

Land restitution and selective violence: Evidence from Colombia¹

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Abstract

Victims' reparation policies and solving agrarian disputes are fundamental aspects to build peace after a civil conflict. In 2014, in Colombia, a ceasefire with the oldest Latin-American guerrilla took place and a peace agreement was signed in 2016. Although started in 2011, one of the peacebuilding and victims' reparation policies was the Land Restitution Policy (LRP) oriented to restore property rights of forcibly displaced victims. In this paper, we explore the effect of the LRP on violence against social leaders. These actors represent the interests of their communities, oppose the expansion of illicit activities in their territories and are guarantors of informal property rights in most of Colombian rural areas. Hence, in this article we determine whether or not a comprehensive intervention, such as the LRP, had spillover effects in social leaders' exposure to violence. We show that the LRP reduced social leaders' killings. Yet, the effect depends on both the intensity of the policy's implementation and its interaction with improved territorial security conditions. Our results suggest a reduced rate of social leaders' killings in municipalities in which LRP was more intense (measured by the number of active processes registered in the program) after the ceasefire with the FARC. In absence of the LRP, after the ceasefire with FARC, the rate of social leaders' killings would have been 1.8 times higher. We explain our findings by an improvement of socioeconomic conditions, an increase in trust within beneficiaries' communities, and the design of a security intelligence mechanism implemented within the policy.

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Introduction

Violence and civil conflict are one of the dimensions development economics constantly explores. Even if there are improvements regarding civil conflicts, there are latitudes where these phenomena remain unsolved. In some contexts, such as Colombia, one of the most vulnerable actors during conflict are social leaders. These leaders represent their communities- interests and needs- and are, in some occasions, the only link between communities and formal State institutions (Velásquez Ospina, 2017). Moreover, in a context of conflict, social leaders oppose illegal armed groups and the expansion of illicit economies (Arjona, 2016; Lobo & Vélez, 2022). This opposition to the interests of land dispossession perpetrators increases their risk and it is one of the main causes of their killings (Ibáñez, 2008; Garay Salamanca et al., 2011; Velásquez et al., 2021; Saffon & Sánchez, 2019). Hence, these actors remain in constant exposure to violence and their risk threatens conflict resolution as it weakens social cohesion and expose entire communities to violent actors in their absence (Gutiérrez, 2020).

One of the causes of conflict is land property and agrarian disputes (Cramer & Richard, 2011; Cramer & Wood, 2017; Gutiérrez, 2019; Wood, 2003; Thomson, 2011; Arjona 2016). On the one hand, one of the roots of civil conflicts is the unequal distribution of land. In Colombia, the main reason to form left-wing guerrillas was to contest and protest for the unequal possession of land and the exclusion of peasants to formal property rural titles. On the other hand, civil conflict intensifies rural disputes and land property inequality. Intensification of violence displaced peasants from their land and vulnerate their rights for several years. Hence, one of the policies oriented to build peace and to repair victims of conflict is the restitution of their land. Despite the evidence of the association between land property and civil conflict, there is no evidence of the effects of land restitution to victims on violence. These interventions aim to solve agrarian legacies of conflict and to protect systematic victims of conflict through a comprehensive design.

In this paper, we estimate the effect of the Land Restitution Policy (LRP) in Colombia on peacebuilding actors such as social leaders. This policy was design in 2011, prior to the public beginning of the peace process between the Colombian government and the former guerrilla Fuerzas Armadas Revolucionarias de Colombia (FARC). The unilateral ceasefire declared by the FARC occurred in December 2014 and the final agreement was signed in November 2016. The LRP aims to repair victims through the restitution of the land from where victims were forcibly displaced. This policy was designed comprehensively as it included an intelligence mechanism anticipating the potential territorial security throwbacks. Hence, one of its objectives is to maintain territorial stabilization and contribute to peacebuilding. Social leaders, on their hand, worked in the implementation of several peacebuilding policies and their killings increased in recent years (Marín Llanes, 2022; Prem et al., 2021). In absence of special protection and guarantying their lives, the peacebuilding process and the end of conflict is at risk (Gutiérrez, 2020).

To determine the impact of the program on social leaders' killings we used an event study strategy where the treatment group is composed by municipalities where the LRP started, that is, where institutional presence and fieldwork to collect evidence officially begun, and the control group is composed by municipalities in which the victims' request did not trigger a probatory process yet. In those models we controlled for observable characteristics of the

municipalities such as presence of armed groups, institutional capacity, among other variables, and for unobservable time-invariant characteristics. The identification strategy was complemented with a qualitative approach oriented to understand the structure and design of the LRP. Hence, we carried out structured interviews to policy makers, politicians and militaries in charge of the design and implementation of the policy. Our qualitative component included interviews to former President of Colombia, Juan Manuel Santos, that lead the political process to implement the policy, the former director of the LRP and a General in charge of the security component of the policy, among others.

We found a significant reduction in social leaders' killing in those municipalities where the LRP was implemented. However, both the intensity of the policy's implementation and the ceasefire with the FARC mattered. In absence of this intervention, social leaders' killings would have been 1.8 times higher during the period after the ceasefire. Indeed, we find results only in the highest quartile of treated municipalities, that is, where the share of registered land is more intense, pointing to the importance of the scale and comprehensiveness of the intervention to reverse the agrarian legacies of the conflict. These results could be explained by some components we identified in the interviews. On the one hand, the design of the policy anticipated the potential security throwbacks land restitution could have. Therefore, the policy design included an intelligence mechanism oriented to guaranteeing the stabilization and the improvement of security conditions after the intervention. On the other hand, both the empirical evidence and our qualitative results point to a comprehensive improvement on socioeconomic conditions, and community's trust due to the intervention (Bogliciano et al., 2019; Bogliciano et al., 2021; Maldonado et al., 2020). Hence, the reduction in civil conflict violence, oriented to social leaders, caused by the LRP is explained by the comprehensiveness of a policy that simultaneously increased wellbeing and anticipated potential security threats.

Additionally, by investigating the effects of the LRP on community leaders, a specific type of social leaders directly linked to the probatory process of the policy, we found a more direct channel (Velásquez Ospina, 2017). Indeed, across the country there is evidence that community leaders serve as guarantors of informal land transactions. In this case, LRP reduces the likelihood of community leaders being killed by offering a comprehensive approach to restitution in which security (including that of legal witnesses) is a key consideration during the intervention.

In fact, in most of Colombian municipalities land informality is predominant, contributing to land disputes and conflict (Cramer & Richard, 2011; Cramer & Wood, 2017; Gutiérrez, 2019; Wood, 2003; Thomson, 2011). In these contexts, where the rule of law is ineffective, landownership is largely a sociological question depending on social relationships (Alston et al, 2009). In these regions no land cadaster has been created or updated in years, registry offices are days away in distance, and notary offices have been either destroyed or co-opted by illegal actors. The praxis of landownership in those settings largely depends on social relationships, especially those backed up by community leaders whose signatures on "sell-letters" are recognized by rural dwellers as binding. In these settings, when land restitution programs are implemented – either, during armed conflict as means to gain legitimacy, or after armed conflict as peacebuilding initiatives - their success depend on protecting the lives of social leaders and in particular, community leaders. These actors become targets of

violence as they testify on the identification of *rightful occupants* and therefore, against perpetrators who displaced original occupants or second occupants who bought the land from perpetrators.

Lastly, independently of whether the restitution cases were advanced before or after the ceasefire, we only find significant results after the ceasefire, suggesting that the effects of the LRP on the killing of social leaders only shows its potential with the overall de-escalation of conflict. In municipalities where the LRP was not implemented with enough intensity, it did not reduce social leaders' killings neither before nor after the ceasefire. Additionally, we do not find statistical difference in most of municipality's characteristics between municipalities with higher implementation intensity and control municipalities. We specially focused on rural institutional capacities at the local level and do not find statistical differences. Hence, we guarantee the effects are driven by the interaction between the LRP and the ceasefire instead of being correlated with pre-treatment differences in the capacities of rural institutions.

The contribution of this paper is twofold. Firstly, it speaks to the literature exploring the effects on civil conflict dynamics and violence of specific policies. In this regard, there is evidence of the increase in violence due to antidrug policies such as aerial spraying, illicit crops' substitution and to the power vacuum in gangs (Abadie et al., 2014; Dell, 2015; Marín Llanes, 2022). Moreover, there is evidence of increases in violence explained by changes in opportunity costs due to social insurance programs, fiscal incentives, land reforms and marriage inequality (Albertus & Kaplan, 2013; Fetzer, 2020; Rexer, 2022; Vanden Eynde, 2016). By analyzing the LRP, aiming to repair victims and solving the roots of agrarian conflicts, this study contributes to this literature providing causal evidence of the effects of this intervention on conflict, protecting essential peacebuilding actors, such as social leaders.

Secondly, this paper is related to the literature on peacebuilding policies and reparation of victims. There is evidence of the positive effects of victims' reparation on socioeconomic indicators, financial access and human capital accumulation (Guarín et al., 2022). Moreover, the LRP increased economic wellbeing, microcredit access and community's trust (Bogliciano et al., 2019; Bogliciano et al., 2021; Maldonado et al., 2020). However, prior to this paper, there were not, to the best of our knowledge, empirical approaches exploring spillover effects on conflict dynamics of a victims' reparation policy. This study contributes to fill this gap in the economic literature.

The remaining of this paper is organized as follows: Section 2 describes the Colombian conflict-related context and the LRP. Section 3 develops the methodological approach to estimate the impact of the policy on social leaders' killings. Section 4 presents the main findings of the paper. Section 5 explores mechanisms and Section 6 concludes.

The LRP and the Colombian agrarian war

This section is based on official information about the design of the LRP and complemented with semi-structured interviews to former government officials, Ministers, and a former President of Colombia involved in the design of the program. The LRP's main objective -

since its implementation in 2011- is to restore agrarian peace in the countryside. The agrarian legacy and its relationship with the Colombian conflict has been largely studied in the literature showing how violent conflicts have roots in, and are shaped by, agrarian structures and the struggles to change them, including the establishment of conflict local orders (Cramer & Richard, 2011; Cramer & Wood, 2017; Gutiérrez, 2019; Wood, 2003; Thomson, 2011; Arjona 2016). Extreme inequity, informality in the structure of property rights, and violence interacted in vicious cycles, deepen the armed conflict, and made it difficult for the State to reestablish the rule of law. Some of the mechanisms linking the agrarian question and the Colombian conflict include the establishment of informal settlement by landless peasants to resist persecution and *capture* of entire municipalities by armed groups, including the army, followed by massive land dispossession of informal tenants.

The LRP theory of change involves reversing the agrarian legacies of the war bearing in mind that each territory has its own legacy of agrarian conflicts. Indeed, these wartime legacies on agrarian dynamics range from the deep transformations on both land distribution and its use, to the maintenance of illegal dispossession through networks of front men. In these circumstances, it is not only risky for the victims to make claims for the land, due to the link between their dispossession and the political-military power of the dispossessing elites, but an entire institutional framework is required to intervene in the territory to promote the conditions for legal restitution and effective return of victims.

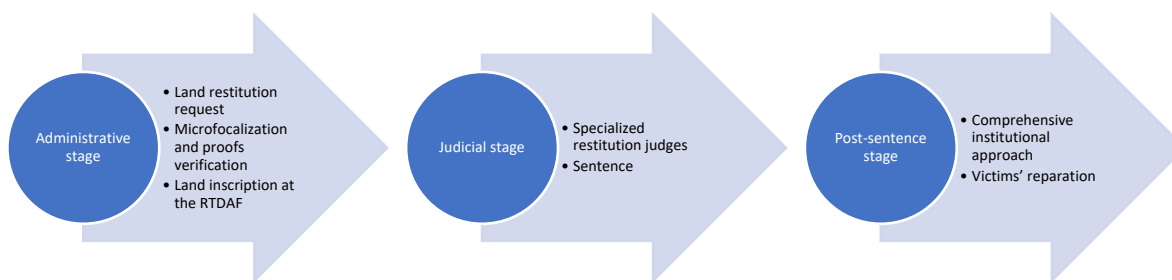
LRP includes municipal-level interventions and inter-agency projects aiming at deactivating the agrarian legacies of the conflict as one of the key components of the war. LRP aims to restore the rights of land dispossession victims by providing formal land titles, connect families to social services, and provide funds to invest in livelihood sustaining activities. In its design, the LRP requires, before the intervention takes place, a security clearance from an intelligence board. The details of the design of the LRP and the evolution of the policy are presented in the next subsection.

In June 2016, the Colombian government and the FARC signed a historical Peace Agreement formally ending 60 years of civil conflict. The LRP was recognized and endorsed within the agreement. The peace negotiation meant the demobilization of the oldest Latin America guerrilla and an opportunity to re-incorporate former combatants. For the Colombian State, it represented an opportunity to ensure its presence in the territory, to increase political participation and to repair conflict victims through the establishment of rural property rights attacking the roots of the agrarian conflict. It also drastically increased the number of municipalities intervened through the LRP.

LRP process

A typical LRP process involves three stages depicted in Figure 1: (i) administrative, (ii) judicial and (iii) post-sentence. The administrative stage begins with the restitution request, continues with the security and intelligence analysis to microfocalize territories, and ends with the formal inscription of the request whereby the probatory process is conducted in the municipality.

Figure 1. LRP stages



In the first stage, displaced victims make a formal restitution request to the URT, which is the administrative agency in charge of the implementation of the policy. Subsequently, an Integrated Intelligence Board starts an evaluation to determine whether the URT can begin the evidentiary process associated with the requested land. This stage is coordinated between the URT, the Armed Forces, the Intelligence Board and other state institutions. In the security analysis, called microfocalization, the board evaluates 17 variables related to territorial security in order to decide if the conditions enable the URT's intervention. The result of the security analysis could also constraint the intervention or suggest intervening militarily these territories to enable the subsequent intervention of the policy⁵.

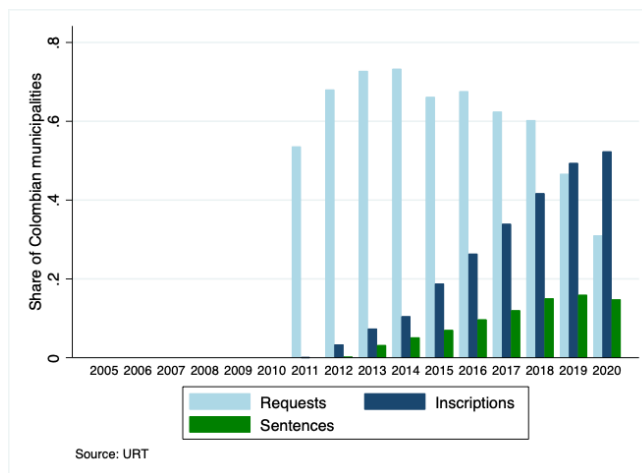
If security conditions are met and microfocalization begins, URT verifies the requirements stipulated in the Victims Law (1448 of 2011): (i) the requester had to forcibly abandon the land in the context of the conflict, (ii) the requester effectively occupied the requested land and (iii) the facts occurred after January 1st, 1991. URT visits the requested land to corroborate the information through social cartographies, interviews, among others. If the request meets the legal criteria, the land is inscribed in the Dispose and Forcely Abandon Land Register (RTDAF by its Spanish acronym). The inscription of the land is the last administrative requirement for the requester to present the lawsuit to a specialized land restitution judge. Lastly, the post-sentence stage consists of the accomplishment of the judiciary orders by multiple state agencies, coordinated through the URT.

Figure 2 presents the evolution of the LRP stages across time. This descriptive evidence suggests an increasing proportion of municipalities with land restitution requests until 2014, when 73% of Colombian municipalities had at least one request. From 2014 onwards, the share of municipalities fell to 31% in 2020. The proportions of municipalities with at least one inscription and one sentence show a secular increase. During the first year, less than 1% of Colombian municipalities had lands inscribed in the RTDAF. The share of municipalities

⁵ The 17 variables considered at the intelligence boards are tertiary roads, presence of antipersonnel mines, incidents with antipersonnel mines, permanent presence of Armed Forces, terrorist attacks, municipalities controlled by terrorists attacks, kidnappings, killings of land requesters, treats and kidnappings of land requesters, early alerts of land restitution's security system, illicit use crops, changes in homicides rate, forced displacement, armed plans of illegal groups, presence and actions of armed groups.

with inscriptions in 2020 was 52%. In terms of sentences, the proportion grew from 1% in 2012 to 15% in 2020. These descriptive statistics suggest the advances of the LRP since its start through increases in the intervened territories.

Figure 2. LRP evolution



Methods

To estimate the effect of the LRP on social leaders' killings we followed a difference-in-differences strategy allowing for differences in the period in which each municipality is treated. This methodology is an event study as the treatment does not occur in the same period across municipalities. The treatment is defined since the period in which the URT inscribed the first land plot at the RTDAF as this is the moment in which government institutions effectively intervened the territories because functionaries visited the land and validated the fulfillment of the criteria. Even with a single inscription, there is an entire institutional structure present at the territory, as well as the development of the intelligence analysis.

The restitution request is not an intervention as it does not involve the action of government agencies and it is a demand-side selection. Also, the judiciary sentence does not represent the beginning of the LRP either. To achieve a sentence in the land restitution process it is necessary to go through all the previous interventions such as the microfocalization and the verification of the evidence to register the land at the RTDAF. Additionally, the sentence is an individual process focusing on repairing victims rather than a comprehensive intervention affecting violence dynamics.

The purpose of this paper is to estimate spillover effects on violence in municipalities intervened by the LRP instead of impacts on beneficiary households. Therefore, as mentioned before, the treatment group is defined by municipalities with at least one land plot inscribed at the RTDAF and each municipality within this group is considered treated since the first land plot inscription. Formally, the specification is defined as follows:

$$y_{m,t} = \mu_m + \mu_t + \delta d_{m,t} + \beta X_m + \varepsilon_{m,t} \quad (1)$$

where $y_{m,t}$ is the social leaders' killing rate by 100.000 inhabitants in municipality m and period t ; μ_m and μ_t are municipality and time fixed effects, respectively; $d_{m,t}$ is the treatment dummy equals one from the period t when the first land was registered at the RTDAF in municipality m and onwards; X_m is a covariates matrix including baseline municipality's characteristics such as armed groups presence, illegal activities, fiscal revenues, PDET, PNIS and victims rate to control for pre-treatment observable differences between municipalities (see Table 1); and $\varepsilon_{m,t}$ is the stochastic error term clustered at the municipality level to control for potential serial correlation.

We also explore differences in the effect of the LRP after the ceasefire with the FARC. Thus, we estimate the following model to test for these heterogenous effects:

$$y_{m,t} = \mu_m + \mu_t + \delta_1 d_{m,t} + \delta_2 d_{m,t} * Cease_t + \beta X_m + \varepsilon_{m,t} \quad (2)$$

In equation (2) we included an interaction term between the treatment with the $Cease_t$ dummy variable which equals 1 from 2015 and onwards, as in December 2014 the FARC signed the unilateral ceasefire with the Colombian government. This model allows to determine if the de-escalation of conflict and the armed retirement of the FARC enabled the LRP intervention to generate differential effects on social leaders' security.

In classic difference-in-differences settings, the underlying assumption is parallel trends. Even though it is not empirically feasible to test the accomplishment of this assumption, it is possible to determine if the treatment had anticipation effects. Thus, we estimate a dynamic event study model that computes the effect of each individual period prior and after the treatment. This lead and lags model allows to determine the horizon in which the treatment had an impact on the outcome. Formally, the specification of the model is the following:

$$y_{m,t} = \mu_m + \mu_t + \sum_{t=-8}^{-2} \gamma_t d_{s,t} + \sum_{t=0}^7 \gamma_t d_{s,t} + \beta X_m + \varepsilon_{m,t} \quad (3)$$

where the structure of equation (1) is maintained except for the sums capturing the dynamic effect of the treatment. In equation (3), $d_{s,t}$ is a dummy variable equals one in period t , relative to the treatment period, for municipalities in the treatment group. Therefore, γ_t represents a vector of average treatment effects for each period t relative to the beginning of the effective treatment. To test the potential fulfillment of the parallel trends' assumption, it is necessary that every estimate prior γ_{-2} is statistically equal to zero.

The definition of the treatment allows us to determine the period in which the state intervened municipalities through the LRP. However, there are additional challenges to estimate an unbiased effect. First, the inscription of a land in the RTDAF depends on a demand-side request that could be endogenous to municipality characteristics. For this reason, we selected a sample of Colombian municipalities with at least one request. Therefore, the universe of municipalities in our sample is selected conditionally on the demand-side request and we estimated the effect within a sample of potentially treated units.

As robustness tests, we consider two additional sample selections to address potential concerns about the identification strategy. It is possible that some municipalities with restitution requests won't be treated as any of the requested plots met the law criteria. Hence, we tested baseline models with two different selection criteria. The first is to be above the

median of a conflict index or above the median victims' rate per municipality to guarantee that the sampled municipalities are effectively affected by conflict and likely to be treated as they potentially fulfill the law requirements. Second, we restricted the sample to treated municipalities. This empirical test is demanding as all municipalities are treated and the unique variation exploited is the time-difference of the treatment. Therefore, this approach is the most robust in terms of selection of the municipalities. Nevertheless, this selection reduces the statistical power of the sample, reduces the variation between treated and not-yet-treated municipalities, and increases the likelihood of biased estimates (Callaway & Sant'Anna, 2020; Gardner, 2021; Goodman-Bacon, 2021). Reassuringly, all the results of these robustness tests are consistent with our main findings.

A second concern could be the confounding effect of the LRP with the security analysis as this process was a requirement to inscribe a land in the RTDAF. Therefore, the treatment effect is certainly composed by the restitution itself and by the comprehensiveness of the policy, including the security mechanism⁶. However, we test whether or not the estimated effects are mainly driven by the lag of microfocalization. Hence, we rely on robustness tests models including a dummy variable for municipalities that were microfocalized, one or more years, prior to the first inscription. The results suggest that the effect is not driven by the lag of the security analysis. Additionally, with the dynamic event study model specified in Equation (3), we do not observe anticipation effects prior the first inscription.

Furthermore, considering recent developments on staggered timing of the treatment we employed valid inference procedures to test for consistency with our two-way fixed effects results (Callaway & Sant'Anna, 2020; Gardner, 2021; Goodman-Bacon, 2021). As Roth et al. (2022) mentioned, recent inference methods result in similar findings. Hence, we followed Callaway & Sant'Anna (2020) procedure to test for these potential sources of bias and, reassuringly, found consistent results. We present these results in Appendix 1.

The data to estimate the impact of the LRP on social leaders' killings come from two main sources. The URT provided detailed information on the number of requests, inscriptions and sentences at the municipality level on a yearly basis. Additionally, we use *Somos Defensores'* data, a Non-Governmental Organization (NGO) that monitors social leaders' killings since 2005. By employing their database, it is possible to build a panel at the municipality level with the number of social leaders killed. There are other sources reporting social leaders' killings with some differences in their reports. However, Ball et al. (2018) suggest a similar trend in the data reported by all sources of information and *Somos Defensores* is the unique source reporting data since 2005 up to 2020 disaggregating the information at the municipality level (Marín Llanes, 2022; Orbegozo, 2021; Prem et al., 2021).

Other sources of information are the CEDE municipality panel that has general characteristics of the municipalities such as its population, distance to the department capital, among other. UNODC reports annually the acres of illicit crops and for 2014 reported the presence of mining activities at the municipality level; Osorio et al. (2019) present the presence of armed groups; the ART provided information about municipalities with

⁶ An additional concern regarding the microfocalization to estimate the impact of the LRP on social leaders' killings could be that the treatment is endogenously determined by the outcome. However, as our interviews with members of the intelligence board suggested, neither social leaders' killings nor their activism are included in the 17 variables analyzed by the intelligence board.

Development Plans with Territorial Approach (PDET by its Spanish acronym) and beneficiaries of the National Integral illicit use crops Substitution Program (PNIS by its Spanish acronym); the homicides rate and the number of massacres is reported by the National Colombian Police and information on conflict victims is retrieved from the Unique Victims Register (RUV by its Spanish acronym). These variables are employed in the empirical strategy to control for observable differences among municipalities prior the treatment and to define alternative samples as will be presented later in this section. These data were organized in a balanced municipality-level panel from 2005 to 2020.

Table 1 presents descriptive statistics by treatment group in observable characteristics prior the Victims Law. First, it is worth mentioning the balance in number of observations between treatment (58%) and control (42%) groups. As expected, the treatment group has a higher victims' rate, a higher share of municipalities with armed groups presence, substitution programs (PNIS) and PDET. Additionally, this group has, in average, higher fiscal income that could represent a more dynamic economic activity and a larger institutional capacity. However, there are no statistical differences in our outcome by treatment groups. As there are observable differences, these variables at the baseline period interacted with period-fixed effects are included in the estimations.

Table 1. Municipality's pre-treatment characteristics by treatment groups

	Control group		Treatment group		Mean differences (p-value)
	N	Mean (se)	N	Mean (se)	
Social leaders' killings rate	424	0.093 (0.818)	587	0.125 (1.164)	-0.031 (0.636)
Share of municipalities with sentences	424	0 0	587	0.518 (0.500)	-0.518
Conflict victims (100.000 inhabitants)	422	197.706 (749.533)	587	302.898 (791.316)	-105.192** (0.033)
Fiscal income (log)	401	6.208 (1.843)	583	6.577 (1.736)	-0.370*** (0.001)
Mining presence	424	1.126 (3.368)	587	0.857 (2.762)	0.268 (0.165)
Armed groups presence	424	0.160 (0.367)	587	0.228 (0.420)	-0.068*** (0.008)
PNIS	424	0.040 (0.196)	587	0.066 (0.249)	-0.026* (0.071)
PDET	424	0.137 (0.344)	587	0.191 (0.393)	-0.054** (0.023)
Coca crops	424	61.014 (221.067)	587	60.974 (344.810)	0.040 (0.998)

*** p<0.01, ** p<0.05, * p<0.1

Results

Main results

This section presents the main findings of the paper. We find a negative effect of the LRP on social leaders' killings after the ceasefire with the FARC. Table 2 presents results for equations (1) and (2). The first column computes the average treatment effect of the LRP on treated municipalities. The average effect of this intervention on the rate of social leaders' killings is -0.064, however the effect is statistically insignificant. In column 2, the effect is decomposed by the period prior and posterior to the ceasefire. This result suggests a statistically significant reduction in social leaders' killing rate of 0.365 caused by the LRP in the period after the ceasefire⁷. The statistical significance of the results as well as its magnitude is economically relevant. In absence of the intervention, the killing rate would have been 1.8 times higher in this period⁸.

The third column of Table 2 presents a slight modification of equation (2) as it does not include time-fixed effects for every period in order to compute the estimate for the ceasefire fixed-effect. This model allows to estimate the positive association between social leaders' killings and the ceasefire itself, suggesting that the negative effect of the interaction between the inscription and the ceasefire is not driven by the ceasefire.

Table 2. PRT effect on social leaders killing rate

VARIABLES	Social leaders killing rate		
	(1)	(2)	(3)
Inscription	-0.064 (0.074)	0.247* (0.133)	0.247* (0.133)
Inscription * Cease		-0.365** (0.148)	-0.365** (0.148)
Cease			0.978*** (0.302)
Municipality fixed effects	✓	✓	✓
Period fixed effects	✓	✓	
Controls	✓	✓	✓
Pre-treatment mean	0.110	0.110	0.110
Observations	15,701	15,701	15,701
R-square	0.164	0.164	0.055

Standard errors, clustered at the municipality level, in parenthesis.

*** p<0.01, ** p<0.05, * p<0.1

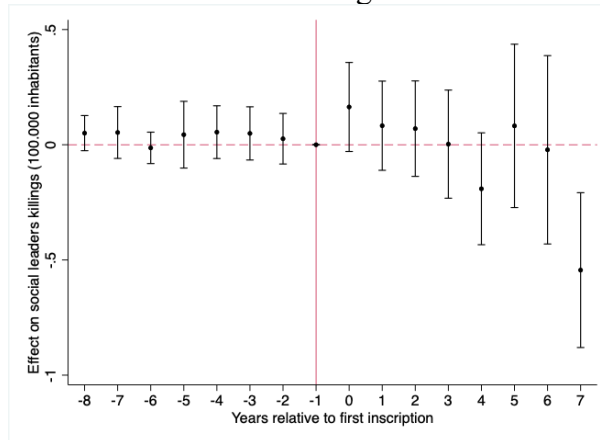
This method requires the potential fulfillment of the parallel trends assumption to attribute the effect to the policy. Figure 1 presents the graphical results of equation (3). Prior to the implementation of the policy there are no statistically significant effects. Therefore, the

⁷ These results are consistent with the Callaway & Sant'Anna (2021) estimation procedure both in sign of the effect, statistical significance, and magnitude. Employing those methods, we found a -0.286 estimate at the 95% of confidence as presented in Table A.1.

⁸ The mean killing's rate in treated municipalities was 0.457 between 2015 and 2020. Therefore, in absence of the treatment the rate would have been 0.823 instead of 0.457.

identification assumption is likely to hold in this context enabling the causal interpretation of the effects presented in Table 2.

Figure 3. Dynamic model of social leaders' killings



We present two additional results: i) the effect of each phase of the LRP and ii) the decomposition of the effect by type of social leaders.

Table 3 suggests the inscription phase is the driver of the LRP's effect on social leaders' killings. In contrast, controlling for inscriptions and sentences, municipalities with requests after the ceasefire are correlated with higher rates of social leaders' killings. This result is consistent with Prem et al. (2021) as they show a larger effect of FARC's demobilization on social leaders' killings in municipalities with land restitution requests.

In terms of the sentence, there are no statistically significant effects prior nor subsequent to the ceasefire. The unique negative and statistically significant effect is found in municipalities with registered land after the ceasefire. These findings are reasonable as the inscription of the land in the RTDAF is the only phase requiring an active intervention of government institutions in the territory through the microfocalization and the verification of law requirements. Neither the requests nor the sentences involve integral interventions as those processes are individual and one could expect individual instead of spillover effects.

Table 3. Effect on social leaders' killings by stages of PRT

VARIABLES	(1) Social leaders killings rate	(2)
Request	0.099** (0.039)	0.003 (0.048)
Request * Cease		0.179*** (0.062)
Inscription	-0.099 (0.077)	0.107 (0.140)
Inscription * Cease		-0.282* (0.161)
Sentence	0.027 (0.101)	0.414 (0.340)
Sentence * Cease		-0.417 (0.334)

Municipality fixed effects	✓	✓
Period fixed effects	✓	✓
Controls	✓	✓
Observations	15,766	15,766
R-square	0.159	0.160

Standard errors, clustered at the municipality level, in parenthesis.

*** p<0.01, ** p<0.05, * p<0.1

Note: The sample for these estimations is composed by every municipality below 200.000 inhabitants because one of the treatments was define through restitution requests.

Lastly, Table 4 presents the effect by type of social leader: Afro Colombian, Community, Indigenous, Victim Representative and Peasant leaders.

These results point to an effect on community leaders (column 3) as the driver of our results. Consistently with our theoretical framework, community leaders, usually those who sign “sell-letters” to certify informal rural property, are more involved in the implementation of national or local policies and are, in most cases, intermediate political representatives of communities to local authorities (Somos Defensores, 2020; Velásquez Ospina, 2017). Furthermore, these actors play a fundamental role between local institutions and local communities. Community leaders are a keystone in the informal rural property structure as previously mentioned. These results serve as a falsification test, as we did not find any statistical effect on leaders of ethnic communities such as Afro Colombian or Indigenous leaders. As the LRP is mostly an individual process, it is reasonable that we do not find effects on ethnic leaders as for whom demands on collective property are more important⁹.

Table 4. PRT effect by type of social leader

VARIABLES	(1) Social leaders killings	(2) Afro Colombian	(3) Community	(4) Indigenous	(5) Victim	(6) Peasant
Inscription	0.247* (0.133)	0.006 (0.008)	0.084 (0.076)	0.076 (0.000)	0.080* (0.045)	0.010 (0.029)
Inscription * Cease	-0.365** (0.148)	-0.001 (0.013)	-0.149* (0.087)	-0.093 (0.000)	-0.077 (0.047)	0.004 (0.028)
Municipality fixed effects	✓	✓	✓	✓	✓	✓
Period fixed effects	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Observations	15,701	15,701	15,701	15,701	15,701	15,701
R-square	0.164	0.126	0.102	0.148	0.080	0.118

Standard errors, clustered at the municipality level, in parenthesis.

*** p<0.01, ** p<0.05, * p<0.1

Robustness checks

It is possible that municipalities with requests are not potentially treated as the requested lands do not fulfill the Victims Law requirements, particularly being affected by the armed

⁹ There is a collective route to request restitution for collective property of Afro Colombians or indigenous communities. However, most of the processes are individuals and we ran a model controlling for municipalities with collective requests and did not find any variations in our main results. These results are available upon request to the authors.

conflict. Therefore, we restricted the sample to municipalities effectively affected by the armed conflict. This definition arises from a conflict index composed by the standardized measures of conflict victims and massacres' rates. Municipalities are selected in the sample if they are above the median of the index or above the median of the conflict victims' rate. Second, we restricted the sample to municipalities that effectively received the treatment, with at least one land plot registered, and exploit the time-variation of the treatment. In this case, there are not never-treated municipalities enabling to address any concern on selection limitations. Table 5 presents results with these two definitions of the sample and the results are consistent with the baseline findings. Reassuringly, the magnitude and the statistical significance of the estimates are similar to the ones estimated with the baseline sample.

Table 5. PRT effect on social leaders killing rate

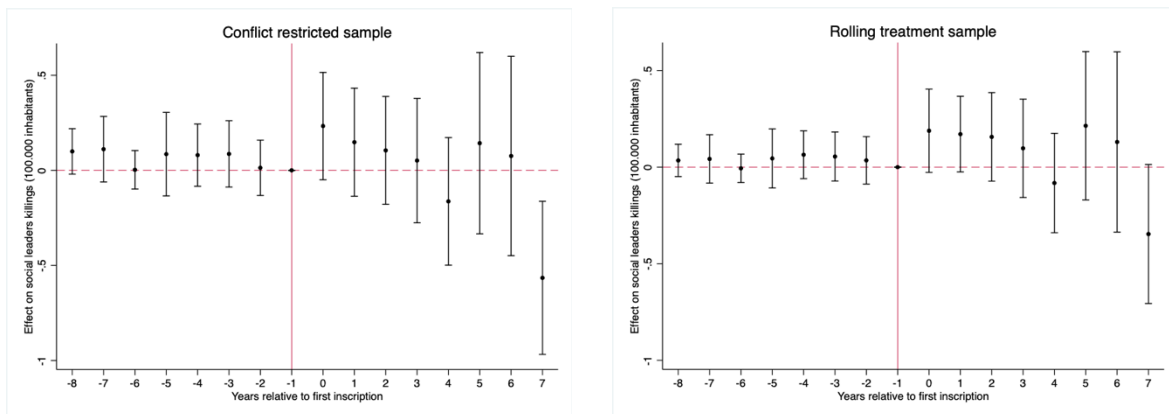
VARIABLES	(1) Conflict sample	(2) Conflict sample	(3) Rolling treatment sample	(4) Rolling treatment sample
Inscription	-0.070 (0.108)	0.245* (0.147)	-0.001 (0.074)	0.265* (0.141)
Inscription * Cease		-0.404** (0.177)		-0.360** (0.163)
Municipality fixed effects	✓	✓	✓	✓
Period fixed effects	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Observations	8,954	8,954	9,328	9,328
R-square	0.184	0.185	0.226	0.227

Standard errors, clustered at the municipality level, in parenthesis.

*** p<0.01, ** p<0.05, * p<0.1

Consistent with the main results and the methodological section, we proved with these additional samples any anticipation effects of the treatment and the potential fulfilment of the parallel trends assumption. These results are presented in Panel 1, in which the left graph corresponds to the model with the conflict restricted sample and the right one to the rolling treatment model.

Panel 1. Robustness dynamic models



Mechanisms

We explore differences on the effect of LRP conditioning on some characteristics of the intervention by municipality. As mentioned in the data subsection, we account for the number of restitution requests and inscriptions in the RTDAF at the municipality level. Therefore, we compute an intensity measure of the intervention for each municipality.

$$Intensity_{m,T} = \frac{\sum_{t=2011}^T Inscriptions_{m,t}}{\sum_{t=2011}^T Requests_{m,t}} * 100$$

where $Inscriptions_{m,t}$ represents the number of lands registered at the RTDAF in municipality m in period t , and $Requests_{m,t}$ is the number of restitution requests. Therefore $Intensity_{m,T}$ captures the level at which administrative agencies responded to the demands for restitution in municipality m through period T . This measure is increasing in the share of victims integrated to the reparation system and to the supply of public services.

In order to create a categorical variable that measures the intensity of the intervention, we computed quartiles of $Intensity_{m,T}$ and the factorial model we estimated is defined as follows:

$$y_{m,t} = \mu_m + \mu_t + \sum_{q=1}^4 \delta_q QIntensity_{q,m,t} + \beta X_m + \varepsilon_{m,t} \quad (4)$$

where $QIntensity_{q,m,t}$ corresponds to a dummy variable equals one if the municipality m belongs to the q^{th} intensity quartile. The rest of the structure of this model is the same from equation (1). Therefore, every scalar of vector δ_q represents the effect of the LRP on social leaders' killings in the q^{th} quartile of intensity.

In this case we are interested in determining if the effect is concentrated in municipalities with higher intensity of the policy independently from the ceasefire period. Similarly, to the baseline results, we specified a model interacting each intensity quartile with the ceasefire period as follows:

$$y_{m,t} = \mu_m + \mu_t + \sum_{q=1}^4 \delta_q QIntensity_{q,m,t} * Cease_t + \beta X_m + \varepsilon_{m,t} \quad (5)$$

Table 6 presents results for equations (4) and (5). From the first column, the effect of the LRP is concentrated in municipalities above the median of the intensity measure. However, when including the ceasefire interaction term, the corresponding estimates of the 3rd and 4th quartile are not negative nor statistically significant. The entire effect occurred in municipalities corresponding to the higher intensity quartile during the period posterior to the ceasefire¹⁰. The magnitude of the effect is 1.4 times larger than the average effect estimated in equation (2). One concern could be that municipalities reached the 4th quartile

¹⁰ Employing the Callaway & Sant'Anna (2020) procedure, we found similar results as the LRP effect on social leaders' killings occurred in the 3rd and 4th quartile in the period post-ceasefire. The differences lie on the 3rd quartile post-ceasefire and as we do not estimate significant results unconditional to the period in the Callaway & Sant'Anna (2020) estimation. These results are presented in Table A.4.

only after the ceasefire. Hence, we test if the effect was only present in municipalities that reached the highest quartile after the ceasefire, and we find that the results are independent from the period in which the municipality reached this quartile.

Table 6. Intensity effect of the PRT on social leaders' killings

VARIABLES	Social leaders killings	
	(1)	(2)
1 st quartile	0.082 (0.116)	0.128 (0.218)
2 nd quartile	-0.081 (0.093)	0.183 (0.193)
3 rd quartile	-0.175* (0.095)	0.294 (0.414)
4 th quartile	-0.241** (0.098)	0.226 (0.229)
1 st quartile * Cease		-0.075 (0.248)
2 nd quartile * Cease		-0.315 (0.210)
3 rd quartile * Cease		-0.533 (0.422)
4 th quartile * Cease		-0.515** (0.234)
Municipality fixed effects	✓	✓
Period fixed effects	✓	✓
Controls	✓	✓
Observations	15,701	15,701
R-square	0.164	0.165

Standard errors, clustered at the municipality level, in parenthesis.

*** p<0.01, ** p<0.05, * p<0.1

These results show that in municipalities with a higher intensity of the policy intervention, the LRP reduced social leaders' killings. Hence, we explore some observable characteristics that could explain why a municipality was categorized at some point in the higher quartile to determine if these pre-treatment differences could explain the effect on social leaders' killings. Table 7 presents mean differences between each inscription quartile and never treated municipalities (with restitution requests but with no inscription). This descriptive evidence suggests that municipalities in the higher quartile only present statistical differences with the control group in terms of illegal activities such as mining and coca crops. In the rest of the observable characteristics, these two groups are statistically similar. One relevant variable included in this analysis is the rural cadastral update. This variable is the latest year in which the administrative authority updated the cadastral register in the rural area of the municipality. In case of finding differences in this variable, it would be possible that municipalities in the 4th quartile have a stronger rural institutional capacity unrelated to the LRP. However, the difference is statistically equal to zero, reassuring that there are no observable differences between the 4th quartile's municipalities and the control. Therefore, the effect computed in this paper is not explained by these observable characteristics as there are no differences between these municipalities in most of conflict-related variables, nor being at the top quartile is related to pre-treatment characteristics of the municipalities.

Table 7. Municipality's characteristics by quantile in pre-treatment

	Control	Q1 (de)	Mean dif (p-value)	Q2 (de)	Mean dif (p-value)	Q3 (de)	Mean dif (p-value)	Q4 (de)	Mean dif (p-value)
Coca acres	72.259	120.692	-48.433	54.073	18.186	40.53	31.73	7.617	64.642**
	335.209	440.592	0.194	240.274	0.555	227.608	0.279	37.952	0.019
Mining activities	0.176	0.188	-0.012	0.139	0.037	0.185	-0.01	0.094	0.082**
	0.381	0.392	0.759	0.347	0.308	0.39	0.79	0.293	0.017
Neo-paramilitary groups	0.123	0.333	-0.21***	0.19	-0.067**	0.192	-0.069**	0.154	-0.031
	0.329	0.473	0	0.394	0.048	0.395	0.034	0.363	0.326
Left-wing armed groups	0.11	0.162	-0.053	0.088	0.022	0.093	0.017	0.06	0.049*
	0.313	0.37	0.12	0.284	0.456	0.291	0.553	0.239	0.078
Armed groups	0.233	0.496	-0.263***	0.277	-0.044	0.285	-0.052	0.215	0.018
	0.504	0.665	0	0.578	0.383	0.534	0.281	0.444	0.694
Fiscal income (log)	6.226	6.785	-0.559***	6.689	-0.463***	6.753	-0.527***	6.183	0.043
	1.828	2.107	0.005	1.681	0.009	1.707	0.002	1.422	0.795
PNIS	0.042	0.128	-0.086***	0.073	-0.031	0.06	-0.018	0.02	0.022
	0.2	0.336	0	0.261	0.138	0.238	0.366	0.141	0.222
PDET	0.136	0.299	-0.163***	0.161	-0.024	0.172	-0.036	0.168	-0.032
	0.343	0.46	0	0.368	0.475	0.379	0.278	0.375	0.342
Massacres	0.08	0.224	-0.143***	0.096	-0.016	0.124	-0.044	0.056	0.024
	0.357	0.547	0.001	0.241	0.625	0.501	0.242	0.119	0.417
Victims	241.124	408.701	-167.577*	241.088	0.036	264.755	-23.631	209.235	31.889
	1024.384	517.95	0.087	418.381	1	442.074	0.783	547.156	0.717
Homicides rates	25.987	36.059	-10.073***	32.543	-6.556**	29.025	-3.038	22.799	3.188
	33.08	31.305	0.004	36.206	0.05	30.052	0.321	23.377	0.28
Rural cadastral update	2008.582	2009.582	-1.000	2008.951	-0.369	2008.548	0.034	2008.136	0.446
	6.389	5.999	0.145	5.611	0.547	6.185	0.955	6.179	0.455
Urban cadastral update	2008.115	2009.218	-1.103	2009.538	-1.422**	2008.696	-0.581	2008.533	-0.418
	6.590	5.892	0.101	4.433	0.016	5.125	0.319	5.293	0.469
Total inscriptions	0	7.529		19.366		45.323		117.515	
	0	10.434		36.720		66.167		199.197	
Inscriptions (% of total legal rural properties)	0	0,212		0,330		0,603		2,000	
	0	0,387		0,529		0,969		3,733	

*** p<0.01, ** p<0.05, * p<0.1

Lastly, we considered information on the lag of the microfocalization with respect to the first land inscription as mentioned in the methodological section. In that model we included a dummy variable that equals one if the microfocalization occurred at least one year prior the inscription in order to determine if the results are driven by this process. Results of this estimation do not vary from baseline results nor the estimate corresponding to the lag variable is statistically significant. Secondly, we included a dummy variable equals one if the municipality has collective requests from Afro Colombian or indigenous communities. Here again, results were similar to the baseline model and the corresponding estimate of the

collective requests was statistically insignificant. Lastly, judiciary sentences can have different mandates as the judges can restitute the land to the victims and include productive projects, housing subsidies, credit alleviation, among others. We estimate a model considering different intensities of the judiciary sentences at the municipality level and we did not find any heterogenous effects at this level. These results are not presented in this paper but are available upon request to the authors.

Conclusion

We estimate the effect of the LRP in Colombia on social leaders' killings. LRP is an unprecedented policy aiming to restore property rights of forcibly displaced victims in Colombia. We found that in absence of the policy, in the period after the ceasefire with the FARC, the rate of social leaders' killings would have been 1.8 times higher. Therefore, in the period corresponding to the de-escalation of the civil conflict in Colombia, the LRP contributed to protect social leaders and stabilize conflict-related territories. Community leaders, in particular, were protected by the policy, a consistent result with the informal rural property structure in Colombia as they are a keystone to certify victims' informal property and are the link between local institutions and communities (Velásquez Ospina, 2017).

The LRP's effects on social leaders' killings were concentrated in municipalities where the policy was implemented with higher intensity. Moreover, our findings are not sensible to the period in which the policy reached the highest implementation intensity level. Moreover, we do not find robust evidence suggesting unbalance on observable characteristics between municipalities in which effects were estimated versus control municipalities. Therefore, we conclude results are driven by the interaction of the policy with the de-escalation of the conflict.

There are specific aspects of the policy design explaining the estimated effects. First, the joint effort of the Armed Forces and state institutions in the integrated intelligence boards suggest an alternative to guarantee security conditions on conflict-affected territories. This in turn seems to have a positive spillover effect on the stabilization of these regions through the reduced risk of social leaders. These mechanisms could be replicated in other policy interventions such as illegal crop substitution programs (Marín Llanes, 2022). Second, LRP is oriented to improve victims' socioeconomic conditions through an integrated approach. The policy certainly considers the formalization of rural property, but it also includes an integrated state presence through credit alleviation, productive projects, among others. Moreover, the policy also generates trust within beneficiaries (Bogliacino et al., 2019). Former functionaries argued that victims mentioned the LRP as the first policy in which victims were put in the first place and felt the support of formal institutions.

These results tell a story of comprehensive rural interventions as effective mechanisms contributing to reverse agrarian legacies of conflict. In the absence of LRP, the ceasefire between the FARC and the Colombian government do not reduce lethal violence against social leaders. However, in municipalities where both the LRP was implemented and ceasefire coincide, a more secure environment emerges due to the integrality of the transformation in rural dynamics. The ceasefire enabled a de-escalation of conflict, reducing more direct legacies of the conflict and enabled a complete operation of the policy. With the

LRP the land question is being addressed, including social inclusion through welfare programs, and some of the agrarian legacies of the conflict affecting social leaders. The overall effect improves the social environment in which social leaders operate.

In this paper we explore the effects of an agrarian policy aiming at reestablishing victims' property rights on selective violence. However, it will be worth to explore its effects on other measures of violence to have a more holistic understanding. Evidence on violence or criminal activity of property formalization is scarce and should be pursued.

Lastly, recent violence against LRP' officers and land claimants evidence the risk of land restitution in a peacebuilding context. At the same time social leaders' killings have increased in recent years suggesting a potential new wave of violence in Colombia (Gutiérrez, 2020; Marín Llanes & Vélez, 2021). Hence, the role of the LRP in the actual context is relevant and the extended term of the Victims Law must be exploited to consolidate peace: ensuring security in conflict-affected territories, guaranteeing protection to local political actors such as social leaders, and repairing victims. Evidence from the new term should be explored to identify new potential gaps and the long-term effects of LRP.

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Appendix 1: Results employing Callaway & Sant’Anna (2021) procedure

In this paper we estimated our models employing the Callaway & Sant’Anna (2021) procedure in order to test if our TWFE are robust to a more transparent inference method (Gardner, 2021; Goodman-Bacon, 2021). This method consists of estimating individual effect for each cohort of treated municipalities employing never treated municipalities as controls and employing no negative weights to average the effects. Both the structure and the variables included in our TWFE model are maintained in these robustness models.

The only variation we made, seeking for stronger assumption requirements, is that we do not interact variables for heterogeneous effect. Instead, we subsample conditioning on some characteristics in order to test if assumptions hold individually. For example, instead of interacting the effect of the LRP with the period posterior to the ceasefire, we ran a separate model for the period previous to the cease and an another for the period post. Thus, we can estimate dynamic effects for each model and test for its consistency. Moreover, this approach enables to test jointly for pre-treatment effects equal to zero for each cohort and each time-period.

All the results employing the Callaway & Sant’Anna (2021) inference method and defining the control group as never treated municipalities are consistent with the TWFE findings presented in the paper. There are slight variations such as we find null effects for the higher quartiles unconditional to the time period. We only find statistically significant results for the 3rd and 4th quartile in the post-cease period.

Table 1. LRP ATTs on social leaders’ killings

VARIABLES	(1) Full sample	(2) Pre-cease	(3) Post-cease
ATT	-0.117 (0.117)	0.360 (0.312)	-0.286** (0.143)
Baseline controls interacted with fixed effects	✓	✓	✓
Municipality fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Pre-treatment equal to 0 (χ^2)	111.606	84.418	9.337
P-value	0.117	0.346	0.500
N	15,701	9,809	4,638

Robust-cluster standard errors in parenthesis

*** p<0.01, ** p<0.05, * p<0.1

Panel 1. Dynamic ATTs by sample period

a. Full sample

b. Pre-cease

c. Post-cease

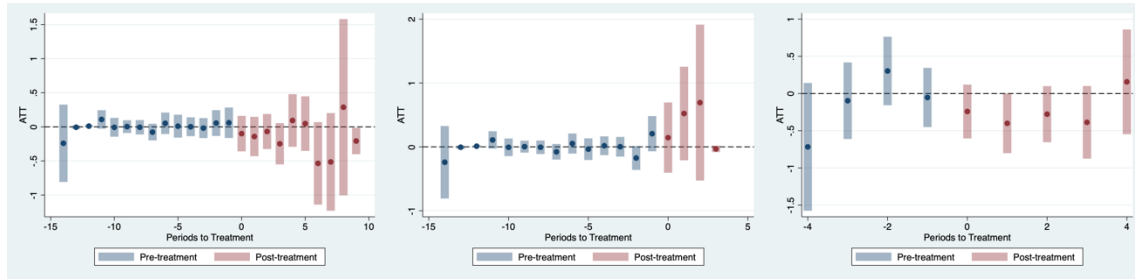


Table 2. LRP ATTs on social leaders' killings – Robustness checks

VARIABLES	(1) Conflict-related sample			(5) Rolling treatment sample		
	Full sample	Pre-cease	Post-cease	Full sample	Pre-cease	Post-cease
ATT	-0.116 (0.175)	0.453 (0.379)	-0.394* (0.217)	-0.233* (0.131)	0.177 (0.175)	-0.473* (0.244)
Baseline controls interacted with fixed effects	✓	✓	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Pre-treatment equal to 0 (χ^2)	163.307	113.616	9.950	125.268	83.779	2.482
P-value	0.000	.005	0.445	0.017	0.364	0.004
N	8,953	5,593	2,382	8,281	5,830	1,615

Robust-cluster standard errors in parenthesis

*** p<0.01, ** p<0.05, * p<0.1

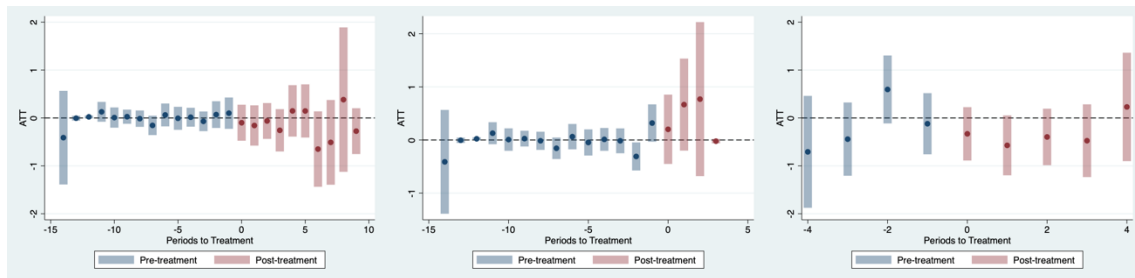
Panel 2. Dynamic ATTs by simple period and selection

I. Conflict-related sample

a. Full sample

b. Pre-cease

c. Post-cease



II. Rolling treatment sample

a. Full sample

b. Pre-cease

c. Post-cease

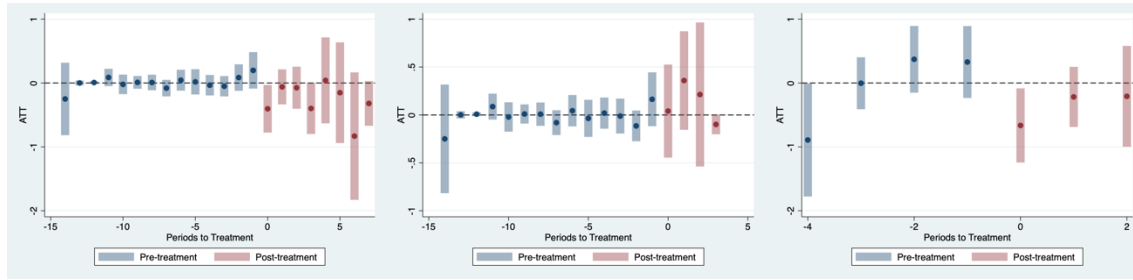


Table 3. LRP ATTs on social leaders' killings by leadership type

	(1)	(2)	(3)
ATT	Full sample	Pre-cease	Post-cease
Community leaders	-0.131*	0.028	-0.187*
	(0.073)	(0.102)	(0.106)
Pre-treatment equal to 0 (χ^2)	70.112	40.921	7.013
P-value	0.893	0.998	0.724
Afro leaders	-0.000	0.004	-0.012
	(0.014)	(0.010)	(0.023)
Pre-treatment equal to 0 (χ^2)	25.816	14.948	10.396
P-value	0.999	1.000	0.238
Peasant leaders	0.049	0.153	0.028
	(0.052)	(0.172)	(0.045)
Pre-treatment equal to 0 (χ^2)	53.46	41.333	8.748
P-value	0.992	0.996	0.556
Indigenous leaders	-0.037	0.063	-0.059
	(0.073)	(0.235)	(0.063)
Pre-treatment equal to 0 (χ^2)	62.849	45.455	10.228
P-value	0.587	0.693	0.421
Victims leaders	0.021**	0.083*	0.017
	(0.010)	(0.049)	(0.016)
Pre-treatment equal to 0 (χ^2)	102.130	75.410	4.291
P-value	0.056	0.308	0.637
Baseline controls interacted with fixed effects	✓	✓	✓
Municipality fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
N	15,701	9,809	4,638

Robust-cluster standard errors in parenthesis

*** p<0.01, ** p<0.05, * p<0.1

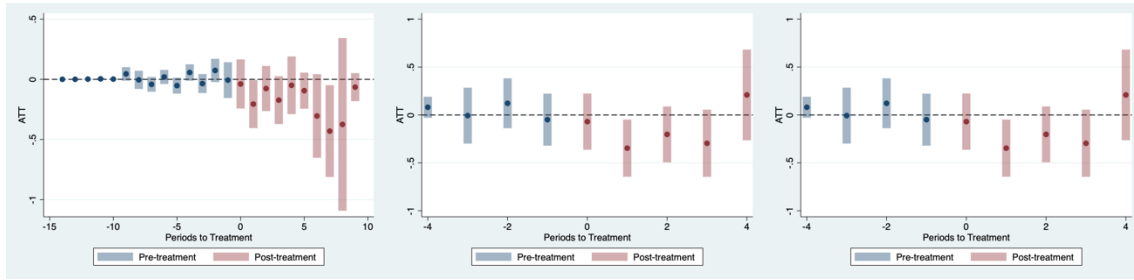
Panel 3. Dynamic ATTs by leadership type

I. Community

a. Full sample

b. Pre-cease

c. Post-cease

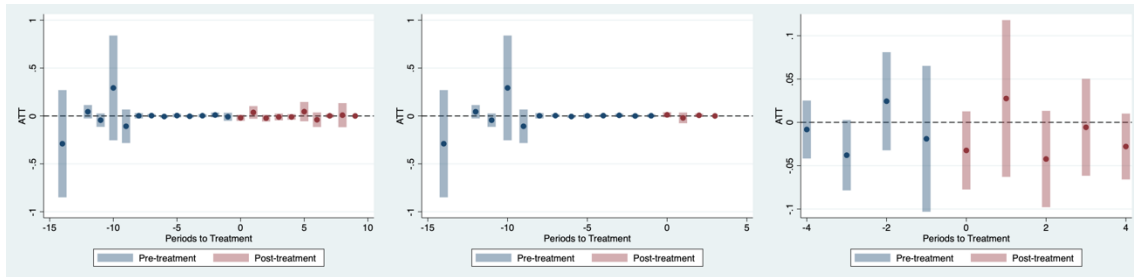


II. Afro

a. Full sample

b. Pre-cessé

c. Post-cessé

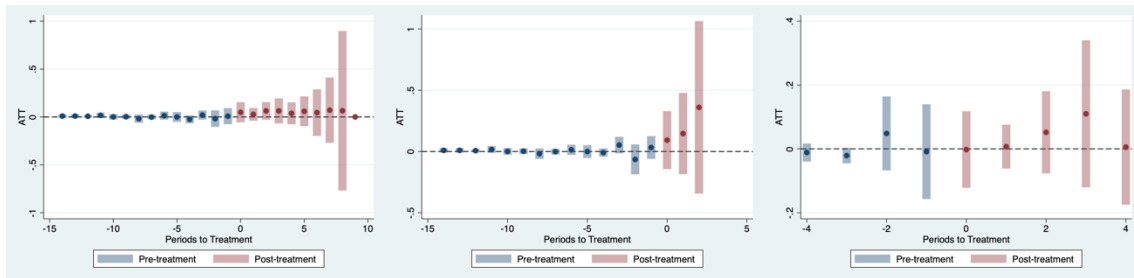


III. Peasant

a. Full sample

b. Pre-cessé

c. Post-cessé

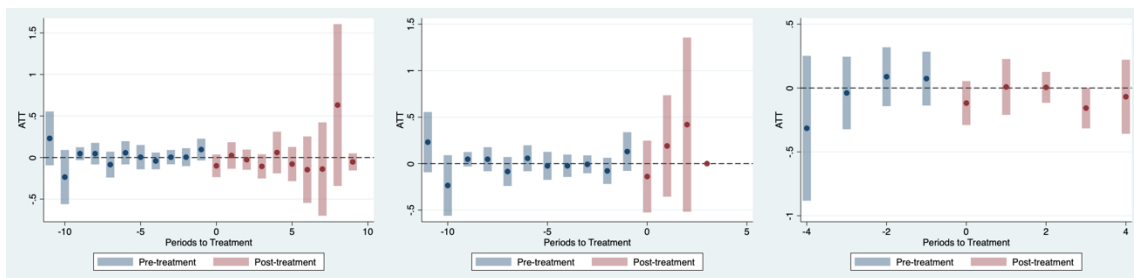


IV. Indigenous

a. Full sample

b. Pre-cessé

c. Post-cessé



V. Victims

a. Full sample

b. Pre-cessé

c. Post-cessé

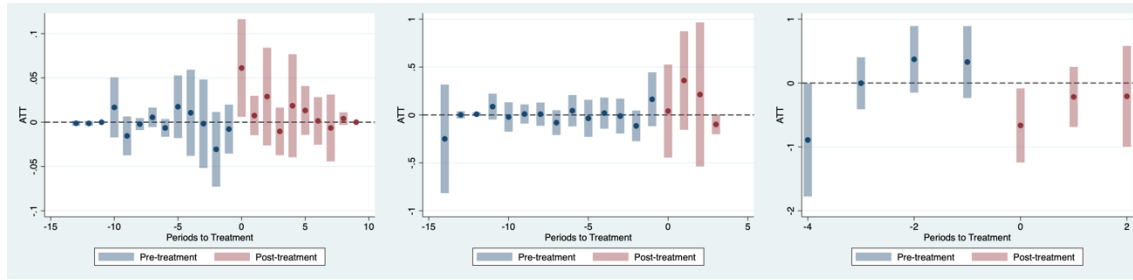


Table 4. LRP ATTs by policy implementation intensity

ATT	(1) Full sample	(2) Pre-cease	(3) Post-cease
1st Quartile vs never treated	0.114 (0.272)	1.307 (1.205)	0.084 (0.323)
Pre-treatment equal to 0 (χ^2)	239.679	168.818	4.767
P-value	0.000	0.000	0.906
N	8,277	5,169	3,054
2nd Quartile vs never treated	-0.256 (0.189)	0.332 (0.276)	-0.386 (0.259)
Pre-treatment equal to 0 (χ^2)	382.002	343.812	9.873
P-value	0.000	0.000	0.452
N	8,677	5,419	3,012
3rd Quartile vs never treated	-0.040 (0.164)	0.371 (0.490)	-0.296* (0.172)
Pre-treatment equal to 0 (χ^2)	173.982	77.062	11.004
P-value	0.000	0.412	0.357
N	8,901	5,559	2,910
4th Quartile vs never treated	-0.082 (0.207)	0.374 (0.458)	-0.430** (0.211)
Pre-treatment equal to 0 (χ^2)	123.011	65.430	6.603
P-value	0.028	0.880	0.762
N	8,965	5,599	2,844
Baseline controls interacted with fixed effects	✓	✓	✓
Municipality fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓

Robust-cluster standard errors in parenthesis

*** p<0.01, ** p<0.05, * p<0.1

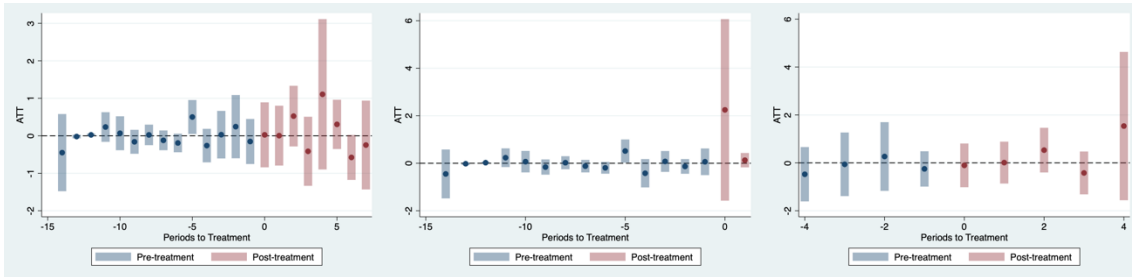
Panel 3. Dynamic ATTs by policy implementation intensity

I. 1st Quartile

a. Full sample

b. Pre-cease

c. Post-cease

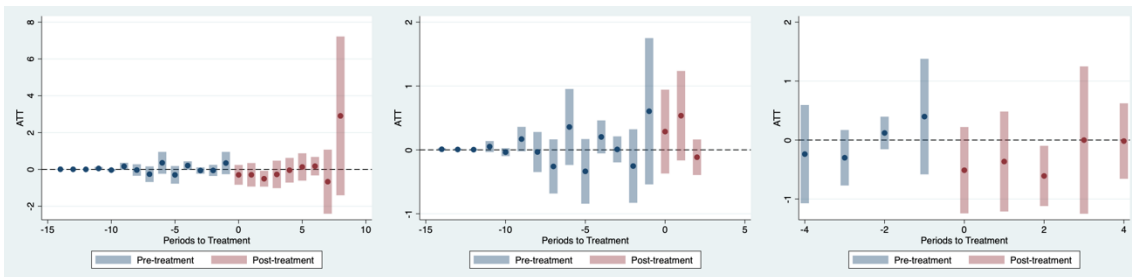


II. 2nd Quartile

a. Full sample

b. Pre-cess

c. Post-cess

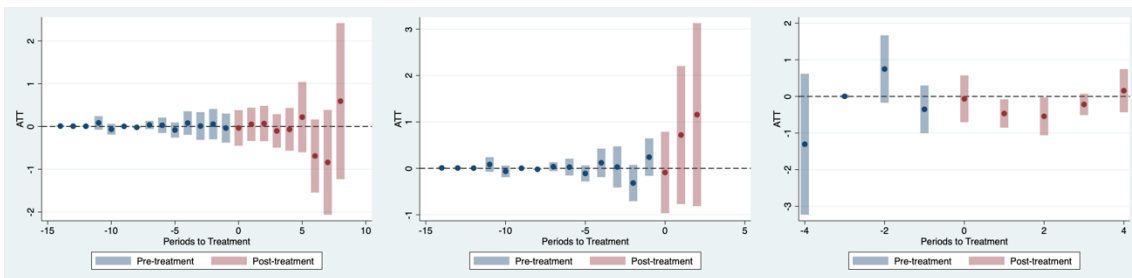


III. 3rd Quartile

a. Full sample

b. Pre-cess

c. Post-cess



IV. 4th Quartile

a. Full sample

b. Pre-cess

c. Post-cess

