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Defending the Peace: Causes, Consequences, and Responses to Postconflict Violence

Author
Wyer, Frank Christopher

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Defending the Peace
Causes, Consequences, and Responses to Postconflict Violence

A dissertation submitted in partial satisfaction
of the requirements for the degree
Doctor of Philosophy in Political Science

by

Frank Christopher Wyer

2023

ABSTRACT OF THE DISSERTATION

Defending the Peace
Causes, Consequences, and Responses to Postconflict Violence

by

Frank Christopher Wyer
Doctor of Philosophy in Political Science
University of California, Los Angeles, 2023
Professor Darin Eugene Christensen, Co-Chair
Professor Chad J. Hazlett, Co-Chair

In recent decades, negotiated settlements between governments and rebel groups have become an increasingly common solution to civil wars. Yet negotiating agreements that can endure is difficult, and a number of countries have reached settlements only to see persistent or recurrent conflict. What are the threats or obstacles to peace in countries emerging from armed conflict? I study the case of Colombia, where a 2016 peace agreement ended a decades-long conflict with the FARC, Colombia's largest and oldest rebel group. Early on, the peace agreement was lauded as an enormous success, and won the country's president a Nobel Peace Prize. Yet within only a few years of the agreement's ratification, dissident rebel factions that rejected peace had recaptured roughly half of their former territory, bringing a new wave of violence and instability. My dissertation is composed of three papers that address: (1) the causes of the rebel defection and origins of the FARC dissident factions; (2) the direct effects of the FARC dissident's defection on the peace agreement's implementation, and in particular the Disarmament, Demobilization, and Reintegration (DDR) program; and (3) the

political effects of these setbacks on Colombian public's support for the peace agreement.

In the first chapter, I investigate the origins of conflict resurgence: why did FARC factions defect from the peace agreement and return to war? Drawing on literatures on rebel fragmentation, peace process spoilers, and material explanations for rebellion, I argue that these dissident commanders returned to arms to exploit opportunities to profit from drug production and trafficking that, ironically, were intensified by the partial success of the peace agreement. I show several lines of evidence for this argument. Among areas previously controlled by the FARC, those with cocaine production prior to the peace agreement were significantly more likely to see dissident factions emerge by 2020 than areas without significant production. Using soil and weather conditions to instrument for cocaine production produces similar results. Further, I use a novel measure of how critical each municipality is to drug trafficking to show that areas that are theoretically most important for drug trafficking are also more likely to see FARC resurgence. Finally, I theorize and find that competition over resources from rival armed groups weakens the relationship between cocaine production capacity and FARC resurgence. These results highlight an important challenge for peacebuilders: in conflicts characterized by resource competition, demobilizing a rebel group may have the unintended consequence of increasing the opportunities for profit for the group's competitors and defectors.

In Chapter 2, I focus on the consequences of the FARC dissidents' defection on the peace agreement's implementation, and specifically, the threat it poses to Colombia's Disarmament, Demobilization, and Reintegration (DDR) program. Since 2016, the former FARC combatants who demobilized have experienced a wave of violent attacks. Existing research on challenges to DDR typically focuses on proclivities for criminality among former combatants or stigmatization by civilians. I document a distinct challenge to DDR that I argue emerges when rebel groups factionalize over the decision to demobilize; namely, fratricide by rebel splinter groups that reject peace. I argue that the success of DDR threatens the interests of rebel splinter groups, and that violence against DDR participants is a strategic

response to this threat. Using a difference-in-differences approach, I show that the emergence and expansion of FARC factions opposed to Colombia's 2016 peace agreement caused a surge in fatal attacks against DDR participants. I also provide qualitative evidence illustrating the mechanisms driving this pattern. These findings highlight the need for a distinct DDR model for rebel groups at risk of factionalization.

Finally, Chapter 3 investigates how the defection of FARC dissidents has affected the Colombian public's support for the peace agreement. A longstanding conventional wisdom in the peacebuilding literature holds that violence during and after a peace process undermines public support for peace. Yet the empirical record is mixed, and in a few high-profile cases such as the Omagh bombing in Northern Ireland, public support for peace surged despite—or even in response to—incidents of violence. Building on the literature on public opinion formation, I argue that the effect of violence on attitudes towards peace may be moderated or exacerbated by political messaging about who or what is to blame. In Colombia, the peace agreement's political supporters and opponents offer competing messages that blame postconflict violence on either the government's implementation failures or on noncompliance by dissident rebel commanders. I fielded a survey experiment with 1466 respondents in conflict and non-conflict zones, pairing recent news about postconflict violence with information supporting these competing political messages. I find that messaging blaming rebel commanders for failing to comply reduced respondents' support for future peace negotiations, but I do not find strong evidence that blaming poor government implementation had a countervailing effect. While the treatment emphasizing rebel commanders' noncompliance increased perceptions that rebels alone were to blame, citizens responded to the treatment emphasizing government implementation failures by blaming *both* parties, limiting the moderating effect of this message. These results suggest that political messaging during episodes of postconflict violence can influence what citizens learn from these episodes about the viability of peace processes, but that there may be an asymmetry in citizens' propensity to assign blame that favors political opponents of peace.

The dissertation of Frank Christopher Wyer is approved.

Barbara Geddes

Leslie Nicole Johns

Chad J. Hazlett, Committee Co-Chair

Darin Eugene Christensen, Committee Co-Chair

University of California, Los Angeles

2023

To my parents

to whom I owe

my love of learning

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VITA

2015	B.A. (International Politics and Economics), Middlebury College.
2019	M.A. (Political Science), UCLA, Los Angeles, California.
2017–2022	Teaching Assistant, Political Science Department, UCLA.
2020–2023	PhD Candidate, Political Science Department, UCLA.

CHAPTER 1

Peace vs. Profit: Rebel Defection & Conflict Resurgence in Colombia

Since the landmark peace agreement with the FARC in 2016, roughly half of the territory once controlled by the group has seen a resurgence of rebel activity under dissident FARC commanders. Why? Drawing on literatures on rebel fragmentation, peace process spoilers, and material explanations for rebellion, I argue that these dissident commanders returned to arms to exploit opportunities to profit from drug production and trafficking that, ironically, were intensified by the partial success of the peace agreement. I show several lines of evidence for this argument. Among areas previously controlled by the FARC, those with cocaine production prior to the peace agreement were up to 37 percentage points more likely to see dissident factions emerge by 2020 than areas without significant production. Using soil and weather conditions to instrument for cocaine production produces similar results. Further, I use a novel measure of how critical each muncipality is to drug trafficking to show that areas that are theoretically most important for drug trafficking are also more likely to see FARC resurgence. Finally, I theorize and find that competition over resources from rival armed groups weakens the relationship between cocaine production capacity and FARC resurgence. These findings highlight an important challenge for peacebuilders: in conflicts characterized by resource competition, demobilizing a rebel group may have the unintended consequence of increasing the opportunities for profit for the group's competitors and defectors.

1.1 Introduction

In 2016, a peace agreement formally brought an end to a decades-long insurgency by the FARC, Colombia's oldest and largest rebel group. This agreement won international acclaim, including a Nobel Peace prize for Colombia's president, and succeeded in demobilizing more than 13,000 members of the FARC, who turned in their weapons and formed a legal political party. Within just a few years however, these successes were under threat, as FARC factions that rejected peace—known as FARC dissidents—had reemerged in roughly half of the former FARC's territory and brought with them a resurgence of violence (Acosta and Murphy 2019; Posso et al. 2021).

Colombia is not alone in this experience of conflict resurgence. In recent decades, negotiated settlements between governments and rebel groups have become an increasingly common solution to civil wars (Toft 2009; Fazal 2018). Yet negotiating agreements that can endure is difficult, and a number of countries have reached settlements only to see persistent or recurrent conflict. Rebel defection is a key challenge in many of these cases, with some rebel factions accepting a peace deal while others reject the deal and return to war (Rudloff and Findley 2016).

Several strands of the literature address the causes and consequences of rebel defection from peace agreements. One body of research focuses on why armed groups fragment (Woldemariam 2018; Bakke, Cunningham, and Seymour 2012; Staniland 2012), and how peace negotiations might catalyze fragmentation (Duursma and Fliervoet 2020). This literature often emphasizes group structure or identity: groups with stronger internal factions (e.g., groups with various ethnic, religious, or tribal factions) may be more likely to see certain factions defect during or after peace processes, as these factions compete over their relative power or the distribution of economic benefits under a peace agreement (Duursma and Fliervoet 2020; Woldemariam 2018; Christia 2012). A second strand of the literature addresses the problem of spoiling. Typically, this literature focuses on ideologically extreme

or hardline members of a group, who prefer to keep fighting or even intentionally sabotage an agreement rather than make concessions that more moderate members accept (De Mesquita 2005; Perkoski 2019).

Viewed from the perspective of these prevalent explanations for defection, the emergence of dissident FARC factions after Colombia's 2016 peace agreement presents an important puzzle. While a few dissident leaders cited ideological reasons for rejecting the peace agreement, most dissident FARC factions appear largely devoid of ideology (Cárdenas et al. 2019; Suárez 2021). And prior to the peace agreement, the FARC was not characterized by a high degree of factionalism.

I argue instead that the emergence of dissident FARC factions is best explained by profit-seeking. The same logic of opportunism cited by the literature on conflict initiation (Fearon and Laitin 2003; Collier and Hoeffler 2004), may drive rebel commanders to defect from peace agreements when war is more profitable than peace. In Colombia, I argue that the massive profits available from the drug trade and the vacuum of control over it caused by the FARC's demobilization offered former FARC commanders both the incentive and opportunity to defect from the peace agreement and form dissident factions.

I provide evidence for this argument by showing that the territorial pattern of dissident FARC factions' emergence and expansion is consistent with a logic of profit-seeking. Among areas previously controlled by the FARC, those with cocaine production prior to the peace agreement were up to 37 percentage points more likely to see dissident factions emerge by 2020 than areas without significant production. Using soil and weather conditions to instrument for cocaine production produces similar results. Further, I use a novel measure of how critical each municipality is to drug trafficking to show that areas that are theoretically most important for drug trafficking are also more likely to see FARC resurgence. Finally, I theorize and find that competition over resources from rival armed groups weakens the relationship between cocaine production capacity and FARC resurgence.

My research builds most directly on previous work on the relationship between rebel

resource endowments and group cohesion (Staniland 2012; Lidow 2011), as well as research on how rebel commanders threaten defection as a strategy to extract payoffs from governments (De Waal 2015; Brenner 2015). My contribution to this literature is to show how access to valuable resources can interact with the dynamics of peace processes to produce defection. My research is also relevant to the literature on spoilers, which typically portrays spoiling as a deliberate strategy employed by groups opposed to a peace agreement (Reiter 2015; Stedman 1997). My results highlight a variety of spoiling that is not a deliberate strategy, but the side-effect of profit-seeking behavior. My findings regarding drug trafficking also contribute to the literature on resources and conflict. While this literature typically focuses on the effect of resources on the areas where they are produced, my findings add to a growing number of studies that suggest the effects of natural resources can also be felt where they are bought, sold, and trafficked (Dell 2015; Walsh et al. 2018; Idler 2020). Last, my findings are relevant to the literature on conflict recurrence (Collier and Sambanis 2002; Walter 2004). Previous work in this literature has shown that conflict recurrence is more common in countries with valuable natural resources (Rustad and Binningsbø 2012), and my evidence from Colombia suggests one plausible mechanism for this relationship.

This explanation has important implications for the design and implementation of peace processes in contexts like Colombia where valuable resources fuel the risk of rebel defection. First, where access to resources gives mid-level commanders the incentive and capacity to defect, settlements must do more to obtain buy-in from these commanders and cannot afford to focus exclusively on a group's central leadership. Second, the finding that dissident expansion may have been impeded in areas where other armed groups had consolidated their control suggests both that it might be possible for governments to prevent splinter groups from emerging by consolidating their own control in such territory, and also that if the government does not consolidate control, demobilizing one armed group or faction may simply make space for its former competitors.

1.2 Background

The FARC originated in central Colombia in the early 1960s as a communist-inspired rural insurgency. In the 1980s the FARC gradually became an important player in the drug trade, with taxes on cocaine eventually becoming a key source of the group's financing. The FARC reached the peak of its power around the year 2000, at which point it operated in over a third of Colombia's 1122 municipalities and had up to 20,000 members. Over the next decade however, the group faced major setbacks, first when right-wing paramilitary groups waged a scorched-earth campaign in several of the FARC's strongholds, and later when the Colombian military assassinated several of the FARC's top commanders in rapid succession. Beginning in 2012, the FARC, now reduced to around 14,000 members in 260 municipalities (Semana 2016; Indepaz 2013), entered negotiations with the government that culminated with a peace accord in 2016. The agreement required FARC's members to disarm and demobilize in exchange for guaranteed political representation and conditional amnesty, among other concessions.

Judging both from the reaction of the international community and results on the ground, Colombia's peace agreement initially appeared poised for success. The agreement won then-President Juan Manuel Santos a Nobel Peace Prize, and both the negotiation process and the agreement it ultimately produced were hailed by observers as models whose success might be replicated elsewhere (Vulliamy 2016; Siegfried 2016). And the initial results of the agreement appeared to support such optimism. The FARC's ceasefire, which began during the negotiation phase, brought conflict-related violence to it's lowest level in 50 years (Mora 2016). Moreover, the FARC's initial disarmament and demobilization was relatively smooth, with the UN Verification mission reporting that 100% of the group's weapons had been turned in and destroyed by mid-2017 ("United Nations" 2017). Meanwhile, the FARC's members demobilized at 26 transition camps located across the country.

This peace process did not however, include the other major armed groups in Colombia,

which at the time consisted of the National Liberation Army (ELN) and the Gaitanist Self-defence Forces of Colombia (AGC). As of 2012, when negotiations with the FARC began, each of these rival groups numbered between 1500-2000 members (Indepaz 2013). Both groups were deeply involved in the cocaine trade and frequently fought the FARC and each other for control over territory used for drug production and trafficking.[1]

A few midlevel FARC commanders whose units added up to around 300 members initially refused to demobilize, and the FARC's central command denounced and expelled them (Acosta 2017; Barrios 2016). Over the ensuing years however, dozens of new dissident factions have appeared across roughly half of the FARC's former territory, and their numbers have expanded from just a few hundred to several thousand strong (Posso et al. 2021). Most of these dissident factions are led by mid-level FARC commanders who spent at least a few months in the demobilization camps before returning to the conflict. While they share the designation of FARC dissidents, some of these factions have formed loosely-organized alliance networks, while others act completely independently, and violent clashes between different factions are not uncommon.

The emergence and expansion of the FARC dissidents has contributed to the uneven success of Colombia's peace agreement. The initial ceasefire led to a dramatic decline in conflict-related violence throughout the FARC's territory, and many former conflict zones have become relatively stable. However, in areas affected by FARC dissidents, these early gains are evaporating. Dissidents are responsible for assassinating dozens of local leaders, forcibly recruiting minors, perpetrating mass killings, and displacing entire communities.

[1]What had been a third major armed group, the Rastrojos, suffered a total collapse when its top leaders were captured in mid-2012.

1.3 Theory and Hypotheses

1.3.1 Why do rebels defect from peace processes?

Why do rebels defect from peace processes? In this section I first address the dominant explanations in two strands of the existing literature on rebel group fragmentation and spoiling, showing that neither offers a satisfactory explanation for the defection of dissident FARC factions. I argue that the literature on conflict initiation suggest a distinct logic for defection from peace processes that focuses on the material opportunities or incentives for rebellion, and that this logic is better suited to explain the case of Colombia.

1.3.1.1 Ethno-religious factions defect from deals that advantage rival factions

One cause of rebel defection from peace processes, suggested by the literature on rebel group cohesion and fragmentation, concerns how peace agreements interact with pre-existing divisions or structural weaknesses within groups. Broadly, this literature emphasizes the distinction between unified groups with strong social ties between members and an established leadership hierarchy, and groups whose members are divided into ethnic or religious subgroups, and whose leadership may be factionalized (Asal, Brown, and Dalton 2012; Staniland 2012). In internally divided groups, members and leaders of various factions are concerned about the relative balance of power of factions within the group, and even positive shocks like the receipt of external support or battlefield losses may cause groups to fragment if they improve the position of a particular faction disproportionately (Christia 2012; Woldemariam 2018). Peace processes have significant implications for the balance of power between factions, as groups must choose who should represent a group in negotiations, what concessions should be demanded, accepted, or rejected, and how political posts and economic benefits should be distributed (Duursma and Fliervoet 2020). Thus, factions of internally-divided groups that are relative losers under a peace agreement may choose to defect from the peace agreement and continue fighting.

Yet the FARC was not characterized by the types of divisions or structural problems that this literature cites as risk factors. The FARC was not divided into racial, ethnic, religious, or other identity-based factions, nor do dissident FARC factions reflect such divisions. On the contrary, the evidence suggests that the FARC was a highly unified group. First, when the FARC's central command agreed to a unilateral ceasefire as a condition of negotiations with the government, compliance with the ceasefire was extremely high across its units (Ávila Martínez 2013). Second, compared to groups with factionalized leadership, where leadership transitions are often a catalyst for infighting, the FARC's leadership transitions were remarkably smooth. For example, when the FARC's founding commander Manuel Marulanda died of a heart attack in 2008, he was smoothly replaced by Alfonso Cano, and when the latter was killed in action by the military in 2011, the FARC announced his successor, Rodrigo Londoño (Timochenko), almost immediately (Hinchliffe 2011).

1.3.1.2 Extremists defect to sabotage deals that make unacceptable concessions

A second cause of rebel defection suggested by the literature emanates from members or leaders seeking to disrupt, or "spoil", a peace agreement they disagree with (Reiter 2015; Stedman 1997). In particular, ideologically extreme members of a group might reject a peace deal that involves concessions inconsistent with their ideology (De Mesquita 2005; Kydd and Walter 2002). By defecting from a peace agreement and continuing to fight, spoilers aim to undermine political will for its ratification and implementation, and thereby force either a renegotiation of the deal or a return to war (ibid).

There is little evidence however, that members of dissident FARC factions are more ideologically extreme than FARC members that remained within the peace agreement, or that the dissidents had ideological objections to the peace agreement. Indeed, the vast majority of FARC dissident commanders initially approved of the peace agreement and participated in the DDR process for at least a few months before establishing dissident groups. And, if anything, these dissident factions exhibit signs of being less concerned with ideology or politics

than the FARC was (Suárez 2021). For example, while the FARC engaged in "political work" in territory under its control, which in practice involved pressuring community-members to attend meetings about communism, reports indicate that such activity is sporadic to non-existent in territory controlled by FARC dissidents (Cárdenas et al. 2019).

1.3.1.3 Opportunists defect for material gain

Why then, did the FARC dissidents defect from the 2016 peace agreement? My basic argument is that profit-seeking behavior by rebel commanders can cause defection from peace processes even in groups that are not characterized by ideological extremism or ethnic factionalism. This argument has its roots in research on civil war initiation that emphasizes the importance of opportunities for rebellion over the existence of grievances (Fearon and Laitin 2003; Collier and Hoeffler 2004; Grossman 1991). While acknowledging that grievances and identity may be important motivations, this body of research argues that rebellions are more common in contexts where the state is too weak to stop them and there are valuable resources to be captured.

I argue that this logic of material opportunity extends not only to violent entrepreneurs initiating conflict, but also to rebel commanders as they consider exiting conflict under a peace agreement. The same opportunities for profit that induce violent entrepreneurs to initiate a conflict might induce a rebel commander to defect from a peace agreement and continue fighting. This argument is most closely related to (De Waal 2015), which suggests that commanders defect from peace agreements to demand a larger payout from the government.[2] But where this prior work frames defection as a negotiating tactic, I suggest that in some scenarios, defection represents the optimal profit-maximizing strategy for rebel commanders even if they do not expect government payouts.

Typically, peace agreements are designed to alter the opportunity structure of rebel

[2]See also the discussion of "greedy spoilers" in Stedman (1997), and Brenner (2015) on government-induced defection of rebel commanders in Myanmar.

commanders in favor of peace. Requiring groups to disband and turn in their weapons increases the cost of returning to war, as does the extension of state control over rebel territory via stabilization forces. Meanwhile, granting rebel commanders political positions or integrating them into the national military can make peace as or more profitable than war.

In Colombia however, rebellion may have remained more attractive than peace for many former FARC commanders. First, for mid-level commanders specifically, the potential income from rebellion, and the drug trade in particular, contrasted starkly with the meager benefits offered by the peace agreement.[3] While a handful of members of the FARC's secretariat received important political positions, the peace agreement offered most mid-level commanders the same package as the lowest rank-and-file member: 90% of the minimum wage and support for agricultural projects. At the time, a Colombian analyst observed that these benefits were effectively "a joke" to mid-level FARC commanders who had managed the FARC's massive illicit revenues (Vélez 2016).

Second, the peace agreement did not foreclose the opportunity for rebellion in Colombia. A program to eliminate illicit drugs by enrolling coca farmers in crop substitution programs was poorly funded and administered: just over half of the nearly 200,000 families eligible for the program were ever formally enrolled, and two years into the program, the UN reported that only 10% of participants had received the full payments they were entitled to, and more than 40% had received nothing at all (Puerta and Chaparro 2019). Similarly, a stabilization plan intended to state control over former FARC territory was never fully realized. Almost a year after the peace agreement was ratified, an International Crisis Group report noted that, "the army and police have dedicated most of their personnel to protecting areas around the 26 cantonment sites"; meanwhile, "in many conflict-affected rural areas, it is still rare to see military or security forces." ("Colombia's Armed Groups" 2017). Instead of closing

[3]Note that while I focus on the mid-level commanders who abandoned the peace agreement, many remain compliant with the peace agreement and play an important and positive role in peacebuilding (Shesterinina 2020).

off opportunities for rebellion, the peace agreement created a vacuum of power, opening the door to rival armed groups like the AGC and ELN seeking to consolidate control over previously contested territory (ibid.), and to potential defectors from the peace agreement.

1.3.2 Observable implications

My theoretical argument suggests that the formation of dissident FARC factions was driven by opportunities for profit-seeking rebellion. This argument has testable implications for the pattern of the emergence of dissident FARC factions across Colombia; namely, these dissident factions should be more likely to emerge and expand in areas with better opportunities for profit-seeking. In what follows, I focus on two key sources of variation in such opportunities: spatial variation in (1) the distribution of valuable resources and (2) competition from rival armed groups.

1.3.2.1 H1: Drug production and trafficking facilitates emergence and expansion of dissident factions

There is considerable spatial variation in the distribution of valuable resources in Colombia. Prior work has exploited this spatial variation to study the impact of oil price shocks on conflict intensity in oil-rich regions (Dube and Vargas 2013), and shocks to the price of cocaine on conflict intensity in cocaine-producing areas (Angrist and Kugler 2008). I focus on cocaine, a resource that generates an estimated $8-12 billion USD (2.6%-4% of Colombia's GDP) in Colombia annually (Alsema 2021). I hypothesize that cocaine represents the type of opportunity for profit-seeking that could give rise to defection.

Armed groups can profit from the drug trade at least two important ways. First, where drugs are produced, armed groups can act as "protection rackets" by establishing territorial control and imposing taxes on production. Thus, I hypothesize that *dissident FARC factions will be more likely to emerge in territory valuable for cocaine production (**H1A**)*

Second, armed groups can profit from the drug trade by establishing control over key points in drug trafficking routes. This second observation draws on recent research that has emphasized how armed groups profit from natural resources not only at production sites, but also at other points in the supply chain. For example, cross-national data suggests that rebel groups are about equally likely to obtain financing from the smuggling of illicit resources as they are from directly extorting resources as they are produced or extracted (Walsh et al. 2018). In the case of Mexico, Dell 2015 demonstrates that drug trafficking organizations compete with each other (and the authorities) over the most efficient routes between drug producing-areas and the US border. In Colombia, Idler 2020 shows that armed groups value control over different parts of the cocaine supply chain, including not only production sites but also trafficking hubs. I hypothesize that *dissident FARC factions will be more likely to emerge in territory valuable for cocaine trafficking (**H1B**).*

1.3.2.2 H2: Competition over resources from incumbent groups impedes expansion of dissident factions

A second source of variation in opportunities for profit-seeking relates to competition: I argue that the effect of valuable natural resources in motivating or facilitating defection is moderated by competition over these resources from other armed groups. Rebel commanders forming dissident factions must consider not only the value that could be extracted from a piece of territory but also the costs of capturing it. The classic literature on profit-seeking rebels considers such costs primarily in terms of state capacity; i.e. the ability of a state to suppress a rebellion (Fearon and Laitin 2003; Collier and Hoeffler 2004). However, other non-state armed groups may also have the capacity and incentive to crush potential competitors, particularly if those competitors are seeking control over the same resource base (Fjelde and Nilsson 2012).

This argument has important implications for understanding the emergence and expansion of FARC dissident factions. In some parts of the FARC's territory, the group's demobi-

lization under the peace agreement left behind a near-complete vacuum of power. In these areas, I hypothesize that a newly-established dissident factions could relatively easily gain control of valuable resources like cocaine.

In other parts of the FARC's former territory however, the FARC faced fierce competition, sometimes latent and sometimes quite active, from two other large armed groups primarily: the AGC and the ELN.[4] In these areas, the FARC's demobilization allowed its long-time competitors to consolidate their control. At the same time that former FARC combatants were leaving for demobilization camps, pamphlets appeared in a number of municipalities proclaiming that the AGC were arriving "to control, organize and recuperate territory from the FARC", and announcing a "social cleansing", a common euphemism for the killing or expulsion of suspected criminals and guerrilla sympathizers (Alsema 2017). Similarly, the ELN "strengthened its control in territories formerly shared with the FARC" ("Colombia's Armed Groups" 2017). Prem et al. 2022 document how the AGC and ELN selectively assassinated hundreds of community leaders starting as early as 2014, as they moved to consolidate control in the FARC's territory. In these contested areas, the cost of entry for a dissident faction would be much higher—to establish a monopoly on violence it would have to defeat and displace a larger incumbent.

In the empirical section of this paper, I test the hypothesis that *dissident FARC factions are less likely to emerge and expand in drug-producing territory when it is contested by incumbent armed groups (**H2**).*

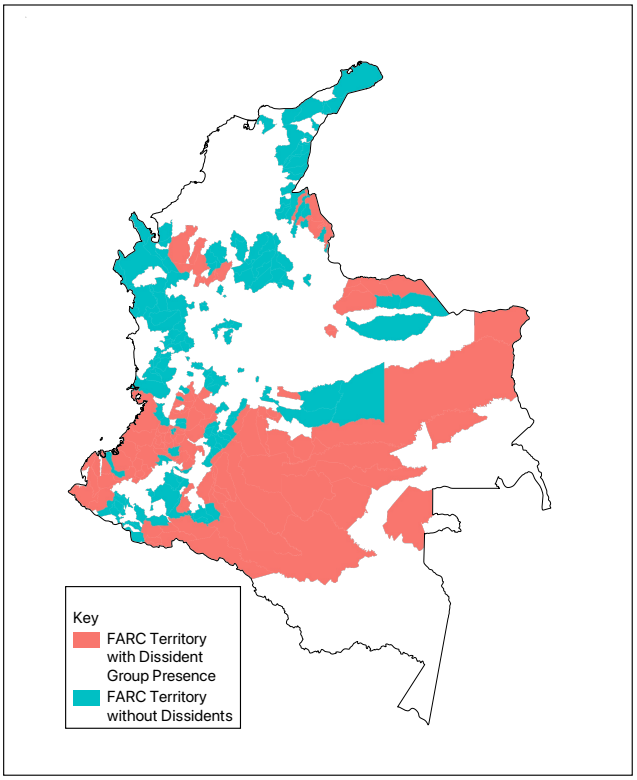
[4]I focus on the AGC and ELN as the two armed groups with national-level presence, a high level of organization, and strong military capacity during the period of the FARC's demobilization. I intentionally omit from this discussion smaller, local groups like the Pelusos and Puntilleros, the Rastrojos, who collapsed just prior to the peace agreement's ratification, and the Águilas Negras, whose numbers, structure, and regional presence is unknown or disputed. Unlike the AGC and ELN, it is not clear ex ante that these groups would have the capacity to consolidate control in the FARC's absence.

1.4 Data & Empirical Strategy

1.4.1 Descriptive evidence

I first show evidence for the descriptive claim that FARC dissidents were more likely to emerge in areas that were valuable to the drug economy but not contested by rival groups. Here, I use binary measures of cocaine cultivation and armed group presence, and limit the analyses to municipalities with FARC presence prior to the beginning of peace talks in 2012. Figure 1.1 maps the pattern of dissident FARC faction resurgence in former FARC municipalities.

Figure 1.1: Mapping Dissident Resurgence in Former FARC Territory



Data on armed group presence come from the Colombian think-tank Indepaz.[5] I start

[5]Indepaz releases periodic reports on the presence of armed groups at the municipal level which are produced by synchronizing across five sets of sources: (1) a comprehensive database of news reports; (2)

with 260 Colombian municipalities with FARC presence in 2012, the year negotiations began. I operationalize competition as a binary variable that takes a value of 1 in municipalities with AGC or ELN presence and 0 otherwise: of the 260 municipalities with FARC presence in 2012, 105 also had either ELN presence, AGC presence, or both. I operationalize FARC dissident resurgence as a binary variable that takes a value of 1 if Indepaz recorded dissident FARC presence in the municipality between 2016 and 2020, and 0 otherwise.

Data on cocaine production comes from the Colombian Ministry of Justice's Drug Observatory (ODC). The ODC uses satellite imagery to precisely locate coca farms, which it aggregates to produce an estimate of the area in each municipality dedicated to cocaine cultivation each year. Here I define a binary variable that takes a value of one if a municipality had at least 100 hectares of cocaine cultivation between 1999-2012 and 0 otherwise.

Table 1.1: Dissident Emergence by Cocaine Cultivation and Competition

		Cultivation	
		Yes	No
Competition	Yes	34%	21%
	No	76%	37%

The results of this descriptive exercise (Table 1.1) demonstrate that FARC dissidents emerged more frequently in territory with cocaine cultivation, but that this pattern disappears in territory with the presence of competitors. The comparison between cocaine-producing areas with and without competition is particularly notable—FARC dissidents are present in 76% of municipalities with cocaine production and no competition, compared to only 37% of cocaine-producing municipalities with competition.

official sources such as the police, the office of the attorney general, and the human rights observatory, among others; (3) reports from independent national NGOs; (4) communication with local or regional organizations; and (5) field work by Indepaz researchers (Posso et al. 2021, Indepaz 2013).

1.4.2 Cocaine production

In this section I analyze the effects of cocaine production on the emergence of dissident groups. Table 1.2 shows the results of three empirical strategies.

Table 1.2: Effects of cocaine production on dissident FARC emergence

	Dependent variable: Dissident Presence (binary)				
	(1) Difference-in-Means	(2) Difference-in-Means	(3) Covariate-Adjusted	(4) Instrumental Variables	(5) Instrumental Variables
(Intercept)	0.31^{***}	0.29^{***}	0.21^{*}	-0.10	-0.16
	(0.04)	(0.04)	(0.09)	(0.23)	(0.23)
Cocaine Production *Binary*	0.27^{***}		0.37^{***}	0.30^{**}	
	(0.06)		(0.10)	(0.09)	
Cocaine Production *Continuous*		0.04^{***}			0.05^{***}
		(0.01)			(0.01)
Agriculture Control				✓	✓
Flood Control				✓	✓
Num. obs.	260	260	258	260	260
First-stage F-stat				240.27	242.14
Anderson-Rubin CI				[0.10, 0.49]	[0.03, 0.08]

Notes: The dependent variable is a binary indicator for the presence of FARC dissident factions in a municipality as of 2020. The independent variable in models 1 and 3 is the logged maximum annual cocaine cultivation (in hectares) in a municipality between 1999-2012, and in models 2, 4, and 5 a binary indicator that takes a value of one if annual cocaine cultivation exceeded 100 ha. in any year during this period. The full sample consists of 260 FARC municipalities, with 2 observations dropped on model 3 due to missingness in the covariates. In model 3 I generate weights on the covariates of interest (literacy, electricity, population, percent rural, rough terrain, highway coverage, and the three distance variables) with kernel balancing, and estimate the model using weighted least squares. The instrument in models 4 and 5 is a municipality-level index of soil (acidity, nitrogen, carbon, and drainage) and weather (temperature, sunlight hours, annual rainfall, and humidity) variables combined using OLS in for the continuous cocaine measure, and Logit for the binary measure. The IV models include controls for agricultural productivity, defined as the logged maximum annual hectares under cultivation with legal crops between 2009 and 2014, and flooding, defined as the average number of flood-related alerts per municipality per year between 2017-2020. All models use HC2 heteroskedasticity consistent standard errors. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Columns 1 and 2 show the unadjusted difference in means for the effect of cocaine production, operationalized as a binary variable (column 1) or a continuous measure (column 2). The coefficient estimate of 0.27 in column 1 indicates that going from a non-cocaine producing municipality to a cocaine-producing municipality increases the probability of seeing

a dissident faction by 27%, while the coefficient estimate of 0.04 in column 2 indicates that doubling a municipality's hectares of cocaine cultivation is associated with an increase of 0.04 (i.e. roughly 4 percentage points) in the probability of dissident presence in a municipality.

While this relationship between cocaine production and the emergence of FARC dissidents is consistent with my hypotheses, it is vulnerable to confounding by other variables that might affect conditions for both drug production and rebellion. In the subsequent sections I outline two strategies to address such problems.

1.4.2.1 Covariate adjustment

The first strategy involves identifying a set of plausible confounding variables and adjusting for them directly. For example, cocaine is often cultivated in relatively poor, rural areas, and comparing these areas to relatively wealthy towns would risk confounding the effects of cocaine, poverty, and remoteness. However, this problem could be resolved by comparing municipalities with and without cocaine production that are otherwise equally poor and remote, either by identifying municipalities with matching characteristics, or by weighting certain municipalities such that the weighted distribution of remoteness or poverty matched the distribution in the cocaine-producing municipalities.

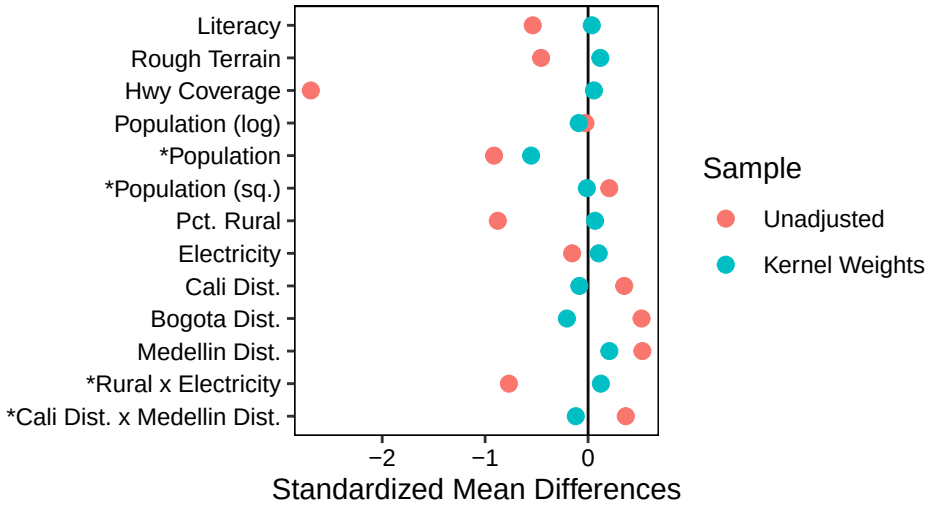
I adjust for several categories of potential confounders. The first category includes basic features of a municipality, such as its population size and whether it is rural or urban, based on census data. The second category includes variables that correspond to features identified as important in previous conflict research, such as ruggedness of terrain, infrastructure, wealth, and education (Fearon and Laitin 2003; Collier and Hoeffler 2004). I include a proxy for wealth using census data on the percent of the population with access to electricity, education using the percent of the population that is literate, infrastructure by calculating the length of highway per square kilometer, and rough terrain as the standard deviation of elevation. Last, I include a set of geographic variables (the distance from three major cities), to account for spatial clustering. This last step ensures that I compare municipalities that

not only have similar characteristics, but are also in the same parts of the country.

I employ kernel balancing to adjust for these covariates (Hazlett 2020), a method that can achieve approximate balance on a range of highly flexible and complex functions of covariates while minimizing the worst-case bias from imbalance on unknown or unspecified functions of the covariates. In practice, there are multiple plausible nonlinear relationships in the data: perhaps rebellion is most likely in areas that are neither cities nor entirely depopulated, implying some kind of u-shaped relationship; or, perhaps the effect of rough terrain on rebellion is only present where infrastructure is poor, implying some kind of interaction. These nonlinearities suggest that kernel balancing is the most best-suited approach for covariate adjustment, but I also show in Appendix 1.7 that the results are broadly consistent when using more standard mean balancing or matching procedures.

Figure 1.2 shows that the balancing weights dramatically decrease the difference in mean values of the covariates between cocaine and non-cocaine producing municipalities.

Figure 1.2: Balance on Observables for Drug Producing and non-Drug Producing Municipalities



Note in particular that before this adjustment, cocaine-producing municipalities are less connected to the highway network, are further from major cities, and have lower access to

electricity than other municipalities. Without adjusting for these variables, it is impossible to rule out the possibility that the correlation between cocaine cultivation and the emergence of dissident factions is caused by the fact that these municipalities are generally more remote or poorer than others. Moreover, this balancing procedure not only improves balance on the covariates explicitly included, but also on transformations and interactions between them (noted with a star). For example, there is a notable imbalance in the interaction between the electricity access variable and rurality variable that improves with kernel weights, even though this interaction is not explicitly included.

Column 3 of table 1.2 shows the result of this approach. Because the dependent and independent variables are both binary in this model, the result of this analysis indicates that going from a non-cocaine producing municipality to a cocaine-producing municipality increases the probability of seeing a dissident faction by 37 percentage points (95% CI = 17 to 58).

A causal interpretation of this estimate relies on the assumption of no unobserved confounding. In other words, there is not some other unobserved or omitted variable that causes both cocaine production and rebel resurgence. In the section that follows, I describe a final strategy that seeks to address potential unobserved confounders.

1.4.2.2 Instrumental variables

Even after adjusting for observed confounders, there is still a risk that some omitted or unobserved variable might bias the result. For instance, suppose that corrupt local officials inhibit effective government action against both cocaine production and armed groups, allowing both to thrive in their jurisdictions in return for kickbacks. Such corruption would be difficult to observe or measure, but this relationship would bias estimates of the effect of cocaine production on dissident expansion.

Instrumental variable analysis addresses concerns about unobserved confounders by ex-

ploiting exogenous variation in the independent variable, here cocaine production. Like most crops, coca plants require a specific set of climactic and soil conditions, and as a result some parts of Colombia are better-suited for its cultivation than others. I rely on the assumption that these instruments are exogenous; that is, it is unlikely that some other variable causes soil and weather conditions and causes rebel resurgence. However, I also address potential violations of this assumption by controlling for variables like agricultural productivity that are plausibly correlated with both the instrument and the outcome, as well as by performing sensitivity analysis to test the robustness of the results to such violations.

A large number of soil and weather conditions might plausibly affect cocaine cultivation. In my preferred specification, I use a list of weather and soil variables derived from expert analyses of the cocaine plant. This list includes measures of soil acidity, nitrogen and carbon content, and drainage capacity, as well as temperature, sunlight hours, annual rainfall, and humidity. I regress these variables on historical cocaine production, and use the predicted values to combine them into a single index variable for cocaine cultivation suitability.[6]

For cocaine cultivation, I use both the binary variable from the previous analysis, and a continuous measure that takes the logged maximum annual cocaine cultivation between 1999-2012 from the Ministry of Justice's ODC data. Soil data comes from the International Soil Reference and Information Centre's 2005 estimates for Latin America (Batjes 2005). This database maps values from soil samples to polygons, which I merge with the spatial boundaries of Colombian municipalities. Where these polygons do not overlap perfectly with municipality boundaries, I use the values from the profile with the larger overlap. For weather, I rely on readings from 2047 Colombian weather stations spanning 1981-2010. Interpolation allows me to combine information from multiple stations to produce estimates for each municipality. Available indicators include measures of temperature, rainfall, sunlight hours, and humidity.

[6]I show further details of the instrument construction in Appendix 1.6.3.1 and map the distribution of these indexes in Appendix 1.5.

Last, in some specifications I control for licit agricultural productivity and flooding incidents. I use historical data on crop productivity from the Colombian ministry of agriculture and data on flood events from Colombia's disaster management agency to generate these control variables.

Columns 4 and 5 of table 1.2 show the results of these analyses of the effect of cocaine production on the presence of dissident factions. Model 5 shows the result with a continuous measure of cocaine production, while model 4 shows the results with the binary measure. These results do not diverge sharply from the unadjusted and covariate-adjusted difference in means estimates, suggesting that the extent observed or unobserved confounding drove those effects was limited.

IV analyses depend on a set of identifying assumptions: the instrument must have a strong effect on the treatment variable (relevance), it must not be correlated with other variables that affect the outcome (ignorability), and it must affect the outcome only through the treatment variable (exclusion restriction) (Angrist, Imbens, and Rubin 1996).

Straightforward diagnostics allow me to test the assumption of relevance, or rule out what is commonly termed the problem of "weak instruments". The first stage F-statistic of 231 is a very strong first-stage, well above commonly cited benchmarks such as 10. Additionally, the Anderson-Rubin confidence intervals (Anderson and Rubin 1949), which are robust to weak instruments (Staiger and Stock 1994), exclude a negative or null effect.

Weather and soil affect not only cocaine crops but also licit agriculture, which could violate the exclusion restriction assumption. If the weather and soil conditions that favor cocaine cultivation also favor licit crops, then it might be that municipalities with such conditions are wealthier than others and this wealth, rather than the cocaine production, might attract rebel groups. Conversely, if the weather and soil conditions that favor cocaine cultivation have negative effects on licit crops, then the inhabitants of these areas might be particularly impoverished, and more vulnerable to recruitment by a rebel group (see, e.g. Caruso, Petrarca, and Ricciuti 2016). Similarly, the weather and soil conditions that permit

cocaine cultivation might also affect rebel groups by causing floods. Flooding could wash out roads and bridges and make certain areas difficult for the state to access and govern, thereby allowing rebel groups to thrive.

Models 4 and 5, control for both of these potential violations. I control for municipalities' agricultural productivity, which I operationalize as the maximum hectares under cultivation with any legal crop in any year between 2009 and 2014. I also control for the average number of flood-related alerts per municipality per year between 2017-2020. In this case, I intentionally use data from the "post-treatment" period, because the relationship of interest is whether the instrument is correlated with flooding during the period where FARC dissident factions emerged. In both cases, the IV estimates for the effect of cocaine remains positive and significant, which alleviates the concern that the effect of the instrument might be driven by these other variables.

In the appendices, perform a number of additional robustness tests. I show that the first stage and reduced form regressions are robust to spatial correlation in the data (Appendix 1.9), using Conley standard errors to adjust for two-dimensional spatial dependence (Conley 1999). I also address a further set of exclusion restriction violation concerns specifically associated with using rainfall as an instrument (Mellon 2021). I drop annual rainfall from the list of soil and weather variables included in the coca suitability index, and show that the estimate is of similar magnitude and significance (Appendix **??**).

It is still possible however, that some other omitted variable exists that violates the exclusion or ignorability assumptions. I use sensitivity analyses (Table 1.3) to assess the threat to inference posed by unknown violations of the exclusion or ignorability assumptions (Cinelli and Hazlett 2020). The intuition behind this approach is to quantify how strong the relationship between the instrument, the outcome, and some other unobserved variable would have to be to change the substantive result of the analysis.

The sensitivity analysis yields several quantities of interest. In particular, the quantity $RV_{q=1}$ represents how strong an unobserved confounder must be to bring the point estimate

Table 1.3: Sensitivity Analysis of Reduced Form

	Outcome: *Dissident Presence (binary)*						
	Est.	S.E.	t-value	$R^2_{Y\sim D\|X}$	$RV_{q=1}$	$RV_{q=1,\alpha=0.05}$	DF
Weather & Soil Instrument	0.05	0.01	4.18	6.3%	22.8%	12.8%	258

Notes: The dependent variable is a binary indicator for the presence of FARC dissident factions in a municipality as of 2020. The instrument is a municipality-level index of soil (acidity, nitrogen, carbon, and drainage) and weather (temperature, sunlight hours, annual rainfall, and humidity) variables, combined using the predicted values from an OLS regression of on historical cocaine production. I use HC2 heteroskedasticity consistent standard errors.

to zero, while $RV_{q=1,\alpha=0.05}$ represents how strong unobserved confounding would have to be to bring the estimate to a range where it is no longer statistically different from 0, at the significance level of 0.05. Here, an unobserved confounder would have to explain more than 22.8% of the residual variance of both the weather and soil instrument and dissident emergence to bring the reduced form estimate to 0, or 12.8% for the reduced form estimate to lose statistical significance. This result indicates that the IV analysis is robust to even moderately strong violations of the key assumptions.

In sum, there is extensive evidence for the argument that cocaine production facilitated the emergence and expansion of dissident factions (Hypothesis 1A). In the next section, I provide evidence that the relationship between cocaine and conflict resurgence can be observed not only in locations of cocaine cultivation, but also sites of cocaine trafficking.

1.4.3 Cocaine Trafficking

1.4.3.1 Measurement & estimation

The second set of analyses in this paper focus on the effect of drug *trafficking* on dissident faction emergence. As with drug production, the risk of confounding by unobserved variables is clear. Drug traffickers want to avoid interdiction and may favor routes through areas where the state is particularly weak, or where government officials are particularly corrupt for example, and these same factors might facilitate rebellion. Studying this relationship is further complicated by the fact that unlike in the case of drug production, where fields of

coca bushes are directly observed by remote sensing, there is not a direct measure of drug trafficking.

As with drug production, I address the problem of unobserved or omitted confounders by using exogenous sources of variation in drug trafficking, this time exploiting features of geography that make territory useful for drug trafficking. The intuition behind my approach is simple: cocaine must travel from the fields where it is cultivated to land or sea borders, and basic features of geography constrain the set of plausible routes between cultivation sites and the borders. I use these geographic features to simulate realistic trafficking routes, and use these simulated routes to construct measures that capture the importance of territory for drug trafficking. Because these measures are derived from exogenous geographic features, they comprise instruments for drug trafficking that are unaffected by factors like local government or politics that could be confounders.[7]

The technical procedure for generating these instruments is as follows. I use the municipalities where cocaine is produced as starting points for routes and generate roughly 200 evenly spaced points along Colombia's land and sea borders for the end points. I then use a least-cost-path algorithm to map the most efficient routes between each producer-municipality and every border point (Etten 2017). Functionally, I input pairs of start and end points, as well as a high-resolution map (raster) with rivers, roads, mountains, and other surfaces and their associated travel speeds (Weiss et al. 2018), and the algorithm generates a route that minimizes the cost of travel (in time) between the start and end point.

After creating these hypothetical routes for every drug-producing municipality, I can generate a measure of what I term *centrality* by identifying municipalities intersected by a

[7]Related work by Dell (2015) and Wright (2016), uses road networks and known transit points to simulate trafficking routes. My approach diverges in three key ways. First, recognizing that drug traffic occurs not only on roads, but also along rivers, and even unpaved paths, I use a method that allows for non-road travel where efficient. Second, rather than choosing known exit points, which may be endogenously determined by factors like local government capacity or corruption, I allow traffic to exit the country at any point along the border. And third, while Wright focuses on the most efficient path, and Dell on spill-overs from the most efficient path to the most likely alternative, my criticality measure attempts to quantify how important a specific piece of territory is to the broader network of drug-trafficking routes.

large number of routes. All else equal, municipalities that are intersected by many paths from cultivation sites to the border should be more likely than others to be valuable for trafficking.

I supplement the centrality measure with a second measure, which I term *criticality*, that captures how pivotal a particular municipality is to drug trafficking routes. I take this dual approach because it is possible that some municipalities fall along many possible routes, but are not actually critical chokepoints for those routes. Consider for instance an area with a dense network of roads and rivers that create many viable and low-cost routes between a production site and a port town from which drugs are trafficked overseas. A municipality in this area might be intersected by a major highway and therefore be at the center of many efficient routes, but because many low-cost alternative routes exist, control over this municipality does not give an armed group control over drug trafficking. Compare this to a scenario where a single paved road connects a drug-producing municipality to the transportation network, and bypassing the road would require transporting the drugs by burro or on foot. Control over a single municipality along this road could allow an armed group effective control over all traffic out of the production site.

I operationalize this concept in the following manner. For each municipality, one at a time, I create a barrier that prevents travel through the municipality and calculate new routes from every production site to border point. I then calculate the total percentage change in route costs caused by preventing travel through the municipality. Preventing travel through municipalities that are truly critical to trafficking routes should increase route costs significantly, while preventing travel through municipalities along routes with multiple low-cost alternatives should not. Appendix 1.6.5.1 provides a step-by-step guide to the creation of both the centrality and criticality measures.

These measures of centrality and criticality are valid only if they produces strong instruments for drug trafficking. In the absence of a direct measure of drug trafficking, I instead test the relationship between these instruments and the volume of seizures of drug shipments

recorded by the Colombian Ministry of Justice's Drug Observatory. Seizure data do not fully reflect the total volume of drug traffic, as they measure only drug traffic that is detected and interdicted by authorities. Nevertheless, strong instruments for drug trafficking should also be strongly correlated with seizures of drug shipments. My preferred measure of drug trafficking is the log maximum yearly seizure of cocaine paste in a municipality between 1999-2012. Coca paste is a form of cocaine that has been partially processed; it is often transported in this form but not typically consumed without further refinement.

1.4.3.2 Results

My hypothesis 1B proposes that the effect of cocaine on the emergence of dissident factions should hold not only in territory where drugs are produced, but also where they are trafficked. Because there is not a direct measure of drug trafficking, and because actual drug trafficking routes are likely influenced by confounding factors like state capacity and corruption, I use geospatial tools to create routes between cocaine production and the borders that are based on exogenous geographic features, and use these simulated routes to predict a municipality's importance for cocaine trafficking. I create an instrument for a municipality's *centrality* to cocaine-trafficking routes by calculating the proportion of simulated routes that pass through it, and its *criticality* by measuring how much the transit costs of the simulated routes increase if they have to avoid the municipality. In the section that follows I first validate these instruments by showing that they strongly predict seizures of cocaine shipments, and then show that these instruments for drug trafficking also have strong positive effects on the emergence of dissident factions.

Table 1.4 tests the relationships between the criticality and centrality measures and cocaine paste seizures.

These results indicate that both measures are positively correlated with cocaine seizures. They suggest that a one standard-deviation increase in the measure of criticality is associated with a roughly a 33% increase in the amount of cocaine paste seized (exponentiating the

Table 1.4: Relationship Between Trafficking Instruments and Observed Seizures

	Dependent variable: Cocaine Paste Seizures (log kg.)			
	(1)	(2)	(3)	(4)
(Intercept)	−2.734	2.315	−4.291***	2.065
	(1.525)	(1.690)	(1.230)	(1.503)
Criticality Measure	0.379*	0.290*		
	(0.171)	(0.147)		
Centrality Measure			0.425**	0.705***
			(0.137)	(0.145)
Municipality Size	✓	✓	✓	✓
Coca Cultivation		✓		✓
Instrument ptl. R^2	0.019	0.015	0.036	0.084
R^2	0.147	0.312	0.156	0.367
Num. obs.	260	260	260	260
F statistic	28.129	53.046	35.884	65.952

Notes: In all models, the dependent variable is the maximum annual seizure of cocaine paste in a municipality in logged kilograms. The independent variable is the criticality measure in models 1 and 2 and the centrality measure in models 3 and 4, and both measures are in standard deviation units. All models control for municipality size, in logged hectares. Models 2 and 4 also control for cocaine cultivation in logged hectares. All models use HC2 heteroskedasticity consistent standard errors. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

coefficients on the logged variables), while a one standard-deviation increase in the measure of centrality is associated with a doubling of the amount of seizures. Appendix 1.6.5.2 uses maps to visualize the relationships between these variables.

The municipality of Santander de Quilichao, identified by Colombian authorities as an important trafficking hub (see Appendix 1.6.7), provides a useful sanity check. The two instruments for drug trafficking developed in this paper capture the municipality's potential importance for drug trafficking—it is in the top 3rd of municipalities on the criticality measure, and scores among the top 10% of municipalities on the centrality measure. This importance can also be seen in the data on interdictions; seizures of coca paste in Santander de Quilichao have exceeded 460 kilograms in a single year.

Having established that the trafficking instruments are correlated with seizures of cocaine,

I next show in Table 1.5 that they also have an effect on the emergence of dissident factions.

Table 1.5: Relationship Between Trafficking Instruments and Dissident Faction Presence

	Dependent variable: Dissident Faction Presence (binary)			
	(1)	(2)	(3)	(4)
(Intercept)	−0.34	0.20	−0.62*	0.15
	(0.31)	(0.41)	(0.26)	(0.40)
Criticality Measure	0.07**	0.07**		
	(0.02)	(0.02)		
Centrality Measure			0.06	0.09**
			(0.03)	(0.03)
Municipality Size	✓	✓	✓	✓
Coca Cultivation		✓		✓
Add. Controls		✓		✓
Num. obs.	260	259	260	259
F statistic	22.36	11.62	11.13	8.02

Notes: In all models, the dependent variable is a binary indicator for the presence of FARC dissident factions in a municipality as of 2020. The independent variable is the criticality measure in models 1 and 2 and the centrality measure in models 3 and 4, and both measures are in standard deviation units. All models control for municipality size, in logged hectares. Models 2 and 4 also control for cocaine cultivation in logged hectares, as well as the proportion of households in a municipality with electricity access, and municipality's terrain measured by taking the standard deviation of elevation in a high resolution raster DEM. Models 2 and 4 drop one observation due to missingness on the electricity variable. All models use HC2 heteroskedasticity consistent standard errors. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

I interpret these results as the reduced form of an instrumental variables analysis in which the first stage is missing. In a standard two-stage-least-squares (2SLS) equation, the reduced form estimate (also called the intent-to-treat (ITT)) is the numerator and is divided by the first stage or "compliance ratio" to estimate the local average treatment effect (LATE). Thus, the fact that the reduced form estimate is positive and (in most models) statistically significant suggests that there is a positive relationship between drug trafficking and the emergence of dissident factions, but because the ratio in the denominator is missing, I cannot estimate the size of this effect.

Last, as in any other instrumental variables analysis, it is important to rule out potential violations of the key assumptions. In Models 2 & 4 I address three in particular. First,

some municipalities that are useful for trafficking cocaine also produce it, so I control for production to isolate the effect of trafficking. Second, by construction, the instruments are influenced by terrain and by transportation networks. This introduces two potential exclusion restriction violations. First, rough terrain is thought to be favorable to insurgency, and it is possible that the criticality measure might assign larger weight to municipalities with important passes through a mountainous region, as such municipalities may be particularly costly to circumvent. I address this concern by including a control variable for rough terrain (Yamazaki et al. 2017). Second, both measures assign higher weight to municipalities along potential transportation networks, but proximity to transportation networks might make municipalities generally wealthier, and therefore more attractive targets for armed groups. I address this concern by including a control variable for the proportion of the population with electricity access, which in this context is a proxy for wealth or income.

In both cases, including these additional control variables fails to diminish the estimated effects of the trafficking instruments on dissident emergence, suggesting that these variables do not represent problematic violations of the exclusion restriction.

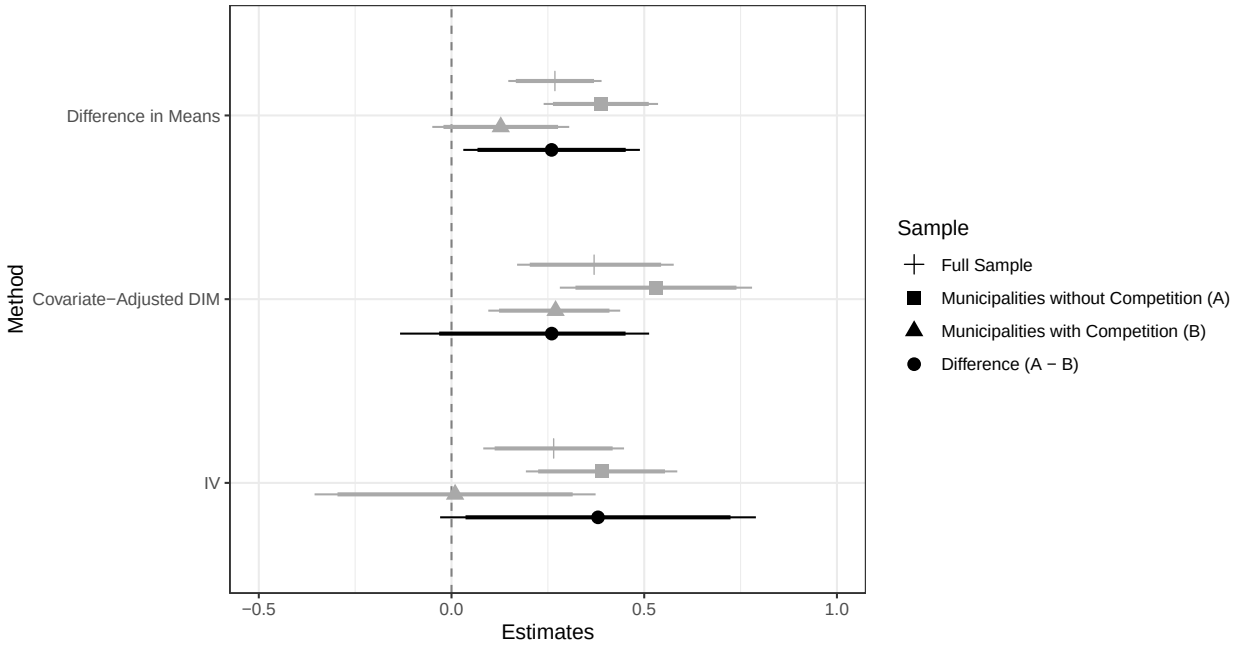
1.4.4 Competition

1.4.4.1 Measurement & estimation

The final set of analyses in this paper investigates whether competition from other armed groups, specifically the AGC and ELN, mitigated the effect of cocaine production on the emergence of dissident FARC factions. Here I employ the same set of empirical strategies as before, first showing unadjusted estimates, then adjusting for observed covariates and using instrumental variables to address potential unobserved confounders. To evaluate the heterogeneous effects with regards to competition, I split the sample into municipalities with and without competition, estimate the covariate-adjusted or instrumented effects of cocaine production in each sample, and take the difference.

Figure 1.3 presents the results of these empirical strategies. For each approach, I show estimates for the effect of cocaine on dissident resurgence in the full sample, in only municipalities without competition (A), in only municipalities with competition (B), and the difference between A and B, which represents the extent to which competition reduces the effect of cocaine on dissident resurgence.

Figure 1.3: Estimates for Effect of Cocaine Production in Full & Split Sample



Notes: In all models, the dependent variable is a binary indicator for the presence of FARC dissident factions in a municipality as of 2020, and the independent variable is a binary indicator for cocaine production, which takes a value of 1 if the municipality's annual cocaine cultivation ever reached at least 100 hectares, and 0 otherwise. The full sample consists of 260 FARC municipalities, with 2 observations dropped in the covariate-adjusted models due to missingness. I split the sample into municipalities without competition (A), which are municipalities with no AGC or ELN presence as of 2012, and municipalities with competition (B), which are municipalities with AGC or ELN presence as of 2012. In the covariate-adjusted model, I generate weights on the covariates of interest (literacy, electricity, population, percent rural, rough terrain, highway coverage, and the three distance variables) with kernel balancing, and estimate the model for each subsample using weighted least squares. In the IV model, the instrument is a municipality-level index of soil (acidity, nitrogen, carbon, and drainage) and weather (temperature, sunlight hours, annual rainfall, and humidity) variables combined using predictions from a logistic regression. All models used HC2 heteroskedasticity consistent standard errors. Confidence intervals for the difference between the covariate-adjusted estimates were generated using 10000 bootstrap simulations. Thick and thin bars represent 90% & 95% confidence intervals respectively. For a table version, see appendix 1.11

The results are broadly consistent with hypothesis 2. Across all models, the point estimates for the effect of cocaine production on dissident emergence in municipalities with no competition are consistently larger than the point estimates in the subsample with compe-

tition. Estimates for this difference range from 0.26 in the unadjusted difference in means model to 0.38 in the instrumental variables model. The IV estimates imply that cocaine-producing FARC municipalities were 39 percentage points more likely to see the emergence of dissident FARC factions in the absence of competition from rivals, but that this effect was reduced to about 1 percentage point in contested territory. These results should be interpreted with some caution however, as the difference is not statistically significant at conventional levels in the covariate-adjusted model, and is significant only at the 90% level in the IV model.

In addition to the assumptions required for covariate adjustment or instrumental variables discussed in previous sections, these results for the differential effect of cocaine production in areas with and without competition rely on a further assumption that competition and cocaine production are independent, or at least independent after conditioning on the covariates. I provide evidence for such independence in appendix 1.12.

1.5 Conclusion

I set out to explain the emergence of dissident factions in the aftermath of Colombia's peace agreement. Drawing on previous research on civil war initiation, I proposed a logic of profit-seeking, in which dissident commanders defected to profit from the illicit drug trade.

I presented an array of descriptive and statistical evidence for these arguments. First, I showed that dissident FARC factions were much more likely to emerge and expand in cocaine-producing areas, exploiting variation in cocaine cultivation caused by weather and soil to address the concern that this relationship might be driven by omitted variables. Second, I created a set of instruments for cocaine trafficking by simulating routes from production sites to the border, and showed that these measures predicted dissident faction emergence even after adjusting for cocaine production. Finally, though the statistical evidence is inconclusive, I show that this effect may have been weaker in territory where dissident factions faced

competition over these resources from rival armed groups.

Broadly, my results highlight a mechanism for rebel defection during peace processes that previously received little attention in the literature—opportunities for profit-seeking by rebel commanders. This result has implications for peacebuilding both in Colombia and beyond. In Colombia, shortly after President Gustavo Petro took office, he opened the door to talks with all of the country's armed groups, including the ELN, AGC, and the FARC dissidents (Guerra and Hege 2022). My findings suggest that any agreement with these groups must address the underlying economic incentives created by the drug trade, or it risks replicating the pattern of rebel defection and conflict resurgence that has hindered previous peace processes. A similar logic may apply in other contexts like the DRC, where the number of armed groups in resource-rich regions like Kivu has expanded despite the implementation of three successive DDR programs intended to demobilize them (Vlassenroot, Mudinga, and Musamba 2020).

This research has a number of limitations that leave fruitful ground for future research. First, while my analysis focuses on the presence of dissident factions at the municipality level to make inferences about their origins, finely-grained quantitative data on the strength of dissident factions or qualitative evidence on dissident commanders might allow future research to more precisely assess the facilitating or motivating effects of resources. Second, this analysis treats the value of territory as static and therefore independent of strategic interactions among groups. Future research could apply more dynamic empirical models in which control over certain production sites renders certain drug trafficking routes particularly important, for example, or where competition with one incumbent group might lead to cooperation with another. Finally, my finding that incumbent armed groups may have impeded the expansion of dissident factions raises the question of whether consolidation of *state* control could have had a similar effect. Recent interventions to improve governance quality and capacity in areas contested by armed groups provide a promising path forward on this front (R. A. Blair et al. 2022), and future studies of interventions specifically tailored

to resource-rich areas might be particularly fruitful.

1.6 Supporting Information

1.6.1 Data Sources

Table 1.6: Data Sources

Data	Source	Site
Weather	Institute of Hydrology, Meteorology and Environmental Studies (IDEAM)	ideam.gov.co
Soil	Soil and Terrain Database for Latin America and the Caribbean (SOTERLAC)	data.isric.org
Census	National Administrative Department of Statistics (DANE)	datosabiertos.esri.co; dane.gov.co
Terrain	Multi-Error-Removed Improved Terrain DEM	hydro.iis.u-tokyo.ac.jp
Highways	National Roads Institute (INVIAS)	inviasopendata-invias.opendata.arcgis.com
Cocaine	Ministry of Justice and Law (MJD)	minjusticia.gov.co
Flooding	National Unit for Disaster Risk Management (UNGRD)	portal.gestiondelriesgo.gov.co
Crops	Ministry of Agriculture and Rural Development (MADR)	agronet.gov.co
Armed Group Presence	Institute for Peace and Development Studies (Indepaz)	indepaz.org.co
Transit Cost Raster	Malaria Atlas Project (MAP)	malariaatlas.org

1.6.2 Covariate Adjustment Robustness

Table 1.7: Balancing Weights at Varied kbal Bandwidths

Bandwidth	Estimate of Y ∼ D	Effective Sample Size (Control)	Omnibus F-stat for D ∼ X
15.37	0.37	12.13	0.78
10.00	0.39	12.37	0.37
5.00	0.42	17.76	4.86
2.50	0.44	20.06	4.97

Notes: Kernel balancing allows for manual tuning of the bandwidth scaling factor in the calculation of gaussian kernel distance equivalent to the entire denominator $2\sigma^2$ of the exponent. The default is to search for the value which maximizes the variance of the kernel matrix. Here, each row shows the results of manually varying this parameter, with the second column showing the weighted estimate for outcome of dissident presence variable on the binary treatment of cocaine production, the second column shows the effective sample size of the weighted control units, and the fourth column shows the omnibus F-statistic for the weighted regression of the treatment variable on all covariates.

1.6.3 Production Instrument Data

1.6.3.1 Construction

Table 1.8: Coefficients for Regression-based Instrument Construction

	Dependent Variable:	
	Coca Production (Log ha.)	*Coca Producer (Binary)*
	(1)	(2)
(Intercept)	1.822	3.416
	(2.301)	(2.441)
Soil PH	-1.340^{***}	-1.803^{***}
	(0.305)	(0.373)
Temperature Range	-0.001	-0.009
	(0.027)	(0.033)
Soil Nitrogen	0.098	0.015
	(0.127)	(0.126)
Soil Carbon	-0.002	-0.001
	(0.002)	(0.002)
Sunlight Hours	0.942^{**}	0.709^{*}
	(0.301)	(0.313)
Humidity Range	-0.030^{***}	-0.050^{***}
	(0.007)	(0.015)
Annual Rainfall	0.002^{***}	0.001^{**}
	(0.000)	(0.000)
Soil Drainage	3.163^{***}	2.082^{***}
	(0.487)	(0.556)
Num. obs.	260	260

Notes: Model 1 is an OLS regression where the dependent variable is a continuous measure of coca cultivation (log max annual production pre-2012) and the independent variables are the soil and weather variables that comprise the instrument. Model 2 is a logistic regression where the dependent variable is a binary measure of coca cultivation (max annual production pre-2012 > 100 in any year) and the independent variables are the soil and weather variables that comprise the instrument. Both models use HC2 standard errors. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

1.6.3.2 Output

Figure 1.4: Distribution of Suitability Index (OLS)

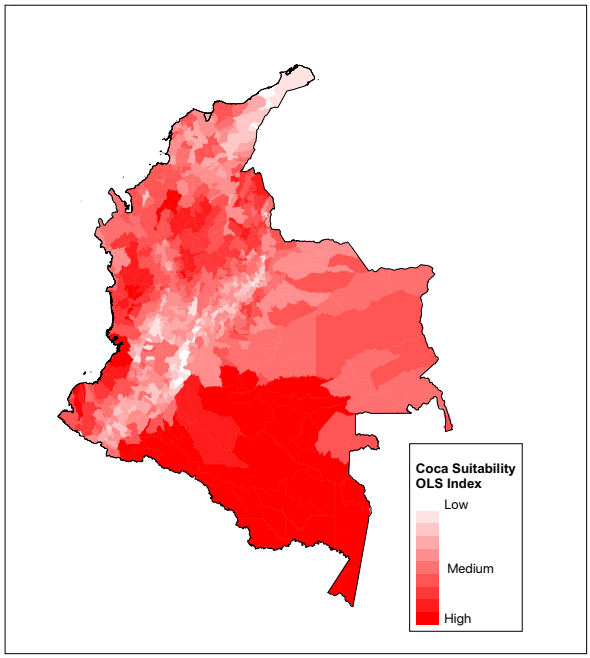
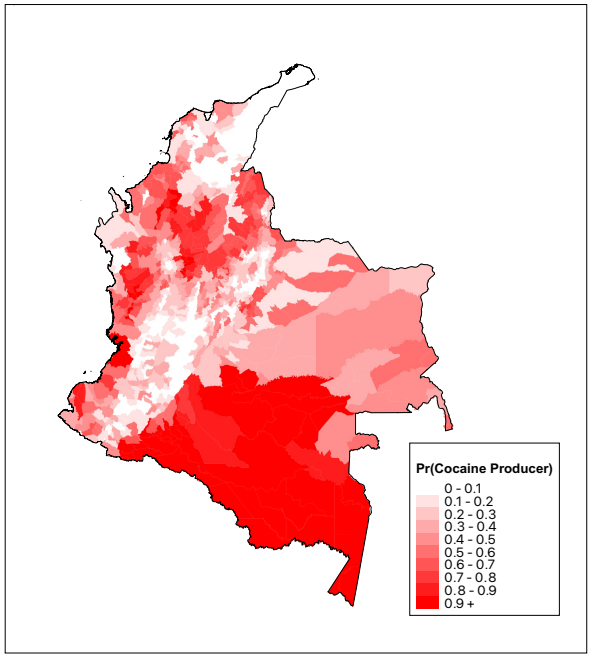


Figure 1.5: Distribution of Suitability Index (Logit)



1.6.4 Production Robustness Checks

Table 1.9: IV Reduced Form with Spatial (Conley) Standard Errors

	Dependent Variable: *Dissident Presence (Binary)*				
	(1)	(2)	(3)	(4)	(5)
Radius	1000km	750km	500km	200km	100km
(Intercept)	0.25***	0.25***	0.25**	0.25**	0.25**
	(0.06)	(0.07)	(0.08)	(0.09)	(0.08)
Weather & Soil	0.05*	0.05*	0.05	0.05*	0.05**
Instrument	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)
Num. obs.	260	260	260	260	260

Notes: The instrument is a municipality-level index of soil (acidity, nitrogen, carbon, and drainage) and weather (temperature, sunlight hours, annual rainfall, and humidity) variables combined using principal components analysis. The reduced form model regresses dissident presence, a binary indicator for the presence of FARC dissident factions in a municipality as of 2020, on the instrument. All models use Conley standard errors to adjust for potential spatial dependence in the error term (Conley 1999). I use municipality centroids and distance cutoffs for potential clustering of 100, 200, 500, 750, or 1000 km. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

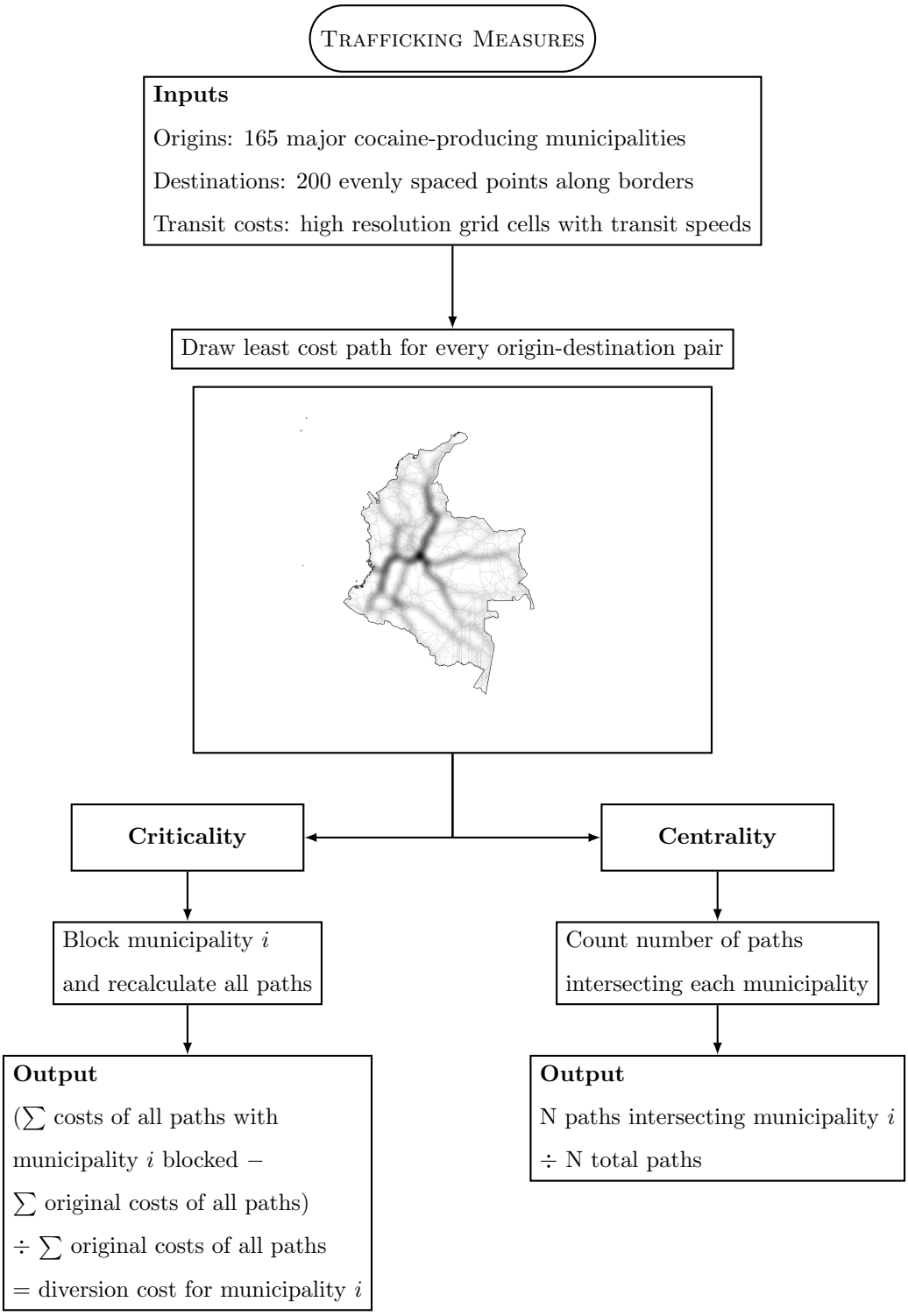
Table 1.10: Production IV: Drop Rain from Weather & Soil Index

	(1)	(2)	(3)	(4)
(Intercept)	0.19***	−0.22	0.31***	−0.10
	(0.05)	(0.23)	(0.05)	(0.23)
Coca Cultivation (log ha.)	0.07***	0.07***		
	(0.01)	(0.01)		
Coca Cultivation (binary)			0.27**	0.28**
			(0.10)	(0.10)
Agriculture Control		0.05		0.05*
		(0.03)		(0.03)
Floods Control		−0.03		−0.05
		(0.07)		(0.07)
Num. obs.	260	260	260	260
First-stage F-stat	169.33	169.77	234.90	231.02

Notes: The dependent variable is a binary indicator for the presence of FARC dissident factions in a municipality as of 2020. The independent variable is a binary indicator that takes a value of one if annual cocaine cultivation in a municipality exceeded 100 hectares between 1999-2012. The instrument is a municipality-level index of soil (acidity, nitrogen, carbon, and drainage) and weather (temperature, sunlight hours, and humidity) variables combined using predictions from ols or logistic regression. The 2SLS model with controls includes controls for agricultural productivity, defined as the logged maximum annual hectares under cultivation with legal crops between 2009 and 2014, and flooding, defined as the average number of flood-related alerts per municipality per year between 2017-2020. All models use HC2 heteroskedasticity consistent standard errors. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

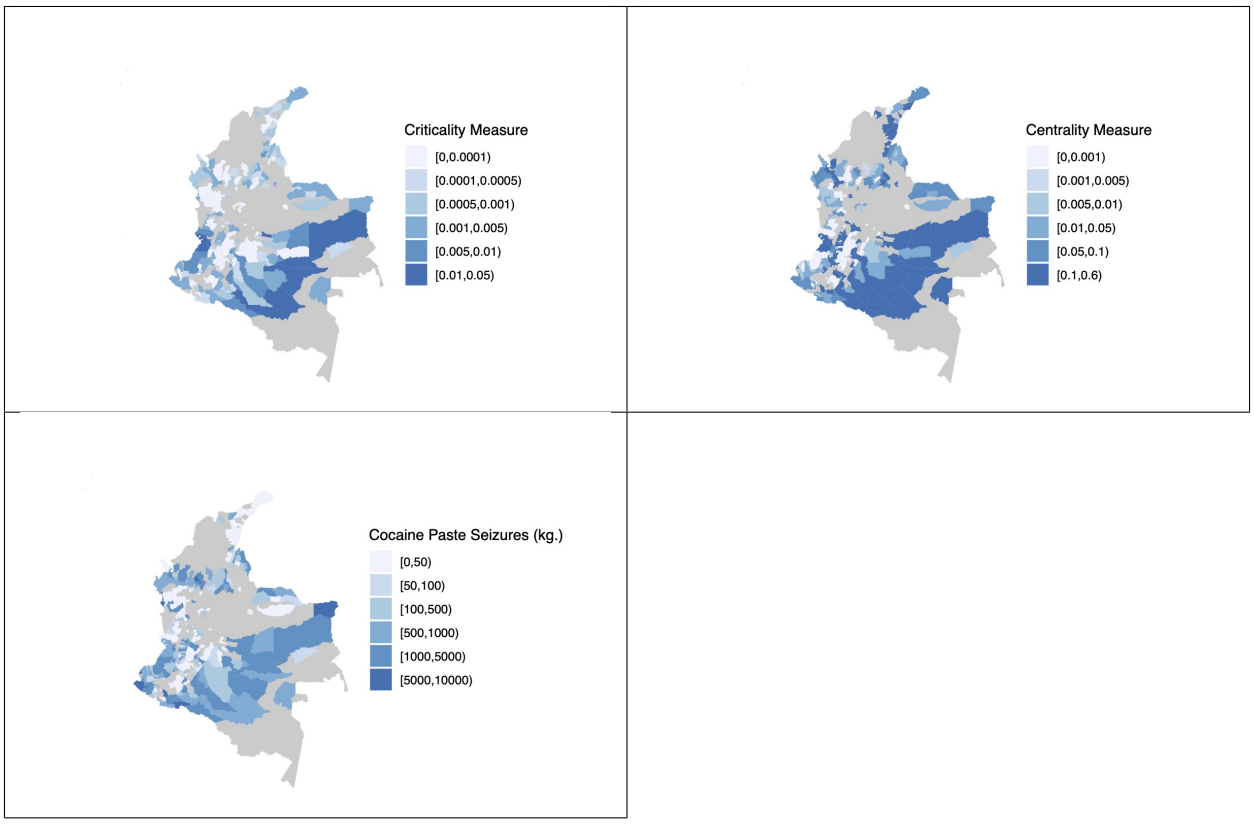
1.6.5 Trafficking Instrument Data

1.6.5.1 Measurement Strategy



1.6.5.2 Mapping Trafficking Measures and Seizures

Figure 1.6: Mapping Cocaine Trafficking in Former FARC Territory: Instruments and Cocaine Seizures



Notes: Each pane represents a map of Colombia shaded at the municipality level. Municipalities in light grey are municipalities with no FARC presence as of 2012. Shades of blue represent levels of the three variables of interest. Cocaine paste seizures are measured as the maximum annual seizure in a municipality between 1999-2012. The centrality and criticality measures are described in section 4.3.1.

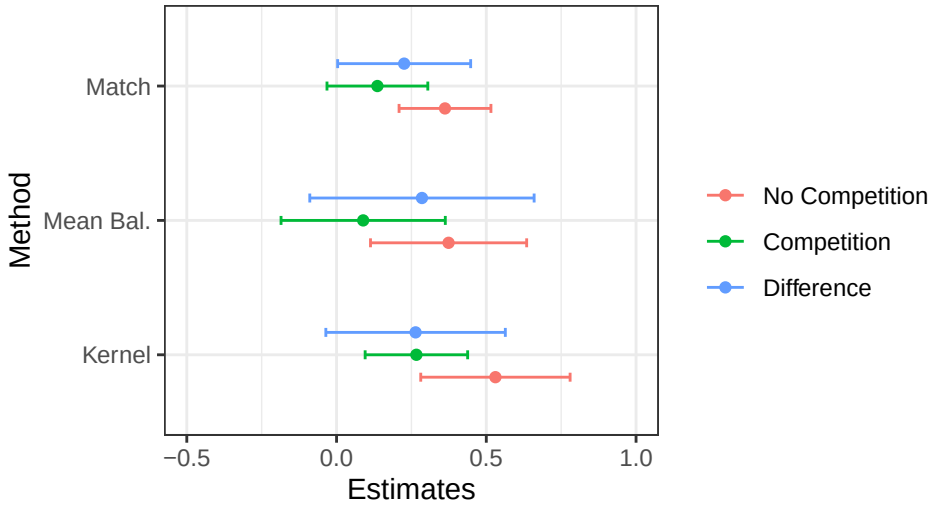
1.6.6 Competition Results

Table 1.11: Split Sample Estimates for Cocaine Cultivation by Competition Variable

Method	Sample	Est.	95% CI	90% CI
Difference in Means	Full Sample	0.27	0.15, 0.39	0.17, 0.37
Difference in Means	No Competition Municipalities (A)	0.39	0.24, 0.54	0.26, 0.51
Difference in Means	Competition Municipalities (B)	0.13	-0.05, 0.31	-0.02, 0.28
Difference in Means	Difference (A - B)	0.26	0.03, 0.49	0.07, 0.45
Covariate-Adjusted	Full Sample	0.37	0.17, 0.58	0.2, 0.54
Covariate-Adjusted	No Competition Municipalities (A)	0.53	0.28, 0.78	0.32, 0.74
Covariate-Adjusted	Competition Municipalities (B)	0.27	0.1, 0.44	0.12, 0.41
Covariate-Adjusted	Difference (A - B)	0.26	-0.13, 0.51	-0.03, 0.45
IV	Full Sample	0.27	0.08, 0.45	0.11, 0.42
IV	No Competition Municipalities (A)	0.39	0.19, 0.59	0.22, 0.55
IV	Competition Municipalities (B)	0.01	-0.36, 0.37	-0.3, 0.31
IV	Difference (A - B)	0.38	-0.03, 0.79	0.04, 0.72

Notes: In all models, the dependent variable is a binary indicator for the presence of FARC dissident factions in a municipality as of 2020, and the independent variable is a binary indicator for cocaine production, which takes a value of 1 if the municipality's annual cocaine cultivation ever reached at least 100 hectares, and 0 otherwise. The full sample consists of 260 FARC municipalities, with 2 observations dropped in the covariate-adjusted models due to missingness. I split the sample into municipalities without competition (A), which are municipalities with no AGC or ELN presence as of 2012, and municipalities with competition (B), which are municipalities with AGC or ELN presence as of 2012. In the covariate-adjusted model, I generate weights on the covariates of interest (literacy, electricity, population, percent rural, rough terrain, highway coverage, and the three distance variables) with kernel balancing, and estimate the model for each subsample using weighted least squares. In the IV model, the instrument is a municipality-level index of soil (acidity, nitrogen, carbon, and drainage) and weather (temperature, sunlight hours, annual rainfall, and humidity) variables combined using predictions from a logistic regression. All models used HC2 heteroskedasticity consistent standard errors. Confidence intervals for the difference between the covariate-adjusted estimates were generated using 10000 bootstrap simulations.

Figure 1.7: Comparing Results of Matching and Balancing Strategies



Notes: In all models, the dependent variable is a binary indicator for the presence of FARC dissident factions in a municipality as of 2020, and the independent variable is a binary indicator for cocaine production, which takes a value of 1 if the municipality's annual cocaine cultivation ever reached at least 100 hectares, and 0 otherwise. The full sample consists of 260 FARC municipalities, with 2 observations dropped in the covariate-adjusted models due to missingness. I split the sample into municipalities without competition (A), which are municipalities with no AGC or ELN presence as of 2012, and municipalities with competition (B), which are municipalities with AGC or ELN presence as of 2012. In the Kernel balancing and Mean Balancing models, I generate weights on the covariates of interest (literacy, electricity, population, percent rural, rough terrain, highway coverage, and the three distance variables) with kernel balancing and entropy balancing respectively, and estimate the model for each subsample using weighted least squares. In the matching model, I match on these variables using mahalanobis-distance nearest-neighbor matching. All models used HC2 heteroskedasticity consistent standard errors.

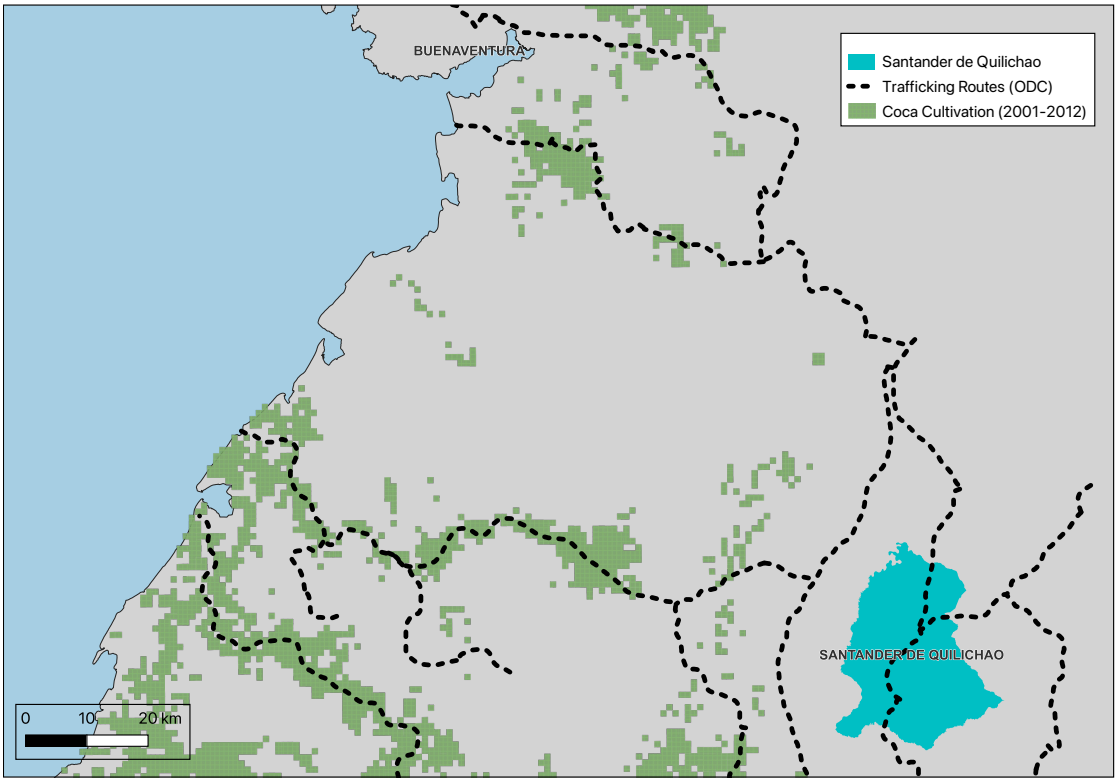
Table 1.12: Conditional Independence of Competition & Cocaine

	Dependent variable: AGC or ELN Presence (Binary)		
	(1) Matching	(2) Mean Balancing	(3) Kernel Balancing
(Intercept)	0.36***	0.42***	0.65***
	(0.05)	(0.05)	(0.13)
Cocaine Cultivation	0.07	0.04	−0.22
(binary measure)	(0.06)	(0.14)	(0.14)
Adj. R-sq.	−0.00	−0.00	0.04
Num. obs.	204	258	258
F statistic	1.20	0.07	2.48
N Clusters	102		

Notes: In all models, the dependent variable is a binary indicator for the presence of AGC or ELN in a municipality as of 2012. The independent variable is a binary indicator for a maximum annual cocaine cultivation (in hectares) above 100 hectares in a municipality between 1999-2012. I match and generate weights on the following covariates of interest: literacy, electricity, population, percent rural, rough terrain, highway coverage, and the three distance variables. Matching is implemented using mahalanobis distance nearest-neighbor matching without replacement. Mean balancing weights are generated using Hainmueller's entropy balancing procedure (Hainmueller 2012). All models use HC2 heteroskedasticity consistent standard errors. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

1.6.7 Santander de Quilichao as a Trafficking Hub

Figure 1.8: Santander de Quilichao at the Intersection of Trafficking Routes



Notes: Trafficking routes (dotted black) are reproduced from maps included in Ministry of Justice and UNOCD reports on Cauca (*Cauca* 2016) and Valle del Cauca (*Valle del Cauca* 2013). Geolocated coca cultivation data at 1km resolution (green) are from the Ministry of Justice's Drug Observatory (ODC), from 2001-2012.

CHAPTER 2

Either with us or against us: Rebel fratricide against DDR participants in postconflict Colombia

Disarmament, Demobilization, and Reintegration (DDR) programs have become a standard feature of civil war peace agreements, but they face many challenges. In Colombia, a carefully-designed DDR program for 14,000 former combatants from the FARC rebel group has been plagued by violent attacks against its participants by armed groups. Existing research on challenges to DDR typically focuses on proclivities for criminality among former combatants or stigmatization by civilians. In this paper, I document a distinct challenge to DDR that I argue emerges when rebel groups factionalize over the decision to demobilize; namely, fratricide by rebel splinter groups that reject peace. I argue that the success of DDR threatens the interests of rebel splinter groups, and that violence against DDR participants is a strategic response to this threat. I find evidence for this argument in the pattern of violence against former FARC combatants in Colombia. Using a difference-in-differences approach, I show that the emergence and expansion of FARC factions opposed to Colombia's 2016 peace agreement caused a surge in fatal attacks against DDR participants. I also provide qualitative evidence illustrating the mechanisms driving this pattern. These findings highlight the need for a distinct DDR model for rebel groups at risk of factionalization.

2.1 Introduction

On September 7th, 2022, armed men shot and killed Reude Suárez Guerrero in his home in Tibú, a town located on Colombia's northeastern region of Catatumbo (Ramírez 2022). Guerrero was a former member of the FARC, Colombia's largest rebel group, who had demobilized when the FARC signed a peace agreement with the government in 2016. In the years following the peace agreement, he had worked with a cooperative set up by former combatants called the Association for Reconciliation and Peace in Catatumbo. Several months after the incident, Colombian authorities captured several men involved in Guerrero's killing, and found that they belonged to the FARC's 33rd Front, a faction of the FARC that had rejected the 2016 peace accord. According to officials, the 33rd Front had pressured Guerrero to join, and when he refused, ordered his killing ("A la cárcel" 2023).

Guerrero's is not an isolated case. As of early 2023, more than 350 out of the roughly 14,000 members of the FARC who demobilized under the peace agreement had been killed in violent attacks, with many more injured or forcibly displaced ("El Secretario" 2023). And, while the majority of these incidents remain unsolved or under investigation, a striking number of cases involve deliberate attacks by groups like the 33rd Front; that is, FARC splinter groups that rejected the peace agreement and remain at war ("48,21%, avance" 2020).

Organized violence against former combatants in DDR programs is an important and understudied phenomenon. Both earlier DDR programs in Colombia such as one involving the Popular Liberation Army (EPL) in the 1990s, as well as DDR programs in other countries like the Democratic Republic of Congo have seen a pattern of deliberate attacks on former combatants by organized armed groups (Buitrago Roa and Suárez Gutiérrez 2017; Richards 2016). This type of violence poses a critical threat to peace, as demobilized may be compelled to return to arms to protect themselves, while members of other armed groups in negotiations with the government may refuse to demobilize after observing the fate of DDR participants.

The existing literature on DDR programs offers few explanations for why DDR participants are targets of organized violence. Some studies focus on criminality as a cause of violence against DDR participants, proposing job training or small business loans to prevent unemployed excombatants from turning to high-risk activity (Blattman and Annan 2016). Other studies focus on the problem of societal stigmatization, proposing activities to foster forgiveness and acceptance and reduce the demand for retribution (G. Blair et al. 2021). Yet neither jobs training nor reconciliation with the civilian population are adequate solutions to violence perpetrated by organized armed groups.

In the theoretical section of this paper, I argue that when rebel groups fragment during a peace process, successful demobilization programs pose a threat to the strategic interests of splinter groups that remain at war. Successful DDR programs deprive these splinter groups of experienced members, bring sensitive intelligence within reach of authorities, and to the extent that demobilized rebels are politically active, undermine splinter groups' legitimacy. In light of these threats, splinter groups have a strong incentive to disrupt DDR programs, including through violence.

In the empirical section, I provide evidence linking the emergence of dissident FARC splinter groups after the 2016 peace agreement to the surge in violence against demobilized FARC members. I employ a difference-in-differences design, leveraging the timing of splinter group emergence in different regions of Colombia, to show that these splinter groups increase rates of fatal attacks for DDR participants. I further demonstrate that this effect is unique to DDR participants, and does not apply to the broader civilian population. I also include analyses that rule out several alternative explanations for this pattern.

I build on and contribute to several areas of the literature on civil war and peacebuilding. My results advance scholarly understanding of rebel group fragmentation by highlighting the unique challenges and incentives splinter groups face as they confront both the state *and* other rebel factions. My findings also advance an extensive literature evaluating DDR programs by identifying an important but largely overlooked challenge to the success of

such programs. In this regard, my findings also have policy implications for the design and implementation of peace agreements—in agreements involving groups at risk of fragmentation, DDR programs may require additional resources to ensure the physical security of participants.

2.2 Theoretical Framework

2.2.1 The State of Evidence on Violence and DDR Programs

Why do DDR programs like Colombia's often struggle to achieve their goals? While research on DDR programs is extensive and diverse (Sharif 2018), two key sets of challenges dominate this literature: one concerns DDR participants' involvement in crime and violence; and a second concerns societal stigmatization of DDR participants.

Research on criminality and violence emphasizes DDR participants' skills with weapons and combat, their network ties, and their lack of skills and opportunities in the formal economy (Gilligan, Mvukiyehe, and Samii 2013; Blattman and Annan 2016; Daly, Paler, and Samii 2020). Studies in a variety of settings have found high rates of criminal recidivism among demobilized combatants, a phenomenon that threatens to perpetuate the cycle of violence and instability in postconflict countries. An important policy implication of this research is that DDR programs must include components like job training or small business loans that allow former combatants to earn a livelihood in the formal economy.

Research on DDR programs also highlights challenges associated with societal stigma against former combatants (Podder 2012; Salih et al. 2018; G. Blair et al. 2021). Civilians may perceive demobilized combatants as dangerous, untrustworthy, or spiritually tainted by bloodshed. Moreover, the civilian populations victimized by rebel violence may desire retribution against former combatants beyond what a negotiated settlement stipulated. In the extreme, civilians may take matters into their own hands and participate in extrajudicial reprisals. However, even less extreme manifestations of stigma such as job discrimination

or social shunning can impede former combatants' reintegration into civilian life. Proposed solutions to this problem range from formal reparations and apologies by former rebels, to interventions by religious or traditional authorities designed to promote reconciliation.

While both criminality and societal stigma could cause violence against DDR participants, such violence is likely to be sporadic and unorganized. I contend that deliberate and systematic violence against DDR participants like the pattern observed in Colombia requires a distinct explanation. I develop a theoretical argument that builds on insights from the literatures on wartime desertion and defection and on rebel group fragmentation.

2.2.2 Violence as a Strategic Response by Splinter Groups to the Threat of DDR

Research on rebel group fragmentation suggests that peace negotiations often catalyze factional infighting within rebel groups (Duursma and Fliervoet 2021). Rebel commanders may have ideological disagreements over whether proposed concessions are consistent with their aims, or pragmatic disputes over the distribution of power and benefits. This literature also suggests that fragmentation can have violent consequences, as rebel splinter groups compete for dominance (Cunningham, Bakke, and Seymour 2012). While this existing literature on fragmentation focuses on violence between *armed* factions, I argue that splinter groups also have a strong incentive for violence against *demobilized* rebels.

DDR programs threaten rebel splinter groups first, because they represent a source of competition for members. Even in conflict zones, combat experience can be a rare and valuable resource, and the greater the share of the rebel group that participates in DDR, the fewer experienced recruits the splinter group is able to retain. This not only damages the group's fighting capacity directly, but also forces it to invest resources into attracting and training fresh recruits. Moreover, a successful DDR program may increase the risk of desertions for the splinter group by demonstrating the viability of life outside of the group for former members. Desertion not only costs armed groups valuable members, but also

poses a threat to their internal cohesion and battlefield performance (Lyall 2020).

A second reason DDR programs threaten splinter groups is that DDR participants may share intelligence with the state. This could include both intelligence pertinent to a splinter group's operations and strategy, as well as testimony that might implicate splinter group members in war crimes.

Most modern civil wars involve irregular warfare, a style of conflict in which information is valuable and scarce (Kalyvas 2006). Examples of potentially damaging strategic intelligence that DDR participants might share include a rebel groups' sources of weapons and financing, its command structure, the location of camps, and the identities of covert operatives. Moreover, even if DDR participants preferred to withhold information from the state, they could not credibly commit to refrain from intelligence sharing, particularly if the state employs coercive methods.

Many peace processes also involve truth and reconciliation provisions, requiring or seeking testimony from former combatants regarding atrocities. Such testimony might implicate members of the splinter group in war crimes, increasing the likelihood of prosecution and severe punishment if they are captured.

Last, a successful DDR program may threaten splinter groups' legitimacy. Peace agreements increasingly include provisions granting demobilized rebel groups avenues for political participation by granting them seats in the legislature or the opportunity to compete in elections (Matanock 2017). To the extent that demobilized rebels are able to demonstrate that the group's agenda can be achieved through nonviolent political means, the raison d'etre of the splinter group's violent campaign is called into question. Such a threat to legitimacy may affect the splinter factions' ability to maintain loyalty and cohesion, attract recruits, and govern the population.

In light of these threats, I argue that splinter groups have a strong incentive to disrupt DDR programs, including through fratricide. First, as a response to the problem of recruit-

ment and desertion, violence may offer splinter groups both a means to deter and punish DDR participation by their own members, and a way to coercively recruit rebels who demobilized. Second, targeted violence against DDR participants known to possess the most sensitive information about the group may both prevent them from sharing this information with state security forces, as well as deter other DDR participants from sharing such information. Again, targeted assassinations of suspected informants is a common feature of civil war violence. And third, violence targeting politically active DDR participants may disrupt their ability to campaign and deter further political action, a strategy rebel groups frequently employ to undermine political opponents.

2.3 Study Setting

2.3.1 Colombia's Peace Agreement and DDR Program

In 2016 the Colombian government reached a comprehensive peace agreement with the FARC, the country's largest and oldest rebel group. Within the first year of the deal's ratification, roughly 13,000 FARC members (about 95% of the total membership) disarmed and demobilized at 26 camps located in former conflict zones.

Among other provisions, the peace agreement stipulated that former combatants would receive a stipend worth 90% of the minimum wage, amnesty or reduced sentences for crimes conditional on providing testimony to the truth and reconciliation commission, and the ability to participate in elections under the auspices of a legal political party. After completing the DDR process, some former combatants left the DDR camps to seek opportunities elsewhere, while others remained to work on subsidized economic projects located in or around the camps.

2.3.2 The emergence of Dissident FARC factions

As the vast majority of FARC members turned in their weapons and entered the DDR program, several mid-ranking FARC officers announced their opposition to the peace agreement and formed splinter factions commonly referred to as "FARC dissident" groups. Composed of just a few hundred members at the beginning of the process, dozens of FARC dissident groups emerged in the ensuing years and expanded rapidly by attracting additional defectors from the peace process and recruiting new members. As of 2022, an estimated 4500 FARC dissidents were active in 162 Colombian municipalities (Valdés 2022).

2.3.3 Violence against DDR participants

The FARC dissidents' defection has not been the only setback to the success of the peace agreement; of the roughly 14,000 former FARC combatants who participated in DDR, more than 350 were killed within six years ("El Secretario" 2023). Violent attacks have injured dozens more and forcibly displaced several hundred from their homes.

The causes of this violence remain poorly understood. The FARC's political party blames the government for failing to provide adequate protection, while statements by the FARC's political opponents insinuate that the violence is related to criminal activity by the victims ("Las pullas" 2020). Meanwhile, early scholarly efforts have offered several theories, ranging from societal stigma and persecution against former combatants (Valencia Agudelo 2021), to the idea that they are caught in the crossfire in disputes for territorial control (Charles, Baysal, and Forero 2020).

Official statistics however, reveal a surprising pattern. While the government has yet to identify the perpetrators for the majority of attacks, the cases that have been solved reveal that the most common perpetrators were FARC dissidents. 44% of solved attacks were perpetrated by FARC dissidents compared to just 11% for the next most common perpetrator, a guerrilla group called the ELN ("48,21%, avance" 2020).

These numbers provide at least face plausibility to the argument I intend to test; namely, that violence by rebel splinter groups is a significant threat to DDR programs. Nevertheless, these statistics alone are not conclusive evidence of this claim. First, because the majority of cases remain unsolved, the true pattern could be different, particularly as the government may have a stronger incentive to investigate the cases where the suspected perpetrator is a rebel group. Second, and perhaps more importantly, without further analysis, it is impossible to rule out the possibility that the emergence FARC dissidents in has caused a general increase in fatalities for the civilian population as a whole, and FARC DDR participants are not being uniquely singled out.

2.4 Observable Implications

My theoretical framework predicts that when rebel groups fragment during peace processes, splinter factions have a strong incentive for violence against DDR participants. An observable implication of this argument for the Colombian case is that the emergence and expansion of FARC dissident groups in the aftermath of Colombia's peace agreement should cause increased violence against FARC DDR participants. I focus on temporal and spatial variation in the pattern of violence and dissident group expansion. Formally, I hypothesize that: *(H1) the emergence of FARC dissidents in a region should increase the level of violence against former FARC DDR participants in that region.*

My theoretical argument suggests that DDR participants are not just incidental victims caught in the crossfire of conflict, but are singled out as targets by splinter groups. In the context of Colombia, I argue that the FARC dissidents are deliberately targeting former FARC members in the DDR program. Formally, I hypothesize that *(H2) the rate of violence against former FARC DDR participants should increase more due to the emergence of FARC dissidents relative to the rate of violence against other civilians.*

2.5 Data

2.5.1 Dependent Variable: Violence Against FARC DDR Participants

The main outcome of interest in this analysis is violence against former FARC combatants in the DDR program. I operationalize this variable as an annual department-level rate of fatal attacks against former FARC combatants. In Colombia, departments are administrative units analogous to states or provinces. I employed a two-pronged strategy to collect data on victims of fatal attacks. First, I synthesized reports of such attacks compiled by three separate groups:

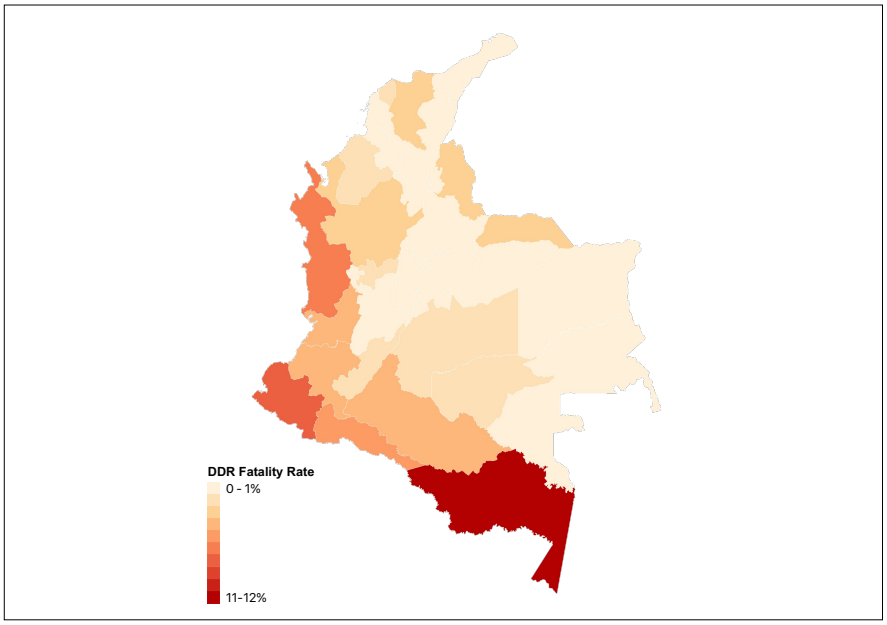
- The Institute for Peace and Development Studies (Indepaz), a Colombian think tank and conflict monitor.
- The National Council for Reincorporation (CNR), the FARC's delegation to the monitoring and verification commission established by the peace agreement.
- The Center for Research and Public Education (CINEP), a conflict monitoring NGO with unique coverage of remote areas due to its access to reports from local Catholic Churches.

Second, I independently verified these cases by searching local and national media, as well as press releases from the public prosecutor's office. I also collected supplementary details on each case such as the victims' age, sex, and occupation, among others. This data spans 2017-2022.

To calculate the *rate* of fatal attacks, I also collected data on the department-level population of FARC DDR participants. For 2017, I use the initial count of FARC DDR participants collected at the 26 official demobilization camps as well as detention facilities from which FARC POWs were released under the peace agreement. For the following years, I use department-level population data from Colombia's National Agency for Reincorporation (ARN). These statistics allow me to calculate a precise, time-varying per capita measure of

fatal attacks on DDR participants. Figure 2.1 maps the fatality rate by department, summed over the entire study period.

Figure 2.1: DDR Fatality Rate by Department (2017-2022)



2.5.2 Independent Variable: Dissident Group Expansion

The main independent variable in this analysis is FARC dissident presence in an area. I operationalize dissident FARC presence at the department-year level as a dummy variable that takes the value of 1 starting the first year that FARC presence is reported in the department in that year.

The primary source for data on dissident FARC presence come from the Colombian think-tank Indepaz.[1] Indepaz produces lists of departments with FARC dissident presence each year, which I clean and transform into a department-year panel covering 2017-2022.

[1]Indepaz releases periodic reports on the presence of armed groups at the municipal level which are produced by synchronizing across five sets of sources: (1) a comprehensive database of news reports; (2) official sources such as the police, the office of the attorney general, and the human rights observatory, among others; (3) reports from independent national NGOs; (4) communication with local or regional organizations; and (5) field work by Indepaz researchers (Posso et al. 2021, Indepaz 2013).

2.6 Empirical Strategy

To test the first hypothesis, I employ a difference in differences strategy that exploits variation in the emergence of dissident FARC groups in time and space. I compare the difference in fatal attacks before and after the emergence of dissident groups in a department to the difference in the same period across departments where dissident groups did not emerge. Critically, this approach addresses both time-invariant differences between different regions that might lead to differences in fatal attacks as well as general trends in attacks over time.

This design addresses a core problem of inference, in that areas where FARC dissidents emerge are likely to be significantly different from other areas in important ways. For example, dissident FARC groups were more likely to emerge in areas valuable for drug production and trafficking, and such areas might have high levels of violence regardless of whether or not dissident FARC groups were present.

For the difference-in-difference design, I need not assume that departments with and without FARC dissidents are otherwise identical. Instead, I rely on the weaker assumption of parallel trends; i.e., that the trend over time in fatalities in departments with and without FARC dissidents would have remained similar.

To test hypothesis 2, I conduct the same set of analyses but the dependent variable I substitute the department-level homicide rate for the general public. I then take the difference between the estimated effect of dissident FARC emergence on rate of DDR-participant fatalities and on the rate for the general population. This triple-difference approach is included to test whether, as I hypothesize, DDR participants are uniquely vulnerable to splinter group violence.

2.7 Main Results

Table 2.1 shows the results of my main empirical strategy. In columns 1 and 2 I estimate the effect effect of dissident FARC presence on the rate of FARC DDR participant fatalities. Columns 3 and 4 show the results for a nearly identical analysis where the outcome of interest is homicide rates for the general population. In columns 5 and 6 I estimate the triple difference, subtracting the change in the overall homicide rate from the change in the FARC DDR participant homicide rate.

I include results for two estimation strategies. The first (columns, 1, 3, and 5) is a standard two-way fixed-effect estimator, that includes fixed effects for year and department and clusters standard errors at the department level. Recent developments in the literature on difference-in-difference designs suggest that in cases where different units receive treatment at different points in time—as is the case here with dissident FARC factions emerging in different departments in different years—the two-way fixed-effect estimator can be biased if the treatment effect is constant across units but heterogeneous within groups over time (Goodman-Bacon 2021). To address this problem, I follow a procedure developed by Callaway and Sant'Anna (2021) to estimate a "group-time average treatment effect" for each cohort of units that receive treatment at a given point in time, and then aggregating these cohort-level effects into an overall effect of participating in the treatment (columns, 2, 4, and 6).

Consistent with hypothesis 1, the estimates in columns 1 and 2 indicate that the presence of FARC dissidents causes an additional 3-4 fatal attacks per 1000 FARC DDR participants. By contrast, the estimates in columns 3 and 4 show no discernible effect of FARC presence on civilian homicide rates. Consistent with hypothesis 2, the estimates in columns 5 and 6 confirm that differencing out the negligible effects of FARC dissident presence on civilian homicide rates does not significantly attenuate the estimated effect on DDR participant fatalities.

Table 2.1: Effect of FARC Dissident Presence on DDR Fatality Rates

	DV = DDR Rate		*DV = Civilian Rate*		*Difference*	
	(1)	(2)	(3)	(4)	(5)	(6)
FARC Dissident Presence	$3.22^{\dagger}$ (1.85)	3.91^{*} (1.76)	0.01 (0.01)	0.01 (0.02)	$3.21^{\dagger}$ (1.85)	3.89^{*} (1.76)
Model	TWFE	C&S	TWFE	C&S	TWFE	C&S
Year FE	✓	✓	✓	✓	✓	✓
Dept. FE	✓	✓	✓	✓	✓	✓

Notes: The unit of analysis in all models is the department-year. The independent variable in all models is a binary indicator that takes a value of 1 if FARC dissidents are present in a department-year and 0 otherwise. The dependent variable in models 1 and 2 is the rate of fatal attacks on FARC DDR participants per 1000. The dependent variable in models 3 and 4 is the rate of homicides for all civilians per 1000. Models 5 and 6 estimate the difference and standard error of the difference between the estimates in models 1 and 3 and models 2 and 4. TWFE models use department and year fixed effects only. C&S models implement the Callaway and Sant'Anna (2021) staggered difference in differences estimator, using the not-yet-treated departments as the control group. Standard errors in all models are clustered on Department. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$; $^{\dagger}p < 0.1$

In the supporting information, I show that this result holds using alternative operationalizations of the dependent variable (SI 2.3). I also use an event-study design to show that estimates are not driven by pre-existing trends in fatality rates (SI 2.2).

2.8 Mechanisms

Why is the presence of FARC dissidents causing an increase in fatal attacks on FARC DDR participants? In the theoretical section of this paper, I proposed three potential reasons for splinter groups to target DDR programs, related to issues of recruitment and retention, information sharing, and political legitimacy.

2.8.1 Recruitment and Retention

My theoretical argument proposes first, that DDR programs threaten rebel splinter groups because they inhibit the group's ability to recruit experienced former combatants. In the

case of Colombia, I find evidence that the FARC dissident groups sought to recruit DDR participants, *and* that they used violence to punish and deter refusal.

The case of alias Ruede Guerrero described in detail in the introduction illustrates this mechanism. Reports indicate that the Frente 33 dissident group had repeatedly tried to recruit Guerrero and he had refused. The same accounts noted that not only did Frente 33 kill Guerrero, but they then threatened his family and forced them to flee ("A la cárcel" 2023). These details suggest that the Frente 33 intended to signal to other DDR participants that refusing recruitment would be severely punished. Several other documented cases follow a similar pattern, with DDR participants being killed by FARC dissident groups after refusing recruitment ("Fiscalía" 2020; "Hombres armados" 2020).

2.8.2 Information

My theoretical argument suggests that DDR programs threaten rebel splinter groups because DDR participants may have valuable and potentially damaging information. In the Colombian context, some evidence suggests that FARC dissident groups feared that participants in the demobilization program might serve as informants for their adversaries.

For example, the department of Cauca is one of the most dangerous zones for FARC DDR participants, with a total of 55 fatal attacks during the study period. Several FARC dissident groups emerged in the department of Cauca soon after the ratification of the peace agreement. The *Frente Dagoberto Ramos* dissident group operated in close proximity to one of the department's official DDR camps in the village of Monterredondo. Engaged in a fierce conflict with both the Colombian government and other armed groups active in Cauca, the *Frente Dagoberto Ramos* viewed the DDR participants as potential informants (Cardozo 2022). After multiple attacks on DDR participants in the area, the group eventually delivered an ultimatum to the village council demanding the expulsion of all remaining DDR participants. The message alleged that the DDR program was augmenting the network of informants in the area, and called the demobilization camp a "nest of toads", a common

epithet for informants (Soto 2020).

2.8.3 Legitimacy

Last, I argued that DDR programs might threaten rebel splinter group legitimacy if DDR participants engage in political activity. Under Colombia's peace agreement, the FARC was guaranteed seats in the legislature and allowed to form a political party, which initially called itself the "FARC Party" before later rebranding. Compared to the FARC, the FARC dissidents appear less focused on political ambitions and more focused on profits in the illicit economy. Nevertheless, whether for instrumental reasons or not, dissident groups largely continued to use the FARC's name, symbols, and ideology. Both the dissident FARC groups and the demobilized FARC's political party claimed to represent the legitimate continuation of the FARC and criticized the other as disloyal to the cause ("Iván Márquez" 2021; "Márquez" 2019).

Some of the violence experienced by DDR participants may have stemmed from this conflict over the FARC's political legitimacy. For example, during the 2018 election cycle, a dissident FARC group in the department of Arauca called *Frente 10* ambushed members of the FARC's political party at a campaign event, killing a bystander and destroying a campaign vehicle in the process ("Caravana" 2018). Later in the same year, dissidents attacked another of the party's political caravans in the department of Norte de Santander (Castillo 2018). In 2020, FARC dissidents attempted to assassinate the president of the FARC party ("Colombia foils" 2020), and in the 2022 election year, they planted a bomb in the party's political headquarters ("Policía atribuye" 2022).

2.9 Alternative Explanations

Next, I evaluate potential alternative explanations for this pattern. As noted earlier, a significant focus of the existing literature on threats is criminal activity by DDR participants

and stigmatization by civilians. Because my research design relies on the timing of dissident FARC splinter groups' emergence in different regions of Colombia, for this alternative mechanisms to invalidate my results, it must also affect these regions in a similar pattern over time.

The literature on DDR programs suggests that participants who lack opportunities in the formal economy may turn to crime and be exposed to high levels of violence as a consequence. This argument suggests that both "push factors" such as the opportunities for employment in the formal economy, and 'pull factors" such as the opportunities in the illicit economy, might affect DDR participant criminality.

I use the department-level unemployment rate as a measure of time-varying "push factors". This measure is intended to capture negative economic shocks to a department that might make finding a legal job more difficult for DDR participants, potentially causing them to turn to crime. While poor economic conditions likely affect crime propensity for the general population, former combatants might be more likely than others to feel the effects for the reasons noted earlier such as stigma from employers and lack of formal work experience.

I use the department-level unemployment statistics for 2017-2022 from Colombia's National Administrative Department of Statistics. Because the ministry responsible calculates this measure only for the more populated departments, I drop the 10 least populated departments.

To capture time-varying "pull factors", I use a department-level measure of cocaine production. Cocaine is the largest component of Colombia's criminal economy; large increases or decreases in cocaine production might affect the opportunities for illicit activity by DDR participants, particularly given the FARC's deep involvement in the cocaine trade during the armed conflict.

I use a department-level measure of hectares of cocaine cultivation derived from data from the UN Office on Drugs and Crime and the Colombian Ministry of justice. These agencies

use remote-sensing to detect cocaine cultivation sites and estimate their annual production. As of mid-2023, data on cocaine production were available through 2021, so I drop 2022 for this part of the analysis.

As before, I employ a two-way fixed-effects model that adjusts for both time-invariant differences between departments and general trends over time.

Table 2.2: Alternative Explanations for DDR Fatality Rates

	DV = DDR Fatalities					
	(1)	(2)	(3)	(4)	(5)	(6)
FARC Dissident Presence		1.87*	2.77†		1.11	3.99†
		(0.81)	(1.55)		(0.73)	(2.25)
Unemployment Rate	0.37	0.32	✓			
	(0.43)	(0.44)				
Cocaine Production (log)				0.16	0.09	✓
				(0.59)	(0.61)	
Model	TWFE	TWFE	C&S	TWFE	TWFE	C&S
Year FE	✓	✓	✓	✓	✓	✓
Dept. FE	✓	✓	✓	✓	✓	✓
Num. obs.	132	132	132	150	150	150

Notes: The unit of analysis in all models is the department-year. The unemployment rate variable is the department-level annual unemployment rate. The cocaine production variable is the logged annual number of hectares of cocaine cultivation. The FARC Dissident Presence variable is a binary indicator that takes a value of 1 if FARC dissidents are present in a department-year and 0 otherwise. The dependent variable in all models is the rate of fatal attacks on FARC DDR participants per 1000. TWFE models use department and year fixed effects only. C&S models implement the Callaway and Sant'Anna (2021) staggered difference in differences estimator, using the not-yet-treated departments as the control group. Standard errors in all models are clustered on Department. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$; $^{\dagger}p < 0.1$

I find little evidence for an effect of either changes in unemployment or the quantity of cocaine production on fatality rates for DDR participants. In models 1 and 3, estimates of their effect on DDR fatalities are small and statistically insignificant. Because of data availability limitations for the unemployment and cocaine production measures, including these variables as controls in the main analyses involves dropping significant numbers of observations. However, models 2, 3, 5, and 6 show that even after the data is truncated in this manner, the direction and magnitude of estimates remain similar when including these variables as controls.

2.10 Conclusion

This paper set out to explain the puzzling wave of violence DDR participants experienced in the aftermath of Colombia's 2016 peace agreement with the FARC. I developed a theoretical explanation that focused on the threat that DDR programs pose to rebel splinter groups in cases of rebel fragmentation. My difference-in-differences analysis of attacks on FARC DDR participants support this argument, indicating that the rate of fatal attacks increased where FARC splinter groups expanded. Results from a similar empirical strategy do not support alternative explanations for violence prevalent in the existing literature on DDR programs, which blame violence on unemployment and criminality among DDR participants. In-depth analysis of common patterns of these attacks also lend support to the idea that splinter groups perceived DDR programs as threatening their recruitment, compromising sensitive intelligence, and challenging their legitimacy.

As an increasing number of armed conflicts conclude in negotiated settlements (Fazal 2018), it becomes increasingly likely that cases like Colombia's will emerge in which rebel groups fragment into splinter groups during the peace process (Duursma and Fliervoet 2021). Because disarming, demobilizing, and reintegrating former combatants is an integral component of most peace processes, my findings regarding the threat rebel splinter groups pose to DDR programs highlight a critical challenge to peace processes. DDR programs cannot succeed if governments cannot even guarantee the survival of participants.

My results highlight several policy issues that warrant further attention from scholars and policymakers. First, I demonstrate that violence against DDR participants emerged as a consequence of rebel fragmentation. While a growing body of research has identified several risk factors for rebel group fragmentation, more work is needed both on the causes of fragmentation and on how governments can manage or prevent it during and after negotiations. Second, my results indicate that the model of DDR implemented in Colombia, which involved cantonment in former conflict zones, is vulnerable to violent disruption. Further

research could evaluate a range of DDR programs and identify models that are resilient to the threat of postconflict violence.

2.11 Supporting Information

2.11.1 Alternative Operationalizations

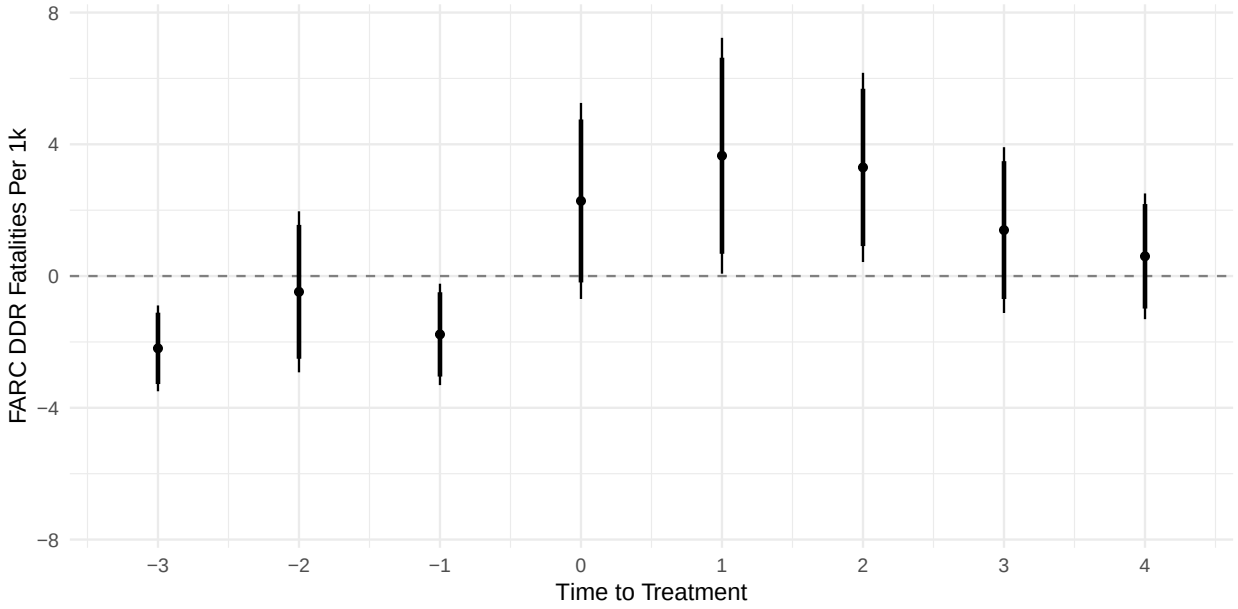
Table 2.3: Effect of FARC Dissident Presence on (logged) DDR Fatalities

	DV = Logged DDR Fatalities	
	(1)	(2)
FARC Dissident Presence	0.18^{*}	$0.23^{\dagger}$
	(0.07)	(0.13)
DDR Population (logged)	✓	✓
Model	TWFE	C&S
Year FE	✓	✓
Dept. FE	✓	✓
Num. obs.	198	198

Notes: The unit of analysis in all models is the department-year. The independent variable in all models is a binary indicator that takes a value of 1 if FARC dissidents are present in a department-year and 0 otherwise. The dependent variable in models 1 and 2 is the logged count of fatal attacks on FARC DDR participants. Both models control for the logged count of FARC DDR participants per department. TWFE models use department and year fixed effects only. C&S models implement the Callaway and Sant'Anna (2021) staggered difference in differences estimator, using the not-yet-treated departments as the control group. Standard errors in all models are clustered on Department. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$; $^{\dagger}p < 0.1$

2.11.2 Event Study

Figure 2.2: Event Study Plot for Effect of FARC Dissident Presence on DDR Fatality Rate



Notes: The dependent variable in this analysis is the rate of fatal attacks on FARC DDR participants per 1000. The independent variables are leads and lags representing years before and after treatment. I use CR0 standard errors clustered at the department level. Thick and thin bars are 90% and 95% confidence intervals respectively.

CHAPTER 3

Who's to blame? How postconflict violence affects public support for peace

A longstanding conventional wisdom in the peacebuilding literature holds that violence during and after a peace process undermines public support for peace. Yet the empirical record is ambiguous, and in a few high-profile cases such as the Omagh bombing in Northern Ireland, public support for peace surged despite—or even in response to—incidents of violence. Building on the literature on public opinion formation, I argue that the effect of violence on attitudes towards peace may be moderated or exacerbated by political messaging about who or what is to blame. I test this argument in Colombia, a country that has seen persistent postconflict violence after a 2016 peace agreement, and where the agreement's political supporters and opponents offer competing messages that blame either the government's implementation failures or noncompliance by dissident rebel commanders. I fielded a survey experiment with 1466 respondents in conflict and non-conflict zones, pairing recent news about postconflict violence with information supporting these competing political messages. I find evidence that messaging blaming rebel commanders for failing to comply reduced respondents' support for future peace negotiations, but I do not find strong evidence that messages attempting to shift blame to poor government implementation had a countervailing effect. In a probe of the mechanisms, I find suggestive evidence that while the treatment emphasizing rebel commanders' noncompliance increased perceptions that rebels alone were to blame, citizens inferred from the treatment emphasizing government implementation failures that *both* parties were to blame, limiting the moderating effect of this message. These results

suggest that political messaging during episodes of postconflict violence can influence what citizens learn from these episodes about the viability of peace processes, but that there may be an asymmetry in citizens' propensity to assign blame that favours political opponents of peace.

3.1 Introduction

In countries emerging from conflict, armed actors opposed peace often perpetrate violent attacks in the post-conflict period, and a conventional wisdom in political science holds that such violence can undermine citizens' support for the peace process (Kydd and Walter 2002). Implicit in this argument is a set of assumptions about how citizens interpret such setbacks and assign blame; they interpret violence as evidence that the peace process is failing, and respond by withdrawing their support. Proponents of this view point to examples like the Israel-Palestine conflict, where evidence suggests that voters responded to violence by electing hardliners opposed to negotiations (Getmansky and Zeitzoff 2014). Yet other examples demonstrate that this response is not inevitable. When dissident Republicans perpetrated bombings after the ratification of the Good Friday Agreement for instance, the citizens of Northern Ireland rallied in support of the Agreement (Darby 2006). These conflicting examples suggest a need to revisit the conventional wisdom around postconflict violence: when does violence galvanize public support for peace, and conversely, when does it undermine public confidence in the peace process?

I develop a theoretical argument with roots in an extensive literature on public opinion, which emphasizes the ways in which partisan messaging and beliefs can moderate the effects of material conditions on attitudes. I propose that how citizens react to postconflict violence depends on their beliefs about who or what is to blame; beliefs that are based on information that is often ambiguous or politically charged. In particular, it is often unclear whether post-conflict violence is the fault of untrustworthy rebels exploiting a weak or ineffective peace agreement, or the fault of an incompetent government failing to implement a well-designed peace agreement. In this context, messaging about blame from partisan elites who support or oppose a peace agreement could play an important role in shaping attitudes and beliefs.

I argue that different beliefs about who is to blame for postconflict violence matter because they can have dramatically different implications for how citizens might react to

postconflict violence. If postconflict violence is a symptom of rebel noncompliance, then it may have the effect of undermining public confidence, in line with the aforementioned conventional wisdom on spoiling. By contrast, if postconflict violence is a symptom of poor government implementation, then it may have the opposite effect—galvanising citizens to demand better implementation from their government.

I test this argument using a survey experiment conducted in Colombia in 2022, a period in which postconflict violence was escalating and blame for the violence was contested and politicized. I developed a set of interventions that paired recent news about postconflict violence with information supporting these competing political messages. I tested the effect of these messages on confidence in the 2016 agreement as well as support for future peace negotiations among a sample of 1466 respondents from a mix of conflict and non-conflict zones.

The main result emerging from this experiment is that information replicating the messaging from political elites emphasizing rebel culpability for postconflict violence decreased the public's appetite for future peace deals. Specifically, respondents who received news about recent violence paired with information implicating rebel non-compliance with the 2016 peace agreement were less likely to support future negotiated settlements with rebel groups than respondents who either received no information, or who received information about the violence without additional information about blame. There is also some evidence this effect is stronger for respondents in former conflict zones.

I do not however, find that information implicating a lack of government implementation as a cause of violence had the anticipated positive effect on any of the outcomes of interest, a result that suggests that messaging by political supporters of peace attempting to shift blame to the government may not have its desired effect. In fact, respondents appear to infer from this treatment that neither the government nor the rebels are fulfilling their commitments.

These results build on a growing body of research emphasizing the pivotal role of partisan elites and messaging can play in either sustaining or dismantling public support for peace

deals (Matanock, García-Sánchez, and Garbiras-Díaz 2020). I contribute to this literature by demonstrating how partisan messaging weaponizes and interacts with developments on the ground in post-conflict settings, and in particular episodes of post-conflict violence. My results suggest that elite messaging during such episodes can influence what citizens *learn* from these episodes about the viability of peace processes, but is effective only when messages are carefully crafted and reach the right audience.

3.2 Theoretical Framework

3.2.1 Postconflict Violence and Models of Public Opinion

Prior research on the topic of "spoilers" argues that violence can be an effective strategy for extremists opposed to peace (Stedman 1997). Postconflict violence signals to the public that the peace agreement is not self-enforcing and rebels cannot be trusted to abide by their commitment to refrain from violence (Kydd and Walter 2002; Braithwaite, Foster, and Sobek 2010; Findley and Young 2015). As a consequence, public support for negotiated solutions declines.

Research in this vein relies implicitly on a model of public opinion in which members of the public observe positive or negative changes in their material circumstances and update their views of the responsible policies or elected officials accordingly. Proponents of this model of public opinion point to evidence that incumbents often lose support if they preside over economic downturns (Lewis-Beck and Stegmaier 2000), military casualties (Karol and Miguel 2007), or terrorist attacks (Bali 2007).

An alternative model of public opinion emphasizes the role of partisanship and elite messaging in moderating how the public responds to changes in material circumstances. This view suggests that members of the public rely on cues from trusted copartisan elites when forming opinions on policy issues (Zaller 1992; Lupia, McCubbins, Arthur, et al. 1998). Proponents of this view point to evidence that even on seemingly objective material conditions

such as the state of the economy, public opinion is often sharply divided along partisan lines (Bartels 2002).

Recently, scholars have applied this insight to the study of violence and peace processes. Berrebi and Klor (2008) offer evidence that partisanship might moderate the effect of violence on public attitudes towards peace. In an observational study, they show that terrorist attacks in Israel seem to have polarized the electorate, increasing support in right wing strongholds for right-leaning parties opposed to concessions to Palestinian groups, but increasing support for left-leaning parties that supported concessions in left-leaning strongholds.

Matanock and Garcia-Sanchez (2017) provide suggestive evidence that elite division over peace negotiations was responsible for polarization in public opinion in the run-up to Colombia's 2016 peace referendum. In more recent work, they offer further experimental evidence that cues from copartisan elites about the *contents* of Colombia's peace agreement affected respondents' attitudes towards the agreement, and were not attenuated by additional factual information about the agreements' provisions (Matanock, García-Sánchez, and Garbiras-Díaz 2020).

Matanock et al's finding that elite cues about the contents of a peace agreement appear to outweigh the factual information provided by enumerators provides strong face validity to the claim that the postconflict setting is one where elite cues can play a powerful role. Moreover, Berebi and Klor's finding that postconflict violence polarized the Israeli electorate is consistent with a model in which partisans react to violence differently due to receiving divergent cues from partisan political elites. I build on and contribute to this emerging literature by arguing that cues from political elites are particularly powerful in shaping responses to postconflict violence because who or what is to blame for violence may be ambiguous or contested.

3.2.2 The Role of Blame

When members of the public observe conflict-related violence after a peace agreement, one of at least two actors could be responsible. Both rebel commanders and elected officials are critical to the success of a peace agreement: rebel commanders are responsible for complying with disarmament and demobilization and elected officials for implementing agreed-upon concessions. Postconflict violence could be a result of rebel commanders reneging on demobilization, or from elected officials reneging on implementation.

Which of these sets of actors is responsible for violence may be ambiguous or contested. In many peace processes, international observers are necessary in part because governments and rebels frequently accuse each other of violating agreements and neutral arbiters are required (Fortna 2004).

Who citizens blame for violence is important for how violence affects their attitudes towards peace. If elected officials are reneging on implementation, then citizens have agency—they can demand that their elected officials implement the agreement, and if they ignore these demands, vote them out of office. I argue that because citizens' possess this agency over elected officials, if they blame elected officials for violence, this violence may not affect their overall confidence in the peace process.

Conversely, if rebels renege on demobilization, citizens have less agency. Unlike elected officials, rebel commanders are not susceptible to citizen pressure. I argue that because citizens' lack agency over rebel commanders, if they blame rebel non-compliance for violence, this violence may undermine their confidence in the potential effectiveness of the peace agreement.

But how do members of the public decide who or what is to blame? I argue that in this uncertain context, partisan messaging about blame for postconflict violence is potentially quite powerful. Pro-agreement political elites may seek to frame violence as evidence that the peace agreement is being poorly implemented and requires more resources to succeed.

Political elites opposed to the peace agreement on the other hand, may seek to frame violence as evidence that rebels are untrustworthy and the conflict cannot be resolved through peaceful means.

3.3 Study Setting

I study postconflict violence in Colombia, where a 2016 peace agreement with the FARC, the country's largest rebel group, sought to end a conflict that had lasted more than 50 years. The agreement has seen both major successes and setbacks. Early on, it successfully disarmed and demobilized roughly 95% of the FARC's members and reduced the level of violence in many former conflict zones ("United Nations" 2017; Mora 2016). Over time however, several factions of the FARC led by commanders who rejected the agreement called "FARC Dissidents" reemerged in roughly half of the FARC's territory (Posso et al. 2021), causing a resurgence of violence in these areas. In other areas the FARC's former competitors, primarily a rival guerrilla group called the National Liberation Army (ELN) and a paramilitary group called the Gaitanist Self-defense Forces of Colombia (AGC), seized control. These dynamics have resulted in a resurgence of violent clashes, forced displacement, and assassinations of community leaders ("Pico más alto" 2022). This resurgence has not been uniform however; as of 2020 roughly half of former FARC municipalities had seen dissident groups reemerge, while the other half remained relatively peaceful.

The question of who or what is to blame for persistent postconflict violence in Colombia is politically contentious. Critics of the 2016 peace agreement like former presidents Álvaro Uribe and Iván Duque frequently blame violence on the the peace agreement itself, which they argue was too lenient on the FARC. As evidence for their argument, they highlight the fact that the peace agreement offered amnesty to former FARC commanders, some of whom formed the dissident FARC factions that are now perpetrating violence ("Álvaro" 2019). Uribe has explicitly argued that in response to persistent violence, the government should

amend or even abandon the peace agreement (Echavarría 2019).

By contrast, prominent supporters of the peace agreement blame poor implementation by the government for the persistence of violence. They contend that the Duque administration, which held office from 2018 to 2022, neglected or mismanaged many of the agreement's key provisions ("Petro propone" 2022). For example, planned police and military stabilization forces failed to arrive in many conflict zones, creating a vacuum that allowed both the FARC Dissidents and other armed groups to take control ("Colombia's Armed Groups" 2017). A program to address narcotrafficking by subsidizing alternative crops for farmers enrolled less than half of eligible families and paid subsidies to just a fraction of those who ultimately enrolled (Puerta and Chaparro 2019). And, millions of dollars intended for local development projects in conflict zones were siphoned off by corrupt officials (Vanegas 2022). Proponents of this view argue that in response to persistent violence, the government must do more to fulfill its commitments under the peace agreement ("Álvaro" 2019).

In sum, Colombians are presented with two dramatically different political narratives about what is to blame for postconflict violence and the appropriate response. One narrative seeks to weaponize post-conflict violence to weaken support for the peace agreement, while the other seeks to use it to galvanize citizen pressure in support of the agreement.

3.4 Empirical Strategy

I designed a survey experiment that sought to evaluate both the general effect of postconflict violence in Colombia on attitudes towards peace processes, as well as the moderating effect of messaging about blame by partisan political elites.[1]

[1]A pre-analysis plan for this study, registered after data collection but prior to statistical analyses, is available on the OSF registry: https://archive.org/details/osf-registrations-c4vy3-v1

3.4.1 Interventions

To test the effect of postconflict violence on attitudes towards peace processes, I developed an intervention using information about recent conflict-related violence in the country. Specifically, I excerpted information from international agencies like the Red Cross about a surge in conflict-related forced displacement that was widely reported on by mainstream Colombian news agencies.[2] This information is factual, nonpartisan, and unambiguous in its message of large-scale postconflict violence.

To test the effect of messaging about blame, I developed interventions based interviews, press releases, and social media posts by prominent political figures on both sides of the issue. Pro-peace agreement political elites blamed postconflict violence on the government's implementation failures, accusing right-wing president Ivan Duque's administration (2018-2022) of sabotaging the peace agreement. Their messaging amplified news about funding shortfalls, delays, and mismanagement of programs established under the peace agreement. For the the treatment blaming government implementation (T2A), I paired the information in T1 with excerpts from a report showing that the government had implemented less than 1/3rd of the peace agreement's provisions (Kroc Institute 2021). While this information was factually accurate, it was the type of information that the pro-peace agreement elites amplified in their messaging as it reflected poorly on the government's implementation of the agreement. Moreover, paired with the information on postconflict violence, it plausibly implies that the government's implementation is to blame.

By contrast, political elites opposed to the peace agreement blamed rebels for postconflict violence. They built a narrative around the peace agreement that criticized its perceived lenience towards untrustworthy rebels, and in particular, its offer of conditional amnesty and political posts to FARC commanders. They amplified news stories about the non-compliance

[2]For example, statistics on conflict-related displacement from the Red Cross appear regularly in both El Tiempo and El Espectador (Amat 2019; "Pico más alto" 2022), Colombia's two newspapers with national circulation.

of FARC commanders who defected from the peace agreement to form FARC "dissident" groups, arguing that these commanders should have been in prison. For the anti-peace treatment (T2B), I paired information from T1 with excerpts from a report on the rapid expansion of dissident groups led by FARC commanders who abandoned the peace agreement (Posso et al. 2021). Again, while this information is factual, it is information that the peace agreement's political opponents amplified in their messaging as it fit their narrative about rebel non-compliance.

3.4.2 Outcomes

I test the effects of these treatments on three outcomes of interest, which I measure on 4-point scales. The first outcome, which I label "peace accords confidence" asks respondents to rate their confidence in the eventual success of the 2016 peace agreement. Given the uncertainty around implementation, the wording of the question specifically seeks to isolate respondents' beliefs about whether the peace agreement *will* succeed if it is implemented.

The second and third outcomes of interest concern support for future rounds of negotiations with rebel groups. Specifically, the second outcome question asked respondents to rate support for negotiations with the ELN, Colombia's second largest rebel group, and the third outcome question asked respondents to rate their level of support for negotiations with the FARC Dissidents. In the early summer of 2022, when this survey was being enumerated, the idea of peace talks with these groups was plausible, but no timeline or framework had been established. Subsequent to the survey, in fall of 2022, the newly-elected Petro Administration announced its intention to pursue talks with both groups under a policy called "Total Peace" ("Paz Total").

Each outcome question aims to measure an important element of attitudes towards peace. The question about the 2016 peace accord seeks to measure confidence in the existing peace process. It is also plausible however, that citizens *learn* from the experience of postconflict violence when considering future peace negotiations. The question about negotiations with

the FARC Dissidents—successor groups led by rebels who defected from the original peace agreement—tests whether postconflict violence might damage the prospect of future negotiations with (some of) the same rebels. The question about negotiations with the ELN on the other hand, tests whether postconflict violence damages the future prospects for negotiated settlements writ large.

3.5 Hypothesized Treatment Effects

3.5.1 Main Effects

3.5.1.1 Postconflict Violence Only (T1)

The conventional wisdom in the literature suggests that priming respondents with information about postconflict violence should have a negative impact on confidence in or attitudes towards peace agreements. A negative effect of T1 relative to control on the support for peace outcomes would be consistent with this conventional wisdom. Conversely, a positive or more neutral effect of T1 on the outcomes would be consistent with the idea that postconflict violence can have the opposite effect of galvanizing citizens in support of peace agreement implementation.

3.5.1.2 Postconflict Violence and Messaging about Poor Government Implementation (T2A)

I argue that information about the stalled implementation of the peace agreement can moderate any negative effects of news about postconflict violence on attitudes towards peace. Thus I hypothesize that the treatment effect of Treatment 2A on the support for peace outcomes will be more positive compared to information about postconflict violence alone.

3.5.1.3 Postconflict Violence and Messaging about Rebel Noncompliance (T2B)

I argue that information about rebel defection or noncompliance with the peace agreement can exacerbate any negative effects the effect of news about postconflict violence on attitudes towards peace. Thus I hypothesize that the treatment effect of Treatment 2B on the support for peace outcomes will be more negative relative to information about postconflict violence alone

3.5.2 Hypothesized Sources of Heterogeneity

The informational treatments I outline aim to increase awareness or salience of certain pieces of information about postconflict violence. Several factors might plausibly condition the susceptibility of respondents to these treatments. I preregistered four potential sources of heterogeneity: ideology, political engagement, security, and past conflict exposure.

I include ideology because a key focus of my theory is messaging by partisan elites. In Colombia, historically conservative or right-leaning parties and politicians have taken a hard line against the guerrilla groups, while left-leaning parties and politicians have been more favorable to negotiating with these groups. Left- or right-leaning voters might be particularly susceptible to arguments that are typically propogated by their copartisan elites. I measured political ideology by asking respondents to rate themselves on a numeric scale from left to right.

Second, I include political engagement because I expect respondents to vary in the extent to which they are exposed to national-level political debates about the peace process. Respondents who pay close attention to national politics may already know and believe the arguments on a particular side of the debates, while respondents who are less politically engaged may be more open to considering arguments for either side. I constructed an index of political engagement that includes questions about political knowledge, voting participation, and news consumption.

Third, I expect the treatment effects to vary with respondents' past conflict exposure. Respondents in former conflict zones have more at stake with the success or failure of the peace process than respondents in areas unaffected by conflict. I measured conflict exposure using municipality level data on FARC presence prior to the 2016 peace agreement.

Last, I expect the treatment effects to vary with what I term the "security trajectory"; i.e., whether security conditions improved or deteriorated after the ratification of the 2016 peace agreement. I argue that the trend in respondents' local security conditions may influence their responsiveness to a set of treatments that focus on worsening postconflict violence. I measure security trajectory by comparing municipality-level homicide rates in the six years before and six years after the peace agreement's ratification.

3.5.3 Study Sample and Implementation

The sample consists of 1466 respondents from 28 Colombian municipalities. Municipalities were chosen using a stratified random sampling strategy that ensured representation of each of Colombia's geographic regions, as well as current and former conflict zones.[3] Within municipalities, the sample includes respondents from both the populated centers and rural periphery, in proportion to the distribution population within the municipality. Within these units, respondents are randomized into either the pure control or one of the three treatment conditions.

3.5.4 Estimation

The main estimating equation for the treatment effects is as follows:

$$Y_i = \alpha + \beta_1 T1_i + \beta_2 T2A_i + \beta_3 T2B_i + \theta X + \epsilon_i$$

[3]In a small number of cases documented in the appendix, enumerators were sent to preselected substitutes due to either intense armed group activity or flooding.

Where Y_i is the outcome for respondent i, $T1$, $T2A$, and $T2B$ are dummy variables for assignment into one of the treatment conditions, and X is a vector of covariates. Following Lin 2013, X represents centered covariates interacted with each treatment condition.

To increase the precision of estimates and adjust for any random imbalances, I include a list of pre-registered covariates that include demographic variables—age, sex, ethnicity, religion, education, socioeconomic status, and region[4]—as well as theoretically-motivated variables—ideology, political engagement, local security conditions, security trajectory, and FARC presence prior to the peace agreement.

3.6 Results

3.6.1 Main Treatment Effects

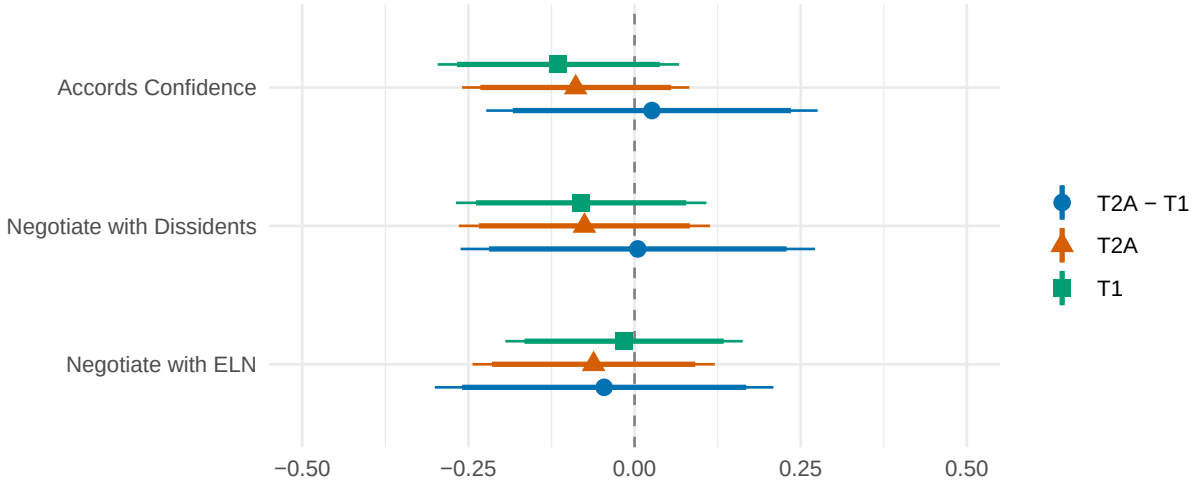
3.6.1.1 Weak Effects for Message Blaming Government Implementation

Figure 3.1 plots the average treatment effects of the messaging from the peace supporters' camp blaming poor government implementation relative to the control group that received no information and relative to the T1 group that received only information about violence for all three outcomes of interest.

Contrary to my expectations, I do not find that this treatment mitigates the anticipated negative effect of information about postconflict violence. Across all three outcomes, T2A is statistically indistinguishable from T1 and from the control group. I thus do not find evidence that the message seeking to shift the blame for violence to the government's poor implementation was successful. In the section on mechanisms that follows (3.6.3), I suggest that this failure might be due to respondents inferring from the lack of government implementation that neither the rebels, nor the government, were fulfilling their commitments.

[4]I deviate from the pre-analysis plan only in controlling for region rather than municipality due to dimensionality constraints.

Figure 3.1: Effects of Message Blaming Government Implementation (T2A)



Notes: Outcomes are measured on a 4-point scale. For each outcome, I show the treatment effect of T1 versus control, T2A versus control, and the difference. Models are estimated using the full set of controls described in section 3.5.4. Thick and narrow bars represent 90% and 95% confidence intervals respectively. I use HC2 heteroskedasticity-consistent standard errors.

3.6.1.2 Negative Effects for Message Blaming Rebel Non-compliance

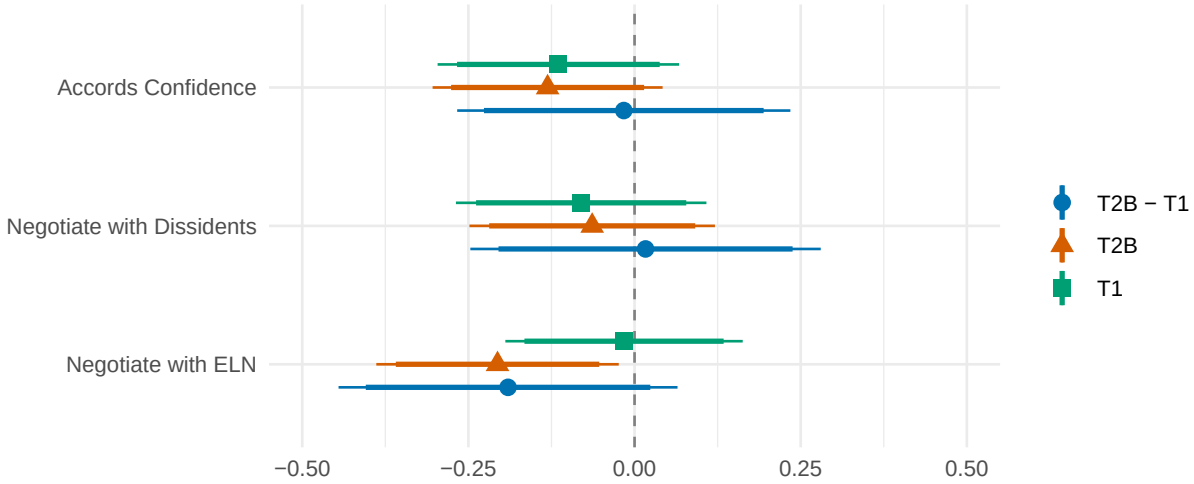
Figure 3.2 plots the average effects of the treatment based on messaging from the peace agreement's political opponents blaming rebel non-compliance for violence, compared to the control group that received no information, and compared to the T1 group that received only information about violence for all three outcomes of interest.

The effects of this message are negative for all three outcomes of interest, and statistically significant for the outcome measuring support for negotiations with the ELN. The estimated effect of T2B on this outcome of -0.2 scale points relative to control and -0.19 relative to T1 represent movement in the range of 6-7% of the control group mean.[5] I interpret this result as evidence for my hypothesis that messaging emphasizing rebel non-compliance should decrease public support for peace processes.

While for the full survey sample, I find a statistically significant effect of T2B only for the

[5]Per my pre-analysis plan, I also estimated the differences between the effects of T2A and T2B (see Appendix 3.5). While the direction of these effects are broadly consistent with my hypotheses, the estimates are imprecise.

Figure 3.2: Effects of Message Blaming Rebel Non-compliance (T2B)



Notes: Outcomes are measured on a 4-point scale. For each outcome, I show the treatment effect of T1 versus control, T2B versus control, and the difference. Models are estimated using the full set of controls described in section 3.5.4. Thick and narrow bars represent 90% and 95% confidence intervals respectively. I use HC2 heteroskedasticity-consistent standard errors.

ELN negotiations outcome, in the section that follows I show that this treatment also had meaningful effects on support for negotiations with the FARC Dissidents among important pre-registered subgroups. In the mechanisms section, I also consider why this treatment had the anticipated effect while the messaging emphasizing government implementation did not.

3.6.2 Heterogeneity: Who Reacts Strongly to Rebel Non-compliance?

Table 3.1 includes the results for the rebel noncompliance treatment (T2B) with the four pre-registered sources of heterogeneity.[6] Ideology and political engagement do appear predictive of baseline levels of support for peace negotiations. However, I find little evidence that the treatment effect varies with political engagement, ideology, or the security trajectory. I do find however, that the treatment emphasizing rebel noncompliance (T2B) has consistently stronger negative effects for respondents in areas with FARC presence prior to the peace agreement. This effect is largest and statistically significant for the outcome measuring

[6]See Appendix 3.5 for the full set of heterogeneous effects for T1 and T2A.

support for negotiations with FARC Dissidents. For respondents in former FARC territory, treatment T2B decreased support for negotiations with FARC Dissidents by 0.55 scale points, or about 19% of the control group mean.

Table 3.1: Effect Heterogeneity for Rebel Non-compliance Treatment

	DV = *Peace Accords Confidence*	DV = *Negotiate with ELN*	DV = *Negotiate with Dissidents*
	Model 1	Model 2	Model 3
(Intercept)	2.848***	2.987***	2.941***
	(0.063)	(0.063)	(0.068)
T2B	−0.131	−0.206*	−0.064
	(0.088)	(0.093)	(0.094)
Ideology	−0.062	−0.135***	−0.122***
	(0.037)	(0.034)	(0.037)
Political Engagement	0.068	0.154***	0.078
	(0.039)	(0.037)	(0.041)
FARC Presence	0.016	0.053	0.013
	(0.172)	(0.164)	(0.177)
Security Trajectory	−0.001	0.004	−0.002
	(0.006)	(0.005)	(0.006)
T2B x Ideology	−0.035	0.031	0.046
	(0.051)	(0.052)	(0.053)
T2B x Political Engagement	−0.021	−0.027	−0.008
	(0.053)	(0.053)	(0.055)
T2B x FARC Presence in 2012	−0.177	−0.080	−0.551*
	(0.234)	(0.251)	(0.255)
T2B x Security Trajectory	−0.005	−0.002	−0.010
	(0.008)	(0.008)	(0.008)
Num. obs.	1270	1339	1335

Notes Outcomes are measured on a 4-point scale. Models are estimated using the full set of controls described in section 3.5.4. I use HC2 heteroskedasticity-consistent standard errors. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

I interpret this result as consistent with my argument that inhabitants of former conflict zones stand to gain the most if a peace agreement succeeds and lose the most if a peace agreement fails. For these respondents with direct experience with the FARC, blaming rebel non-compliance for violence may be a particularly effective message.

3.6.3 Mechanisms

Why did the treatment emphasizing rebel noncompliance have the anticipated effect on the negotiations outcome while the treatment emphasizing poor government implementation did not? For the proponents of peace processes in Colombia, this question is an important puzzle, particularly if postconflict violence continues to escalate.

I asked several questions post-treatment to assess whether the treatments had the intended effects. Two particularly relevant questions are first, whether respondents believe the government is fulfilling its role in implementing the peace agreement, and second, whether respondents believe the FARC rebels are complying with the peace agreement.

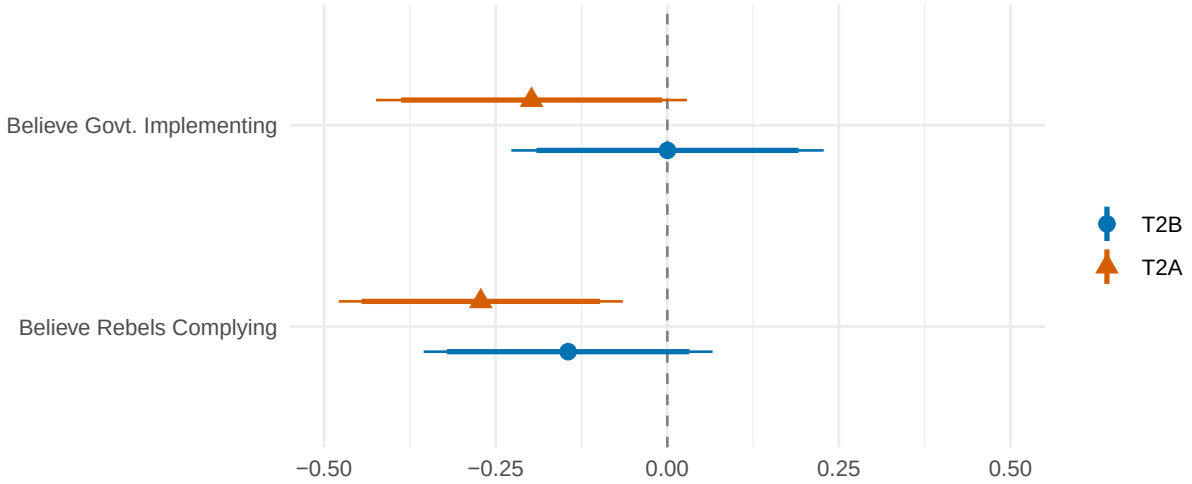
The messaging about poor government implementation in T2A is intended to shift blame for postconflict violence to the government. Here I expect to observe a downward shift in beliefs that the government is implementing the agreement, but do not expect movement regarding rebel compliance.

The messaging about rebel noncompliance in T2B is intended to shift blame for postconflict violence to the rebels. Here I expect to observe a downward shift in beliefs that the rebels are complying, but do not expect movement regarding government implementation.

For T2B, I indeed observe some negative movement on beliefs that rebels are complying, and no change in beliefs that the government is implementing. In other words, the treatment appears somewhat successful at increasing the extent to which respondents blame the rebels.

For T2A, however, I observe negative movement in both beliefs that the government is implementing the agreement, and that rebels are complying with the agreement. In other words, despite the fact that T2A does not explicitly mention the FARC, respondents seem to have inferred from the information about government implementation failures that both the FARC *and* the government are to blame for the violence. Plausibly, some respondents assumed that the governments' failure to implement the agreement might be a *response* to actions by the FARC.

Figure 3.3: Effects of Treatments on Beliefs about Compliance and Implementation



Notes: Mechanism questions use a 6-point scale. For each mechanism, I show the treatment effect of T2A versus control and T2B versus control. Models are estimated using the full set of controls described in section 3.5.4. Thick and narrow bars represent 90% and 95% confidence intervals respectively. I use HC2 heteroskedasticity-consistent standard errors.

In the appendices, I further test the validity of these mechanisms first, by demonstrating a strong correlation between perceptions of government implementation, rebel compliance, and the outcomes of interest among the control group (SI Table 3.4), and second, by showing that the areas that saw large treatment effects on the mechanism questions also saw corresponding effects on the outcomes of interest (SI Figure **??**).

In sum, while the messaging about rebel non-compliance succeeded in shifting blame to the rebels and lowering support for peace negotiations, the messaging about government implementation failures appears to have generated perceptions of shared blame and thus failed at its intended goal of mitigating the effects of violence on support for peace.

3.7 Conclusion

This paper investigated how postconflict violence affects public support for peace processes. Drawing on insights from the literature on public opinion, I hypothesized that the effect of violence can be moderated by messaging from partisan political elites about who or what is

to blame. My results from a survey experiment in Colombia indicate that political messaging about blame can indeed moderate the effect of violence on attitudes towards peace, but such messaging has important limitations.

As anticipated, whereas news about violence alone had little noticeable impact on attitudes towards peace, paring this news with messaging that explicitly blamed rebel behavior decreased support for future peace processes. There was also some evidence to suggest that this effect was strongest for individuals in conflict zones who would be most vulnerable to renewed violence. Contrary to expectations however, messaging that blamed postconflict violence on poor government implementation failed to have a countervailing effect. Instead, citizens inferred from messaging about government implementation failures that both the government and the rebels deserved blame for the violence.

Taken together, these results support my argument that partisan political messaging can play an important role in moderating or exacerbating the effect of postconflict violence on public attitudes. Yet they also highlight an important asymmetry in the effectiveness of such messaging—citizens are prone to blame rebels for postconflict violence, regardless of whether the available information indicates that rebels or the government is more responsible.

This finding may be discouraging for proponents of peace, and suggests that more research is needed to identify a more effective set of messages that can sustain public support for peace in the face of postconflict violence. One promising avenue for future research on this topic is how peace agreement *design* might counteract the effects of postconflict violence on public opinion. Tellez (2019) finds that including specific provisions in a peace agreement can have strong effects on citizens' support for the agreement. If, as my results suggest, citizens infer from the experience of postconflict violence that rebels cannot not be trusted to comply, then one plausible solution is to strengthen monitoring and verification mechanisms. Further research on this topic could investigate whether including stronger monitoring and verification provisions in proposed peace agreements and emphasizing these provisions in messaging is an effective antidote to citizens' concerns about rebel noncompliance.

3.8 Supporting Information

3.8.1 Experimental Treatments

3.8.1.1 Structure of Treatment Groups

- **C:** No information treatment (pure control)
- **T1:** Information about postconflict violence only
- **T2A:** Information about postconflict violence + information implicating government implementation failures
- **T2B:** Information about postconflict violence + information implicating rebel commanders

3.8.1.2 Full treatment and control conditions

Condition	**Sample Text**
Pure Control (C)	*(Respondents move directly from demographic questions to outcome measurement.)*
Information about postconflict violence only (T1)	Recent reports from the UN and the Red Cross have noted an intensification of the armed conflict in Colombia in recent years. Since the Peace Agreement was signed in 2016, approximately 142,000 Colombians have been victims of forced displacement due to the armed conflict.

Information about post-conflict violence + information implicating government implementation failures (T2A)	Recent reports from the UN and the Red Cross have noted an intensification of the armed conflict in Colombia in recent years. Since the Peace Agreement was signed in 2016, approximately 142,000 Colombians have been victims of forced displacement due to the armed conflict. Independent reports reveal that the Colombian government has made very little progress in implementing the 2016 peace agreement. As of 2021, the government had still not fully implemented more than two-thirds of the agreement's provisions.
Information about post-conflict violence + information implicating rebel commanders (T2B)	Recent reports from the UN and the Red Cross have noted an intensification of the armed conflict in Colombia in recent years. Since the Peace Agreement was signed in 2016, approximately 142,000 Colombians have been victims of forced displacement due to the armed conflict. Independent reports indicate that former FARC commanders such as Iván Márquez, who rejected the peace accords, are responsible for much of the recent violence. These FARC dissidents now have up to 5,200 members and operate in up to 123 municipalities.

3.8.2 Outcome Questions

The first outcome measure seeks to elicit respondents' general level of confidence or optimism regarding the peace process. To operationalize this concept, I offer respondents a set of choices that best describe their level of optimism regarding the peace agreement's successful implementation. At one extreme is the statement that "the peace agreement will almost

certainly not succeed, regardless of implementation," while at the other extreme is the statement that, "the peace agreement will almost certainly succeed with better implementation". The full set of options on the 4 point scale is as follows:

1. The peace agreement will almost certainly not succeed, regardless of implementation (1)
2. The peace agreement will probably not succeed, but there is a small chance of success with better implementation (2)
3. The peace agreement will probably succeed with better implementation, but there is a small chance it will not succeed (3)
4. The peace agreement will almost certainly succeed with better implementation (4)

This question wording is designed to elicit respondents' confidence or optimism in the potential success of the peace agreement if it were implemented.

To evaluate support for negotiations with armed groups, I asked in separate questions, the following:

- In the coming years, if a peace agreement is negotiated with the ELN, would you support it? *(Definitely yes, probably yes, probably no, definitely no)*
- In the coming years, if a peace agreement is negotiated with the FARC dissidents, would you support it? *(Definitely yes, probably yes, probably no, definitely no)*

3.8.3 Ethical Considerations

Because this survey involves human subjects, serious attention was given to ethical considerations such as the consent of the subjects, and mitigation of potential security or health risks for participants. In what follows, I discuss each of these topics and the measures taken to ensure the study adheres to the highest ethical standards. The survey text was also submitted to and approved by UCLA's Institutional Review Board (IRB#21-001054).

3.8.3.1 Informed Consent

To ensure fully informed consent, potential participants were given the following details before the interview began:

- Researcher name and affiliation
- The topic area of the survey
- The approximate length of the survey
- The name of the firm conducting enumeration
- A telephone number in Colombia to contact for more details

Potential participants were also told they could refuse to answer questions or stop the interview at any point if they felt uncomfortable. Following the survey, respondents were debriefed. The debrief reiterated details about the sponsor and purpose of the survey, and reminded survey participants that the information provided in the survey was incomplete and a diversity of perspectives exist on the issues discussed.

3.8.3.2 Respondent and Enumerator Security

Despite the 2016 peace agreement and the demobilization of the FARC, as of mid-2022 when this survey was implemented illegal armed groups continued to pose a potential security threat in many of the sampled municipalities.

Several precautions were taken to avoid exposing respondents and enumerators to potential security risks. First, the survey avoided asking about potentially sensitive topics, such as the presence or activities of local armed groups. Second, respondents were reminded that they could at any time either drop out of the survey or decline to respond to a question if they felt uncomfortable. And third, in a small number of instances where enumerators reported credible security concerns to their supervisors, they were redirected to a pre-selected list of secondary sites. These cases are documented in detail in section 3.8.4.4.

3.8.3.3 Covid-19 Measures

During the planning stages of this survey, the trajectory of the Covid-19 pandemic was highly uncertain. To avoid contributing to the spread of the virus, enumerators were instructed to follow a strict set of protocols around masking, sanitation, and social distancing.

3.8.4 Enumeration

3.8.4.1 Timeline and Context

Enumeration occurred between June 4th and June 29th, 2022. This period coincided with Colombia's Presidential Election campaign, with a first-round election occurring on May 29th, 2022, and a runoff on June 19th, 2022.

3.8.4.2 Flooding and Landslides

Seasonal flooding and the resulting landslides prevented access to one municipality Rioblanco, Tolima entirely, and the pre-selected rural zone of Guaca, Santander. Chaparral, Tolima was chosen as a substitute for Rioblanco, and enumerators were sent to a different rural zone of Guaca.

3.8.4.3 Transportation

Transportation issues prevented enumeration in the municipality of Santa Rosa del Sur, Bolívar. Specifically, the poor conditions of the roads and rivers required a multi-day journey beyond what had been budgeted for. The municipality of María la Baja, Bolívar, was chosen as a substitute.

3.8.4.4 Security Concerns

In five municipalities, security threats prevented enumerators from reaching the preselected rural zone of the municipality. In these cases, enumerators were alerted that armed groups were active in the area and would not permit access by outsiders. In each instance, enumerators were sent to a second rural location within the same municipality.

3.8.4.5 Summary of Substitutions

Table 3.3: List of Substitutions and Causes

Substitution Type	Original Location	Substitute Location	Cause
Municipality	Rioblanco, Tolima	Chaparral, Tolima	Flooding/Landslides
Municipality	Santa Rosa del Sur, Bolívar	María la Baja, Bolívar	Difficult Travel Conditions
Rural Zone	Baraya, Guaca, Santander	Tabacal, Guaca, Santander	Flooding/Landslides
Rural Zone	Nabusikame, Pueblo Bello, Cesar	Minas del Iracal, Pueblo Bello, Cesar	Security Conditions
Rural Zone	Majagua, María la Baja, Bolívar	Nueva Esperanza, María la Baja, Bolívar	Security Conditions
Rural Zone	Villa Paz, Puerto Lleras, Meta	Caño Rayado, Puerto Lleras, Meta	Security Conditions
Rural Zone	Villa Paz, Puerto Lleras, Meta	Caño Rayado, Puerto Lleras, Meta	Security Conditions
Rural Zone	Bocas de Mayorquín, Buenaventura, Valle	Córdoba, Buenaventura, Valle	Security Conditions
Rural Zone	El Caracol, Arauca, Arauca	Manhattan, Arauca, Arauca	Security Conditions

3.8.4.6 Limitations Arising from Security-based Substitutions

It is plausible to conclude that the five rural zones enumerators were unable to access due to armed group activity represent the conditions of the more insecure areas of the country. Even though enumerators were able to complete interviews in other parts of the municipality, the fact that they were able to do so suggests that those areas were likely more secure.

My inability to survey the most insecure rural areas affects my interpretation of the eventual results in two ways. First, in addition to other limitations on the representativeness

of the survey sample, this sample is not representative of Colombia's most insecure rural zones. Second, with regards to the heterogeneous effects of security, I cannot rule out a different relationship at the most extreme level of insecurity.

3.8.5 Additional Mechanisms Analyses

My theoretical argument asserts that blame is an important moderator of the effect of violence on attitudes towards peace. I argue that citizens who blame rebel noncompliance for violence may lose confidence in the viability of peace processes, while people who blame government implementation for violence may continue to believe in peace processes and direct their energy towards pressuring the government to improve its implementation.

To assess the face validity of this argument, I test for a relationship between perceptions of government implementation, perceptions of rebel compliance, and the outcome questions. Specifically, I focus on the difference between respondents' ratings of government implementation and their ratings of rebel compliance. A positive score means that respondents gave a worse assessment to rebel compliance than to government implementation, potentially indicating a readiness to blame the rebels more than the government for setbacks to the peace agreement. My theory predicts a negative relationship between this variable and support for peace processes.

Table 3.4 confirms that perceptions of government implementation and rebel compliance are indeed correlated with support for peace. As predicted, respondents who have a more positive assessment of government implementation than rebel compliance are less confident in the 2016 peace accord and are less supportive of negotiations with the ELN and FARC dissidents.

Figure 3.4 provides additional evidence for this argument by showing that areas where the treatment caused large changes in perceptions about blame also saw large effects on support for peace. Each point represents a department, the administrative unit above municipality,

Table 3.4: Correlation Between Mechanism Questions and Outcomes in Control Data

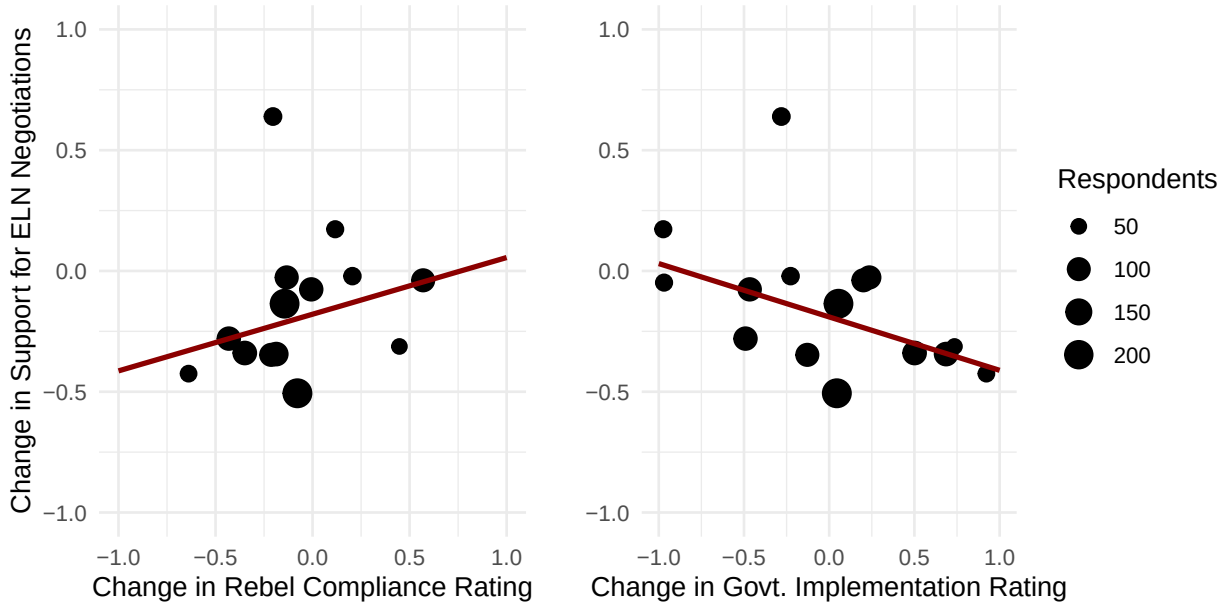
	DV = *Peace Accords Confidence*	DV = *Negotiate with ELN*	DV = *Negotiate with Dissidents*
	Model 1	Model 2	Model 3
(Intercept)	2.870***	3.030***	2.980***
	(0.062)	(0.063)	(0.065)
Govt. Implement Rating − Rebels Comply Rating	−0.090*	−0.109**	−0.150***
	(0.036)	(0.035)	(0.037)
Num. obs.	311	339	336

Notes: Only respondents assigned to control are included in these analyses. The independent variable in all models is the difference between respondent ratings of government implementation and ratings of rebel compliance, both on six-point scales from low (1) to high (6). The dependent variables are all measured on 4 point scales from low (1) to high (4). I use HC2 heteroskedasticity-consistent standard errors. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

and the size of the point is scaled to the number of respondents in each department. On the y-axes, I plot the estimated effect of T2B on support for ELN negotiations, and on the x-axes, I plot the effect on perceptions of rebel compliance and government implementation respectively.

These plots indicate that departments where treatment increased perceptions that the rebels were to blame (that is, caused respondents to perceive the government to be implementing the agreement but perceive rebels as failing to comply) also saw decreased support for negotiations with the ELN.

Figure 3.4: Correlation Between T2B Effects on Respondents' Perceptions of Blame and Support for Negotiations with the ELN at the Department Level

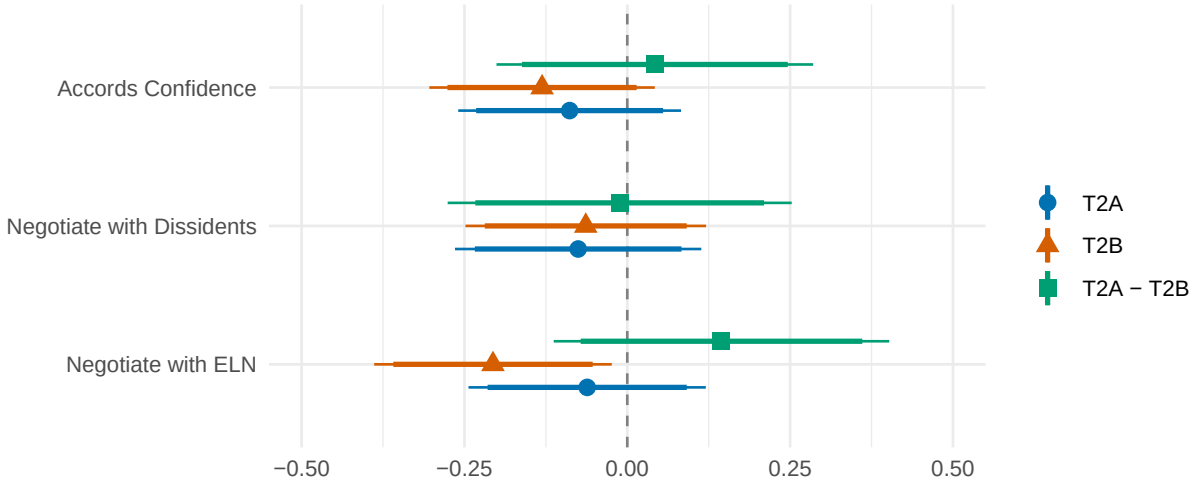


Notes: The y-axis plots the effect of T2B on support for negotiations with the ELN, measured on a 4-point scale. The x-axis plots the effect of T2B on the rating of government implementation (right) and the rating of rebel compliance (left), both measured on 6-point scales. I use the Lin (2013) estimator with individual-level controls (i.e. sex, religion, education, socioeconomic status, political engagement, ideology). Each point is a department; i.e. the administrative unit above municipality. The size of each point represents the number of respondents in each department. The red line is from a weighted linear regression where municipality-zones are weighted by the number of respondents.

3.8.6 Additional Preregistered Treatment Effects

3.8.6.1 Differences in Effects of T2A and T2B

Figure 3.5: Difference in Treatment Effects of T2A vs T2B



Notes: Outcomes are measured on a 4-point scale. For each outcome, I show the treatment effect of T2A versus control, T2B versus control, and the difference. Models are estimated using the full set of controls described in section 3.5.4. Thick and narrow bars represent 90% and 95% confidence intervals respectively. I use HC2 heteroskedasticity-consistent standard errors.

Table 3.5: Main and Heterogeneous Effects

	DV = *Peace Accords Confidence*	DV = *Negotiate with ELN*	DV = *Negotiate with Dissidents*
	Model 1	Model 2	Model 3
(Intercept)	2.848***	2.987***	2.941***
	(0.063)	(0.063)	(0.068)
T1	−0.115	−0.016	−0.080
	(0.093)	(0.091)	(0.096)
T2A	−0.089	−0.062	−0.075
	(0.087)	(0.093)	(0.096)
T2B	−0.131	−0.206*	−0.064
	(0.088)	(0.093)	(0.094)
Ideology	−0.062	−0.135***	−0.122***
	(0.037)	(0.034)	(0.037)
Political Engagement	0.068	0.154***	0.078
	(0.039)	(0.037)	(0.041)
FARC Presence	0.016	0.053	0.013
	(0.172)	(0.164)	(0.177)
Security Trajectory	−0.001	0.004	−0.002
	(0.006)	(0.005)	(0.006)
T1 x Ideology	0.060	0.023	−0.006
	(0.053)	(0.050)	(0.054)
T2A x Ideology	0.062	0.076	0.090
	(0.053)	(0.052)	(0.054)
T2B x Ideology	−0.035	0.031	0.046
	(0.051)	(0.052)	(0.053)
T1 x Pol. Engage	−0.064	−0.088	0.024
	(0.054)	(0.052)	(0.056)
T2A x Pol. Engage	−0.065	−0.068	−0.004
	(0.051)	(0.053)	(0.056)
T2B x Pol. Engage	−0.021	−0.027	−0.008
	(0.053)	(0.053)	(0.055)
T1 x FARC Presence	−0.024	−0.163	−0.357
	(0.252)	(0.239)	(0.253)
T2A x FARC Presence	−0.365	−0.188	−0.363
	(0.229)	(0.248)	(0.254)
T2B x FARC Presence	−0.177	−0.080	−0.551*
	(0.234)	(0.251)	(0.255)
T1 x Security Trajectory	0.002	−0.010	−0.006
	(0.008)	(0.008)	(0.008)
T2A x Security Trajectory	−0.014	−0.005	−0.002
	(0.007)	(0.008)	(0.008)
T2B x Security Trajectory	−0.005	−0.002	−0.010
	(0.008)	(0.008)	(0.008)
Num. obs.	1270	1339	1335

Notes Outcomes are measured on a 4-point scale. Models are estimated using the full set of controls described in section 3.5.4. I use HC2 heteroskedasticity-consistent standard errors.

3.8.7 Analysis of Missingness

Table 3.6: Correlation Between Outcome Missingness and Treatment Conditions

	DV = Accords Outcome Missing	*DV = ELN Outcome Missing*	*DV = Dissident Outcome Missing*
	Model 1	Model 2	Model 3
T1A	−0.03	0.01	0.01
	(0.02)	(0.02)	(0.02)
T2A	−0.02	0.02	−0.00
	(0.03)	(0.02)	(0.02)
T2B	−0.02	0.01	0.01
	(0.03)	(0.02)	(0.02)
Num. Missing	196	127	131
Num. obs.	1466	1466	1466

Notes The dependent variable in each model is a binary indicator that takes a value of 1 if the outcome of interest is missing for that observation and 0 otherwise.. Models are estimated using the full set of controls described in section 3.5.4. I use HC2 heteroskedasticity-consistent standard errors. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

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