non-state armed groups that did not participate in the 2016 peace agreement.*

This study focuses on the period from March 2017 to June 2017, which captures a critical juncture in Colombia's post-conflict transition. Between October 2016 and January 2017, FARC insurgents were still entrenched in their traditional strongholds. However, on January 31, 2017, FARC members began relocating to 26 designated "*Zonas Veredales Transitorias de Normalización*" (ZVTNs) to initiate the disarmament and reintegration process. By June 27, 2017, the United Nations verified that all registered individual arms had been handed over, marking the official end of the FARC as an armed organization. During the months of March through June, FARC combatants underwent registration and identification procedures in the ZVTNs, and the disarmament process—commencing in March—gradually advanced throughout this window. With both the Colombian government and the FARC insurgency absent from the latter's former strongholds, a power vacuum emerged that other non-state armed groups sought to fill. Consequently, this period is when such groups were most likely to employ selective violence in an effort to expand their territorial control.

# 4 Empirical Strategy

## 4.1 Data

I utilized conflict data from Universidad del Rosario, which provides detailed information on violent events, including the armed actor involved, location, date, and other relevant characteristics.[4] This dataset enables the identification of four primary non-state armed groups: the FARC insurgency, the ELN, paramilitary groups, and other

[4] The conflict data used was compiled using reports from *Revista Noche y Niebla*. These statistics are highly reliable, as they are derived from news reports from 25 major Colombian newspapers, supplemented by detailed reports from Catholic priests documenting incidents of political violence. Furthermore, these events are cross-verified with official government reports to ensure accuracy.

groups. I focus on the number of monthly one-sided attacks against the civilian population initiated by these non-state armed groups from March 2017 to June 2017, transforming this variable by standardizing it to have a mean of zero and a standard deviation of one.

I incorporated voting data from the *Registraduría Nacional del Registro Civil*, which provides detailed results from the peace referendum. Specifically, I calculate the vote share for both options—supporting and rejecting the peace agreement—at a municipal level. Additionally, I analyze voter turnout to gauge the level of political engagement. I also include data from the 2014 presidential elections at the municipal level obtained from the same source, and data on electoral risk from *Misión de Observación Electoral* (MOE). This electoral data provides additional context for understanding the political landscape of each municipality prior to the peace agreement.

Finally, I incorporate data on various municipal characteristics from multiple sources. First, I use data on the presence of coca crops from the *Integrated Monitoring System for Illicit Crops* (SIMCI) by the *United Nations Office on Drugs and Crime* (UNODC). Legal mining statistics are obtained from the *Ministry of Mines and Energy* in Colombia, while information on illegal mining comes from Prem et al. (2020b). Additionally, I include socioeconomic data from a municipal panel provided by *Centro de Estudios sobre Desarrollo Económico* (CEDE) at Universidad de los Andes. The final database is a pooled panel of municipal-level microdata spanning from March 2017 to June 2017.

Table 1 provides an overview of key variables relevant to the study. It includes the share of municipalities that supported the peace agreement and voter turnout. Additionally, it presents the number of attacks perpetrated by non-state armed groups, broken down by specific groups such as the FARC, ELN, paramilitary groups, and other armed factions. Table 1 indicates that voter turnout was relatively low, even compared to typical levels in Colombia. Despite the peace agreement with the FARC insurgency, reports show that FARC dissidents—combatants who did not adhere to the peace deal—continued to engage in violent activities. Furthermore, the data reveals that the majority of attacks were carried out by unidentified actors.

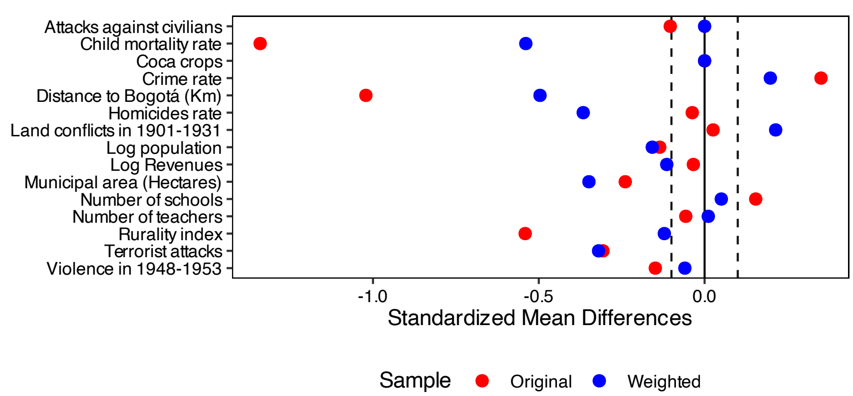
Table 1. Summary statistics

| | Mean | Std. Dev. | Min | Max |
|---|---|---|---|---|
| *Peace referendum (October 2, 2016)* | | | | |
| Proportion of municipalities in favor of peace | 0.516 | 0.500 | 0.000 | 1.000 |
| Voting turnout | 35.288 | 8.321 | 3.386 | 62.411 |
| *Violent attacks (per 10,000 people)* | | | | |
| Total number of one-sided attacks | 0.012 | 0.092 | 0.000 | 1.854 |
| One-sided attacks perpetrated by FARC | 0.002 | 0.053 | 0.000 | 2.630 |
| One-sided attacks perpetrated by ELN | 0.002 | 0.042 | 0.000 | 1.485 |
| One-sided attacks perpetrated by paramilitary groups | 0.003 | 0.052 | 0.000 | 1.810 |
| One-sided attacks perpetrated by an unknown actor | 0.007 | 0.058 | 0.000 | 1.140 |
| One-sided attacks perpetrated by state forces | 0.001 | 0.019 | 0.000 | 0.737 |

Summary statistics are calculated for the sample studied (October 2016 - October 2017) using monthly averages.

Figure 3 highlights the distinct characteristics of municipalities that rejected the peace agreement compared to those that supported it. Municipalities rejecting the peace deal are typically closer to Bogotá, have lower child mortality rates, and are smaller in geographical size. They also tend to have fewer coca crops, are less rural, and have experienced fewer terrorist attacks. On the other hand, these municipalities report higher crime rates and higher homicide rates, indicating a different set of social and security challenges compared to areas that supported the peace agreement.

Figure 3. Covariate balance




Covariate balance for a set of municipal characteristics in 2015 assessed using *cobalt* in R (Greifer, 2024).

### 4.2 Estimation

The difference in the number of one-sided attacks between municipalities that rejected the peace agreement and those that supported it is insufficient to derive a causal estimate of the effect of the peace referendum results on one-sided attacks by non-state armed actors. Multiple observable and unobservable individual, and municipal characteristics in Colombia affect preferences for peace. As Figure 3 illustrates, municipalities that supported the peace agreement experienced more violence in the past and were more significantly affected by the presence of coca crops, among other notable differences.

Omitted variable bias may affect the correlation between the peace referendum results and violence. Various unaccounted factors, such as economic conditions, historical grievances, and local leadership, could simultaneously impact both the referendum results and the levels of selective violence. Additionally, reverse causality is a concern; it is plausible that existing selective violence levels influenced preferences for peace, thereby affecting the referendum outcomes. For example, Kibris (2011) finds that Turkish voters are sensitive to experiencing terrorism. Similarly, Berrebi and Klor (2008) and Getmansky and Zeitzoff (2014) provide evidence from Israel supporting this finding.

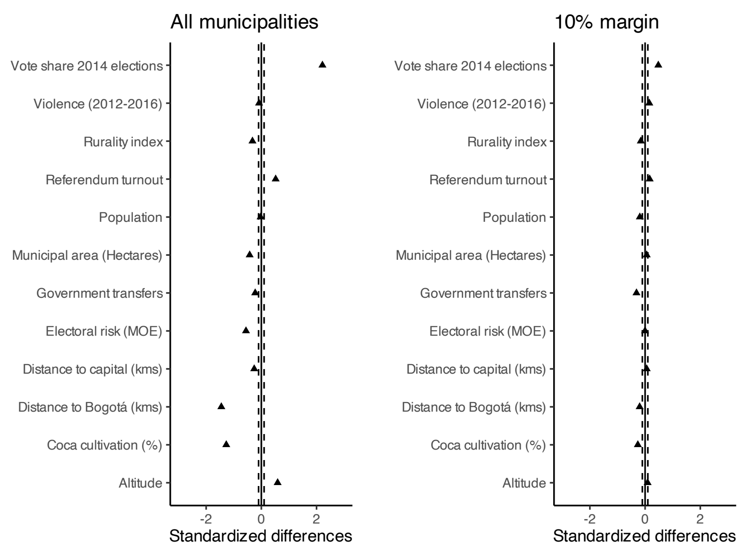
To estimate the causal effects of the peace referendum results on one-sided attacks (selective violence) perpetrated by non-state armed actors not involved in negotiations with the Colombian government, I employ a regression discontinuity design, using the vote share reported in the peace referendum as a score variable. Although the referendum's outcome depends solely on the total votes cast at the national level, local preferences regarding peace offer information that non-state actors can use strategically. The empirical model employs a regression of the following form:

$$y_{it} = \beta_1 + \beta_2 \times D_{it} + \beta_3 \times f\left(X_{it}\right) + \beta_4 \times D_{it} \times f\left(X_{it}\right) + \epsilon_{it} \tag{1}$$

In this context, $y_{it}$ represents the outcome variable for municipality $i$ in month $t$, which is a standardized transformation of the number of one-sided attacks committed by non-state armed groups excluded from peace negotiations. The variable $D_{it}$ is a binary treatment indicator that denotes whether a municipality rejected the peace agreement during the referendum. The term $f\left(X_{it}\right)$ is a polynomial function of the score variable, and $\epsilon_{it}$ is an idiosyncratic error term. Here, $X_{it}$ is calculated as the vote share rejecting the peace agreement minus the vote share supporting it, with each vote share representing a fraction of the total votes cast. Therefore, the treatment variable $D_{it}$ equals 1 if $X_{it} > 0$ and 0 otherwise.

The coefficient of interest is $\beta_2$, which represents a discontinuous change in the outcome variable at the threshold where the score variable is zero. I estimate $\beta_2$ both parametrically and nonparametrically within a narrow bandwidth, following the approach of Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019). Additionally, I test the robustness of the results by using different bandwidths and applying local linear and quadratic polynomials. The causal interpretation of $\beta_2$ relies on two main assumptions: first, covariates other than the outcome variable change smoothly at the threshold, indicating that any abrupt change in selective violence by non-state armed groups can be attributed solely to the rejection of the peace agreement; and second, that there is no systematic manipulation of the referendum results around the score threshold.

Figure 4. Standardized difference between treatment and control groups




The figure presents the standardize differences of municipal characteristics between treated and control municipalities. Left panel includes all municipalities, while right panel includes only municipalities reporting a 10% margin favoring either option during the referendum.

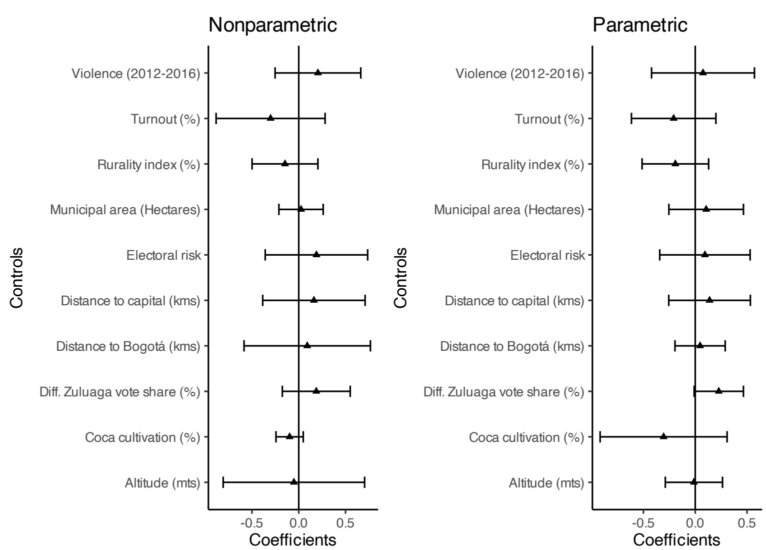
To ensure that the treatment and control groups are more comparable, I employ a regression discontinuity design that focuses on a subgroup of observations near the threshold. This approach allows me to compare municipalities that narrowly rejected the peace agreement with those that narrowly accepted it, thereby reducing potential differences between the groups. Figure 4 presents the standardized differences between the treatment and control groups across various municipal characteristics. The left panel includes all municipalities, displaying a broader range of differences. In contrast, the right panel focuses solely on municipalities within a 10% margin of the threshold, resulting in more comparable observations.

By concentrating on observations close to the threshold, I can more accurately isolate the effect of the peace referendum results on subsequent selective violence, ensuring that the municipalities in both groups are similar in terms of observable and unobservable characteristics. This enhances the validity of the causal estimates and provides a clearer understanding of the impact of the peace agreement's rejection on the behavior of non-state armed actors.

### 4.3 Identification

Two important assumptions must be met for the regression discontinuity design to be interpreted as causal: first, covariates other than the outcome must change smoothly at the threshold. This makes sure that any sudden changes in the outcome variable are due to the treatment. Second, there must be no systematic manipulation of the assignment variable around the threshold, meaning that the assignment mechanism is not manipulated.

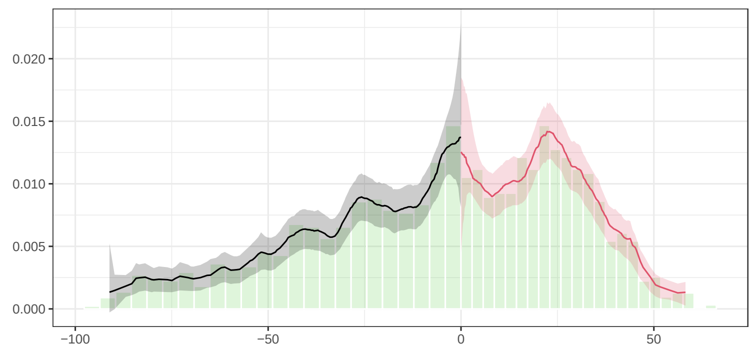
Figure 5. Continuity assumption




Point estimates accompanied by confidence intervals at the 95% level.

Based on Calonico et al. (2014), Figure 5 shows the differences between the treatment and control groups for a number of municipal characteristics at the point where the score variable is zero, before the peace referendum. This figure suggests that the first key assumption for a regression discontinuity design is likely to hold. Specifically, there are no statistically significant differences between the treatment and control municipalities around the threshold for these set of observable characteristics. This lack of significant differences supports the plausibility that covariates change smoothly at the threshold.

Figure 6. Score density




Manipulation test based on Cattaneo et al. (2020), where p-value is 0.449.

I use a manipulation test that Cattaneo et al. (2020) proposed, which modifies the McCrary test, to evaluate the second identifying assumption (McCrary, 2008). Figure 6 shows the distribution of the score variable around the threshold. A discontinuous jump in this distribution would suggest that it was more or less likely to see a narrow win for the rejection of the peace agreement during the referendum. However, Figure 5 demonstrates that there is no significant increase in density at the threshold (p-value = 0.449), indicating no evidence of score manipulation.[5] I test for manipulation in the score variable across different quartiles of the electorate's empirical distribution, as shown in Figure A1, to check for score manipulation in municipalities with larger electorates. The findings reveal no evidence of manipulation in the score variable for any electorate quartile, supporting the assumption that there is no systematic score manipulation around the threshold.

The data appear to support the two key identifying assumptions of the regression discontinuity design. First, covariates other than the outcome variable seem to

[5]Additionally, when performing the McCrary (2008) test, the results show no apparent sorting on the score variable (p-value = 0.022).

be continuous around the threshold, and second, there is no evidence of manipulation in the score variable. This suggests that the results derived from this analysis have a causal interpretation, providing a reliable estimate of the impact of the peace referendum results on selective violence perpetrated by non-state armed actors.

# 5 Results

## 5.1 Main Results

Table 2. Effect of peace referendum rejection on selective violence, March 2017 – June 2017

| Dependent variable | Standardized non-state selective violence events (Non-FARC) | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: Nonparametric estimates and bias-corrected standard errors - Kernel: Uniform* | | | | | | |
| Rejection share (%) | 0.273** | 0.279** | 0.313*** | 0.319*** | 0.336** | 0.274*** |
| | (0.097) | (0.093) | (0.095) | (0.088) | (0.110) | (0.085) |
| Bandwidth | 9.983 | 10.516 | 10.288 | 14.503 | 8.919 | 11.229 |
| Observations | 948 | 988 | 964 | 1,312 | 876 | 1,056 |
| Municipal controls | | | ✓ | | | ✓ |
| Political controls | | | | ✓ | | ✓ |
| Conflict controls | | | | | ✓ | ✓ |
| Clustered SE | | ✓ | ✓ | ✓ | ✓ | ✓ |
| *Panel B: Parametric estimates* | | | | | | |
| Rejection share (%) | 0.279* | 0.279 | 0.306* | 0.286* | 0.266* | 0.214* |
| | (0.119) | (0.155) | (0.149) | (0.135) | (0.111) | (0.094) |
| Bandwidth | 10.516 | 10.516 | 10.516 | 10.516 | 10.516 | 10.516 |
| Observations | 988 | 988 | 988 | 988 | 988 | 988 |
| Municipal controls | | | ✓ | | | ✓ |
| Political controls | | | | ✓ | | ✓ |
| Conflict controls | | | | | ✓ | ✓ |
| Clustered SE | | ✓ | ✓ | ✓ | ✓ | ✓ |

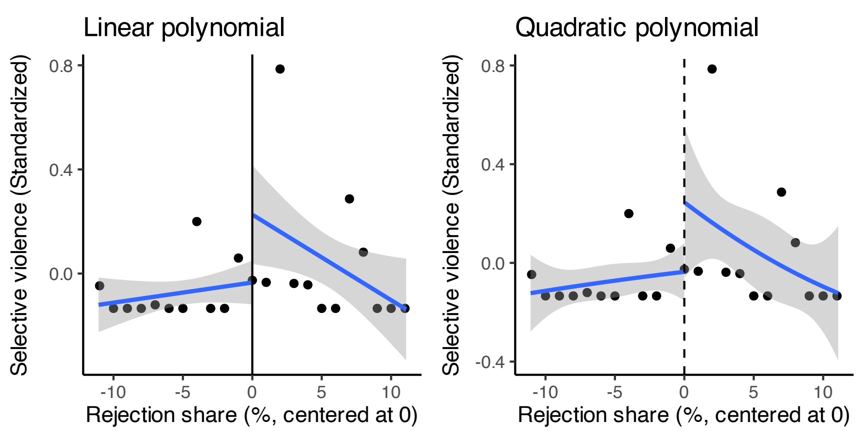
Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019).

Table 2 presents the main results. Panel A reports the non-parametric estimates based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019), while Panel B displays the corresponding parametric estimates. Column 1 shows the results from the most basic empirical model. Column 2 incorporates clustered standard errors at the department-month level for more robust inference.

Column 3 adds municipal controls, including municipal area size, population size, distance to Bogotá, and a rurality index. Column 4 includes political controls such as the vote share in the 2014 presidential elections and an electoral risk index. Column 5 accounts for prior violence by including the number of attacks before the peace referendum. Finally, Column 6 combines all the aforementioned controls, providing a comprehensive view of the factors influencing the relationship between the peace referendum results and subsequent violence.

All estimates consistently show a positive and statistically significant relationship between the share of votes rejecting the peace referendum and the number of one-sided attacks carried out by non-state armed actors during the disarmament process, from March 2017 to June 2017. As an example, the results in Panel A, Column 2, show a one-standard-deviation increase in the rejection share of the peace referendum is associated with a 0.27-standard-deviation increase in one-sided attacks against the civilian population from March 2017 to June 2017. When I compare this point estimate to the mean of the dependent variable, it represents almost an increase of 211% in one-sided attacks. This signifies a notable escalation in violence linked to the rejection of the peace agreement.

Figure 7. Effect of the peace referendum rejection on selective violence committed by non-state armed groups




Bins within Calonico et al. (2014) and Calonico et al. (2019) optimal bandwidths are displayed for linear and quadratic polynomials without additional controls. Standard errors are clustered at the department-month level.

Figure 7 presents the main estimates derived from the parametric approach in Column 2 of Table 2, using both linear and quadratic polynomials with a uniform kernel. Each point on the graph represents the standardized number of one-sided attacks for a specific bin within the optimal bandwidth range, as determined by Calonico et al. (2014) and Calonico et al. (2019). Both figures in the graph indicate a statistically significant increase in one-sided attacks near the threshold, highlighting the impact of the peace referendum rejection on subsequent selective violence.

### 5.2 Robustness Checks

I performed a validity test by manipulating the threshold of the regression discontinuity design at different levels. This approach helps confirm that the discontinuity is only observable at the threshold that actually matters in this context, which is zero. By systematically adjusting the threshold, I should observe null results for other thresholds, indicating that the observed treatment effects are not driven by arbitrary cutoff points. Figure A2 confirms the validity test by demonstrating that the discontinuity is only observable at the threshold of zero.

One important robustness check is the donut hole regression discontinuity design, which excludes observations very close to the threshold to mitigate potential biases (Barreca et al., 2011). Its relevance lies in confirming that the results are not disproportionately driven by data points immediately adjacent to the cutoff, which might be subject to manipulation, anomalies, or measurement error. Figure A3 confirms that there are no observations near the cutoff that could have disproportionately influenced the results.

Different bandwidths can affect the balance between bias and variance in the estimates. Narrower bandwidths may increase precision but introduce bias, while wider bandwidths may reduce bias but increase variance (Calonico et al., 2014, 2019). By examining the consistency of the results across a range of bandwidths, I can confirm that the observed treatment effects are not driven by a specific bandwidth selection. Figure A4 confirms that the results remain consistent across different bandwidths.

I run the main estimates using different bandwidths on both sides of the threshold (Table A1), employing a quadratic polynomial (Table A2), and using both triangular and epanechnikov kernels (Table A3). The results remain consistent across these various specifications, demonstrating that the observed treatment effects are robust to different methodological choices. This consistency across multiple approaches reinforces the reliability of my conclusions and provides strong evidence for the causal impact of the peace referendum results on selective violence perpetrated by non-state armed actors. I also analyzed the effect on different types of targets and found that the rejection of the peace agreement primarily intensified selective violence against civilian communities rather than prominent figures, such as social leaders (Table A4).

Because the main database is a municipal pooled panel, the score variable inherently has multiple mass points. This means that while each municipality has a single fixed value associated with the score variable, the outcome variable varies monthly. As a result, the score variable, which is constant for each municipality, essentially becomes discrete. If this is the case, the continuity-based local polynomial method may no longer be applicable (Lee and Card, 2008). When the score variable contains mass points, local polynomial methods for a regression discontinuity design behave essentially as if we had the same number of observations as mass points. Instead of having a continuous spread of data points around the cutoff, we have clusters of observations at specific values (Cattaneo et al., 2017).

To address the issue of a discrete score variable, I employed a local randomization approach that adjusts for any confounding variables by randomly perturbing the assignment variable within a small window around the threshold (Cattaneo et al., 2015). Table A5 presents the results of a local randomization regression discontinuity design. This approach continues to show a statistically significant increase in selective violence, with the significance range within the 15% threshold that is appropriate for this analysis (Cattaneo and Titiunik, 2022).

Kolesár and Rothe (2018) discusses the issue of using confidence intervals (CIs) based on standard errors that are clustered by the running variable in regression discontinuity designs with a discrete running variable, as recommended by Lee and Card (2008). Kolesár and Rothe (2018) shows that these CIs do not effectively protect against model misspecification and tend to have poor coverage properties. This means that the resulting confidence intervals may not accurately reflect the true uncertainty around the estimated effects, potentially leading to misleading inference. Table A6 reports results following Kolesár and Rothe (2018), and two different smoothness class (Armstrong and Kolesár, 2020). In both cases, one-sided attacks perpetrated by non-state armed groups increases more in municipalities that rejected the peace agreement.

In 2014, Colombia held presidential elections that were widely considered the first informal peace referendum (Weintraub et al., 2014). The two main candidates had opposing views on the peace agreement with the FARC. Incumbent President Juan Manuel Santos, who initiated the peace talks during his first term, was a strong advocate for continuing the peace process. In contrast, his opponent, Oscar Iván Zuluaga, campaigned on a platform that sought to break the peace agreement, presenting a clear choice for voters on the future of the negotiations. Table A7 demonstrates that the increase in violence perpetrated by non-state armed groups is not influenced by the vote share during the 2014 presidential elections. Furthermore, there appears to be no direct disruption between the political agendas of the presidential candidates and the public's preferences regarding peace (see Figure A5).

Past violence could significantly influence electoral preferences, potentially affecting the outcomes of the peace referendum. Communities that have experienced higher levels of violence may develop distinct political attitudes, prioritizing security and stability over other issues (Berrebi and Klor, 2008; Getmansky and Zeitzoff, 2014; Kibris, 2011). Table A8 indicates that violence, at least between 2000 and 2011, does not have a relationship with the results of the peace referendum. Levels of violence experienced during this period did not directly influence how municipalities voted in the peace referendum.

I conducted two additional placebo exercises to further validate the findings. The first exercise examined whether the rejection of the peace agreement influenced one-sided attacks initiated by either FARC or state forces (Table A9), while the second analyzed the effect of the rejection on selective violence during two distinct periods: before the disarmament process (October 2016–February 2017) and after the disarmament process (July 2017–December 2017) (Table A10). Neither of these exercises yielded statistically significant results, reinforcing the robustness of the primary findings.

### 5.3 Heterogeneous Effects

To further understand the factors driving the increase in one-sided attacks perpetrated by non-state armed actors in Colombia, I will test a set of heterogeneous effects. This analysis will include variables such as the presence of other non-state actors, the presence of coca crops, the extent of gold mining activities, the level of civilian resistance mechanisms, and several state presence measures. By examining these factors, the aim is to identify specific conditions and contexts that may influence the escalation of selective violence.

A key driver of violence during the post-demobilization period is the ambition of non-state armed groups that did not participate in the negotiation process to occupy former FARC territories. As shown in Table A11, the estimates from the main empirical model indicate that selective violence is disproportionately concentrated in these former strongholds. Further, this effect is driven by the proximity of these non-signatory armed actors, who were already positioned in neighboring municipalities as shown in Table A12. Consequently, selective violence is particularly pronounced in places that were FARC strongholds and are simultaneously exposed to these non-participating groups. This finding supports *Hypothesis 2*, underscoring the significance of territorial control motivations as an important driver of violence in Colombia.

A second key driver of selective violence following the peace referendum lies in the economic incentives non-state armed groups have to seize the lucrative resources—such as illegal drug production and mining operations—vacated by the FARC. Table A13 shows that selective violence by armed groups excluded from the 2016 peace agreement increases most sharply in municipalities that both rejected the accord and report the presence of coca crops. Moreover, when focusing solely on coca-producing municipalities, the effect is driven exclusively by those previously controlled by the FARC (see Table A14). Similar results emerge when gold-mining areas are considered instead of coca-growing regions (Table A15 and Table A16). Together, these findings support *Hypothesis 3*, underscoring how economic motivations significantly shape patterns of post-agreement violence in Colombia.

A lack of robust civilian resistance can significantly facilitate selective violence by lowering the costs and risks associated with coercion (*Hypothesis 4*). In this study, four indicators capture the concept of civilian resistance: (1) the presence of civilian organizations, (2) the level of protest activity in 2013 at the municipal level, (3) the presence of social leaders (measured through statistics on their killings), and (4) voter turnout in the peace referendum. Where these indicators are strong, communities are better positioned to mobilize, collectively resist, or negotiate with armed groups, thereby raising the costs and limiting the feasibility of violent tactics. Conversely, when civilian institutions and activism are weak, non-state actors encounter fewer barriers to asserting their power, creating conditions in which selective violence against the population becomes a more viable strategy to secure territorial and economic objectives.[6] Overall, the findings indicate that robust civilian resistance is a

[6]Following Kaplan (2017), the presence of civilian organizations measures a community's capacity to both push back against and negotiate with non-state armed groups. The level of protest activity serves as a proxy for civic engagement and the community's willingness to mobilize for collective causes. The presence of social leaders reflects the strength of community organization and leadership, which is crucial for fostering social cohesion and resistance against armed actors. In Colombia, the lack of official registration for civil society organizations complicates the identification of social leaders, so their killings often serve as a proxy for both their presence and level of activity. Finally, voter turnout in the peace referendum indicates the degree of political participation and community involvement in shaping local decisions.

significant mediator of selective violence perpetrated by non-state armed groups that remained outside the Colombian government–FARC negotiations.

In municipalities that rejected the peace agreement and lacked robust civilian organizations, Table A17 shows an increase of selective victimization of nearly 0.2 standard deviations. Furthermore, when comparing across these municipalities without civilian organizations, those that had been under FARC control exhibit selective victimization rates more than five times higher than their counterparts not controlled by the FARC (see Table A18). My measure of social activism presence, indicated by reported killings of social leaders, and the measure of local protest usage yield similar findings regarding selective violence. In municipalities that rejected the peace agreement and exhibit social activist activity, there is an increase in selective violence of about 0.4 standard deviations (see Table A19), with all of this effect concentrated in former FARC-controlled areas (see Table A20). Meanwhile, in municipalities where protests are not commonly used, rejecting the peace agreement corresponds to a marginal effect of 0.46 standard deviations in selective victimization (see Table A21)—an impact that rises to nearly seven times higher in zones previously occupied by the FARC compared to non-FARC areas (see Table A22). Finally, municipalities that rejected the peace agreement and recorded lower voter turnout during the referendum show higher levels of selective violence—approximately 0.47 standard deviations (see Table A23)—an effect entirely concentrated in former FARC-controlled areas (see Table A24). Taken together, these findings underscore the pivotal role of civilian agency in shaping patterns of post-agreement violence. As stated in *Hypothesis 4*, in communities that lack robust mechanisms of resistance—whether through civic engagement, protest, social activism, or electoral participation—non-state armed groups encounter fewer constraints, thereby amplifying selective victimization.

Following the peace agreement, the Colombian government classified certain municipalities into two specialized programs—PDET (Development Programs with a Territorial Focus) and PNIS (National Comprehensive Program for the Substitution of Illicit Crops)—as part of a broader effort to foster development, address the root causes of conflict, and support the transition to peace in regions heavily affected by violence. In our analysis, these designations serve as proxies for state presence, alongside an additional variable measuring each municipality's remoteness from a military brigade.

Table A25 and Table A26 indicate that non-state armed groups considered PNIS municipalities more strategically valuable than PDET municipalities. Specifically, in

PNIS municipalities that rejected the peace agreement, selective violence increases by about 2.28 standard deviations, with the entire effect concentrated in former FARC-controlled areas (see Table A27 and Table A28). This pattern suggests that zones central to the government's strategy for reducing coca cultivation have become focal points of conflict as non-participating armed actors seek to preserve their economic interests. Meanwhile, Table A29 shows that greater military presence helps mitigate selective victimization: municipalities that rejected the agreement and lie far from military brigades report higher levels of selective violence (see also Table A30). Overall, these findings imply that assertive military control, as stated in *Hypothesis 5* of territories formerly held by demobilized groups is currently more effective at stabilizing these regions than purely non-military interventions aimed at addressing the root causes of violence.[7]

## 6 Conclusion

Peace agreements can fail to deter violence due to their inability to address the interests and grievances of all parties involved in a conflict. When such agreements are made with only a subset of non-state actors, they can create an imbalance in power dynamics, leading to unintended consequences. Non-state groups that are excluded from the peace process may view the agreement as an opportunity to further their economic interests, particularly in regions with valuable resources. Thus, this exclusion can incentivize these groups to escalate violence to assert their dominance or protect their economic activities. Consequently, while peace agreements aim to establish stability, they can inadvertently provoke new conflicts by disrupting the status quo among competing factions.

The findings indicate that the rejection of the peace agreement in the 2016 Colombian peace referendum precipitated a surge in one-sided attacks (selective violence) by non-state armed groups excluded from the peace process with the FARC insurgency. Employing a regression discontinuity approach, the empirical analysis reveals an average increase of 211% in events of selective violence between March and June of 2017, highlighting the substantial impact of the referendum's outcome on local security conditions in Colombia after the peace referendum took place.

[7] One of the identification assumptions underlying the main results would be threatened if there were a discontinuous jump in the characteristics used to test heterogeneous effects around the chosen threshold. In such a scenario, the observed jump in the outcome variable could be attributed to these confounding characteristics rather than the mechanism of interest. However, Figure A6). shows no evidence of such a discontinuity, thus reinforcing the robustness of the principal findings.

This study contributes to the broader literature on the relationship between income and violence, and examines the effectiveness of peace agreements in preventing future conflicts. Unlike most research on the effectiveness of peace agreements, which often relies on qualitative and anecdotal evidence, this study provides a quantitative analysis of the causal impact of peace agreements on subsequent violence. By employing a regression discontinuity approach, this paper offers robust evidence on how the rejection of a peace agreement can escalate violence among non-state armed groups, making it one of the first studies to rigorously quantify the causal effects of peace agreements on future violence.

In the Colombian context, the results of the peace referendum signaled which municipalities became focal points for non-state armed groups following the demobilization of the FARC insurgency, particularly those that rejected the peace agreement, as they often harbor strong punishment preferences against armed actors. Selective violence perpetrated by non-state armed groups that did not participate in the peace agreement between the Colombian government and the FARC tends to concentrate in municipalities previously under FARC control, exposed to the influence of these new actors, and endowed with significant economic resources (e.g., coca crops or gold mines). Moreover, these municipalities often exhibit low levels of civilian agency or resistance, as well as an absence of robust state military presence, creating conditions that facilitate the expansion and entrenchment of non-signatory armed groups.

One of the main caveats of this study is that it estimates a local average treatment effect by focusing on municipalities that narrowly rejected or supported the peace agreement. This approach means that the findings are most applicable to areas where the vote was closely contested, and may not fully capture the broader impact of the peace agreement on municipalities with more decisive outcomes. Therefore, while the results provide valuable insights into the localized effects of the peace referendum, they should be interpreted with caution when generalizing to all municipalities across Colombia. Another limitation of this study is that it focuses exclusively on one specific aspect of violence: one-sided attacks perpetrated by non-state armed groups. This narrow focus means that other forms of violence and their potential impacts were not considered. Additionally, the analysis is confined to the effect of the peace agreement as determined by the outcome of the peace referendum. This approach overlooks other possible mechanisms through which the peace agreement could influence violence, such as changes in government policies, shifts in public sentiment, or the implementation of local development programs.

Promising avenues for future research on this topic include exploring the broader impacts of peace agreements on various forms of violence and examining different mechanisms through which peace agreements can influence conflict dynamics. Additionally, comparative studies across different regions or countries that have undergone similar peace processes could provide valuable insights into the generalizability of these findings. Finally, incorporating qualitative methods to complement quantitative analyses could help uncover nuanced local contexts and actors' perspectives, offering a more comprehensive understanding of the complex relationship between peace agreements and violence.

The findings of this study carry several policy implications, particularly for the design of future peace processes. First, peace agreements should strive to include all relevant non-state actors to prevent the exclusion of groups that may feel threatened and respond with increased violence. Comprehensive inclusion can prevent power vacuums that might otherwise be exploited by excluded group. Furthermore, continuous monitoring and evaluation of peace agreements' implementation can help identify and address emerging threats promptly. Integrating local stakeholders and communities in the peace process can ensure that agreements are more attuned to the specific needs and dynamics of different regions, increasing the likelihood of sustainable peace. Moreover, policy measures that enhance security and law enforcement in vulnerable regions can directly reduce the capacity of non-state armed groups to perpetrate violence. Strengthening the presence of state institutions and ensuring the rule of law can create an environment where the strategic use of violence becomes less viable for these groups.

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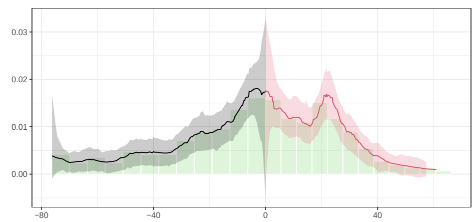
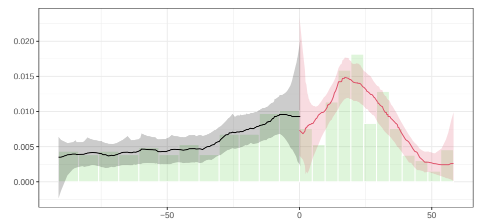
# A Appendix Figures

Appendix Figure A1. Score density by electorate quartile

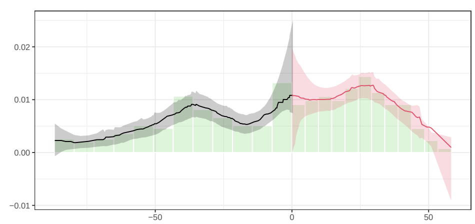
(a) Quartile 1



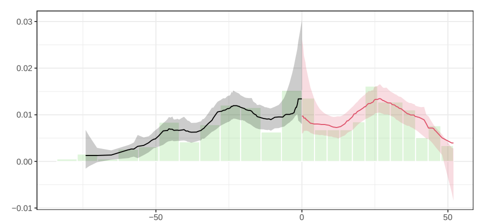

(b) Quartile 2




(c) Quartile 3

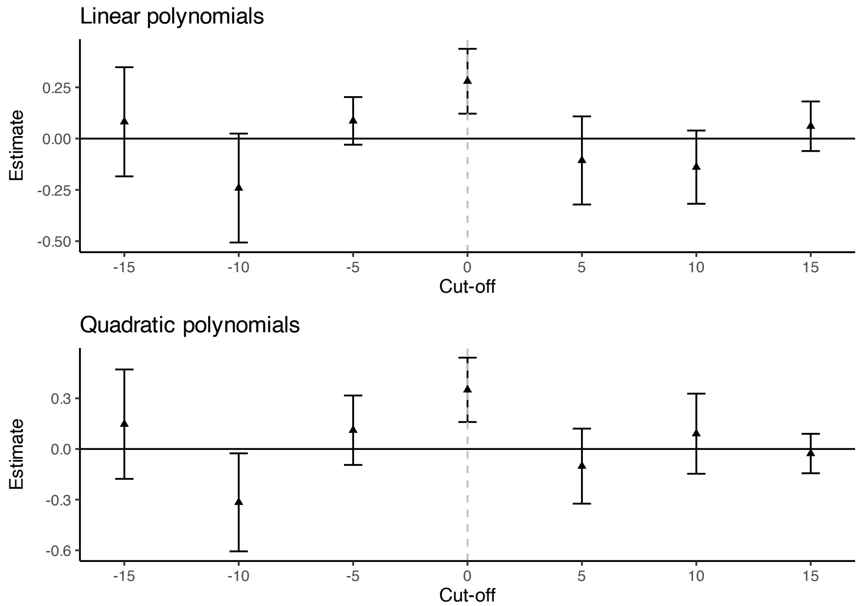



(d) Quartile 4



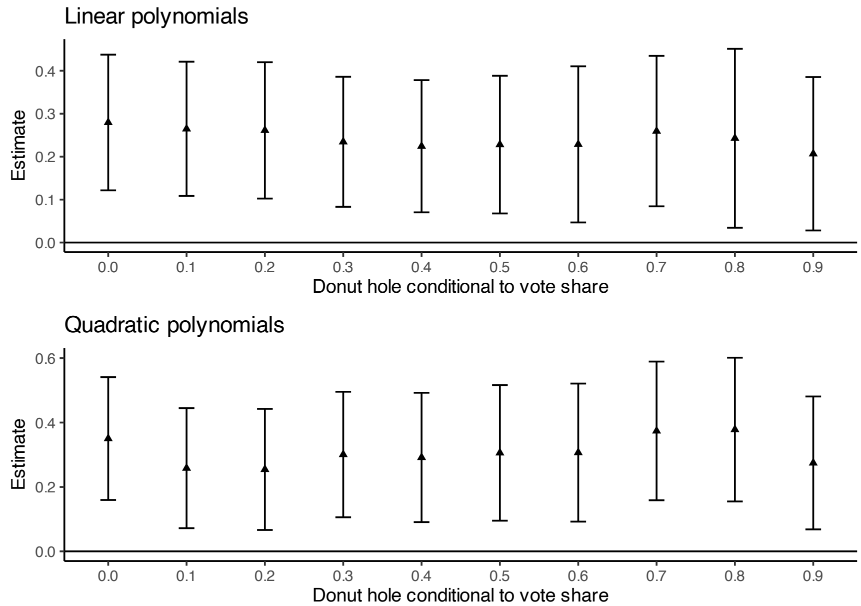

Manipulation test based on Cattaneo et al. (2020). p-values are 0.997 in (a), 0.900 in (b), 0.579 in (c), and 0.803 in (d).

Appendix Figure A2. Sensitivity analysis to different cut-offs



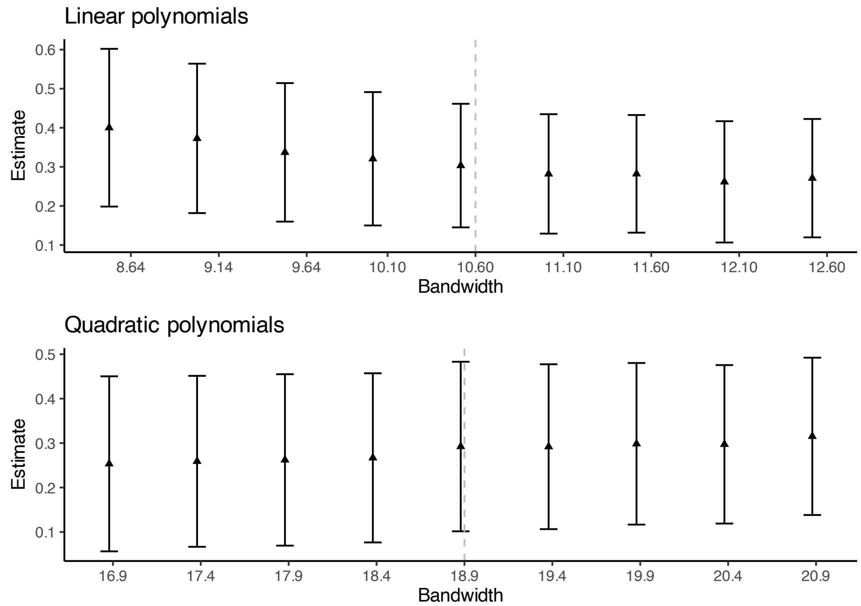

Point estimates for the common support of the score variable with confidence intervals at the 95% level. Parametric estimates using optimal bandwidths of Calonico et al. (2014) based on linear and quadratic polynomials, no controls, and clustered standard errors at the department-month level case.

Appendix Figure A3. Sensitivity analysis to observations near the cut-off



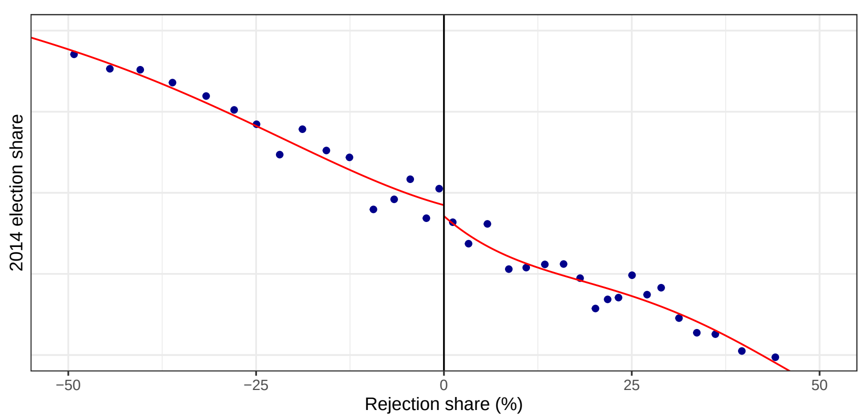

Point estimates for the common support of the score variable with confidence intervals at the 95% level. Parametric estimates using optimal bandwidths of Calonico et al. (2014) based on linear and quadratic polynomials, no controls, and clustered standard errors at the department-month level case.

Appendix Figure A4. Sensitivity analysis to bandwidth choice



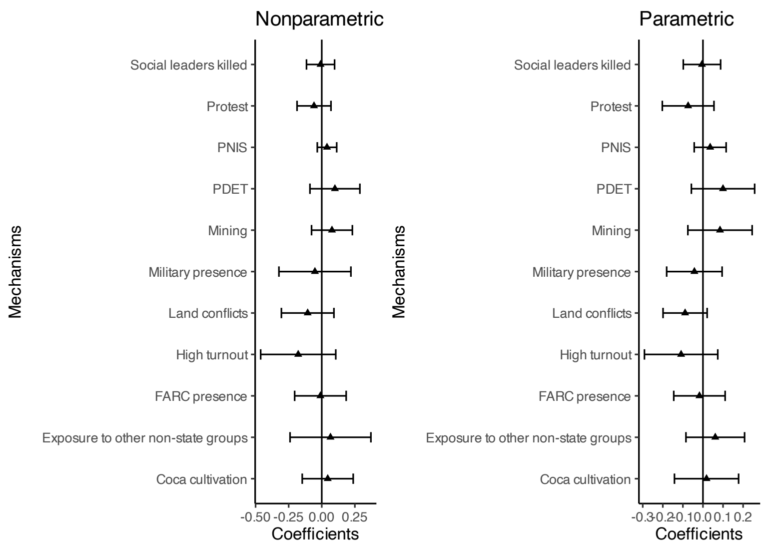

Point estimates for the common support of the score variable with confidence intervals at the 95% level. Parametric estimates using optimal bandwidths of Calonico et al. (2014) based on linear and quadratic polynomials, no controls, and clustered standard errors at the department-month level case.

Appendix Figure A5. 2014 presidential election vote share and 2016 peace referendum results




RD plot based on Calonico et al. (2014).

Appendix Figure A6. Continuity assumption: Heterogeneous effects




Point estimates accompanied by confidence intervals at the 95% level.

# B Appendix Tables

Appendix Table A1. Effect of peace referendum rejection on selective violence, March 2017 – June 2017

| Dependent variable | Standardized non-state selective violence events (Non-FARC) | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: Nonparametric estimates and bias-corrected standard errors - Kernel: Uniform* | | | | | | |
| Rejection share (%) | 0.337** | 0.355** | 0.327** | 0.316*** | 0.338*** | 0.077** |
| | (0.119) | (0.121) | (0.109) | (0.092) | (0.099) | (0.077) |
| Bandwidths | (13.074, 9.244) | (12.796, 9.574) | (11.979, 9.771) | (12.412, 11.299) | (11.278, 9.652) | (11.089, 16.525) |
| Observations | 1,056 | 1,052 | 1,028 | 1,120 | 980 | 1,288 |
| Municipal controls | | | ✓ | | | ✓ |
| Political controls | | | | ✓ | | ✓ |
| Conflict controls | | | | | ✓ | ✓ |
| Clustered SE | | ✓ | ✓ | ✓ | ✓ | ✓ |
| *Panel B: Parametric estimates* | | | | | | |
| Rejection share (%) | 0.312** | 0.312* | 0.322** | 0.299** | 0.339** | 0.307** |
| | (0.118) | (0.149) | (0.145) | (0.129) | (0.116) | (0.106) |
| Bandwidths | (12.796, 9.574) | (12.796, 9.574) | (12.796, 9.574) | (12.796, 9.574) | (12.796, 9.574) | (12.796, 9.574) |
| Observations | 1,052 | 1,052 | 1,052 | 1,052 | 1,052 | 1,052 |
| Municipal controls | | | ✓ | | | ✓ |
| Political controls | | | | ✓ | | ✓ |
| Conflict controls | | | | | ✓ | ✓ |
| Clustered SE | | ✓ | ✓ | ✓ | ✓ | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with unequal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019).

Appendix Table A2. Effect of peace referendum rejection on selective violence, March 2017 – June 2017

| Dependent variable | Standardized non-state selective violence events (Non-FARC) | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: Nonparametric estimates and bias-corrected standard errors - Kernel: Uniform* | | | | | | |
| Rejection share (%) | 0.348*** | 0.350*** | 0.359*** | 0.319*** | 0.349*** | 0.380*** |
| | (0.101) | (0.103) | (0.107) | (0.098) | (0.100) | (0.107) |
| Bandwidths | 18.646 | 18.879 | 18.875 | 23.992 | 22.466 | 17.114 |
| Observations | 1,640 | 1,656 | 1,656 | 2,192 | 2,008 | 1,528 |
| Municipal controls | | | ✓ | | | ✓ |
| Political controls | | | | ✓ | | ✓ |
| Conflict controls | | | | | ✓ | ✓ |
| Clustered SE | | ✓ | ✓ | ✓ | ✓ | ✓ |
| *Panel B: Parametric estimates* | | | | | | |
| Rejection share (%) | 0.373** | 0.373* | 0.390* | 0.406* | 0.404** | 0.404** |
| | (0.133) | (0.184) | (0.180) | (0.008) | (0.148) | (0.138) |
| Bandwidths | 18.879 | 18.879 | 18.879 | 18.879 | 18.879 | 18.879 |
| Observations | 1,656 | 1,656 | 1,656 | 1,656 | 1,656 | 1,656 |
| Municipal controls | | | ✓ | | | ✓ |
| Political controls | | | | ✓ | | ✓ |
| Conflict controls | | | | | ✓ | ✓ |
| Clustered SE | | ✓ | ✓ | ✓ | ✓ | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of quadratic polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019).

Appendix Table A3. Effect of peace referendum rejection on selective violence, March 2017 – June 2017

| Dependent variable | Standardized non-state selective violence events (Non-FARC) | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: Nonparametric estimates and bias-corrected standard errors - Kernel: Triangular* | | | | | | |
| Rejection share (%) | 0.306*** | 0.309*** | 0.321*** | 0.327*** | 0.362*** | 0.293** |
| | (0.094) | (0.096) | (0.097) | (0.090) | (0.110) | (0.102) |
| Bandwidths | 13.033 | 13.286 | 13.254 | 15.391 | 10.461 | 11.629 |
| Observations | 1,228 | 1,240 | 1,236 | 1,384 | 980 | 1,084 |
| Municipal controls | | | ✓ | | | ✓ |
| Political controls | | | | ✓ | | ✓ |
| Conflict controls | | | | | ✓ | ✓ |
| Clustered SE | | ✓ | ✓ | ✓ | ✓ | ✓ |
| *Panel B: Nonparametric estimates and bias-corrected standard errors - Kernel: Epanechnikov* | | | | | | |
| Rejection share (%) | 0.297*** | 0.301*** | 0.320*** | 0.323*** | 0.345*** | 0.288** |
| | (0.092) | (0.094) | (0.094) | (0.088) | (0.101) | (0.093) |
| Bandwidths | 12.355 | 12.578 | 12.518 | 15.387 | 10.895 | 11.917 |
| Observations | 1,172 | 1,184 | 1,176 | 1,384 | 1,020 | 1,128 |
| Municipal controls | | | ✓ | | | ✓ |
| Political controls | | | | ✓ | | ✓ |
| Conflict controls | | | | | ✓ | ✓ |
| Clustered SE | | ✓ | ✓ | ✓ | ✓ | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and triangular kernel (Panel A) or Epanechnikov kernel (Panel B) based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019).

Appendix Table A4. Effect of peace referendum rejection on selective killings, March 2017 – June 2017

| Dependent variable | Standardized non-state selective killings (Non-FARC) | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: Civilian population* | | | | | | |
| Rejection share (%) | 0.191** | 0.193** | 0.214** | 0.207*** | 0.238* | 0.136* |
| | (0.065) | (0.063) | (0.068) | (0.063) | (0.096) | (0.055) |
| Bandwidth | 11.978 | 12.519 | 11.190 | 11.861 | 8.779 | 15.658 |
| Observations | 1,136 | 1,176 | 1,056 | 1,120 | 868 | 1,424 |
| Municipal controls | | | ✓ | | | ✓ |
| Political controls | | | | ✓ | | ✓ |
| Conflict controls | | | | | ✓ | ✓ |
| Clustered SE | | ✓ | ✓ | ✓ | ✓ | ✓ |
| *Panel B: Social leaders* | | | | | | |
| Rejection share (%) | -0.011 | -0.010 | -0.005 | 0.010 | -0.013 | 0.005 |
| | (0.016) | (0.021) | (0.021) | (0.021) | (0.021) | (0.021) |
| Bandwidth | 8.462 | 8.511 | 8.026 | 7.982 | 8.758 | 7.868 |
| Observations | 848 | 856 | 820 | 816 | 868 | 804 |
| Municipal controls | | | ✓ | | | ✓ |
| Political controls | | | | ✓ | | ✓ |
| Conflict controls | | | | | ✓ | ✓ |
| Clustered SE | | ✓ | ✓ | ✓ | ✓ | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019).

Appendix Table A5. Local randomization

| | Difference in means |
|---|---|
| Selective violence | 0.717 |
| Finite sample p-value | 0.079 |
| Window | (-2.554; 2.554) |

Results based on Cattaneo et al. (2015).

Appendix Table A6. Alternative CIs

| | Hölder (1) | Taylor (2) |
|---|---|---|
| Rejection share (%) | 0.273** | 0.402*** |
| | (0.099) | (0.115) |
| Bandwidth | 7.552 | 5.372 |
| Observations | 768 | 596 |

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Results based on Kolesár and Rothe (2018) and Armstrong and Kolesár (2020).

Appendix Table A7. Placebo: 2014 presidential election

| | (1) |
|---|---|
| Zuluaga's vote share (%) | 0.044 |
| | (0.089) |
| Bandwidth | 22.185 |
| Observations | 1,584 |
| Clustered SE | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019).

Appendix Table A8. Placebo: Historical violence

| | Historical violence (1) |
|---|---|
| Rejection share (%) | 0.235 |
| | (0.206) |
| Bandwidth | 21.259 |
| Observations | 471 |
| Clustered SE | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019). The outcome variable includes conflict events reported from January 1, 2000 to December 31, 2011.

Appendix Table A9. Placebo: Selective violence committed by FARC or state forces

| | FARC<br>(1) | State forces<br>(2) |
|---|---|---|
| Rejection share (%) | -0.001 | -0.014 |
| | (0.001) | (0.108) |
| Bandwidth | 2.619 | 20.488 |
| Observations | 288 | 1,824 |
| Clustered SE | ✓ | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019).

Appendix Table A10. Placebo: Selective violence before and after demobilization

| | Before<br>(1) | After<br>(2) |
|---|---|---|
| Rejection share (%) | 0.024 | 0.005 |
| | (0.057) | (0.080) |
| Bandwidth | 14.715 | 14.255 |
| Observations | 1,645 | 1,950 |
| Clustered SE | ✓ | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019). Column (1) includes one-sided attacks reported from October 3, 2016 to February 28, 2017. Column (2) includes one-sided attacks reported from July 1, 2017 to December 31, 2017.

Appendix Table A11. Presence of FARC

| | FARC<br>(1) | Non-FARC<br>(2) |
|---|---|---|
| Rejection share (%) | 0.746*** | 0.095 |
| | (0.225) | (0.093) |
| Bandwidth | 11.353 | 14.539 |
| Observations | 212 | 1,064 |
| Clustered SE | ✓ | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019). The presence of FARC is measured as a dummy indicator of one-sided attacks perpetrated by the FARC insurgency in 2007-2012. Each column reports results for different subsample of municipalities.

Appendix Table A12. FARC-controlled municipalities and exposition to other non-state armed groups

| | Exposed<br>(1) | Not exposed<br>(2) |
|---|---|---|
| Rejection share (%) | 1.092*** | -0.523 |
| | (0.280) | (0.270) |
| Bandwidth | 13.024 | 18.084 |
| Observations | 164 | 108 |
| Clustered SE | ✓ | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019). The estimates include only municipalities reporting FARC presence. Exposure to other armed groups is represented by a dummy variable equal to 1 if the proportion of neighboring municipalities with non-FARC armed groups exceeds the median of its empirical distribution. Each column presents results for a different subsample of municipalities.

Appendix Table A13. Economic incentives: Presence of coca crops

| | Yes | No |
|---|---|---|
| | (1) | (2) |
| Rejection share (%) | 0.652** | 0.169 |
| | (0.226) | (0.093) |
| Bandwidth | 16.124 | 10.438 |
| Observations | 168 | 856 |
| Clustered SE | ✓ | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019). The presence of coca crops is represented by a dummy variable set to 1 if any area within a municipality is cultivated with coca. Each column presents results for a different subsample of municipalities.

Appendix Table A14. Economic incentives: FARC-controlled areas and presence of coca crops

| | FARC | Non-FARC |
|---|---|---|
| | (1) | (2) |
| Rejection share (%) | 1.183*** | -1.016* |
| | (0.341) | (0.491) |
| Bandwidth | 13.468 | 13.146 |
| Observations | 88 | 60 |
| Clustered SE | ✓ | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019). The estimates are restricted to municipalities reporting coca crops. The presence of coca crops is represented by a dummy variable set to 1 if any area within a municipality is cultivated with coca. Each column presents results for a different subsample of municipalities.

Appendix Table A15. Economic incentives: Presence of gold mines

| | Yes<br>(1) | No<br>(2) |
|---|---|---|
| Rejection share (%) | 1.208*** | 0.066 |
| | (0.349) | (0.083) |
| Bandwidth | 11.232 | 10.141 |
| Observations | 752 | 920 |
| Clustered SE | ✓ | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019). The presence of gold mines is represented by a dummy variable set to 1 if any area within a municipality contains legal gold mines. Each column presents results for a different subsample of municipalities.

Appendix Table A16. Economic incentives: FARC-controlled areas and presence of gold mines

| | FARC<br>(1) | Non-FARC<br>(2) |
|---|---|---|
| Rejection share (%) | 1.638*** | 0.004 |
| | (0.465) | (0.004) |
| Bandwidth | 44.460 | 8.755 |
| Observations | 60 | 20 |
| Clustered SE | ✓ | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019). The estimates are restricted to municipalities reporting legal gold mines. The presence of gold mines is represented by a dummy variable set to 1 if any area within a municipality contains legal gold mines. Each column presents results for a different subsample of municipalities.

Appendix Table A17. Civilian resistance: Presence of civilian organizations

| | No | Yes |
|---|---|---|
| | (1) | (2) |
| Rejection share (%) | 0.197* | 0.100 |
| | (0.090) | (0.337) |
| Bandwidth | 8.870 | 21.969 |
| Observations | 820 | 108 |
| Clustered SE | ✓ | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019). The presence of civilian organizations is captured by a dummy variable indicating the establishment of independent organizations by the local civilian population. Each column presents results for a different subsample of municipalities.

Appendix Table A18. Civilian resistance: FARC-controlled areas and presence of civilian organizations

| | FARC | Non-FARC |
|---|---|---|
| | (1) | (2) |
| Rejection share (%) | 0.687* | 0.137* |
| | (0.339) | (0.066) |
| Bandwidth | 12.236 | 9.989 |
| Observations | 184 | 748 |
| Clustered SE | ✓ | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019). The estimates are limited to municipalities with no reported presence of civilian organizations. The presence of civilian organizations is captured by a dummy variable indicating the establishment of independent organizations by the local civilian population. Each column presents results for a different subsample of municipalities.

Appendix Table A19. Civilian resistance: Presence of social activism

| | No<br>(1) | Yes<br>(2) |
|---|---|---|
| Rejection share (%) | 0.391*** | -0.320 |
| | (0.095) | (0.318) |
| Bandwidth | 9.641 | 14.956 |
| Observations | 856 | 96 |
| Clustered SE | ✓ | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019). The presence of social activism is measured by a dummy variable indicating the occurrence of social activist killings. Each column presents results for a different subsample of municipalities.

Appendix Table A20. Civilian resistance: FARC-controlled areas and presence of social activism

| | FARC<br>(1) | Non-FARC<br>(2) |
|---|---|---|
| Rejection share (%) | 1.058*** | 0.073 |
| | (0.221) | (0.095) |
| Bandwidth | 13.076 | 15.508 |
| Observations | 192 | 1096 |
| Clustered SE | ✓ | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019). The estimates are restricted to municipalities without reported social activism presence. The presence of social activism is measured by a dummy variable indicating the occurrence of social activist killings. Each column presents results for a different subsample of municipalities.

Appendix Table A21. Civilian resistance: Absence of protest activity

| | Yes (1) | No (2) |
|---|---|---|
| Rejection share (%) | 0.458*** | 0.007 |
| | (0.121) | (0.150) |
| Bandwidth | 9.810 | 11.724 |
| Observations | 732 | 232 |
| Clustered SE | ✓ | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019). The presence of protests is measured by a dummy variable indicating the the lack of occurrence of protests linked to *minga indígena*, *paro campesino*, or *paro agrario* in 2013. Each column presents results for a different subsample of municipalities.

Appendix Table A22. Civilian resistance: FARC-controlled areas and absence of protest activity

| | FARC (1) | Non-FARC (2) |
|---|---|---|
| Rejection share (%) | 1.254*** | 0.181* |
| | (0.295) | (0.091) |
| Bandwidth | 13.784 | 9.638 |
| Observations | 152 | 232 |
| Clustered SE | ✓ | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019). The estimates are restricted to municipalities where no protests were reported. The presence of protests is measured by a dummy variable indicating the occurrence of protests linked to *minga indígena*, *paro campesino*, or *paro agrario* in 2013. Each column presents results for a different subsample of municipalities.

Appendix Table A23. Civilian resistance: Low voter turnout during the peace referendum

| | Yes (1) | No (2) |
|---|---|---|
| Rejection share (%) | 0.469** | 0.083 |
| | (0.154) | (0.075) |
| Bandwidth | 10.281 | 16.379 |
| Observations | 428 | 832 |
| Clustered SE | ✓ | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019). Low voter turnout is represented by a dummy variable set to 1 if turnout is less than the median of its empirical distribution. Each column presents results for a different subsample of municipalities.

Appendix Table A24. Civilian resistance: FARC-controlled areas and low voter turnout during the peace referendum

| | FARC (1) | Non-FARC (2) |
|---|---|---|
| Rejection share (%) | 1.267*** | 0.090 |
| | (0.346) | (0.136) |
| Bandwidth | 11.240 | 10.079 |
| Observations | 112 | 328 |
| Clustered SE | ✓ | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019). The estimates are restricted to municipalities reporting low voter turnout. High voter turnout is represented by a dummy variable set to 1 if turnout exceeds the median of its empirical distribution. Each column presents results for a different subsample of municipalities.

Appendix Table A25. State presence: PDET municipalities

| | Yes | No |
|---|---|---|
| | (1) | (2) |
| Rejection share (%) | 0.301 | 0.141 |
| | (0.266) | (0.086) |
| Bandwidth | 7.802 | 11.277 |
| Observations | 112 | 912 |
| Clustered SE | ✓ | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019). PDET municipalities are those identified as priority areas for development under the Colombian Peace Agreement, focusing on regions most affected by violence, poverty, and weak state presence. These municipalities are represented by a dummy variable, which is set to 1 if a municipality is part of the PDET program, and 0 otherwise. Each column presents results for a different subsample of municipalities.

Appendix Table A26. State presence: FARC-controlled areas and PDET municipalities

| | FARC | Non-FARC |
|---|---|---|
| | (1) | (2) |
| Rejection share (%) | 0.721 | -0.887 |
| | (0.381) | (0.471) |
| Bandwidth | 7.760 | 25.484 |
| Observations | 72 | 92 |
| Clustered SE | ✓ | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019). The estimates are restricted to PDET municipalities. PDET municipalities are those identified as priority areas for development under the Colombian Peace Agreement, focusing on regions most affected by violence, poverty, and weak state presence. These municipalities are represented by a dummy variable, which is set to 1 if a municipality is part of the PDET program, and 0 otherwise. Each column presents results for a different subsample of municipalities.

Appendix Table A27. State presence: PNIS municipalities

| | Yes (1) | No (2) |
|---|---|---|
| Rejection share (%) | 2.283*** | 0.102 |
| | (0.457) | (0.070) |
| Bandwidth | 12.250 | 16.380 |
| Observations | 48 | 1404 |
| Clustered SE | ✓ | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019). PNIS municipalities refer to areas prioritized for implementing the national program aimed at substituting illegal crops, primarily coca. These municipalities are represented by a dummy variable, which is set to 1 if a municipality is part of the PNIS program, and 0 otherwise. Each column presents results for a different subsample of municipalities.

Appendix Table A28. State presence: FARC-controlled areas and PNIS municipalities

| | Yes (1) | No (2) |
|---|---|---|
| Rejection share (%) | 2.181*** | 0.298 |
| | (0.492) | (0.156) |
| Bandwidth | 9.415 | 18.042 |
| Observations | 32 | 240 |
| Clustered SE | ✓ | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019). The estimates are restricted to FARC-controlled municipalities. PNIS municipalities refer to areas prioritized for implementing the national program aimed at substituting illegal crops, primarily coca. These municipalities are represented by a dummy variable, which is set to 1 if a municipality is part of the PNIS program, and 0 otherwise. Each column presents results for a different subsample of municipalities.

Appendix Table A29. State presence: Remoteness from military brigades

| | Yes | No |
|---|---|---|
| | (1) | (2) |
| Rejection share (%) | 0.583** | 0.061 |
| | (0.192) | (0.144) |
| Bandwidth | 10.064 | 9.267 |
| Observations | 352 | 568 |
| Clustered SE | ✓ | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019). The dummy variable for remoteness from military brigades is defined as a binary indicator that takes the value of 1 if the average distance to 24 military brigades is greater than the median of the empirical distribution of these distances. Each column presents results for a different subsample of municipalities.

Appendix Table A30. State presence: FARC-controlled areas and remoteness from military brigades

| | FARC | Non-FARC |
|---|---|---|
| | (1) | (2) |
| Rejection share (%) | 1.005*** | -0.201 |
| | (0.312) | (0.104) |
| Bandwidth | 13.768 | 20.309 |
| Observations | 128 | 496 |
| Clustered SE | ✓ | ✓ |

Standard errors in parentheses are clustered at the department-month level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Results of linear polynomials with equal bandwidths and uniform kernel based on Calonico et al. (2014), Calonico et al. (2015), Calonico et al. (2018), and Calonico et al. (2019). The estimates are restricted to municipalities situated far from military brigades. The dummy variable for remoteness from military brigades is defined as a binary indicator that takes the value of 1 if the average distance to 24 military brigades is greater than the median of the empirical distribution of these distances. Each column presents results for a different subsample of municipalities.