Unequal Before the Law: Political Intensification and Forbearance of Counternarcotics Enforcement*

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April 22, 2024

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Abstract

Ideally, the state commits to enforcing its laws equitably to uphold security and social order. In practice, however, enforcement is rarely uniform. Why does the state use law enforcement to crack down in some places but not others? Governments face electoral pressures to enforce unequally. In the context of illicit markets like the drug trade, where non-state armed groups are often involved, the stakes of enforcement are higher. When non-state armed actors influence elections, they create differential incentives for the state to crack down in some areas but not others. Using a difference-in-differences design, I demonstrate patterns of forbearance and intensification of forced coca eradication via aerial fumigation in Colombia. Despite widespread increases in eradication in the early 2000s, municipalities with more historical violence by armed groups that shared political alignment with the government—paramilitary groups—experienced less eradication after the election of hardline President Álvaro Uribe. Furthermore, the state engaged in forbearance in paramilitary municipalities where the incumbent outperformed electoral expectations and in areas with recent electoral violence, suggesting that electoral influence is the mechanism that drives the results.

Key words: Law enforcement, state capacity, drugs and politics, non-state armed groups

Word count: 9,739

*Thanks to Elena Barham, Ella Bayi, Carolina Bernal, Stephanie Char, Sarah Daly, John Marshall, Arnaud Maurel, Eduardo Moncada, Beatrice Montano, Vicky Murillo, Oscar Pocasangre, Zara Riaz, Yining Sun, Carolina Torreblanca, Jessie Trudeau, Andres Uribe, and attendees of the 2024 Political Economy of Latin America Conference and the Studies of Organized Crime and Politics Mini-Conference at the 2024 Midwest Political Science Association meeting for helpful comments and suggestions. Iván Ramírez provided outstanding research assistance. This project is supported by an APSA Doctoral Dissertation Research Improvement Grant.

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1 Introduction

Law enforcement is the essential and defining function of the state. Citizens submit to the state to gain the benefits of a peaceful sociopolitical order (Hobbes, 1651; Olson, 1993) since the state is the holder of the monopoly on the legitimate use of violence in its territory (Weber, 1919).[1] Ideally, states would enforce violations of their laws uniformly, creating additional predictability and stability. Indeed, equal protection under the law is a recognized desirable normative principle enshrined in many countries' constitutions as a fundamental human right. Nevertheless, unequal enforcement of the law is the empirical norm in cases as varied as property rights (Holland, 2017), labor statutes (Ronconi, 2010), taxes (Bergman, 2015), and environmental regulations (Bonilla-Mejía and Higuera-Mendieta, 2019).

Why does the state crack down on violations of its laws more harshly in some places and at some times over others? In part, enforcement gaps can result from institutional weakness (Brinks, Levitsky and Murillo, 2019), the inability of the state to enforce where it wants to and when it wants to. However, enforcement efficacy is not only a function of weak state capacity: it can also result from strategic decisions by state actors (Kleinfeld and Barham, 2018; Yashar, 2018).

Focusing on counternarcotics enforcement, I argue that when non-state armed groups immersed in illegal economies influence elections, variation in their characteristics and presence will, in turn, affect the geographic targeting of enforcement. I expect the state to use forbearance as a tool for reprieve to areas under the influence of non-state armed groups with which it is politically aligned, even as these groups threaten the state's monopoly on violence.[2] Conversely, I expect the state will intensify enforcement to repress areas under the influence of unaligned non-state armed groups. Incumbent politicians may use counternarcotics efforts like crop eradication selectively and strategically to maximize the benefits of their implementation while "redistributing" the negative consequences of such policies. This paper uses forced coca crop eradication in Colombia

[1] Of course, states can also provide additional public goods such as education, healthcare, and infrastructure, but the provision of these goods is difficult, if not impossible, in the absence of security.

[2] This tension can manifest even without collusion between the state and armed groups or state capture, although overt cooperation will exacerbate it.

via aerial fumigation, a significant component of counternarcotics strategy (Mejia and Restrepo, 2016), as a case study of these dynamics.

Since 2000, the United States government has spent more than US$20 billion on counternarcotics programs in Latin America and the Caribbean, the region with the highest levels of criminal violence globally (Arjona, 2021). Despite this substantial investment, our understanding of the factors that drive variation in the incidence or intensity of drug enforcement efforts remains limited. Even more puzzling, research on supply-side policies aimed at reducing the amount of illicit drugs in the market via punishing producers or traffickers finds that such an approach to counternarcotics is generally ineffective and results in extensive negative externalities. I argue that these nominal inefficiencies reflect more than weak state capacity: they are endogenous to political factors.

At a national, aggregated level, crackdowns on illicit drugs can benefit incumbent governments by improving their standing with the international community—especially their standing vis-a-vis the United States—and crime-sensitive voters. But, like other policies with diffuse benefits but specific costs, such as environmental regulations, healthcare reforms, and public infrastructure projects, it can be beneficial to selectively target the de facto implementation of antidrug policies at the local level. Even as repressive enforcement practices may create a concentrated backlash among affected citizens, non-state armed groups generate differential incentives for enforcement because they can directly influence electoral behavior or even election results (Acemoglu, Robinson and Santos, 2013; Trudeau, 2022).

I investigate a pivotal period in the expansion of Colombian counternarcotics policies: the late 1990s and the decade of the 2000s. In 2000, the U.S. signed into law Plan Colombia, a bilateral aid initiative that allocated billions of U.S. dollars to military training and equipment, specifically focusing on new Colombian counternarcotics battalions. This initiative greatly expanded the capacity of the central state to implement counternarcotics enforcement. At this time, President Andrés Pastrana (1998-2002) could not run for reelection and was bogged down with low approval ratings resulting from his handling of the security issues the country faced. I thus leverage the election and inauguration of his successor, President Álvaro Uribe (2002-2010), as a critical juncture where the central state would have been incentivized to differentially eradicate depending on the historical influence of competing armed groups. Paramilitary groups favored Uribe, who ran a staunchly militaristic anti-guerrilla campaign and effectively lobbied the Colombian Congress to amend the 1991 constitution, enabling himself to seek a second term.

Using monthly municipal-level data on aerial eradication obtained through an information request to the Colombian Ministry of Justice (*Ministerio de Justicia*) and a difference-in-differences design, I show that during Uribe's government, less eradication was conducted in municipalities with historically high levels of paramilitary violence.[3] These results hold when controlling for coca cultivation and across various measurement strategies. Substantively, for a given municipality, a standard deviation increase in historical paramilitary attacks is associated with 10 fewer hectares of aerial eradication per month on average, about a 5.82% decrease. This translates to around 500 hectares over the course of a 4-year term.[4] I interpret this relationship as resulting from retrospective forbearance toward the paramilitaries, the non-state armed groups more politically aligned with the government (Acemoglu, Robinson and Santos, 2013) in the wake of Uribe's election. Conversely, during this same period, there was more eradication in municipalities with historically high levels of guerrilla violence. The standardized effect for guerrilla violence is similar in magnitude but positive: an increase in average monthly hectares fumigated of about 7.03%.

I argue that armed group electoral influence drives the forbearance results. Late into the first term of Uribe's presidency (2002-2006), the United Self-Defense Forces of Colombia (*Autodefensas Unidas de Colombia*, AUC)—the largest paramilitary umbrella organization—negotiated its demobilization with the administration (Daly, 2016). I provide evidence that forbearance during the 2002-2006 period was driven by municipalities with historically high paramilitary violent presence where Uribe overperformed electoral expectations. After paramilitary demobilization, this relationship between overperformance, historical paramilitary violence, and eradication is

[3] I also conducted fieldwork in Bogotá, Medellín, and four coca-growing municipalities (Anorí, Cáceres, Ituango, and Tarazá) to contextualize the research design and motivate the tests, interviewing coca growers, social leaders, politicians, and military, police, and government officials.

[4] The average size of a coca plot during this time is approximately 1.25 hectares, and about 63% of all total hectares of coca crops detected were cultivated in plots smaller than 3 hectares.

muted. Similarly, from 2002-2006, less eradication was undertaken in municipalities with electoral violence—primarily undertaken by paramilitaries—in 2002. This relationship is attenuated when using electoral violence in 2006 to predict eradication in 2006-2010. These mechanism suggest that when criminal actors successfully deliver votes, they receive relief from repression as a reward.

Existing studies of counternarcotics have focused on the consequences of these policies and their enforcement on violence (Abadie et al., 2014; Calderón et al., 2015; Dell, 2015; Durán-Martínez, 2018; García-Jimeno, 2016; Lessing, 2017; Phillips, 2015; Trejo and Ley, 2020; Snyder and Duran-Martinez, 2009), state capacity (Flores-Macías, 2018; Yashar, 2018), and the drug market itself (Becker, Murphy and Grossman, 2006; Castillo and Kronick, 2020). Relatively less attention, however, has been given to the reasons why the same government can differ in its implementation of these strategies across time and space in the first place: enforcement is treated as absent (Bueno de Mesquita, 2020) or exogenous (Lessing, 2017; Castillo and Kronick, 2020). Torreblanca (2023) studies the electoral consequences of forced eradication of poppy fields in México, showing that eradication engenders decreases in government trust and electoral participation. I build on this contribution by theorizing and testing variation in political expediency to explore the causes of variation in eradication. In doing so, I link together the literature on counternarcotics with that of tough-on-crime or "mano dura" policies (Holland, 2013; Krause, 2014; Ventura, Ley and Cantú, 2024; Visconti, 2020) and electoral incentives (Downs, 1957; Przeworski, Stokes and Manin, 1999). Crop eradication is an example of a repressive approach to crime that may sometimes be ineffective at its stated goals (Blair and Weintraub, 2021) but nevertheless can be implemented strategically for electoral gains (Chevigny, 2003; Holland, 2013; Romero, Magaloni and Díaz-Cayeros, 2016). I also build on the literature on how organized crime groups influence politics (Barnes, 2017; Trudeau, 2022): this study makes a novel contribution in characterizing the circumstances by which the state is more or less likely to leverage or sideline the influence of these groups. More broadly, I view the uneven implementation of law enforcement through counternarcotics as a form of non-material redistribution. In doing so, I contribute to research studying intergovernmental transfers (Bonilla-Mejía and Higuera-Mendieta, 2017; Dixit and Londregan, 1998), acknowledging the role brokers play in changing central government strategy. I also expand the political economy framework of law enforcement (Dewey, Woll and Ronconi, 2021; Holland, 2017) to cases where the state is not the only actor who can control violence. In general, my approach separates the state from its constituent governments or regimes: a particular *government* can benefit from the selective enforcement of its monopoly on violence even as extralegal armed actors threaten the *state's* monopoly on violence.

2 The politics of supply-side counternarcotics

I conceptualize supply-side counternarcotics efforts (e.g., crop eradication, interdiction, or law enforcement operations like the targeting of high-level dealers) as a resource allocation problem that can be affected by political considerations. Crucially, the enforcement of counternarcotics policies typically falls under the jurisdiction of national-level actors but assigns costs and benefits differentially across space. In short, national-level actors reap the national and international benefits sown by enforcing such policies while local-level actors bear the burden—the direct and indirect costs created by enforcement. Therefore, spatial variation in enforcement will reflect this asymmetry.

National-level actors—who earn utility from the achievement of their policy preferences and from reelection where applicable—accrue benefits for supply-side counternarcotics from (1) the international politics of the global drug prohibition regime, especially U.S. bilateral aid, which is typically conditional on cooperation in anti-narcotics enforcement, and (2) broad domestic electoral benefits among the majority of crime-sensitive voters who are not harmed by supply-side policies. The former allows for greater resources to be spent in counternarcotics (if the politician is intrinsically motivated by enforcement) or for other issue areas. For example, U.S. aid allowed Uribe to strengthen the military broadly, a key aspect of his appeal as a hardline candidate who took tough, uncompromising stances on security issues, including counternarcotics. The latter increases reelection probability when security issues are important to the majority of voters in a national constituency and repressive approaches are seen as the most viable solution, a reasonable assumption during the period of study, though this is changing over time (Bewley-Taylor, 2012). Politicians can curry favor with crime-sensitive voters because supply-side efforts against illicit drugs are quite visible and concrete—in the case of crop eradication or interdiction, it is easy to quantify the number of illicit crops eradicated.[5] That said, while international and national electoral incentives encourage the implementation of counternarcotics actions as a whole, they do not necessarily shape their geographic implementation because their benefits are diffuse.[6]

Thus, subnational variation in enforcement reflects benefits and costs to the central government that are realized at the local level. Local-level actors generally bear the net costs of counternarcotics enforcement and can attribute these costs to the central government given the high clarity of responsibility surrounding the issue.[7] Neither local citizens nor politicians are particularly concerned with the international considerations that national-level actors have to keep in mind—these are simply too far removed from their day-to-day lives. Furthermore, the direct costs of enforcement policies are geographically concentrated at the point of the intervention. Aerial eradication has localized environmental (Rincón-Ruiz et al., 2016), health (Camacho and Mejía, 2017), human capital, economic (Rozo, 2014), and political (Ramírez, 2011) costs.[8] Drug seizures can spark violent backlash among drug traffickers (Castillo and Kronick, 2020; Dell, 2015; Flores-Macías, 2018) and citizens could blame politicians for the resulting violence (Marshall, 2023; Pocasan-

[5]This is also why the U.S. uses these as evaluative metrics of effort put into counternarcotics enforcement. Note that demand-side counternarcotics policies do not share this same characteristic, one reason why I exclude these policies from the scope of this theory.

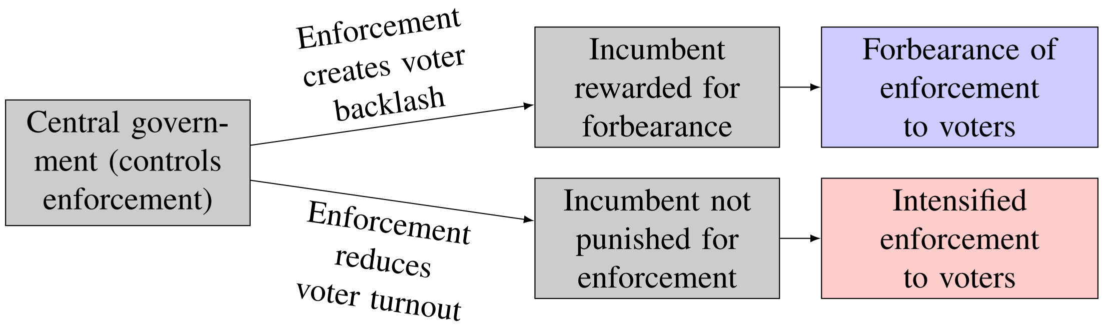
[6]While the U.S. may also have preferences to target particular armed groups such as left-leaning guerrilla groups (Tate, 2015), I assume that these preferences are constant over the post-Cold War period of study. To my knowledge, no public information substantiates the idea that the distribution of eradication efforts influenced U.S. evaluation of cooperation. Available evidence suggests that the U.S. used absolute metrics of hectares of crops eradicated and tons of drugs seized to assess cooperation (Grover, 2020) and did not consider subnational variation. Still, since my empirical strategy leverages changes in the Colombian government, the results cannot be entirely explained through a U.S.-centric lens.

[7]This will be particularly the case in more centralized systems like Colombia. By contrast, Ley (2017) provides evidence from México that suggests that voters are most likely to hold politicians accountable for criminal violence when local and federal governments are politically aligned. However, counternarcotics enforcement specifically has closer implications with central governments given its international dimension and because it affects marginalized populations with little other interaction with the state.

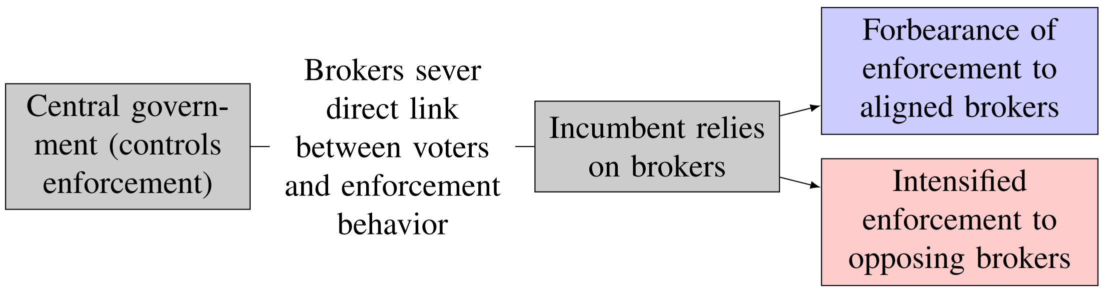
[8]Political costs could become nationalized if opposition to eradication becomes a movement with national prominence. However, this generally has not been the case in Colombia relative to Bolivia or Perú.

Figure 1: Diverging incentives for intensification and forbearance of enforcement.

(a) Enforcement incentives absent brokers.



(b) Enforcement incentives with electoral brokers.



gre, 2022). A similar dynamic occurs with other law enforcement operations, such as missions that attempt to "behead" criminal organizations by arresting or killing leaders of their networks (Calderón et al., 2015; Phillips, 2015), which also create violence and instability. Disruptions to trafficking activities from intensified enforcement from the national level can jeopardize fragile non-violent equilibria between criminal groups at the local level (Lucas, Marshall and Riaz, 2023; Snyder and Duran-Martinez, 2009; Trejo and Ley, 2020).

Therefore, it is unlikely that those who bear the brunt of the enforcement arm of the state will react positively to said enforcement. Affected citizens may respond with formal backlash through anti-incumbent voting or protests, or they may react by disengaging with the state and electoral process, as Torreblanca (2023) finds in México. As Figure 1a stylizes, when repressive enforcement creates a backlash, this incentivizes the government to refrain from enforcing to swing voters (Holland, 2017). When repressive enforcement reduces turnout among affected populations, then the government has incentives to target critical voters (Robinson and Torvik, 2009) or core opposition voters with intensified enforcement. Violent, repressive law enforcement that removes voters from the electorate—directly by killing or displacing them or indirectly by causing them to become disengaged with the electoral process—makes it fruitful to enforce intensely to swing voters. Put differently, the incentives to practice forbearance *on voters* are conditional on the electoral consequences of enforcement.

In either case, however, the presence of electoral brokers—here, non-state armed groups who influence elections—severs this link by making the broker the crucial actor in the process (Mares and Young, 2016), as shown in Figure 1b. Now, the government is incentivized to forbear or intensify enforcement based on the preferences of the broker (the armed actor), not the voters (Acemoglu, Robinson and Santos, 2013).[9] Existing research shows that armed groups can influence electoral behavior and outcomes, thereby molding election results to align with the armed groups' preferences (Hidalgo and Lessing, 2014; Staniland, 2015), including in Colombia (Ch et al., 2018; Hirschel-Burns, 2021; Uribe, 2023).

Under these circumstances, the presence and influence of these groups over their territory create locally differential costs and benefits in the enforcement of supply-side drug policies. When an armed group's preferences align with those of the incumbent government, it may be in the best interests of a government that wants to maximize its probability of reelection and pursue its policy goals to allow the armed group to persist even if the armed group challenges its rule (Acemoglu, Robinson and Santos, 2013). This persistence can be attributed to forbearance in enforcement against aligned armed groups. By contrast, opposition armed groups create incentives to intensify enforcement for parallel reasons. These insights lead to the first hypothesis:

Hypothesis 1 (H1). *Governments will be less (more) likely to enforce or reduce (intensify) counternarcotics enforcement in areas under the influence of non-state armed groups that share aligned (opposing) political preferences.*

If electoral pressures drive these incentives, then one should expect these differential enforce-

[9] Illicit markets are particularly susceptible to attracting non-state armed groups because, by definition, illegal markets cannot use the state to secure their property rights. Violence is the contract enforcement mechanism in illegal markets (Reuter, 2009), and non-state armed groups have a comparative advantage in the use of extralegal force.

ment patterns to be especially salient in areas where armed groups have most influenced or can most influence electoral behavior, which I will operationalize by testing for differential enforcement in swing municipalities and municipalities which experienced recent electoral violence. This logic motivates the second hypothesis:

Hypothesis 2 (H2)**.** *Political forbearance and intensification of counternarcotics enforcement will be greater in areas where the central government gained the most from non-state armed group electoral influence.*

3 Context: aerial coca crop eradication in Colombia

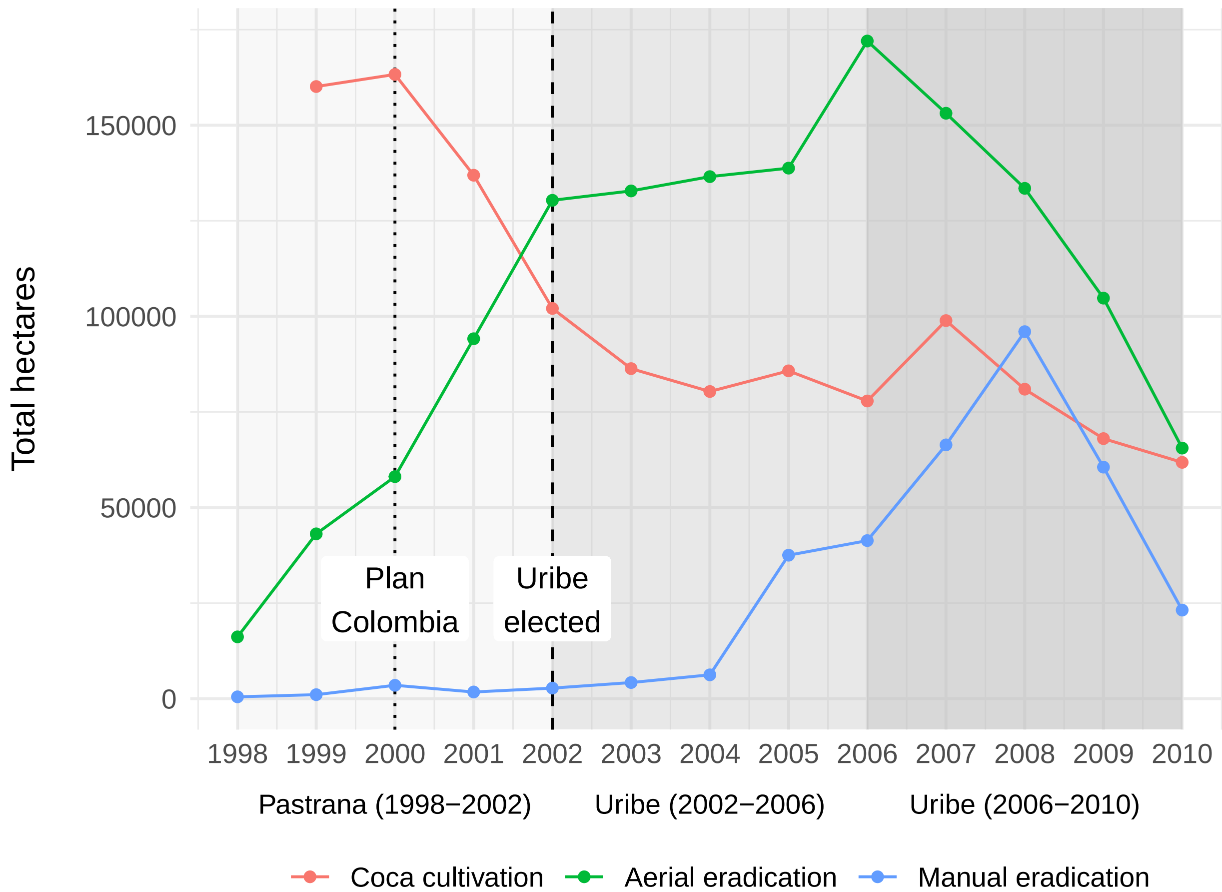
From 1998-2010, more than 1,700,000 hectares of coca crops were eradicated in Colombia, an area almost the size of the U.S. state of New Jersey or the Colombian department of Huila (each measuring approximately 1,900,000 hectares). Figure 2 shows the number of coca hectares cultivated and eradicated each year during this time.[10] This nationwide intensification of eradication came partly due to the passing of Plan Colombia, the bilateral U.S. aid initiative designed to end the armed conflict in Colombia and create a robust counternarcotics strategy. Colombia became the second-largest receiver of U.S. military aid after Israel in these years.

Forced crop eradication can occur in two forms: aerial and manual. Aerial eradication is undertaken by planes or helicopters spraying pesticides, most commonly glyphosate, to destroy coca crops.[11] Manual eradication, meanwhile, involves teams directly on the ground pulling out or fumigating the crop at the root, typically with police or military escorts. Forced eradication in all forms is a paradigmatic case of the dynamics described in Section 2: it is controlled by the *central* government in Colombia, overseen by the executive branch, and executed by the national police or the military. However, this paper focuses on aerial eradication, which has characteristics that make the study of its variation more credible. Aerial eradication is a more indiscriminate

[10]The number of hectares eradicated can be greater than the number of hectares cultivated because cultivation is measured net of eradication at the end of each year. See Section 4 for more details.

[11]Aerial eradication has been halted in Colombia since 2015 because of evidence of its deleterious health consequences.

Figure 2: Number of coca hectares cultivated and eradicated, 1998-2010.



form of enforcement than manual eradication, and there is a greater distance between enforcers and those affected by enforcement. As a result, any differences between the national police and military in implementation are reduced when considering aerial eradication only.[12] Second, aerial eradication is less likely to be affected by other time-varying factors that influence variation in manual eradication, such as organized peasant resistance[13] and safety considerations. Manual eradication is more limited by safety and contemporary military control, while aerial eradication is more affected by exogenous factors such as the weather (Reyes, 2014), although armed actors present some danger to both forms of eradication. Finally, the effects of aerial eradication on local

[12] I conducted interviews with politicians and military, police, and government officials that suggest the two bodies operate similarly, but the data I use does not disaggregate between implementing actor (see Section 4 for more details).

[13] I conducted interviews with social leaders that suggest that sufficiently organized coca growers can sometimes prevent manual eradication but not necessarily aerial eradication.

populations are much stronger than the effects of manual eradication. The indiscriminate nature of aerial eradication makes it particularly undesirable for those affected by it: I conducted interviews with social leaders and coca growers who described how pesticides spilled over onto licit crops, affected water sources, and generated other health and economic consequences. Even though the presence of armed forces that accompany manual eradication teams could cause abuses of power, coca-growing communities particularly despise aerial fumigation (Ramírez, 2011).

Despite these negative consequences, Figure 2 shows that eradication is essential to the Colombian counternarcotics strategy. Foreign aid and electoral benefits that accrue to national-level politicians generate incentives to implement eradication in general.

International benefits include the U.S. aid initiative known as Plan Colombia, which began in 2000 and provided over US$1.2 billion in foreign assistance (Dube and Naidu, 2015) with the expectation of extensive coca crop eradication. The looming threat of reduction of aid also played a role in incentivizing cooperation in counternarcotics among central government officials: a previous official U.S. decertification of Colombia's noncompliance with counternarcotics efforts in 1996 resulted in the cancellation or delay of US$35 million in assistance to Colombia (Crandall, 2002), the suspension of trade preferences for Colombian exports, and vetoes from the U.S. of Colombian requests for funding from international financial institutions, among other consequences (Ramírez, 2011). Decertification's stakes were even higher during the period of Plan Colombia.

Electorally, in Colombia, drug-related issues are particularly salient to voters given the long history of violence in the country and the involvement of armed groups in the drug trade. Appendix Figure A1 uses AmericasBarometer data to show that from 2004-2010, a majority of respondents considered issues that could be classified as related to drugs, crime, and security as the most crucial issue facing the country, except for the years after the Great Financial Crisis.

Although the AmericasBarometer data does not cover the period of the early 2000s, qualitatively, Uribe's 2002 presidential campaign focused on security issues. The election came in the wake of failed peace negotiations with guerrillas by former president Pastrana (1998-2002), so Uribe's campaign focused on a robust military strategy against insurgent groups with heavy use of force. This strategy proved successful as Uribe became the first-ever president elected to office without needing a second round: Uribe won 53% of the vote in 2002, a 21-percentage point margin over his closest challenger. On the day of his inauguration, mortar attacks on the presidential palace—which left about a dozen civilians wounded—helped Uribe further justify his approach: in the subsequent years, Uribe expanded the power of the military. Counternarcotics played an essential role in these counterinsurgency operations, given the blurred lines between these two objectives (Dube and Naidu, 2015). Moreover, crop eradication was explicitly part of Uribe's "democratic security" policy, so results in this area were necessary to keep campaign promises. This approach was generally successful: the level of violence throughout the country decreased, and the government earned vital victories in its battles against the guerrilla groups. Midway through his term, Uribe successfully passed a change in the reelection law: previously, Colombian presidents were not allowed any form of reelection. He ran for president again in 2006 and won again in the first round: his 62% of the vote made for the largest victory for a presidential candidate in Colombian history. Uribe's electoral successes came not despite but rather because of his intense militarized approach to the conflict and counternarcotics. These observations echo research on the electoral success of violent victors (Daly, 2022*b*).

Meanwhile, the costs—both practical and political—of implementing eradication are low for the central government. Estimates suggest that the cost of spraying one hectare of coca crops with glyphosate is approximately US$2,400 (Mejía, 2016).[14] Moreover, coca crops, especially extensive plantations, are relatively easy to identify using satellite imagery (Reyes, 2014). The ecological (Rincón-Ruiz et al., 2016), public health (Camacho and Mejía, 2017), and economic consequences (Rozo, 2014) of crop eradication are geographically concentrated.

As a result of Colombia's centralized process for eradication, the blame for eradication is easily attributable to the national government for the rural farmers, *campesinos*, who are the primary individuals engaged in coca cultivation in Colombia. Interviews with *campesinos* and social leaders reveal that those affected by eradication attribute blame enforcement and its consequences entirely

[14]Manual eradication is more expensive since it requires higher levels of capacity and effort, as do voluntary substitution efforts (Ladino, Saavedra and Wiesner, 2021).

on national-level governments (and the U.S.). This should not be surprising: these communities face very few interactions with the state otherwise.[15] Thus, these peripheral communities and the weak state presence in the areas they live in are particularly susceptible to capture, influence, or coercion by non-state armed groups, which function at the more lucrative refinement and transport stages of the cocaine supply chain (Bergman, 2018) or by taxing growers directly.

Absent armed group involvement in elections, one would expect the central government to eradicate where it is least costly: the military and police may simply eradicate where it is most convenient in practical terms to do so to meet eradication targets. However, coca cultivation alone cannot fully explain variation in eradication efforts. As Table 1 shows, a model predicting aerial eradication in municipality i during year-month t using coca cultivation area in the previous year[16] and municipality and year-month fixed effects only explains between 14% and 25% of the within-sample variation of the outcome.[17] Municipality fixed effects account for any time-invariant geographic characteristics of each municipality: altitude, climate, distance to nearby cities, military installations, and coca suitability, among others. Year-month fixed effects account for any national trends in eradication resulting from, for example, seasonal weather changes.

In sum, time-invariant factors like geography only explain some of the variation in the implementation of eradication. Following the logic of Section 2, I therefore argue that geographic variation in enforcement—forbearance and crackdowns of eradication in some areas over others—should be driven by variation in armed group presence and characteristics.

Indeed, the Colombian context carries the dynamic of various non-state armed groups with

[15]In addition, because of this national politicians cannot necessarily influence voters' perceptions of local politicians with counternarcotics enforcement. The fieldwork interviews also suggest that affected populations see local politicians like mayors and department governors as impotent to the imposition of the central state.

[16]Eradication is measured at the monthly level, while cultivation is measured only at the yearly level—see Section 4 for further details.

[17]This exercise has limitations. The coefficient of determination, R^2, measures how well a linear function using the set of predictor variables predicts the outcome. I do not necessarily argue that political explanations linearly explain a significantly higher portion of the within-sample variation. Instead, I argue that political forbearance and intensification are additional drivers of the heterogeneity in enforcement. Nevertheless, the point that geographic patterns alone do not explain eradication still stands: in addition to considering the R^2 values, notice that the coefficients, while precisely estimated, also imply that cultivation does not fully explain eradication. For example, Column 1 of Table 1 implies that during the period of study, approximately 0.5 hectares of coca were eradicated for each hectare of coca cultivated: there is still substantial heterogeneity to be explored.

Table 1: The relationship between coca cultivation and aerial coca eradication.

	Hectares (1)	Hectares (ln + 1) (2)	Hectares (> 0) (3)
Panel A: Aerial eradication, coca-growing municipalities			
Coca cultivation	0.043***		
	(0.008)		
Coca cultivation (ln + 1)		0.057***	
		(0.009)	
Coca cultivation (> 0)			0.004
			(0.003)
R^2	0.14	0.23	0.21
Observations	37,973	37,973	37,973
Municipalities	299	299	299
Outcome range	[0-17,100.7]	[0-9.75]	{0,1}
Outcome mean	34.61	0.32	0.06
Outcome std. dev	280.80	1.34	0.23
Coca cultivation range	[0-16,523.88]	[0-9.71]	{0,1}
Coca cultivation mean	351.48	2.79	0.60
Coca cultivation std. dev	1,152.26	2.77	0.49
Panel B: Aerial eradication, all municipalities			
Coca cultivation	0.042***		
	(0.008)		
Coca cultivation (ln + 1)		0.065***	
		(0.009)	
Coca cultivation (> 0)			0.014***
			(0.002)
R^2	0.15	0.25	0.23
Observations	142,494	142,494	142,494
Municipalities	1,122	1,122	1,122
Outcome range	[0-17,100.7]	[0-9.75]	{0,1}
Outcome mean	9.26	0.09	0.02
Outcome std. dev	145.85	0.71	0.12
Coca cultivation range	[0-16,523.88]	[0-9.71]	{0,1}
Coca cultivation mean	93.67	0.74	0.16
Coca cultivation std. dev	614.78	1.89	0.37

Notes: All specifications are estimated using OLS and include municipality and year × month fixed effects. Robust standard errors clustered by municipality are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

varying political preferences. The contemporary conflict originates with the formation of several guerrilla groups in the aftermath of *La Violencia*, a civil war fought between the historically dominant Conservative and Liberal Parties. The 1958 arrangement that brought an official end to *La Violencia* did not end the violence in the countryside. In 1964, the left-wing Revolutionary Armed Forces of Colombia (*Fuerzas Armadas Revolucionarias de Colombia*, FARC) formed as a guerrilla group to contest the state. Additional left-wing insurgent groups, such as the National Liberation Army (*Ejército Nacional de Liberación*, ELN), soon followed (Arjona, 2016). Paramilitary groups such as the AUC emerged to combat these insurgencies (Daly, 2016). During the period of the study—from the late 1990s through the 2000s—all of these different groups experienced periods of ascendancy and decline (Ch et al., 2018), and each of these groups became involved with the drug trade and influenced politics.

In particular, paramilitary groups such as the AUC explicitly favored Uribe. The AUC's greatest strength coincided with the period just before the election of President Uribe in 2002, and there was extensive coercion and vote rigging in this election in areas with paramilitary presence. Moreover, politicians who supported Uribe's term limit removal were more likely to be arrested for ties to paramilitaries and more likely to support laws that favored paramilitary groups (Acemoglu, Robinson and Santos, 2013). During his first term, President Uribe negotiated paramilitary demobilization, providing amnesty to most members and limited sentences, although some leaders were extradited.

On the other hand, guerrilla groups opposed Uribe's government and socialized the populations in their influence into their ideology (Hirschel-Burns, 2021). Fergusson et al. (2021) show that paramilitary violence increases in response to the election of a left-wing mayor as a function of traditional elite backlash to threats to de facto political power. Eradication can function as a form of legitimized violence for crackdowns on guerrillas.

In the context of Colombia, H1 implies that after the election of Uribe—who faced reelection incentives and had strong policy preferences for intensified enforcement—one should expect forbearance of eradication efforts in municipalities with historically high levels of paramilitary violence. Conversely, H1 also implies that one should expect greater incidence and intensity of eradication in municipalities with historical guerrilla violence in the 2002-2010 period. H2 implies that these dynamics for paramilitaries should be exacerbated in areas where Uribe gained the most from non-state armed group electoral influence, measured as municipalities where Uribe overperformed electoral expectations—swing municipalities—and municipalities that experienced recent electoral violence likely underaken by paramilitaries in favor of Uribe.

4 Data

To test H1 and H2, I constructed a monthly panel from August 1998 to July 2010, covering the Pastrana and Uribe administrations, Plan Colombia's incidence, paramilitary groups' demobilization, and two presidential elections.

Outcome variable: crop eradication

I sourced data on the outcome measure of interest, crop eradication, via an information request to the Colombian Ministry of Justice (Ministerio de Justicia) from the Colombian Ministry of National Defense (Ministerio de Defensa Nacional). Their reports of aerial eradication aggregate the monthly number of hectares fumigated in each municipality. The starting point of data collection, March 1994, is before the beginning of the time period of study, which corresponds to the inauguration of Pastrana in August 1998. I choose to use metrics of eradication that are reported by the Colombian government not only because it is standard in the literature (Mejia and Restrepo, 2016; Prem, Vargas and Mejía, 2023) but also because any reporting biases that favor a lack of a relationship between political factors and eradication will bias the estimates downward.

Predictor variables: previous armed group violence

The key predictor variables of interest are measures of guerrilla and paramilitary presence across municipalities as proxied by aggregating attacks over time, which follows the empirical literature on the Colombian conflict (Acemoglu, Robinson and Santos, 2013; Ch et al., 2018). Aggregating attacks over many years ensures idiosyncratic year-to-year fluctuations in the conflict do not drive the results and that the results are not entirely a function of the mechanical or contemporary effects of armed group presence on eradication based on safety considerations. While violence-based measures may not necessarily capture territorial control by armed groups (Aponte González, Hirschel-Burns and Uribe, 2023), I argue that historical violence is a prerequisite for presence and influence, in line with the existing literature (Ch et al., 2018).

The primary source of the violence data comes from Restrepo, Spagat and Vargas (2003), a database[18] which counts paramilitary and guerrilla attacks from the Center for Research and Popular Education or *Centro de Investigacion y Educacion Popular* (CINEP)'s *Noche y Niebla* records. CINEP is a Colombian NGO that uses validated media reports, victim testimony, and other sources to construct detailed violence records. Each record is manually classified based on the perpetrating armed group. The raw number of attacks in each municipality over several years is summed together, divided by the total number of months of the time window used, which is then divided by the average of the municipality's population from the National Administrative Department of Statistics of Colombia throughout the time period, and multiplied by 100,000 to create the variable used in the regression models. Thus, the attacks refer to the average number of monthly attacks by each type of armed group in each municipality per 100,000 population. Appendix Figure A3 maps the variation in attacks by plotting the logged values for each armed group.[19] I also construct a binary measure by taking the municipalities in the top quartile of attacks for each armed group for the specified time period. In the appendix, I probe the robustness of the results to various other measurement strategies. Section 5 details these additional specifications.

I group historical violence conducted by paramilitary organizations, primarily the AUC, into a single category of paramilitary violence. Similarly, I group historical violence by different guerrilla groups, such as the FARC or ELN, into a single measure of guerrilla violence. Municipalities in the sample vary cross-sectionally along these two dimensions. Historical violence by one group of

[18]The dataset has been extended by the Universidad del Rosario through 2014.

[19]I use the logged values in the map to facilitate visual interpretation and the raw values per capita in the main specifications to facilitate written interpretation, but the results are robust to the use of either.

armed actors is not exclusive to historical violence by another. On the contrary, many paramilitaries formed primarily to contest the gains made by guerrilla groups in the earlier days of the conflict. That said, there are also municipalities where only a single group of armed actors committed violence in the period I used to generate the predictors. Further, a paramilitary group may have dominated one area of a municipality, while a guerrilla group may dominate another. To account for potential threats to inference generated by this dynamic, in each model, I include measures of the intensity of historical violence by each type of group, assuming that both can affect eradication behavior instead of including each of the measures of armed group violence in separate estimating equations.[20]

Additional variables

In certain specifications, I control for the yearly net hectares cultivated of coca crops in a particular municipality. The source of these data is the Integrated Monitoring System of Illicit Crops (*Sistema Integrado de Monitoreo de Cultivos Ilícitos*, SIMCI) of the United Nations Office on Drugs and Crime (UNODC). On an annual basis since 1999, SIMCI detects areas where coca crops have been cultivated using satellite imagery. Helicopter flights take high-definition photographs to confirm the detection (Abadie et al., 2014). Usefully, since these data come from the UNODC, they are generally independent of the Colombian political system. Appendix Section A.2 describes these data in further detail.

The coca cultivation and eradication data are combined to construct the sample of municipalities used in the study. I use as an estimation sample the 318 (out of 1,122) municipalities in Colombia with *any* aerial eradication or cultivation from 1998-2010. Appendix Figure A2 uses a map to highlight the variation in cultivation and aerial eradication: any municipalities with positive values for either of these variables are included in the sample, covering a wide swath of the Colombian territory.

To test mechanisms, I add electoral data from Pachón, Sánchez Torres et al. (2014) to the panel.

[20] I do not consider their interaction because this would be equivalent to testing whether the gap in enforcement changes before and after the election of Uribe: this is plausible, but a triple interaction would require substantial additional power, and H1 does not concern the differences across groups.

For each municipality, these data present each presidential candidate's first-round vote share in the 1994, 1998, 2002, and 2006 elections. I additionally include data on electoral violence data from the Electoral Observation Mission (*Misión de Observación Electoral*, MOE), an NGO working in Colombia on these topics (Nieto-Matiz, 2019).

5 Forbearance and intensification of eradication

5.1 Empirical strategy

I adopt a difference-in-differences design to test H1, leveraging cross-sectional variation in historical armed group presence alongside temporal variation in the incentives for the government to forbear or intensify enforcement against certain armed groups over others.

The design relies on the changes in incentives for the government of Uribe to use eradication against one group of armed actors over the others. The baseline category is eradication behavior during Pastrana's term (1998-2002). I opt for Pastrana's term as the baseline since Plan Colombia was enacted within this period, substantially enhancing the Colombian government's eradication capabilities. Simultaneously, the passing of Plan Colombia coincided with Pastrana's constitutional ineligibility for seeking reelection. After his presidency, Pastrana did not hold any further political office except for a brief tenure as Ambassador to the U.S. in 2005. Therefore, Pastrana's government would have been less incentivized to leverage or sideline the influence of armed actors through the strategic use of eradication.

After the election of Uribe, however, these incentives were much more potent. Uribe successfully lobbied the Colombian Congress to allow him to run for a second term, so he faced reelection incentives. As Section 3 describes, senators elected by votes in high paramilitary areas disproportionately voted in favor of removing his term limit, and paramilitaries delivered votes via coercion to politicians with preferences relatively close to theirs in executive and legislative elections, all favoring Uribe and his allies (Acemoglu, Robinson and Santos, 2013).[21] When it comes to guerrilla

[21] Forbearance in eradication to paramilitary areas does not rely on explicit collusion between the executive and the

Table 2: Summary of design.

	Pastrana, 1998-2002 ($T=0$)	Uribe, 2002-2010 ($T=1$)
High paramilitary violence ($P_i=1$)	$\mathbb{E}[Y_{i0}(0)\|P_i=1]$	$\mathbb{E}[Y_{i1}(1)\|P_i=1]$
Low paramilitary violence ($P_i=0$)	$\mathbb{E}[Y_{i0}(0)\|P_i=0]$	$\mathbb{E}[Y_{i1}(0)\|P_i=0]$
High guerrilla violence ($G_i=1$)	$\mathbb{E}[Y_{i0}(0)\|G_i=1]$	$\mathbb{E}[Y_{i1}(1)\|G_i=1]$
Low guerrilla violence ($G_i=0$)	$\mathbb{E}[Y_{i0}(0)\|G_i=0]$	$\mathbb{E}[Y_{i1}(0)\|G_i=0]$

groups, incentives to enforce also changed after the election of Uribe. While guerrilla groups have historically opposed the Colombian government, Pastrana was involved in peace negotiations with the FARC. The collapse of those negotiations partly led to Uribe's successful hardline campaign. So, I expect enforcement to be intensified in areas of guerrilla influence during Uribe's term relative to Pastrana. The design can thus be conceptualized as two separate difference-in-differences, stylized using the potential outcomes framework in Table 2, where i indexes municipalities and Y_t represents eradication outcomes in time t. In Section 5, I present results using both continuous and binary measures of historical armed group violence, though I present the predictors as binary in Table 2 for exposition.

For estimation, I use the following specification to predict the intensity and incidence of aerial coca eradication across Colombian municipalities where coca could plausibly be grown and aerially eradicated:

$$\begin{aligned} Eradication_{i,t} = {} & \beta_1 P_i \times \mathbb{1}[2002\text{-}2006] + \beta_2 P_i \times \mathbb{1}[2006\text{-}2010] + \\ & \beta_3 G_i \times \mathbb{1}[2002\text{-}2006] + \beta_4 G_i \times \mathbb{1}[2006\text{-}2010] + \\ & \gamma_i + \delta_t + \varepsilon_{i,t}, \end{aligned} \tag{1}$$

where $Eradication_{i,t}$ is a measure of eradication in municipality i in year-month t. P_i is a time-invariant metric of paramilitary attacks in municipality i, with G_i being the analogous metric for guerrilla attacks. These two variables are interacted with indicators for the months of Uribe's first presidential term (2002-2006) and Uribe's second presidential term (2006-2010), such that

armed group, only for their preferences to be aligned.

Pastrana's term (1998-2002) is the omitted category. Presidential terms begin in August and end in July. Municipality fixed effects γ_i account for any time-invariant confounding municipality characteristics—notably, agroclimatic suitability for the cultivation of coca crops and the size of each municipality—while year-month δ_t fixed effects guard against long-term and seasonal national-level trends. I report robust standard errors clustered at the municipality level.

The key coefficients of interest are the interaction between measures of different armed groups' historical violence and presidential term indicators. Here, β_1, β_2, β_3, and β_4 represent differential growth or decline in eradication behavior across municipalities with variation in guerrilla and paramilitary presence during the 2002-2006 and 2006-2010 presidential terms relative to baseline—Pastrana's term. Based on H1, I expect β_1 and β_2 to be negative: less eradication will be undertaken in areas of historical paramilitary violence during Uribe's two terms relative to Pastrana. Conversely, I expect β_3 and β_4 to be positive: more eradication will be undertaken in areas of historical paramilitary violence during Uribe's two terms relative to Pastrana. The constituent terms of the interactions are not represented in Equation 1 because the municipality fixed effects absorb the time-invariant variables for paramilitary and guerrilla violence while the year-month fixed effects absorb the indicators for presidential terms.

5.2 Results

The results from estimating Equation 1 using continuous measures of historical armed group violence are reported in Table 3, while Table 4 uses binary measures. Within these tables, I use three different outcome measures across columns to show that the results are not sensitive to the distribution of the raw outcome variable, which is particularly right-skewed. Column 1 uses hectares of coca crops eradicated. Next, Column 2 takes the natural log of crop eradication, adding a value of 1 to account for the municipalities with no eradication.[22] Finally, Column 3 evaluates the extensive margin, as the outcome is a binary measure of crop eradication in a municipality. Panel A of each

[22] Interpret the results from this column as percentage effects with caution, since Chen and Roth (2022) show how these transformations depend arbitrarily on the units of the outcome when the treatment affects the extensive margin.

Table 3: Temporal and geographic variation in the intensity and extent of aerial eradication using continuous measures of historical armed group violence.

	Hectares (1)	Hectares (ln+1) (2)	Hectares (> 0) (3)
Panel A: Aerial eradication			
Paramilitary attacks × 2002-2006	-22.799***	-0.111**	-0.016**
	(7.528)	(0.044)	(0.007)
Paramilitary attacks × 2006-2010	-18.603**	-0.169***	-0.031***
	(7.243)	(0.056)	(0.010)
Guerrilla attacks × 2002-2006	5.702	0.057***	0.009***
	(4.296)	(0.020)	(0.003)
Guerrilla attacks × 2006-2010	0.032	0.042*	0.009**
	(2.584)	(0.022)	(0.004)
R^2	0.12	0.22	0.21
Panel B: Aerial eradication, controlling for baseline coca cultivation			
Paramilitary attacks × 2002-2006	-18.678**	-0.091**	-0.014*
	(7.613)	(0.043)	(0.007)
Paramilitary attacks × 2006-2010	-20.305**	-0.159***	-0.029***
	(7.935)	(0.055)	(0.010)
Guerrilla attacks × 2002-2006	3.690	0.047**	0.008**
	(4.083)	(0.019)	(0.003)
Guerrilla attacks × 2006-2010	0.863	0.037*	0.008*
	(2.286)	(0.022)	(0.004)
R^2	0.12	0.23	0.21
Observations	45,792	45,792	45,792
Municipalities	318	318	318
Outcome range	[0-17,100.7]	[0-9.75]	{0,1}
Outcome mean	30.11	0.29	0.05
Outcome std. dev.	258.83	1.27	0.22
Paramilitary attacks range	[0-2.95]	[0-2.95]	[0-2.95]
Paramilitary attacks mean	0.50	0.50	0.50
Paramilitary attacks std. dev.	0.55	0.55	0.55
Guerrilla attacks range	[0-8.39]	[0-8.39]	[0-8.39]
Guerrilla attacks mean	1.15	1.15	1.15
Guerrilla attacks std. dev.	1.36	1.36	1.36

Notes: All specifications are estimated using OLS and include municipality and year × month fixed effects. Baseline category is Pastrana's term from 1998-2002. Robust standard errors clustered by municipality are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Temporal and geographic variation in the intensity and extent of aerial eradication using binary measures of historical armed group violence.

	Hectares (1)	Hectares (ln+1) (2)	Hectares (> 0) (3)
Panel A: Aerial eradication			
Paramilitary attacks × 2002-2006	-21.194**	-0.123**	-0.020**
	(10.119)	(0.051)	(0.008)
Paramilitary attacks × 2006-2010	-13.776	-0.145**	-0.027**
	(8.528)	(0.073)	(0.013)
Guerrilla attacks × 2002-2006	10.710	0.127**	0.021**
	(11.453)	(0.053)	(0.009)
Guerrilla attacks × 2006-2010	-1.345	0.114	0.024*
	(9.028)	(0.076)	(0.013)
R^2	0.12	0.22	0.21
Panel B: Aerial eradication, controlling for baseline coca cultivation			
Paramilitary attacks × 2002-2006	-13.419	-0.088**	-0.016**
	(8.190)	(0.044)	(0.008)
Paramilitary attacks × 2006-2010	-17.170*	-0.130*	-0.024*
	(9.719)	(0.072)	(0.013)
Guerrilla attacks × 2002-2006	1.206	0.084*	0.016*
	(9.009)	(0.049)	(0.008)
Guerrilla attacks × 2006-2010	2.805	0.095	0.020
	(8.702)	(0.078)	(0.014)
R^2	0.14	0.23	0.22
Observations	45,792	45,792	45,792
Municipalities	318	318	318
Outcome range	[0-17,100.7]	[0-9.75]	{0,1}
Outcome mean	30.11	0.29	0.05
Outcome std. dev.	258.83	1.27	0.22
Paramilitary attacks range	{0,1}	{0,1}	{0,1}
Paramilitary attacks mean	0.35	0.35	0.35
Paramilitary attacks std. dev.	0.48	0.48	0.48
Guerrilla attacks range	{0,1}	{0,1}	{0,1}
Guerrilla attacks mean	0.40	0.40	0.40
Guerrilla attacks std. dev.	0.49	0.49	0.49

Notes: All specifications are estimated using OLS and include municipality and year × month fixed effects. Baseline category is Pastrana's term from 1998-2002. Robust standard errors clustered by municipality are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

table reports results from the baseline specification described in Equation 1. In contrast, Panel B reports results from a specification that also controls for the sum of coca hectares detected during Pastrana's term (1999-2001) interacted with the indicators for Uribe's two administrations. This approach exploits variation across municipalities with similar levels of fixed baseline cultivation.

Supporting H1, the results show that relative to Pastrana's term, there was less aerial eradication in municipalities with high levels of historical paramilitary activity and more eradication in municipalities with high levels of historical guerrilla activity during Uribe's presidential terms.

For a given municipality, a standard deviation increase in historical paramilitary attacks per capita is associated with about 10 fewer hectares of aerial eradication per month, translating to approximately 500 fewer hectares fumigated over the course of a 4-year term. Considering the extensive margin, a standard deviation increase in historical paramilitary attacks per capita is associated with a 1.3 percentage point decrease in the probability of any aerial eradication. While the coefficient on guerrilla attacks for the raw hectares outcome is imprecisely estimated, for any given month of Uribe's first or second term, a standard deviation increase in historical guerrilla attacks per capita is associated with about a 1.2 percentage point increase in the probability of any aerial eradication. The results are of similar magnitudes when using a binary measure of historical armed group violence.

5.3 Validation tests and robustness checks

The validity of the difference-in-differences design rests on whether the untreated units of each group are appropriate counterfactuals for treated units.[23] To test this assumption, I conduct two separate tests. First, I test for divergence in pre-trends by dropping the two Uribe administrations from the sample and leading the treatment structure by 1 and 2 years. This tests whether differences in eradication behavior begin not with Uribe's administration but rather an earlier event. Each lead of the treatment can also represent tests of the early stages of Plan Colombia. The results,

[23] Since all units are treated simultaneously, there is no threat to inference from heterogeneous treatment effects over time (Callaway and Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021).

presented in Tables A1 and A2, show that during Pastrana's term, there are few differences in eradication behavior across areas of historically high paramilitary and guerrilla violence after July 2000 or July 2001. The coefficients are imprecisely estimated and substantively small. Second, I estimate event-study models that regress coca eradication on historical paramilitary and guerrilla violence measures interacted with indicators for each year between 1999 and 2010, using 1998 as the reference category. I present the results in graphical form in Figures A4 and A5. Across each of the different outcomes, there is a weak relationship between paramilitary and guerrilla violence until after 2002, which provides support for the identifying assumptions of the design and H1.

Moreover, the results are not a function of differential coca cultivation across municipalities that vary in their historical armed group violence. I estimate two additional specifications that account for coca cultivation in different ways. Instead of controlling for baseline levels of coca cultivation, I control for yearly lags of coca cultivation in Tables A3 and A4. The results here are even more substantial than the main results, though they should be treated with caution since levels of cultivation each year are endogenous to eradication. I also define the outcome as the proportion of coca hectares cultivated in year $t-1$ that were eradicated in year t. In this case, construct a yearly panel instead of year-month panel. For a given year, many municipalities—even those in the coca sample—did not experience cultivation, so I define the outcome as 0 for these municipalities. Conversely, some municipalities experienced greater eradication over the course of the year than coca cultivated (see Section 4 for more details), so I bound the outcome at 1. Regardless, I make similar conclusions from these results, presented in Table A5.

Next, I show that the results are similar across different measurement strategies. First, I generated the predictor variables based on aggregated attacks from the 1988-1997 period instead of the 1988-2001 period. I use the 1988-2001 period in the baseline specification because this range of years includes both eras of paramilitary and guerrilla ascendancy. In particular, the 1998-2001 years include the leadup to the Santa Fe de Ralito pact, where the paramilitary umbrella organization, the AUC, met with nearly 1,000 politicians to strategize a concerted effort to support Uribe's candidacy for presidency in 2002 (Ch et al., 2018). However, while this 1988-2001 range is still measured before the point where Uribe enters office, it is also measured during Pastrana's term. Nevertheless, the results, in Tables A6 and A7, are robust to these changes.

To account for differences across sources of violence data in Colombia (Osorio, 2023), I use an inverse-covariance weighted index of violence data from various sources. I bring together (1) the data from Restrepo, Spagat and Vargas (2003) used in the main results, (2) violence data from the National Center for Historical Memory (Centro de Memoria Histórica), which is a national agency created for truth-seeking and reconciliation related to the Colombian armed conflict (Grupo de Memoria Histórica, 2012), and finally (3) the VIPAA, or Violent Presence of Armed Actors in Colombia (Osorio et al., 2019) database, which uses computerized text annotation to classify violent events. The results of this exercise are reported in Table A8. While these results are more imprecisely estimated than the main ones, especially for the 2002-2006 term, all of the coefficients share the signs expected by H1.

To ensure the results are not being driven by the imposition of a linear functional form when using continuous predictors, I also fit models that transform these predictor variables by $\ln + 1$ in addition to the models that use binary measures of armed group violence. Table A9 accounts for the right skewness of the historical violence data (Ch et al., 2018) by applying this natural log transformation. This set of results is similar to the main results. I also assess the sensitivity of the binary results to the use of the other cutoffs of top quartiles with historical armed group violence. Figures A6 and A7 show the results using the top tercile or quintile as indicators.

Given concerns of potential time-varying unmeasured confounding, I flexibly interact the year-month fixed effects with municipality area, coca suitability, altitude, and distance to Bogotá, as well as measures of the pre-violence right/left lean of the municipality as proxied by the 1986 vote share of Álvaro Gómez Hurtado—a conservative presidential candidate later assassinated by the FARC in 1995—and Jaime Pardo Leal—the candidate of the unofficial political wing of the FARC in this election in Tables A10. Though the inclusion of the latter vote share variables reduces the sample size, the results are similar to the main results. Separately, I include department fixed effects interacted with year-month fixed effects. While these results are estimated less precisely, the results in Tables A12 and A13 are comparable to those from Table 3. Finally, I also use the regression discontinuity design from Fergusson et al. (2021) to show that close elections of right-wing or left-wing mayors do not generally change eradication outcomes[24] in Table A14. Thus, local partisanship does not explain the main results.

6 Mechanisms

The theory described in Section 2 posits that when armed groups act as electoral brokers, government enforcement patterns will depend on the alignment between the government and the armed groups, not necessarily voters. The results in Section 5 provide evidence governments hold back on enforcement to favorable armed groups. H2 posits that this forbearance should result from electoral incentives. To test H2, I hone in on the 2002-2010 years and assess variation in paramilitary electoral influence. I focus on paramilitary influence since paramilitaries affected national elections most directly (Acemoglu, Robinson and Santos, 2013).

6.1 Electoral overperformance

If the logic of forbearance in paramilitary areas during Uribe's terms is correct, then H2 suggests that these results should be driven by municipalities where Uribe overperformed expectations, especially those with high paramilitary presence. In other words, paramilitaries deliver votes the incumbent would not have otherwise received—swing municipalities—they receive relief from repression as a reward.

To test this implication of the theory, I define Δ_i^{2002} as the difference between Pastrana's 1998 vote share and Uribe's 2002 vote share in municipality i. Pastrana was the conservative presidential candidate in the 1998 presidential election; therefore, he serves as the closest analogue for Uribe's anticipated vote share in 2002. Similarly, I define Δ_i^{2006} as the difference between Uribe's 2006 vote share and Uribe's 2002 vote share in municipality i. Higher levels of Δ_i correspond to

[24] Fergusson et al. (2021)'s outcome is armed group violence.

municipalities where Uribe overperformed relative to expectations, likely because of paramilitary influence.

I predict eradication based on Δ_i and armed group presence, as well as their interaction, using the following specification:

$$Eradication_{i,t} = \beta_1 P_i \times \Delta_i + \mathbf{X}_i + \zeta_d + \delta_t + \varepsilon_{i,t}. \quad (2)$$

Here, $\mathbf{X}$ is a vector of controls to account for the cross-sectional nature of the model: coca suitability (Mejia and Restrepo, 2013), municipality area, and population (measured each year) in an attempt to account for the lack of municipality fixed effects—the inclusion of coca suitability drops some municipalities from the analysis. The guerrilla attacks variable G_i is also included in $\mathbf{X}$. Instead of municipality fixed effects, ζ_d represents department fixed effects. As in Equation 1, δ_t represents year $\times$ month fixed effects.

The results are presented in Table 5; estimated separately for the 2002-2006 presidential term (Panels A and B) and the 2006-2010 presidential term (Panels C and D). These results suggest that from 2002-2006, the differential decrease in aerial eradication in areas with high levels of historical paramilitary violence was concentrated in municipalities where Uribe overperformed expectations in the 2002 election. Absent historical paramilitary violence, the relationship between Uribe's electoral overperformance in 2002 and subsequent eradication from 2002-2006 is positive, perhaps suggesting leeway to eradicate more strongly in areas where Uribe overperformed. As historical paramilitary violence increases, however, this relationship reverses: municipalities where Uribe overperformed relative to Pastrana experienced *less* subsequent eradication. From 2006-2010, after paramilitary demobilization, there are no large or statistically detectable differences between Uribe's overperformance in 2006, historical paramilitary violence, and subsequent eradication. By contrast, Table A15 uses Uribe's vote share in each election. Absent historical paramilitary violence, a greater vote share for Uribe is associated with less eradication, suggesting the targeting of core opposition areas.

Table 5: Cross-sectional geographic variation in the intensity and extent of aerial eradication based on Uribe's electoral overperformance, 2002-2010.

	Hectares (1)	Hectares (ln +1) (2)	Hectares (> 0) (3)
Panel A: Aerial eradication (2002-2006)			
Δ^{2002}	101.353*	0.379*	0.053
	(51.764)	(0.218)	(0.035)
Paramilitary attacks	2.046	-0.081	-0.014
	(9.837)	(0.058)	(0.010)
$\Delta^{2002} \times$ Paramilitary attacks	-100.954*	-0.419*	-0.066*
	(58.624)	(0.233)	(0.036)
R^2	0.04	0.10	0.09
Panel B: Aerial eradication, controlling for baseline coca cultivation (2002-2006)			
Δ^{2002}	109.149**	0.413**	0.058*
	(50.396)	(0.207)	(0.033)
Paramilitary attacks	4.861	-0.069	-0.013
	(8.839)	(0.052)	(0.009)
$\Delta^{2002} \times$ Paramilitary attacks	-107.950*	-0.450**	-0.071**
	(57.449)	(0.219)	(0.034)
R^2	0.05	0.12	0.10
Panel C: Aerial eradication (2006-2010)			
Δ^{2006}	-59.238	-0.470	-0.087
	(42.957)	(0.336)	(0.058)
Paramilitary attacks	-12.781	-0.184*	-0.036**
	(10.110)	(0.095)	(0.017)
$\Delta^{2006} \times$ Paramilitary attacks	-1.333	-0.113	-0.018
	(37.945)	(0.427)	(0.079)
R^2	0.05	0.10	0.10
Panel D: Aerial eradication, controlling for baseline coca cultivation (2006-2010)			
Δ^{2006}	-58.201	-0.465	-0.086
	(41.094)	(0.327)	(0.057)
Paramilitary attacks	-15.618	-0.199**	-0.038**
	(9.471)	(0.087)	(0.016)
$\Delta^{2006} \times$ Paramilitary attacks	33.891	0.072	0.009
	(32.391)	(0.390)	(0.074)
R^2	0.07	0.11	0.10
Observations	13,680	13,680	13,680
Municipalities	285	285	285
Outcome (2002-2006) range	[0-17,100.7]	[0-9.75]	{0,1}
Outcome (2002-2006) mean	36.35	0.29	0.05
Outcome (2002-2006) std. dev	329.54	1.28	0.22
Outcome (2006-2010) range	[0-7,131.31]	[0-8.87]	{0,1}
Outcome (2006-2010) mean	34.53	0.4	0.08
Outcome (2006-2010) std. dev	219.57	1.47	0.26

Notes: All specifications are estimated using OLS and include department and year $\times$ month fixed effects. Δ^{2002} ranges from -0.79 to 0.74 with a mean of -0.01 and a std. dev. of 0.24. Δ^{2006} ranges from -0.37 to 0.86 with a mean of 0.16 and a std. dev. of 0.19. Robust standard errors clustered by municipality are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6.2 Electoral violence

I next use electoral violence, defined as reports of threats to use armed violence against voters to support a particular candidate, as a way to assess the relationship between paramilitary influence on elections and subsequent eradication behavior by the central government. Electoral violence data is sourced from the Electoral Observation Mission (*Misión de Observación Electoral*, MOE), a Colombian NGO (Nieto-Matiz, 2019).

If a government is incentivized to hold back on eradication to favorable armed groups because aligned armed groups help the incumbent in elections, then electoral violence by favorable groups should also be associated with forbearance. Unfortunately, the electoral violence data is not disaggregated by the armed actor who committed the electoral violence. However, prior to the 2002 election, paramilitaries were at their highest strength, having signed a pact to support Uribe's candidacy with the goal of the government taking a hardline stance against the guerrillas, and paramilitaries committed most of the instances of electoral violence in this election (Acemoglu, Robinson and Santos, 2013). To assess the relationship between electoral violence and eradication, I fit:

$$Eradication_{i,t} = \beta_1 \text{Electoral Violence}_i + \mathbf{X}_i + \zeta_d + \delta_t + \varepsilon_{i,t}, \tag{3}$$

where historical paramilitary and guerrilla violence is included in the vector of controls $\mathbf{X}_i$ described in Equation 2, with results presented in Table 6. I do not interact electoral violence with the historical time-invariant measures of paramilitary presence because, as a measure of short-term violence, electoral violence is unlikely to reflect consolidated influence (Kalyvas, 2006). Municipalities with strong paramilitary influence could experience electoral violence, but strong paramilitary influence could lead to tampering with election results in other, less violent ways, such as ballot stuffing.

Similarly to Table 5, Table 6 shows a negative relationship between electoral violence in 2002 and eradication in the 2002-2006 term—prior to the demobilization of the paramilitaries. This rela-

Table 6: Cross-sectional geographic variation in the intensity and extent of aerial eradication, 2002-2010.

	Hectares (1)	Hectares (ln+1) (2)	Hectares (> 0) (3)
Panel A: Aerial eradication (2002-2006)			
Electoral violence (2002)	-42.064	-0.235**	-0.037***
	(30.472)	(0.092)	(0.014)
R^2	0.04	0.11	0.10
Panel B: Aerial eradication, controlling for baseline coca cultivation (2002-2006)			
Electoral violence (2002)	-38.444	-0.220**	-0.035***
	(29.381)	(0.088)	(0.013)
R^2	0.06	0.13	0.11
Panel C: Aerial eradication (2006-2010)			
Electoral violence (2006)	-5.210	0.008	0.007
	(4.591)	(0.050)	(0.010)
R^2	0.05	0.10	0.10
Panel D: Aerial eradication, controlling for baseline coca cultivation (2006-2010)			
Electoral violence (2006)	-5.021	0.009	0.007
	(4.381)	(0.050)	(0.010)
R^2	0.06	0.11	0.11
Observations	14,208	14,208	14,208
Municipalities	296	296	296
Outcome (2002-2006) range	[0-17,100.7]	[0-9.75]	{0,1}
Outcome (2002-2006) mean	40.37	0.31	0.05
Outcome (2002-2006) std. dev	339.22	1.34	0.23
Outcome (2006-2010) range	[0-7,131.31]	[0-8.87]	{0,1}
Outcome (2006-2010) mean	36.39	0.42	0.08
Outcome (2006-2010) std. dev	223.87	1.50	0.27

Notes: All specifications are estimated using OLS and include department and year × month fixed effects. Electoral violence (2002) ranges from {0-3} with a mean of 0.03 and a std. dev. of 0.24. Electoral violence (2006) ranges from {0-4} with a mean of 0.07 and a std. dev. of 0.37. Robust standard errors clustered by municipality are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

tionship is attenuated when using electoral violence in 2006—after paramilitary demobilization—to predict eradication from 2006-2010.

7 Conclusion

This paper shows that political incentives influenced the spatial allocation of law enforcement in Colombia. In the wake of extensive foreign aid investments motivated by a desire to reduce coca cultivation, aerial eradication was not implemented apolitically. Using the historical presence of armed groups to generate variation in political incentives to enforce, I show that areas with historically high paramilitary violence experienced differentially less eradication. By contrast, areas with historically high guerrilla violence experienced more eradication. Differences in political alignment explain this differential treatment since Uribe benefited from paramilitary help and built his political image around fighting the guerrillas. The national government used counternarcotics policies to reward aligned armed groups and punish opposition armed groups since the presence of non-state armed actors as electoral brokers reduces the need for the government to cater to voters.

Retrospective vote share considerations partially drive this relationship: during Uribe's first term, there was less eradication in areas with high paramilitary presence where he overperformed relative to expectations; after the official demobilization of the paramilitaries, these relationships are attenuated. In a similar vein, the relationship between electoral violence in the previous election and subsequent eradication is negative in 2002—prior to paramilitary demobilization—and small in 2006—after paramilitary demobilization. These results suggest that the preferential treatment of paramilitaries was partly motivated by their capacity to influence electoral outcomes. The findings help shape our understanding of state-building, development, electoral accountability, peace-building, and the rule of law. State consolidation reflects not only capacity constraints but also willingness: the persistence of armed groups that challenge the monopoly over the use of force can be electorally beneficial.

Colombia is a particular case in many ways given its powerful armed groups with programmatic platforms and ties to national politicians. Despite this, the broader theory should also apply across counternarcotics—and other nationally-driven forms of law enforcement—in different countries. México is an interesting contrasting case: enforcement strategies are decentralized across different levels of government, making accountability for counternarcotics difficult. At the same time, its criminal actors are strong but more focused on state capture than programmatic platforms. Given that eradication demobilizes voters in México (Torreblanca, 2023), national-level enforcement strategies should reflect strategic demobilization, with the additional complications implied by local-federal dynamics (Trejo and Ley, 2020).

Future research should also study demand-side approaches. In building the theory, I chose to focus on supply-side approaches because these approaches have historically been dominant in producer countries in the context of the global drug prohibition regime. Demand-side approaches focus on the root causes of drug abuse and addiction in consumer countries through prevention, treatment, and education.[25] Because of this, demand-side policies display neither the asymmetrical costs and benefits across jurisdictions nor the more apparent clarity of responsibility characteristic of supply-side policies which are necessary conditions for the theory. A different supply-side approach I scope out of the theory is crop substitution. Interviews with National Integrated Illicit Crop Substitution Program (*Programa Nacional Integral de Sustitucion de Cultivos Ilicitos*) officials confirmed that there is little coordination across forced eradication and substitution since they are implemented by different agencies with very different membership and mandates. Nevertheless, the politics of alternative approaches—and the reasons why they have historically been eschewed—are worthy of future study.

The Colombian Constitution enshrines equal protection under the law as a human right. As with many other countries, the lofty ideals of this document fail to live up to practice. Beyond the normative desirability of equal protection, differential enforcement can lead to significant short-term and long-term consequences. In the short run, aerial crop fumigation causes serious health, environmental, and economic damage. Counternarcotics enforcement can also decrease govern-

[25] Over time, as the use of drugs becomes less stigmatized, the incentives that push governments to conduct harsh enforcement strategies may also lessen, but this falls beyond the scope of this paper.

ment trust (Torreblanca, 2023). In the long run, this lack of confidence in institutions and the persistence of non-state armed groups that influence elections can break typical citizen-politician linkages (Kitschelt, 2000; Stokes, Dunning and Nazareno, 2013), and the state might find itself unable to eliminate these threats to its monopoly of violence once the armed groups are no longer electorally useful (Hidalgo and Lessing, 2014). Politicians influenced by armed groups may spend less on public goods and social programs in favor of security (Daly, 2022*a*; Nieto-Matiz, 2023). For a region that already experiences the most criminal violence in the world, forbearance in enforcement can propagate conflict, violence, and development traps that will further lag social, political, and economic progress in Latin America.

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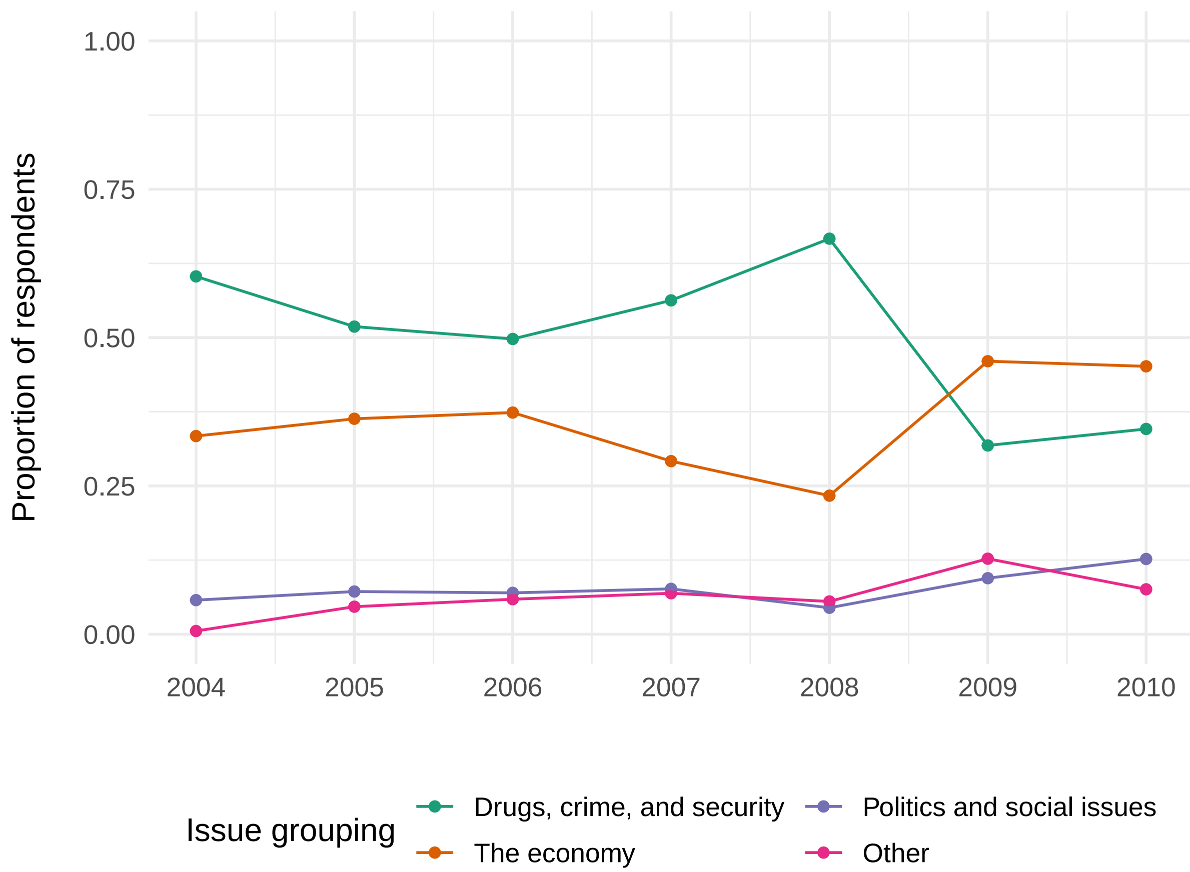
Appendix

Contents

A.1 Public opinion toward security and drugs

The AmericasBarometer question asks respondents what they perceive to be the country's most important problem. Survey enumerators classify their open-ended responses into one of many dozen categories. I group these issues into a number of smaller categories. I classify problems such as inflation, unemployment, and poverty as economic issues; protests, corruption, and problems with service provision as political issues; and issues related to drug trafficking, the armed conflict, and crime are security-related. While the armed conflict might appear to be more all-encompassing than issues of drugs themselves, it is impossible to disentangle the armed conflict from drug trafficking given the involvement of armed groups in the drug trade in Colombia.

Figure A1: Proportion of respondents who indicated an issue falling into the issue grouping as the most important problem facing the country, 2004-2010.

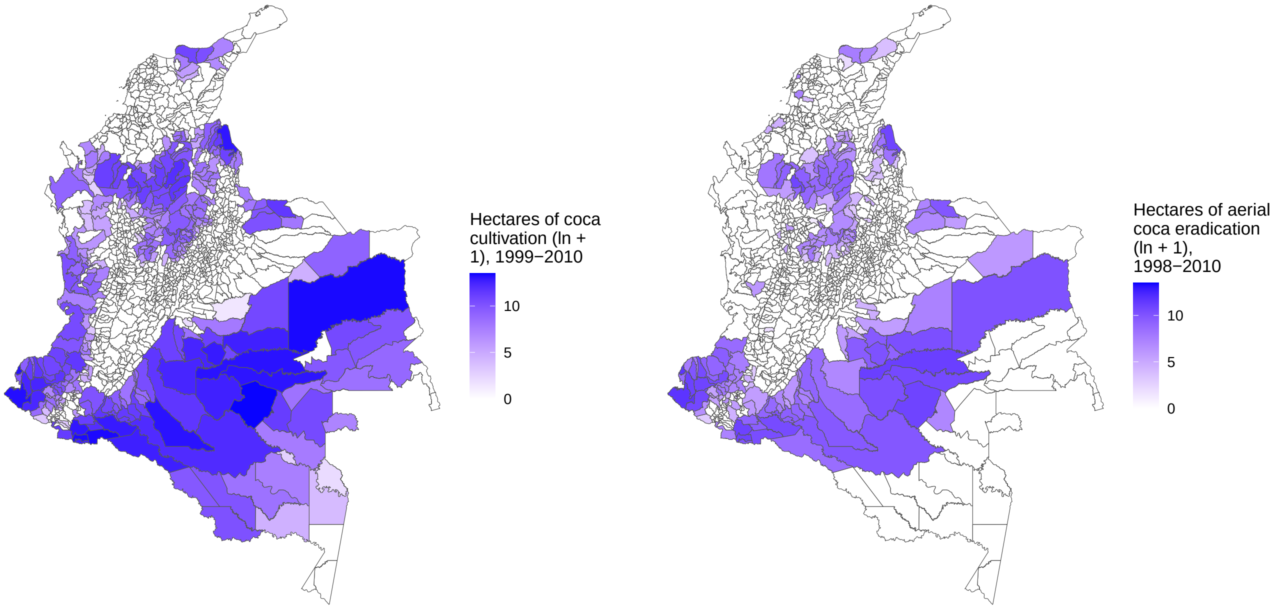


A.2 Detailed description of coca cultivation data

SIMCI's estimations correspond to the nominal date of December 31st—however, the inputs are collected several months around this date, usually between November and February. The measures across years correspond to *net* changes in coca cultivation. Consider, for example, a report of 1,000 hectares of coca crops detected in a particular municipality in year t. In the following year, $t+1$, 500 hectares could be eradicated, but another 1,000 hectares of coca may be planted. The estimated coca cultivation for that municipality in year $t+1$ is thus 1,500 hectares, even though there may have been as many as 2,000 hectares of crops in that municipality at one point. Similarly, it is possible for, say, 1,000 hectares to have been detected in year t and *more than* 1,000 hectares to have been eradicated during year $t+1$, since new cultivation areas can appear during the year. Coca takes approximately 6 months to go from initial planting to initial harvest. Subsequent harvests can occur around every 3 months after the initial harvest. Coca is a crop resilient to eradication; even after fumigation, coca cultivation areas can regrow the crop in a time frame ranging from 6 to 12 months.

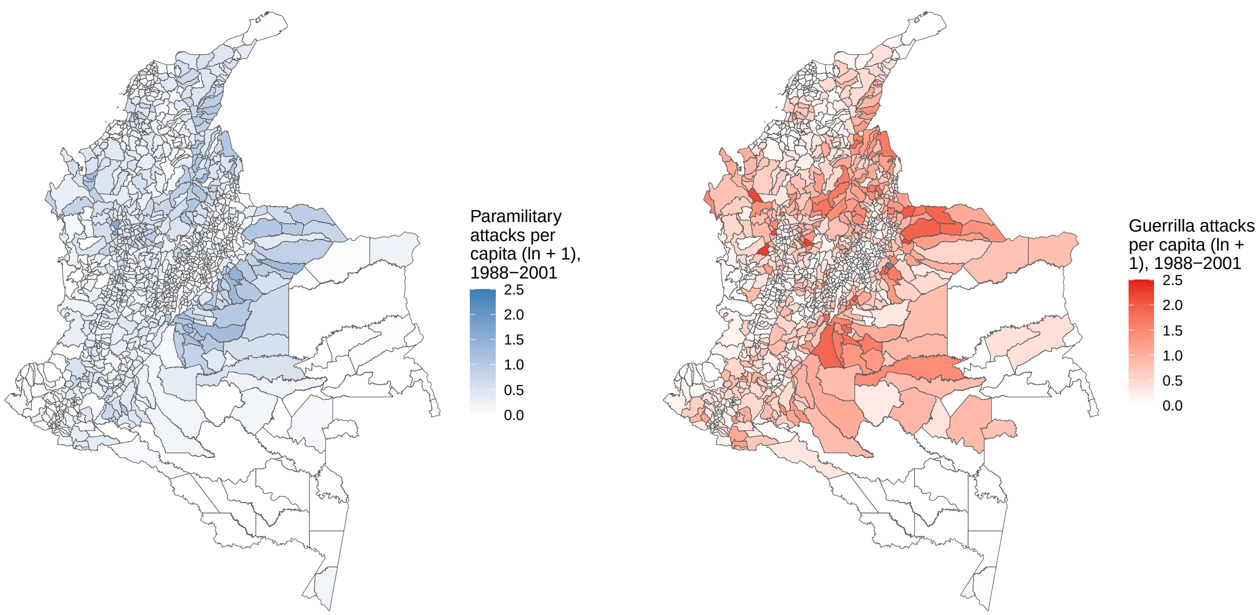
A.3 Variation in coca cultivation and eradication

Figure A2: Coca cultivation and eradication.



A.4 Variation in historical armed group presence

Figure A3: Paramilitary and guerrilla attacks per capita from 1988-2001.



A.5 Difference-in-differences validation checks

A.5.1 Testing parallel trends

To test parallel trends, I fit a model similar to the one described by Equation 1, except I subset the sample to the year-months of Pastrana's term only—thereby representing pre-treatment year-months. I also replace the indicators for Uribe's two presidential terms with placebo treatment indicators for July 2001-July 2002 (Panel A) and July 2000-July 2002 (Panel B).

Table A1: Formal test of parallel trends, using continuous measures of historical armed group violence.

	Hectares	Hectares (ln +1)	Hectares (> 0)
	(1)	(2)	(3)
Panel A: Aerial eradication, July 2001 placebo			
Paramilitary attacks × 2001-2002	-7.338	-0.009	0.001
	(6.095)	(0.036)	(0.007)
Guerrilla attacks × 2001-2002	3.021	0.007	0.000
	(2.572)	(0.014)	(0.003)
R^2	0.10	0.28	0.29
Panel B: Aerial eradication, July 2000 placebo			
Paramilitary attacks × 2000-2002	-4.924	0.017	0.004
	(7.335)	(0.039)	(0.007)
Guerrilla attacks × 2000-2002	6.046*	0.013	0.001
	(3.602)	(0.017)	(0.003)
R^2	0.10	0.28	0.29
Observations	15,264	15,264	15,264
Municipalities	318	318	318
Outcome range	[0-9,649.8]	[0-9.17]	{0,1}
Outcome mean	17.9	0.16	0.03
Outcome std. dev	215.45	0.95	0.17
Paramilitary attacks range	[0-2.95]	[0-2.95]	[0-2.95]
Paramilitary attacks mean	0.50	0.50	0.50
Paramilitary attacks std. dev	0.55	0.55	0.55
Guerrilla attacks range	[0-8.39]	[0-8.39]	[0-8.39]
Guerrilla attacks mean	1.15	1.15	1.15
Guerrilla attacks std. dev	1.36	1.36	1.36

Notes: All specifications are estimated using OLS and include municipality and year × month fixed effects. Robust standard errors clustered by municipality are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Formal test of parallel trends, using binary measures of historical armed group violence.

	Hectares (1)	Hectares (ln +1) (2)	Hectares (> 0) (3)
Panel A: Aerial eradication, July 2001 placebo			
Paramilitary attacks × 2001-2002	-11.016	-0.062	-0.009
	(8.354)	(0.055)	(0.010)
Guerrilla attacks × 2001-2002	11.620	0.049	0.006
	(9.650)	(0.062)	(0.011)
R^2	0.10	0.28	0.29
Panel B: Aerial eradication, July 2000 placebo			
Paramilitary attacks × 2000-2002	-16.232	-0.038	-0.006
	(13.500)	(0.064)	(0.012)
Guerrilla attacks × 2000-2002	23.066	0.047	0.005
	(15.687)	(0.070)	(0.012)
R^2	0.10	0.28	0.29
Observations	15,264	15,264	15,264
Municipalities	318	318	318
Outcome range	[0-9,649.8]	[0-9.17]	{0,1}
Outcome mean	17.9	0.16	0.03
Outcome std. dev	215.45	0.95	0.17
Paramilitary attacks range	{0,1}	{0,1}	{0,1}
Paramilitary attacks mean	0.35	0.35	0.35
Paramilitary attacks std. dev	0.48	0.48	0.48
Guerrilla attacks range	{0,1}	{0,1}	{0,1}
Guerrilla attacks mean	0.40	0.40	0.40
Guerrilla attacks std. dev	0.49	0.49	0.49

Notes: All specifications are estimated using OLS and include municipality and year × month fixed effects. Robust standard errors clustered by municipality are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.5.2 Event study

To generate the event study plots I fit regressions of the following form:

$$Eradication_{i,t} = \sum_{j \neq 2002} \beta_j P_i \times \mathbb{1}[y = j] + \sum_{j \neq 2002} \zeta_j G_i \times \mathbb{1}[y = j] + \gamma_i + \delta_t + \varepsilon_{i,t} \tag{4}$$

where $Eradication_{i,t}$ represents eradication in municipality i in year-month t, and P_i and G_i are variables for historical paramilitary and guerrilla violence. Unlike Equation 1 I do not interact the variables for historical paramilitary and guerrilla violence P_i and G_i with indicators for the beginning of each of Uribe's administrations. Instead, I interact P_i and G_i with indicators for each year $y \in 1998, 1999, \ldots, 2010$ except for 2002, which is the reference category. I also include municipality fixed effects γ_i and year $\times$ month fixed effects δ_t. I expect β_j—the coefficients associated with the interaction of the year indicators and historical paramilitary violence—to be negative after 2002, reflecting forbearance in eradication, and ζ_j—the coefficients associated with the interaction of the year indicators and historical guerrilla violence—to be positive after 2002.

Figures A4 and A5 present the results of these regressions separately for aerial eradication outcomes measured in hectares, $\ln + 1$ hectares, and as a binary indicator. Figure A4 uses continuous measures of historical armed group violence while Figure A5 uses municipalities in the top quartile of violence as a binary measure. I plot estimates of β_j and ζ_j as defined in Equation 4 and 95% confidence intervals.

Figure A4: Event study plots, using continuous measures of historical armed group violence.

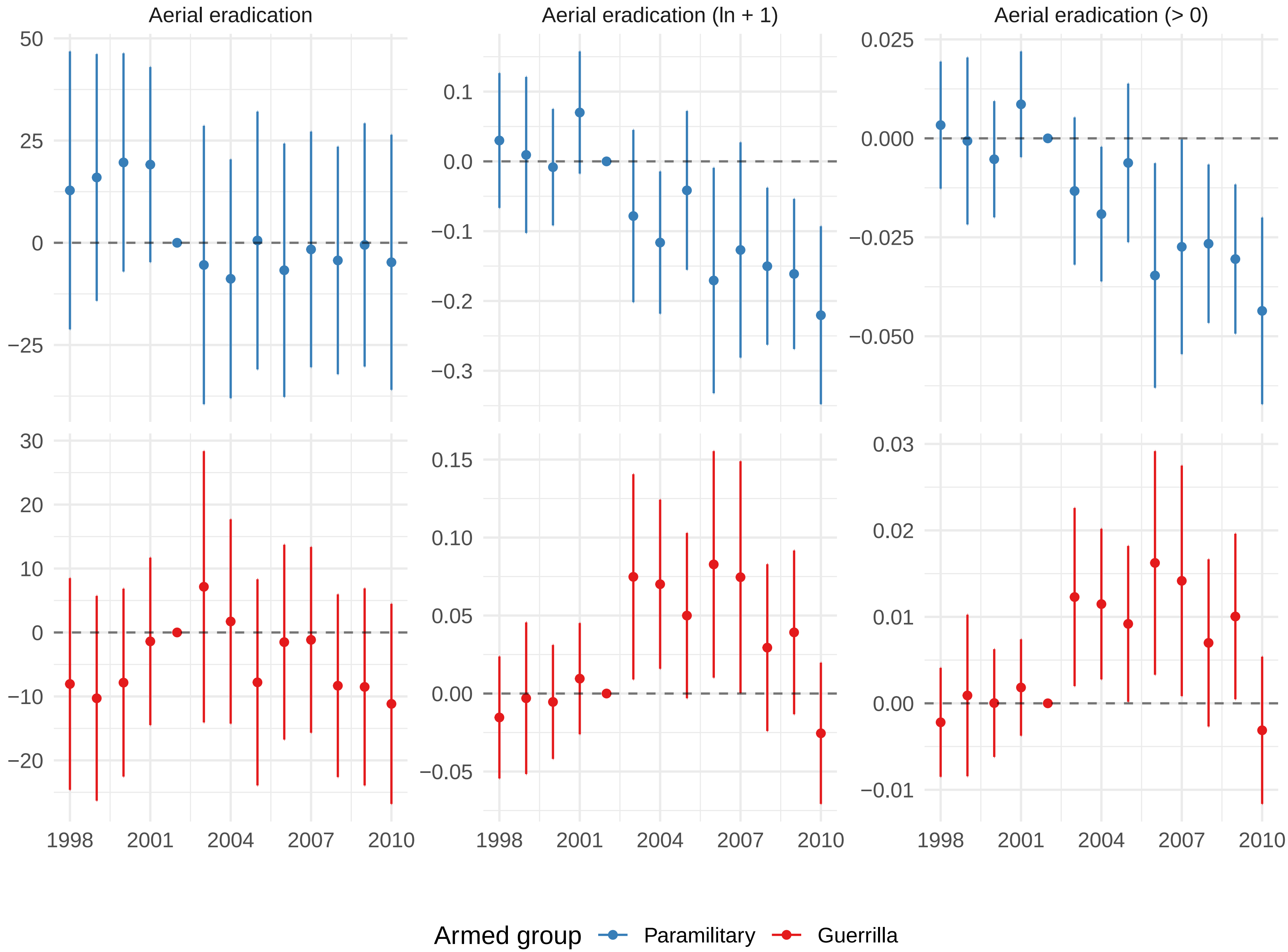
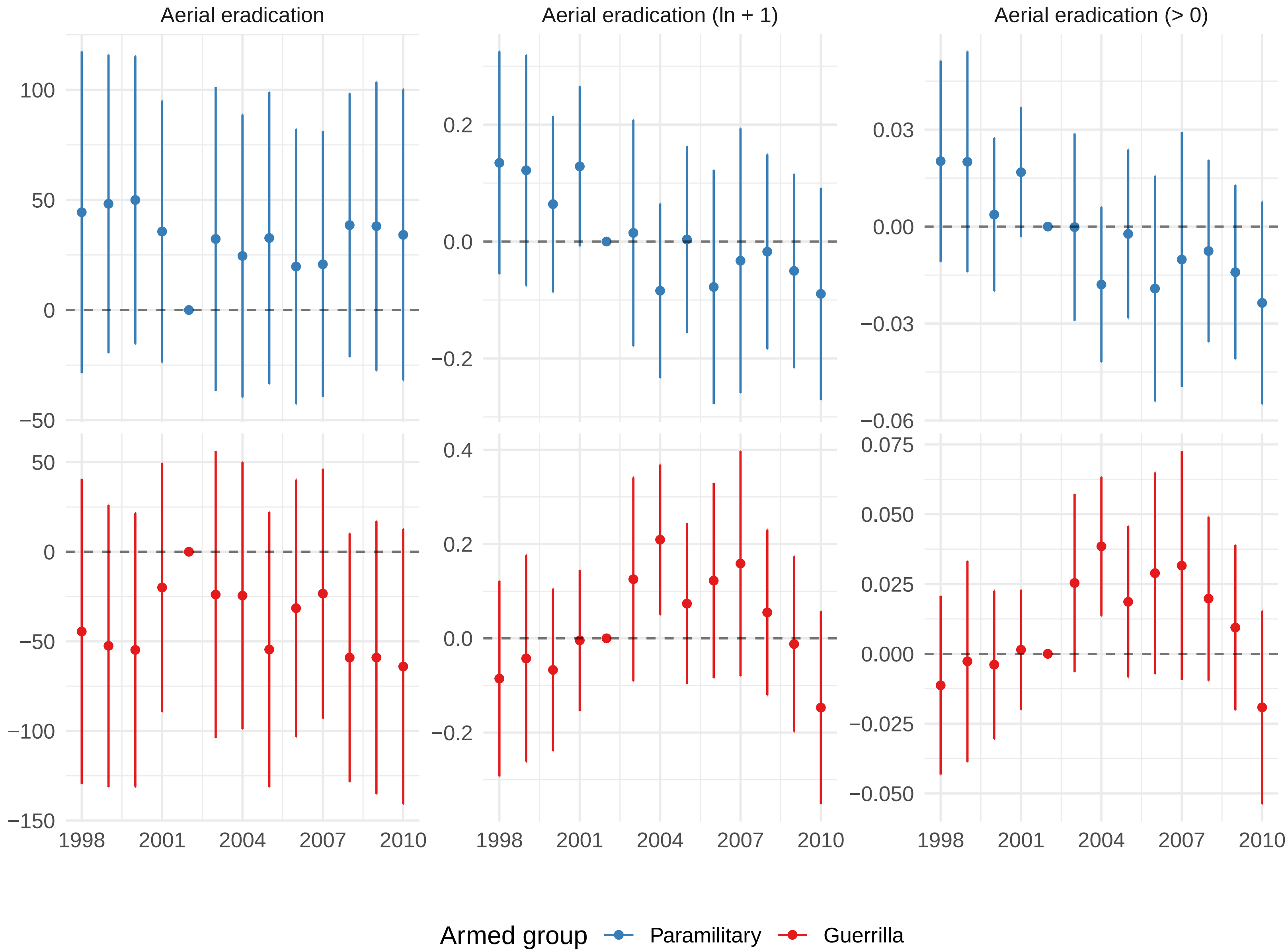


Figure A5: Event study plots, using binary measures of historical armed group violence.



A.6 Robustness checks

The following set of tables present the results of the robustness tests described in Section 5. Tables A3 and A4 use coca cultivation values from $t - 1$ as controls. Table A5 uses as an outcome the proportion of yearly coca cultivated that was aerially eradicated. Tables A6 and A7 use violence data that is always prior to the beginning of Pastrana's term even though it excludes the crucial 1997-2001 period of paramilitary ascendancy (Ch et al., 2018). Table A8 acknowledges the different sources of violence data from the Colombian armed conflict (Osorio, 2023) and uses an inverse-covariance weighted index of three different sources of attacks data: (1) the data from Restrepo, Spagat and Vargas (2003) used in the main results, (2) violence data from the National Center for Historical Memory (Centro de Memoria Histórica), which is a national agency created for truth-seeking and reconciliation related to the Colombian armed conflict (Grupo de Memoria Histórica, 2012), and (3) the VIPAA, or Violent Presence of Armed Actors in Colombia (Osorio et al., 2019), which uses computerized text annotation to classify violent events. Table A9 uses the natural log of attacks to account for the right-skewness of this variable's distribution, adding a value of one such that violence values in municipalities that did not experience violence are well-defined. These results are estimated more precisely than the main results and continue to support H1. Figures A6 and A7 probe the sensitivity of the main binary results based on the cutoff for municipalities with high levels of historical paramilitary or guerrilla violence. Tables A10 and A11 flexibly interact the year-month fixed effects with municipality area, coca suitability, altitude, and distance to Bogotá, as well as measures of the latent right/left lean of the municipality. Tables A12 and A13 add department by year-month fixed effects to Equation 1.

A.6.1 Accounting for lagged coca cultivation

Table A3: Temporal and geographic variation in the intensity and extent of aerial eradication controlling for lagged coca cultivation, using continuous measures of historical armed group violence.

	Hectares (1)	Hectares (ln+1) (2)	Hectares (> 0) (3)
Outcome: aerial eradication			
Paramilitary attacks × 2002-2006	-32.397***	-0.137***	-0.020***
	(8.963)	(0.044)	(0.007)
Paramilitary attacks × 2006-2010	-28.949***	-0.197***	-0.034***
	(8.020)	(0.054)	(0.010)
Guerrilla attacks × 2002-2006	8.448*	0.066***	0.011***
	(4.456)	(0.021)	(0.003)
Guerrilla attacks × 2006-2010	4.734*	0.057**	0.011**
	(2.830)	(0.024)	(0.004)
R^2	0.14	0.23	0.22
Observations	40,386	40,386	40,386
Municipalities	318	318	318
Outcome range	[0-17,100.7]	[0-9.75]	{0,1}
Outcome mean	32.68	0.3	0.06
Outcome std. dev	272.57	1.3	0.23
Paramilitary attacks range	[0-2.95]	[0-2.95]	[0-2.95]
Paramilitary attacks mean	0.50	0.50	0.50
Paramilitary attacks std. dev	0.55	0.55	0.55
Guerrilla attacks range	[0-8.39]	[0-8.39]	[0-8.39]
Guerrilla attacks mean	1.15	1.15	1.15
Guerrilla attacks std. dev	1.36	1.36	1.36

Notes: All specifications are estimated using OLS and include municipality and year × month fixed effects. Baseline category is Pastrana's term from 1998-2002. Robust standard errors clustered by municipality are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Temporal and geographic variation in the intensity and extent of aerial eradication controlling for lagged coca cultivation, using binary measures of historical armed group violence.

	Hectares (1)	Hectares (ln+1) (2)	Hectares (> 0) (3)
Outcome: aerial eradication			
Paramilitary attacks × 2002-2006	-43.105***	-0.174***	-0.025***
	(14.688)	(0.052)	(0.008)
Paramilitary attacks × 2006-2010	-37.845***	-0.203***	-0.033***
	(11.217)	(0.071)	(0.013)
Guerrilla attacks × 2002-2006	35.197**	0.200***	0.031***
	(15.715)	(0.057)	(0.009)
Guerrilla attacks × 2006-2010	30.189***	0.207***	0.036***
	(10.931)	(0.076)	(0.013)
R^2	0.14	0.23	0.22
Observations	40,386	40,386	40,386
Municipalities	318	318	318
Outcome range	[0-17,100.7]	[0-9.75]	{0,1}
Outcome mean	32.68	0.3	0.06
Outcome std. dev	272.57	1.3	0.23
Paramilitary attacks range	{0,1}	{0,1}	{0,1}
Paramilitary attacks mean	0.35	0.35	0.35
Paramilitary attacks std. dev	0.48	0.48	0.48
Guerrilla attacks range	{0,1}	{0,1}	{0,1}
Guerrilla attacks mean	0.4	0.4	0.4
Guerrilla attacks std. dev	0.49	0.49	0.49

Notes: All specifications are estimated using OLS and include municipality and year × month fixed effects. Baseline category is Pastrana's term from 1998-2002. Robust standard errors clustered by municipality are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.6.2 Accounting for proportion of detected coca eradicated

Table A5: Temporal and geographic variation in the proportion of yearly hectares aerially eradicated.

Outcome: proportion of cultivated coca hectares aerially eradicated				
	Continuous measure		Binary measure	
	(1)	(2)	(3)	(4)
Paramilitary attacks × 2003-2006	-0.066**		-0.032	
	(0.028)		(0.033)	
Paramilitary attacks × 2007-2010	-0.070**		-0.044	
	(0.027)		(0.034)	
Guerrilla attacks × 2003-2006	0.044***		0.091***	
	(0.015)		(0.033)	
Guerrilla attacks × 2007-2010	0.027**		0.072**	
	(0.012)		(0.035)	
Paramilitary attacks × 2002-2005		-0.053**		-0.038
		(0.024)		(0.031)
Paramilitary attacks × 2006-2009		-0.087**		-0.057
		(0.034)		(0.040)
Guerrilla attacks × 2002-2005		0.033**		0.079**
		(0.013)		(0.031)
Guerrilla attacks × 2006-2009		0.042**		0.097**
		(0.016)		(0.040)
R^2	0.55	0.56	0.55	0.56
Observations	3,498	3,180	3,498	3,180
Municipalities	318	318	318	318
Outcome range	[0-1]	[0-1]	[0-1]	[0-1]
Outcome mean	0.19	0.19	0.19	0.19
Outcome std. dev	0.37	0.37	0.37	0.37
Paramilitary attacks range	[0-2.95]	[0-2.95]	{0,1}	{0,1}
Paramilitary attacks mean	0.5	0.5	0.35	0.35
Paramilitary attacks std. dev	0.55	0.55	0.48	0.48
Guerrilla attacks range	[0-8.39]	[0-8.39]	{0,1}	{0,1}
Guerrilla attacks mean	1.15	1.15	0.4	0.4
Guerrilla attacks std. dev	1.36	1.36	0.49	0.49

Notes: All specifications are estimated using OLS and include municipality and year fixed effects. Baseline category is Pastrana's term from 1998-2002. Robust standard errors clustered by municipality are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.6.3 Pre-baseline violence data (1988-1997)

Table A6: Temporal and geographic variation in the intensity and extent of aerial eradication using pre-baseline violence data (1988-1997), using continuous measures of historical armed group violence.

	Hectares (1)	Hectares (ln +1) (2)	Hectares (> 0) (3)
Panel A: Aerial eradication			
Paramilitary attacks × 2002-2006	-13.389**	-0.100**	-0.016**
	(5.492)	(0.045)	(0.008)
Paramilitary attacks × 2006-2010	-9.315*	-0.131***	-0.025***
	(5.383)	(0.050)	(0.009)
Guerrilla attacks × 2002-2006	3.198	0.040**	0.007**
	(3.923)	(0.019)	(0.003)
Guerrilla attacks × 2006-2010	-1.079	0.029	0.006
	(3.401)	(0.024)	(0.004)
R^2	0.12	0.22	0.21
Panel B: Aerial eradication, controlling for baseline coca cultivation			
Paramilitary attacks × 2002-2006	-9.536*	-0.082*	-0.014*
	(5.223)	(0.046)	(0.008)
Paramilitary attacks × 2006-2010	-10.853**	-0.122**	-0.023***
	(4.970)	(0.051)	(0.009)
Guerrilla attacks × 2002-2006	1.659	0.033*	0.006*
	(3.781)	(0.019)	(0.003)
Guerrilla attacks × 2006-2010	-0.464	0.025	0.006
	(2.981)	(0.024)	(0.004)
R^2	0.12	0.22	0.21
Observations	45,792	45,792	45,792
Municipalities	318	318	318
Outcome range	[0-17,100.7]	[0-9.75]	{0,1}
Outcome mean	30.11	0.29	0.05
Outcome std. dev.	258.83	1.27	0.22
Paramilitary attacks range	[0-3.41]	[0-3.41]	[0-3.41]
Paramilitary attacks mean	0.45	0.45	0.45
Paramilitary attacks std. dev.	0.61	0.61	0.61
Guerrilla attacks range	[0-7.6]	[0-7.6]	[0-7.6]
Guerrilla attacks mean	1.06	1.06	1.06
Guerrilla attacks std. dev.	1.39	1.39	1.39

Notes: All specifications are estimated using OLS and include municipality and year × month fixed effects. Baseline category is Pastrana's term from 1998-2002. Robust standard errors clustered by municipality are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Temporal and geographic variation in the intensity and extent of aerial eradication using pre-baseline violence data (1988-1997), using binary measures of historical armed group violence.

	Hectares	Hectares (ln +1)	Hectares (> 0)
	(1)	(2)	(3)
Panel A: Aerial eradication			
Paramilitary attacks × 2002-2006	-22.646**	-0.110**	-0.017*
	(10.283)	(0.053)	(0.009)
Paramilitary attacks × 2006-2010	-17.451**	-0.172**	-0.031***
	(8.189)	(0.067)	(0.012)
Guerrilla attacks × 2002-2006	11.146	0.081	0.013
	(11.650)	(0.058)	(0.010)
Guerrilla attacks × 2006-2010	2.345	0.106	0.022*
	(8.888)	(0.074)	(0.013)
R^2	0.12	0.22	0.21
Panel B: Aerial eradication, controlling for baseline coca cultivation			
Paramilitary attacks × 2002-2006	-9.536*	-0.082*	-0.014*
	(5.223)	(0.046)	(0.008)
Paramilitary attacks × 2006-2010	-10.853**	-0.122**	-0.023***
	(4.970)	(0.051)	(0.009)
Guerrilla attacks × 2002-2006	1.659	0.033*	0.006*
	(3.781)	(0.019)	(0.003)
Guerrilla attacks × 2006-2010	-0.464	0.025	0.006
	(2.981)	(0.024)	(0.004)
R^2	0.12	0.22	0.21
Observations	45,792	45,792	45,792
Municipalities	318	318	318
Outcome range	[0-17,100.7]	[0-9.75]	{0,1}
Outcome mean	30.11	0.29	0.05
Outcome std. dev.	258.83	1.27	0.22
Paramilitary attacks range	{0,1}	{0,1}	{0,1}
Paramilitary attacks mean	0.34	0.34	0.34
Paramilitary attacks std. dev	0.47	0.47	0.47
Guerrilla attacks range	{0,1}	{0,1}	{0,1}
Guerrilla attacks mean	0.39	0.39	0.39
Guerrilla attacks std. dev	0.49	0.49	0.49

Notes: All specifications are estimated using OLS and include municipality and year × month fixed effects. Baseline category is Pastrana's term from 1998-2002. Robust standard errors clustered by municipality are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.6.4 Weighted index of violence data

Table A8: Temporal and geographic variation in the intensity and extent of aerial eradication using an inverse-covariance weighted index of three separate sources of violence data from 1988-2001 (Ch et al., 2018; Osorio et al., 2019; Grupo de Memoria Histórica, 2012).

	Hectares	Hectares (ln+1)	Hectares (> 0)
	(1)	(2)	(3)
Panel A: Aerial eradication			
Paramilitary attacks × 2002-2006	-3.292	-0.026	-0.004
	(4.601)	(0.026)	(0.004)
Paramilitary attacks × 2006-2010	-8.455*	-0.090**	-0.015**
	(4.658)	(0.035)	(0.006)
Guerrilla attacks × 2002-2006	0.670	0.025	0.005
	(3.142)	(0.024)	(0.004)
Guerrilla attacks × 2006-2010	3.591	0.030	0.004
	(3.322)	(0.022)	(0.004)
R^2	0.12	0.22	0.21
Panel B: Aerial eradication, controlling for baseline coca cultivation			
Paramilitary attacks × 2002-2006	-1.178	-0.014	-0.002
	(4.192)	(0.025)	(0.004)
Paramilitary attacks × 2006-2010	-9.233**	-0.084**	-0.014**
	(4.340)	(0.035)	(0.006)
Guerrilla attacks × 2002-2006	1.946	0.032	0.006
	(3.470)	(0.023)	(0.004)
Guerrilla attacks × 2006-2010	3.121	0.033	0.005
	(3.423)	(0.022)	(0.004)
R^2	0.12	0.23	0.21
Observations	45,792	45,792	45,792
Municipalities	318	318	318
Outcome range	[0-17,100.7]	[0-9.75]	{0,1}
Outcome mean	30.11	0.29	0.05
Outcome std. dev.	258.83	1.27	0.22
Paramilitary attacks range	[-3.29-4.87]	[-3.29-4.87]	[-3.29-4.87]
Paramilitary attacks mean	-0.12	-0.12	-0.12
Paramilitary attacks std. dev.	1.08	1.08	1.08
Guerrilla attacks range	[-13.98-6.81]	[-13.98-6.81]	[-13.98-6.81]
Guerrilla attacks mean	-0.17	-0.17	-0.17
Guerrilla attacks std. dev.	2.22	2.22	2.22

Notes: All specifications are estimated using OLS and include municipality and year × month fixed effects. Baseline category is Pastrana's term from 1998-2002. Robust standard errors clustered by municipality are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.6.5 Log-transformed violence data

Table A9: Temporal and geographic variation in the intensity and extent of aerial eradication using ln +1 measures of violence data.

	Hectares (1)	Hectares (ln +1) (2)	Hectares (> 0) (3)
Panel A: Aerial eradication			
Paramilitary attacks × 2002-2006	-43.377***	-0.233***	-0.035**
	(14.018)	(0.086)	(0.014)
Paramilitary attacks × 2006-2010	-33.269**	-0.354***	-0.066***
	(13.545)	(0.112)	(0.020)
Guerrilla attacks × 2002-2006	16.915	0.175***	0.028***
	(11.861)	(0.054)	(0.009)
Guerrilla attacks × 2006-2010	0.030	0.152**	0.032**
	(8.107)	(0.070)	(0.013)
R^2	0.12	0.22	0.21
Panel B: Aerial eradication, controlling for baseline coca cultivation			
Paramilitary attacks × 2002-2006	-33.357**	-0.186**	-0.029**
	(14.076)	(0.084)	(0.014)
Paramilitary attacks × 2006-2010	-37.474**	-0.332***	-0.062***
	(15.157)	(0.111)	(0.020)
Guerrilla attacks × 2002-2006	9.191	0.139**	0.024***
	(11.093)	(0.055)	(0.009)
Guerrilla attacks × 2006-2010	3.271	0.136*	0.030**
	(7.706)	(0.071)	(0.013)
R^2	0.14	0.23	0.22
Observations	45,792	45,792	45,792
Municipalities	318	318	318
Outcome range	[0-17,100.7]	[0-9.75]	{0,1}
Outcome mean	30.11	0.29	0.05
Outcome std. dev.	258.83	1.27	0.22
Paramilitary attacks range	[0-1.37]	[0-1.37]	[0-1.37]
Paramilitary attacks mean	0.35	0.35	0.35
Paramilitary attacks std. dev.	0.32	0.32	0.32
Guerrilla attacks range	[0-2.24]	[0-2.24]	[0-2.24]
Guerrilla attacks mean	0.62	0.62	0.62
Guerrilla attacks std. dev.	0.51	0.51	0.51

Notes: All specifications are estimated using OLS and include municipality and year × month fixed effects. Predictors are based on (Ch et al., 2018) data for paramilitary and guerrilla attacks from 1988-2001 from (Restrepo, Spagat and Vargas, 2003) and updated by the Universidad del Rosario, transformed by ln +1. Baseline category is Pastrana's term from 1998-2002. Robust standard errors clustered by municipality are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.6.6 Using alternative cutoffs for binary measures of historical violence

Figure A6: Sensitivity of the binary main results to different cutoffs for high historical paramilitary and guerrilla violence.

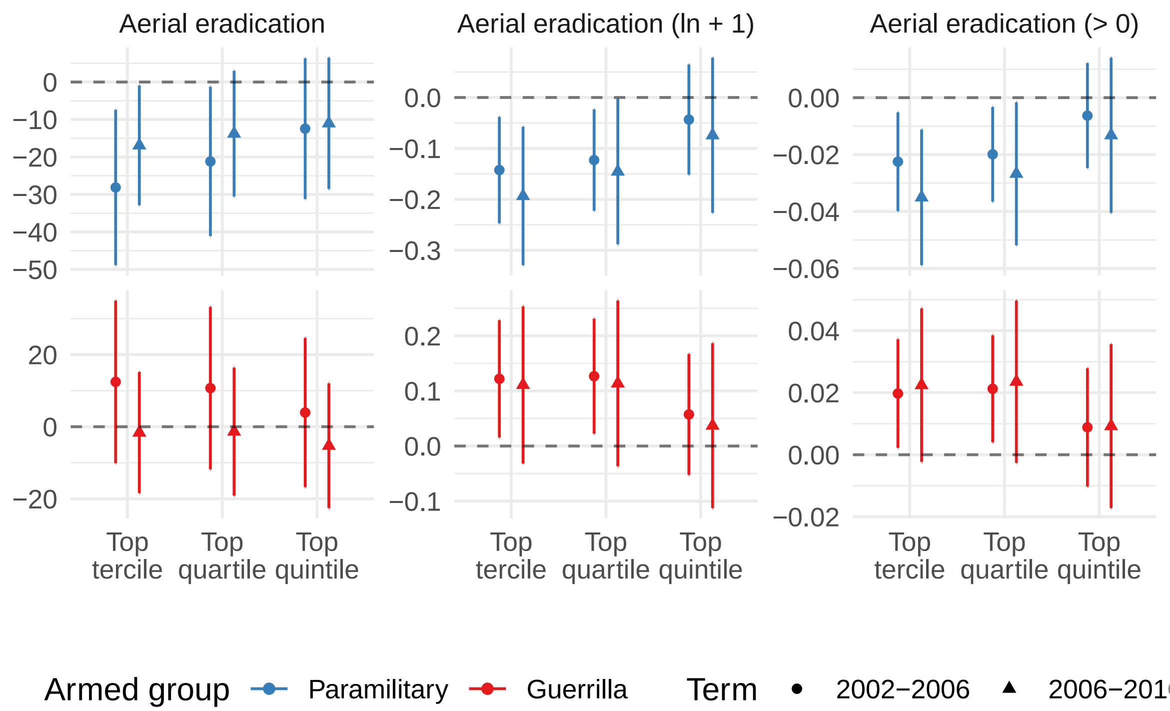
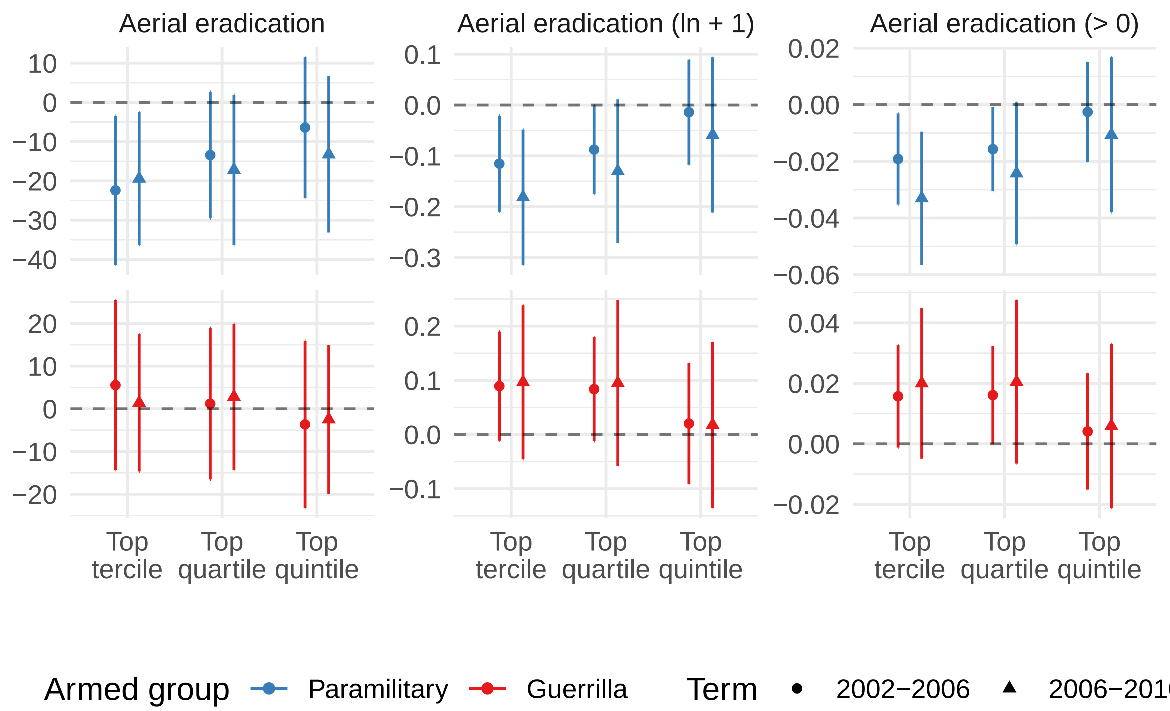


Figure A7: Sensitivity of the binary main results to different cutoffs for high historical paramilitary and guerrilla violence, controlling for baseline coca cultivation.



A.6.7 Additional specifications: flexibly interacting time-invariant characteristics with time

Table A10: Temporal and geographic variation in the intensity and extent of aerial eradication using additional covariates interacted with time, using continuous measures of historical armed group violence.

	Hectares (1)	Hectares (ln+1) (2)	Hectares (> 0) (3)
Panel A: Aerial eradication			
Paramilitary attacks × 2002-2006	-31.235***	-0.147***	-0.022***
	(9.131)	(0.045)	(0.008)
Paramilitary attacks × 2006-2010	-33.515***	-0.305***	-0.054***
	(9.930)	(0.074)	(0.013)
Guerrilla attacks × 2002-2006	1.273	0.034*	0.006**
	(3.105)	(0.019)	(0.003)
Guerrilla attacks × 2006-2010	0.643	0.046*	0.009*
	(3.203)	(0.027)	(0.005)
R^2	0.15	0.26	0.25
Panel B: Aerial eradication, controlling for baseline coca cultivation			
Paramilitary attacks × 2002-2006	-24.800**	-0.125***	-0.019***
	(10.146)	(0.042)	(0.007)
Paramilitary attacks × 2006-2010	-35.069***	-0.294***	-0.052***
	(10.751)	(0.072)	(0.013)
Guerrilla attacks × 2002-2006	0.460	0.031*	0.006*
	(3.011)	(0.018)	(0.003)
Guerrilla attacks × 2006-2010	0.839	0.045*	0.009*
	(2.962)	(0.027)	(0.005)
R^2	0.16	0.26	0.25
Observations	37,584	37,584	37,584
Municipalities	261	261	261
Outcome range	[0-17,100.7]	[0-9.75]	{0,1}
Outcome mean	27.17	0.25	0.05
Outcome std. dev	253.83	1.19	0.21
Paramilitary attacks range	[0-2.95]	[0-2.95]	[0-2.95]
Paramilitary attacks mean	0.56	0.56	0.56
Paramilitary attacks std. dev	0.57	0.57	0.57
Guerrilla attacks range	[0-8.39]	[0-8.39]	[0-8.39]
Guerrilla attacks mean	1.23	1.23	1.23
Guerrilla attacks std. dev	1.36	1.36	1.36

Notes: All specifications are estimated using OLS and include municipality and year × month fixed effects interacted with latent right and left-wing municipality electoral preferences, municipality area, altitude, coca suitability and distance from Bogotá. Baseline category is Pastrana's term from 1998-2002. Robust standard errors clustered by municipality are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A11: Temporal and geographic variation in the intensity and extent of aerial eradication using additional covariates interacted with time, using binary measures of historical armed group violence.

	Hectares (1)	Hectares (ln +1) (2)	Hectares (> 0) (3)
Panel A: Aerial eradication			
Paramilitary attacks × 2002-2006	-32.582***	-0.162***	-0.026***
	(11.462)	(0.052)	(0.009)
Paramilitary attacks × 2006-2010	-24.557**	-0.228***	-0.040***
	(9.861)	(0.082)	(0.014)
Guerrilla attacks × 2002-2006	6.145	0.081	0.016*
	(11.387)	(0.051)	(0.009)
Guerrilla attacks × 2006-2010	0.467	0.096	0.021
	(8.239)	(0.077)	(0.014)
R^2	0.15	0.26	0.24
Panel B: Aerial eradication, controlling for baseline coca cultivation			
Paramilitary attacks × 2002-2006	-21.326**	-0.126***	-0.021***
	(10.306)	(0.044)	(0.008)
Paramilitary attacks × 2006-2010	-27.275**	-0.211***	-0.038***
	(11.273)	(0.080)	(0.014)
Guerrilla attacks × 2002-2006	-3.790	0.049	0.011
	(8.682)	(0.046)	(0.008)
Guerrilla attacks × 2006-2010	2.866	0.081	0.019
	(8.005)	(0.079)	(0.014)
R^2	0.16	0.26	0.25
Observations	37,584	37,584	37,584
Municipalities	261	261	261
Outcome range	[0-17,100.7]	[0-9.75]	{0,1}
Outcome mean	27.17	0.25	0.05
Outcome std. dev	253.83	1.19	0.21
Paramilitary attacks range	{0,1}	{0,1}	{0,1}
Paramilitary attacks mean	0.39	0.39	0.39
Paramilitary attacks std. dev	0.49	0.49	0.49
Guerrilla attacks range	{0,1}	{0,1}	{0,1}
Guerrilla attacks mean	0.44	0.44	0.44
Guerrilla attacks std. dev	0.5	0.5	0.5

Notes: All specifications are estimated using OLS and include municipality and year × month fixed effects interacted with latent right and left-wing municipality electoral preferences, municipality area, altitude, coca suitability and distance from Bogotá. Baseline category is Pastrana's term from 1998-2002. Robust standard errors clustered by municipality are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A12: Temporal and geographic variation in the intensity and extent of aerial eradication using department by year-month fixed effects, using continuous measures of historical armed group violence.

	Hectares (1)	Hectares (ln+1) (2)	Hectares (> 0) (3)
Panel A: Aerial eradication			
Paramilitary attacks × 2002-2006	-12.150*	-0.073	-0.011
	(7.019)	(0.053)	(0.009)
Paramilitary attacks × 2006-2010	-18.908*	-0.148**	-0.028**
	(9.735)	(0.071)	(0.013)
Guerrilla attacks × 2002-2006	1.609	0.029	0.005
	(2.664)	(0.018)	(0.003)
Guerrilla attacks × 2006-2010	0.054	0.034	0.007
	(2.824)	(0.026)	(0.005)
R^2	0.26	0.43	0.42
Panel B: Aerial eradication, controlling for baseline coca cultivation			
Paramilitary attacks × 2002-2006	-11.215	-0.066	-0.010
	(8.003)	(0.053)	(0.009)
Paramilitary attacks × 2006-2010	-19.473*	-0.144**	-0.027**
	(10.217)	(0.071)	(0.013)
Guerrilla attacks × 2002-2006	0.902	0.024	0.004
	(2.640)	(0.017)	(0.003)
Guerrilla attacks × 2006-2010	0.481	0.030	0.006
	(2.513)	(0.026)	(0.005)
R^2	0.26	0.43	0.42
Observations	45,792	45,792	45,792
Municipalities	318	318	318
Outcome range	[0-17,100.7]	[0-9.75]	{0,1}
Outcome mean	30.11	0.29	0.05
Outcome std. dev.	258.83	1.27	0.22
Paramilitary attacks range	[0-2.95]	[0-2.95]	[0-2.95]
Paramilitary attacks mean	0.50	0.50	0.50
Paramilitary attacks std. dev.	0.55	0.55	0.55
Guerrilla attacks range	[0-8.39]	[0-8.39]	[0-8.39]
Guerrilla attacks mean	1.15	1.15	1.15
Guerrilla attacks std. dev.	1.36	1.36	1.36

Notes: All specifications are estimated using OLS and include municipality and department × year × month fixed effects. Baseline category is Pastrana's term from 1998-2002. Robust standard errors clustered by municipality are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A13: Temporal and geographic variation in the intensity and extent of aerial eradication using department by year-month fixed effects, using binary measures of historical armed group violence.

	Hectares (1)	Hectares (ln+1) (2)	Hectares (> 0) (3)
Panel A: Aerial eradication			
Paramilitary attacks × 2002-2006	-11.005	-0.100*	-0.018*
	(8.503)	(0.055)	(0.009)
Paramilitary attacks × 2006-2010	-11.234	-0.113	-0.021
	(10.150)	(0.085)	(0.015)
Guerrilla attacks × 2002-2006	7.710	0.107*	0.020*
	(9.398)	(0.058)	(0.010)
Guerrilla attacks × 2006-2010	7.409	0.162**	0.030**
	(8.455)	(0.078)	(0.014)
R^2	0.26	0.43	0.42
Panel B: Aerial eradication, controlling for baseline coca cultivation			
Paramilitary attacks × 2002-2006	-7.833	-0.078	-0.015
	(8.684)	(0.054)	(0.009)
Paramilitary attacks × 2006-2010	-13.745	-0.102	-0.020
	(12.009)	(0.086)	(0.015)
Guerrilla attacks × 2002-2006	4.568	0.085	0.016
	(8.345)	(0.057)	(0.010)
Guerrilla attacks × 2006-2010	9.897	0.152*	0.029**
	(9.163)	(0.080)	(0.014)
R^2	0.26	0.43	0.42
Observations	45,792	45,792	45,792
Municipalities	318	318	318
Outcome range	[0-17,100.7]	[0-9.75]	{0,1}
Outcome mean	30.11	0.29	0.05
Outcome std. dev.	258.83	1.27	0.22
Paramilitary attacks range	{0,1}	{0,1}	{0,1}
Paramilitary attacks mean	0.35	0.35	0.35
Paramilitary attacks std. dev	0.48	0.48	0.48
Guerrilla attacks range	{0,1}	{0,1}	{0,1}
Guerrilla attacks mean	0.40	0.40	0.40
Guerrilla attacks std. dev	0.49	0.49	0.49

Notes: All specifications are estimated using OLS and include municipality and department × year × month fixed effects. Baseline category is Pastrana's term from 1998-2002. Robust standard errors clustered by municipality are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.7 Fergusson et al. (2021) replication with eradication as outcome

I extend the results of Fergusson et al. (2021), who use a regression discontinuity design around close mayoral elections in Colombia. The authors municipalities which were barely won by a left-wing mayor to those where the left-wing mayor barely lost (or municipalities which were barely won by a right-wing mayor to those where the right-wing mayor barely lost). The authors find that paramilitary violence increases in the wake of an election of left-wing mayor. Instead of using armed group violence as an outcome, I use the same design to predict average eradication undertaken by the central government over the course of the mayor's term. Only the 2003 and 2007 mayoral elections are included.

Table A14: Regression discontinuity design results for the election of right-wing and left-wing mayors on eradcation.

	Avg. yearly aerial eradication hectares (1)	Avg. yearly aerial eradication hectares (ln+1) (2)	Avg. yearly aerial eradication hectares (> 0) (3)
Panel A: Right-wing mayor			
Mayor elected	2.952	0.188	0.049
	(18.430)	(0.406)	(0.087)
Observations	187, 172	187, 172	187, 172
Effective obs.	93, 86	102, 93	95, 88
Bandwidth	0.067, 0.067	0.076, 0.076	0.07, 0.07
Panel B: Left-wing mayor			
Mayor elected	414.558	-0.532	-0.175*
	(577.374)	(1.062)	(0.141)
Observations	41, 44	41, 44	41, 44
Effective obs.	13, 21	12, 20	10, 20
Bandwidth	0.06, 0.06	0.055, 0.055	0.049, 0.049

Notes: Classification of left- and right-wing mayors made by Fergusson et al. (2021). Standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.8 Mechanism test using Uribe's vote instead of overperformance

Table A15: Cross-sectional geographic variation in the intensity and extent of aerial eradication based on Uribe's vote share, 2002-2010, using continuous measures of historical armed group violence.

	Hectares (1)	Hectares (ln+1) (2)	Hectares (> 0) (3)
Panel A: Aerial eradication (2002-2006)			
Vote share Uribe (2002)	-100.528**	-0.553**	-0.090**
	(42.459)	(0.236)	(0.039)
Paramilitary attacks	-55.672**	-0.471***	-0.081***
	(27.224)	(0.163)	(0.027)
Vote share Uribe (2002) × Paramilitary attacks	113.086*	0.777**	0.131***
	(62.907)	(0.311)	(0.050)
R^2	0.04	0.10	0.09
Panel B: Aerial eradication, controlling for baseline coca cultivation (2002-2006)			
Vote share Uribe (2002)	-59.513*	-0.377*	-0.064*
	(34.456)	(0.214)	(0.036)
Paramilitary attacks	-24.722	-0.338**	-0.061**
	(21.079)	(0.144)	(0.025)
Vote share Uribe (2002) × Paramilitary attacks	50.925	0.509**	0.091**
	(40.913)	(0.251)	(0.043)
R^2	0.05	0.12	0.10
Panel C: Aerial eradication (2006-2010)			
Vote share Uribe (2006)	-139.895***	-1.220***	-0.209***
	(46.525)	(0.337)	(0.055)
Paramilitary attacks	-57.673**	-0.640***	-0.115***
	(27.582)	(0.235)	(0.041)
Vote share Uribe (2006) × Paramilitary attacks	79.774*	0.775**	0.134**
	(43.126)	(0.373)	(0.065)
R^2	0.06	0.11	0.10
Panel D: Aerial eradication, controlling for baseline coca cultivation (2006-2010)			
Vote share Uribe (2006)	-119.879***	-1.116***	-0.194***
	(43.851)	(0.331)	(0.055)
Paramilitary attacks	-59.341**	-0.648***	-0.116***
	(25.976)	(0.228)	(0.040)
Vote share Uribe (2006) × Paramilitary attacks	84.126**	0.798**	0.137**
	(40.376)	(0.362)	(0.063)
R^2	0.05	0.12	0.10
Observations	13,680	13,680	13,680
Municipalities	285	285	285
Outcome (2002-2006) range	[0-17,100.7]	[0-9.75]	{0,1}
Outcome (2002-2006) mean	36.35	0.29	0.05
Outcome (2002-2006) std. dev	329.54	1.28	0.22
Outcome (2006-2010) range	[0-7,131.31]	[0-8.87]	{0,1}
Outcome (2006-2010) mean	34.53	0.4	0.08
Outcome (2006-2010) std. dev	219.57	1.47	0.26

Notes: All specifications are estimated using OLS and include department and year × month fixed effects. Vote share Uribe (2002) ranges from 0.04 to 0.91 with a mean of 0.55 and a std. dev. of 0.22. Vote share Uribe (2006) ranges from 0 to 0.91 with a mean of 0.40 and a std. dev. of 0.19. Robust standard errors clustered by municipality are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.