Land Titling, Race, and Political Violence: Theory and Evidence from Colombia

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Abstract

Previous research has found that elites often use de facto political power to counter de jure (re)distribution of material, political, and social power. Yet less is known about the underlying conditions that influence elite decisions regarding the type of reforms to offset. We argue that elites are more likely to use de facto power to undermine racially targeted reforms in states that are stratified by both race and class. Following transitions, structurally disadvantaged racial groups are more likely to be the beneficiaries of de jure reforms and thus the targets of counter-reform violence. We test our theory using original data collected by the Observatorio de Territorios Étnicos y Campesinos (OTEC) on the largest communal land reform program undertaken in Latin America: the titling of collective lands belonging to black communities in Colombia. Using both difference-in-difference and regression discontinuity approaches, we show that the legal recognition of black collective property rights increased right-wing paramilitary attacks in municipalities where black communities mobilized to requisition formal land titles. As a further step, we show that the impact of titling reforms on political violence can be explained by greater state coercive capacity and institutional capture by counter-reform elites. These findings together offer new insights into the puzzling racialization of the Colombian civil conflict since the 1990s.

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1 Introduction

Why would redistributive reforms designed to reduce conflict sometimes prompt an increase in violence? And why do states with sufficient legal and coercive capacity occasionally stand by and let the violence happen? We argue that reform under conditions of ethno-racial stratification—contexts in which group membership determines the distribution of status, material benefits, and political power (Horowitz, 1985; Johnson, 2020; Kohler-Hausmann, 2011)—creates favorable conditions for counter-reform by encouraging greater elite investments in extra-legal forms of power. Reforms that target benefits to racially marginalized groups, a likely condition where democratization occurs within a stratified state, foster greater bonds for elite cohesion and engender a complementary relationship between their use of extra-legal violence (de facto power) and formal institutions (de jure power).[1] Compared to universal reforms (read as reforms that are not racially targeted), a racial reform is more likely to encourage counter-reform in stratified states because elite coherence and cooperation are bolstered by overlapping dimensions of existing privilege: *race and class*. Following Bonilla-Silva (1997) we define systems of racial stratification as societies where economic, political, and social power are (partially) structured by racial categories. Counter-reform violence in racially stratified societies will be more frequent where state coercive capacity and vulnerability to elite capture are high because marginal groups in the post-reform era still lack de jure power on par with elites. In sum, ethno-racial stratification is a facilitating condition for the complementary usage of de facto and de jure power by elites. This explains, in part, the often slow and unstable process of inclusive democratic reforms in racially stratified socieities.

We develop this argument through a longitudinal analysis of the Colombian civil conflict. Immediately upon the implementation of a historic ethno-racially targeted land reform in 1996, "*La Ley de las Negritudes*" ("The Law of Black Communities" or "Law 70"), legally constituted black communities were devastated by an unprecedented wave of targeted paramilitary violence. As of 2018, the Colombian government through Law 70 had formally recognized 181 black collective land titles on formally state-owned land that span a total of 5,396,376.3 hectares (Arango, 2018). By January 2016, an estimated 1.8 million Afro-Colombians had been internally displaced by the conflict (30% of the total displaced

[1] Racially targeted reforms are likely when democratization occurs in stratified states, especially when racial stratification was an integral component to the authoritarian regime, because of the legitimacy issues that continued structural inequality and exclusion creates for democratic governments.

population in Colombia) (Sánchez-Garzoli and Cordoba, 2016).[2] This number is still rising due to the continued displacement of black communities, primarily in the Pacific region.[3] This paper argues that the lopsided, racialized structure of both de jure and de facto power after the passage of Law 70 can explain the differential targeting of black communities by paramilitary violence.

We test this argument using a range of publicly available and hand-coded datasets, most notably a complete roster of black communities that were collectively titled under Law 70, compiled by the Observatorio de Territorios Étnicos y Campesinos at the Universidad Javeriana in Bogotá. We combine the data on black communities with disaggregated reports of armed actor violence at the municipal level and use two different estimation strategies, a difference-in-difference approach and a regression discontinuity design, to show that the surge in armed attacks was much greater in the period after the implementation of black collective titles in the municipality. This finding cannot be explained by pre-trends in violence and we find no evidence of mean reversion. Treated municipalities experienced a rapid escalation in political violence in the weeks after receiving a land title and this trend persists for more than a decade after municipalities received their first land title. From the duration of this effect, we infer that paramilitary violence is a proxy for their continued operation and capture of municipal institutions. We find that paramilitary violence in response to titling was most prevalent in municipalities with high coercive capacity (as proxied by the presence of judicial and law enforcement institutions) and vulnerability to elite capture (as proxied by local conservative governance). In addition, we find that the state capacity and capture mechanisms are unique to black collective titles and distinct from the mechanisms that explain violence against non-racial land titles and the creation of indigenous reserves (*resguardo indígenas*) that occurred both before and contemporaneous with Law 70. The mechanisms of counter-reform that predict paramilitary violence against black communities (high state coercive capacity and high vulnerability to capture) do not predict an increase in guerrilla violence.

Our argument and findings make an important contribution to the literature on de jure and de facto power in developing contexts (Acemoglu and Robinson, 2008; Ch et al., 2018; Fergusson et al., 2020). Existing studies on the persistence of elite power have argued that the smaller size of the elite allows them to invest more efficiently than the masses in de facto power to offset de jure reform. We build on this insight by explaining why elite coordination over de facto power is even more efficient when

[2] This statistic of 30% most commonly cited by activists and NGOs, is in contrast to the estimated 10% according to the Unit for the Assistance and Comprehensive Restitution of Victims. Accessed February 12, 2020.

[3] Further information at Unidad Victamas. Accessed February 12, 2020.

reforms stratify beneficiaries by race or ethnicity. We also specify the conditions under which elites may wield de jure and de facto power in a complementary fashion to first recoup their net losses from reform and then to protect their illicit gains through legal, democratic institutions (Cárdenas, 2012; Grajales, 2011, 2013; Vélez-Torres, 2014). In this way, our argument aligns with the "gaming democracy" framework articulated by Albertus and Menaldo (2014) and Albertus (2015). This is especially likely in ethno-racially stratified states, because race continues to stratify access to state power even after reform. Finally, our argument makes important contributions to the existing literature on the salience of race and ethnicity to the Colombian civil conflict and the general ethnic conflict literature. The ethnic dimension of the Colombian Civil Conflict has been under theorized in existing political science scholarship (Albertus and Kaplan, 2013; Restrepo, Spagat, and Vargas, 2004). Our argument and analysis allow us to examine how the confounded relationship between ethnicity and the proximate causes of conflict, itself the product of the historical stratification of power along ethno-racial lines, can "ethnicize" ostensibly non-ethnic conflict.

The rest of this paper proceeds as follows. In the next section we situate our study within the literatures on the persistence of elite power and civil conflict. Then we introduce the reader to the context of ethno-racial stratification, reform and civil conflict in Colombia. We build on insights from the literature review and Colombian context to outline our paper's core thesis and the observable implications that follow. We introduce the paper's data and methodology in the subsequent sections. We test our central hypotheses and include additional tests for robustness in the following sections. The last section of the paper concludes and discusses the generalizable implications from this paper's findings.

2 Literature Review

Elites that enjoy the status quo distribution of power and resources are often threatened by democratizing reforms. Elites perceive high costs to changes to the status quo, especially where levels of inequality are high. This has been a fundamental proposition in endogenous theories of democratic transitions and consolidation (Boix and Stokes, 2003; Houle, 2009; Przeworski and Limongi, 1997; Moore, 1966; Scheve and Stasavage, 2017). Two of the most widely cited studies of endogenous democratization, Acemoglu and Robinson (2006) and Boix (2003), argue that at extreme levels of economic inequality, elites oppose regime transitions to democracy because the marginal costs of redistribution are greatest.

Recent extensions to the endogenous democratization thesis find that redistribution is less likely under democracy than it is under authoritarianism. These studies still maintain the premise that economic elites fear and resist redistribution (Albertus and Menaldo, 2012; Albertus, 2015).

Land redistribution policies—those that expropriate privately owned land from the land rich to distribute to the land poor—pose a heightened threat to elites with concentrated land wealth (Albertus, Brambor, and Ceneviva, 2018; Boix, 2003). The zero-sum nature of redistributive land reform is a threat to the economic interests of landed elites because land is an immobile asset (Albertus, Brambor, and Ceneviva, 2018; Boix, 2003; Albertus, 2015). As a result, land reforms are strongly associated with enduring social conflict and elite backlash. The threats that elites fear from reform and redistribution are not limited to economic losses. For example, elites anticipating reduced political power due to reform are central to theories of the uneven expansion of the suffrage to women, marginal ethnic groups, and the poor (Caraway, 2004; Engerman and Sokoloff, 2005; Johnson III, 1999; Przeworksi, 2009; Teele, 2018; Ziblatt, 2008).

How do elites respond to reform, given that it poses threats to their power? Acemoglu and Robinson (2008) argue that the greater the democratic advantages afforded to citizens by reform, the greater the level of threat that elites will perceive and the more they will invest in political power to oppose reform. Reforms to political institutions affect the distribution of *de jure political power*. Alongside de jure political power, elites and citizens alike have *de facto political power* which are investments in non-institutional forms of power such as "wealth, weapons, or ability to solve the collective action problem" (Acemoglu and Robinson, 2008, p. 268).

In response to a change to de jure institutions, elites deploy de facto power to offset the transfer of political, economic and social power. Albertus and Menaldo (2014), applying this argument to the context of democratic transitions, find that the relationship between redistribution and democracy is weak in cases where elites retained a significant degree of power and influence over the design of post-transition institutions in order to effectively "game democracy". Ziblatt (2009) examining subnational patterns of electoral fraud in 19^{th} Century Germany, argues that democratic competition is endogenous to the pre-reform social order. Where levels of land-holding inequality were very high, elites were more likely to use electoral fraud to limit political opposition. De jure changes to institutions are most vulnerable to elite sabotage when they fail to substantially restructure access to power for the socially disadvantaged. Elites will invest in de facto power to counter reforms up until the point that the degree of power that non-elites have through de jure institutions, make additional investments in de facto power costlier than redistribution itself (Acemoglu and Robinson, 2008). For this reason, many cases of land reform fall short of meaningful redistribution (Albertus and Kaplan, 2013; Albertus, 2015; Albertus and Menaldo, 2012; Joshi and Mason, 2008; Mason, 1998; Paige, 1996). Likewise, many racial reforms remain thwarted and incomplete, because elites retain institutional footholds and sufficient de facto power to moderate or undo reform (Bonilla-Silva, 1997; Marx, 1994; King and Smith, 2005; Nobles, 2000; Omi and Winant, 1994; Sawyer, 2006).

In the empirical studies of greed and grievances in conflict there is not doubt that Colombia is a case of the former, with little to no reference to ethnic or racial factors.[4] While ethnicity and race are not root causes of the civil conflict, their role has been neglected in empirical studies about the Colombian case. We want to be clear that we are not the first to center ethnicity in the narrative of the civil conflict (Wouters, 2001; Arocha Rodríguez, 2005).[5] We bring this important, yet neglected, frame on race and marginalization into conversation with the empirical literature on the Colombian armed conflict that has mainly studied political violence through the lens of class and land. We argue that ethnicity is a crucial pillar to the civil conflict even though its origins can be traced back to land disputes and political power struggles.

This article demonstrates that in the case of the contemporary Colombian civil conflict, there is a substantial degree of the violence that is predicted by the implementation of black collective land tenure, independent of key factors that have been used to explain the incidence of conflict in previous studies. Colombia has experienced the *ethnicization of the conflict*–the disproportionate impact of an explicitly non-racial conflict on particular ethno-racial communities. There is a substantial literature on the mobilization of ethnic identities in conflict. Ethnic identities incite conflict through group-based grievances (Cederman, Wimmer, and Min, 2010; Esteban, Mayoral, and Ray, 2012; Fearon and Laitin, 2011; Gallagher Cunningham and Weidmann, 2010; Horowitz, 1985) in some cases, while in others ethnic communities provide the technology for conflict (de-)mobilization (Kalyvas, 2008; Lyall, 2010; Weidmann, 2009). Our argument departs from the micro-foundational literature on ethnic conflict, in which ethnicity explains who fights and why, in favor of the macro-structural logic of ethnicization. We contend that counter-reforms that are explicitly understood and articulated through the lens of land and class are

[4] Kaplan (2017) is the exception, who refers to the differential reaction of organized communities, indigenous and peasants, to the conflict.

[5] 2013 interview with Marino Cordoba, president and founder of the National Association of Displaced Afro-Colombians (AFRODES).

central mechanisms for maintaining systems of social, political and economic stratification by race. This is not an especial case of the Colombian conflict. Our argument that violence will increase as a result of a reform extends to situations in which marginal ethno-racial groups are the targeted beneficiaries of reform and the homogeneous composition of political, economic and social power creates the conditions for elite counter-reform violence (Bonilla-Silva, 1997; Mills, 2008; Omi and Winant, 1994).

3 Argument

We argue that the likelihood of counter-reform—elite realization of de facto power to offset the transfer of de jure power to previously excluded groups—is greater in response to racially targeted reforms in stratified states. The argument proceeds in two parts. First, we explain why the conjunction of racial stratification and identity-based reforms creates a focal point for elite grievance and raises the probability of elite counter-reform mobilization. The observable implication is that counter-reform violence will be greatest in response to racially targeted reform. Second, we explain how stratification creates unique conditions for a complementary relationship between elite de facto and de jure power. This leads to the novel prediction that counter-reform violence will be greatest under conditions of high state coercive capacity and vulnerability to elite capture.

Access to political, economic and social power in racially stratified states is unequally distributed across racial categories (Bonilla-Silva, 1997; Horowitz, 1985; Johnson, 2020; Kohler-Hausmann, 2011). It follows that under conditions of stratification the membership of the elite and non-elites is disproportionately drawn from different racial groups. Reforms are de jure transfers of political, economic, and social power to previously excluded groups. Reform is a redistributive transfer, meaning that it is zero-sum. As a result of reform, some power is transferred away from existing elites and toward marginal sectors. We can further distinguish racially targeted reforms from non-racial reforms. Under the structural context of stratification, both racial and non-racial reforms would predict a net transfer of power and resources across racial lines.[6] The difference is that in the latter case, identity crosscuts the category of beneficiaries, while in the former the beneficiaries (and the aggrieved) are racially homogeneous.

We define counter-reform as the use of de facto power to offset the transfer of power through de jure reforms. Acemoglu and Robinson (2008) refer to this as "invariance", when net losses to de jure power

[6] This assumes that marginalized racial groups are not excluded from the benefits of reform by other factors like geography and access to distribution networks.

are offset by net gains to de facto power. To achieve invariance elites need to invest in sufficient de facto power. Elite counter-reform is facilitated by their smaller number and greater expected net gains.

Elites are more likely to coordinate on investments in de facto power if the policy they are trying to undermine guarantees greater buy-in among the elite. The elite counter-reform model builds on the assumption that elites and non-elites prefer different public goods. The interests within each group are presumed to be relatively homogeneous (Acemoglu and Robinson, 2008, p. 270). Class interests provide a strong bond for elite cooperation and has been used in other contexts to explain elite cooperation to invest in counter-reform targeting (non-racial) reforms in previous studies (Albertus, Brambor, and Ceneviva, 2018). Identity-based interests (be they ethno-racial, gender-based, regional, or otherwise) also provide a focal point for elite cooperation. Cooperation could be based on an explicitly group-based motivation of animosity or perceived threat by an out-group. Elite cooperation could also be facilitated by the lower probability that elites will accrue benefits from an racially targeted reform than they would from a non-targeted reform. Some elites facing the decision of whether to coordinate counter-reform against an economically targeted reform—one that will target poor beneficiaries independent of their racial identity—may still perceive gains to accepting reform. For example, political elites with a largely poor constituency would weigh the political costs of opposing reform (electoral, legitimacy, etc.) against the social and economic gains to counter-reform. But elites in a stratified society facing the decision of whether to coordinate counter-reform against a racially targeted reform will likely perceive fewer costs to counter-reform. This is not necessarily because of explicitly racist beliefs (although it may very well be), but mainly because racially excluded groups (the exclusive beneficiaries of racial reform) lack sufficient political power to generate cross-pressures among the elite. If elites are looking at a menu of de jure reforms to undermine, they are more likely to choose the one which guarantees greater group buy-in among their peers.

Policies that stratify group membership by ethnicity make investments in de facto power less costly on the margin, specifically in stratified societies. Law 70, which granted collective land titles to black communities, meets these conditions. Law 70 served as a threat to elites' economic and political interests. Cárdenas (2012) argues that Law 70 made black communities "legible to the state" by transforming them into "political actors who must be contended with, as participants who cannot be simply swept aside" (p. 320). This reform marked a substantial redistribution of de jure political, economic and social power from economic elites to peasants, specifically black peasants, with now formalized claims to their land. Our theory predicts that violence, a significant instrument of de facto elite power, will increase in municipalities where black communities are granted collective titles in an effort to counter the material and political transfer of power. Our theory also predicts that the incidence of paramilitary attacks after black communities are titled will be greater than the incidence of paramilitary attacks after non-racially specified peasant titles are granted. A formal statement of the first two hypotheses follows.

Hypothesis 1: Armed actors (primarily paramilitaries) will commit more violent attacks in municipalities in the period after a collective land title is granted to a black community.

Hypothesis 2: Armed actors (primarily paramilitaries) will commit more violent attacks in response to new collective titles to black communities compared to new non-racially targeted land titles.

Our argument is not that race should drive our understanding of the contemporary conflict in Colombia or civil conflict more generally. Instead, we argue that where race historically structures territorial control and access to resources, an exclusive focus on the non-racial drivers of conflict not only ignores the structural discrimination of violence but misses this crucial component. Racial reforms, as opposed to universal reforms, reinforce an important cleavage that would result in a fundamentally different response from the armed actors and the elite. The same vulnerability that racial reform supposes to remedy are likely to explain an increase in the levels of violence when local armed actors can collude with local politicians to prevent the enforcement of reforms.

A key component of racial stratification is the inequality of access to state power across racial categories. This leads to the counter-intuitive expectation that elite counter reform may occur where state capacity is high. The generally accepted argument in the literature is that elites exploit state weakness to undo the effects of de jure reform (Albertus, 2015; Arjona, 2016; Fergusson et al., 2020; Gibson, 2005; Soifer, 2013). The prior would be that elite use of extra-legal violence would be highest in localities in which the state does not have the ability to monopolize violence. We do not disagree with this general relationship between low state capacity and violence perpetrated by non-state actors. Instead, we add the insight that this relationship should be conditional on the status quo balance of elite influence within state institutions.

Where elites have an outsized level of de jure power relative to non-elites, they can combine their influence over legal institutions with their de facto power. The strong bonds between different spheres of power in stratified sates (political, social, and economic) makes the power disparity between elites and marginal ethno-racial communities especially pronounced.[7] Elites can effectively launder ill-gotten gains from their use of de facto power (in this case extra-legal violence) with the cooperation of a legal institutions like courts, judges and the police. Likewise, the intended beneficiaries of de jure reform have little meaningful ability to use state institutions to oppose counter-reform. The literature on counter-reform violence in response to land reform in Colombia strongly supports this notion that elite de jure and de facto power are complementary (Acemoglu, Robinson, and Santos-Villagran, 2013; Arjona, 2016; Grajales, 2011, 2013; Vélez-Torres, 2014). The robust presence of coercive state institutions may actually facilitate counter-reform, because absent a revolutionary restructuring of power in the post-reform context, elites still maintain disproportionate access to de jure power.

For state coercive capacity to complement counter-reform violence, we would also expect a symbiotic relationship between elites and state actors. Where state actors rely on elite de facto power for votes and other rents, and thus elites are able to "game democracy", state coercive capacity should make elite use of de facto power more efficacious (Albertus and Menaldo, 2014). A state with high coercive capacity, but low vulnerability to capture would deter violence—in this context de jure power and de facto power are not complementary, they are substitutes. A state with high coercive capacity and high vulnerability to capture would encourage violence—in this context de jure power and de facto power are complementary. From these insights on the complementarity of de facto and de jure power, under conditions of high vulnerability to elite capture, we derive the following hypotheses.

Hypothesis 3: Armed actors (primarily paramilitaries) will perpetrate more violent attacks post-titling in municipalities with greater coercive capacity.

Hypothesis 4: Armed actors (primarily paramilitaries) will perpetrate more violent attacks post-titling in municipalities where state actors are more vulnerable to institutional capture.

4 Background

4.1 Land Reform and Black Communities in Colombia

The origins of the Colombian armed conflict can be traced back to the use, possession and appropriation of land (Grupo de Memoria Histórica, 2012; López-Uribe, Sànchez, and Fazio, 2010; López-Uribe

[7] We would also expect this conditional relationship between state capacity and counter-reform violence to be common in contexts where economic inequality is very high.

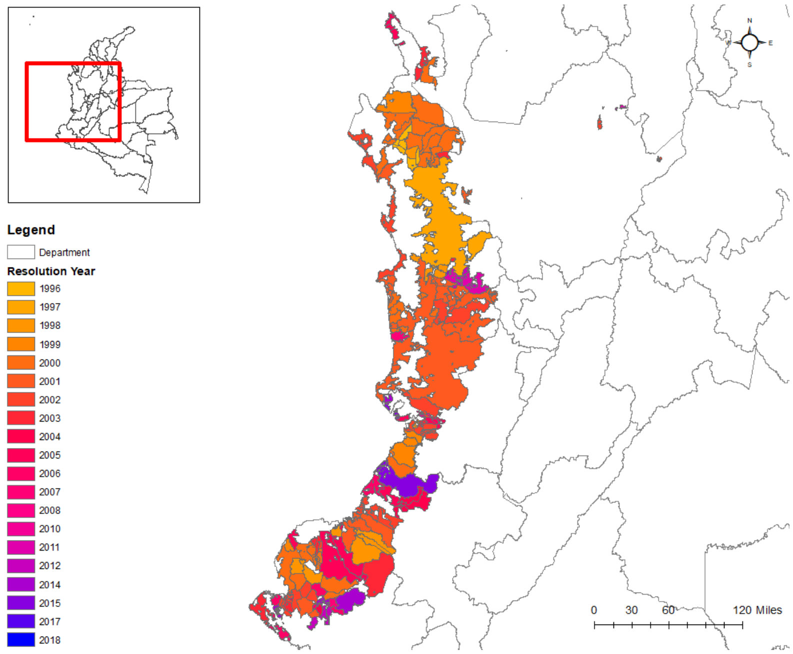


Figure 1: Afro-descendent Communities

and Torres, 2018). Colombia is one of the countries with the highest levels of land inequality in the region (UNDP, 2011), a situation that directly relates to the civil conflict (Albertus and Kaplan, 2013; LeGrand, 2016) and affects political dynamics (Kopas, 2019). Since the expansion of coffee production in the 1920s that incentivized land owners to invade land occupied by peasants, land disputes have played a central role in shaping the conflict (Sánchez, del Pilar López-Uribe, and Fazio, 2010). Recently, several studies have highlighted how armed groups used land disposession in the contemporary civil conflict to obtain economic benefits from their exploitation (García-Jimeno and Ditraglia, 2018; Bandiera, 2019; Steele, 2017; Vargas and Uribe, 2017).[8]

The 1991 Colombian Constitution and the subsequent legislation and decrees codified one of the most expansive racial reforms targeted to afro-descendants in Latin America. In 1993, President Cesar Gaviria signed Law 70, denominated "*La Ley de las Negritudes*" (Law of Black Communities). Roughly a century and a half after the formal end of slavery in Colombia, Law 70 proposed to formalize the political representation and property rights of black communities *as black communities* through collective land titles (Escobar, 2008; Paschel, 2016). Article 1 of Law 70 specified its objective to formalize the land tenure of black communities that lived on barren lands in the Pacific Basin and "other zones of the country" in order to "[protect] the cultural identity and rights of the Black Communities of Colombia as

[8] See Albertus and Kaplan (2013) for a description of the evolution of the land problem, without a specific reference to Afro-descendant community lands.

an ethnic group... and guarantee that these communities have real equal opportunities before the rest of the Colombian society".[9]

Fig. 1 illustrates the limited geographic and temporal implementation of Law 70. It shows the location and timing of titling decisions under Law 70 from 1996-2018. The most intensive period of titling occurred between 1995-2003, after which the approval rate of title requests has dropped off dramatically.[10] Over 85% of black collectively titled land in Colombia is in the Pacific Basin municipalities, despite the fact that the majority of the Afro-descendant population actually lives outside of this region, with a significant portion located on the municipalities on the Atlantic/Caribbean Coast (Cristian et al., 2017; Arango, 2018).[11] Online appendix B provides a complete description of the context of Law 70, its place in the configuration of the racial stratification in Colombia, additional details in the titling process, and its difference to other land (re)distribution initiatives.

4.2 Civil War Actors and Dynamics

The origins of the contemporary conflict can be traced back to the formation of guerrillas in the 1960s. The most prominent guerrilla group, the Revolutionary Armed Forces of Colombia (FARC by its Spanish acronym), was founded as a peasant movement with the self-proclaimed aim of fighting for land redistribution and to represent the rural poor. Other guerrillas, including the National Liberation Army (ELN) and the Popular Liberation Army (EPL) were also conceived as movements that cited land redistribution as their main purpose, among other goals. The guerrillas grew considerably from the 1960s to the 1990s, when they initiated peace talks with the government that subsequently failed (Molano, 2015).

The conflict can be described as low-intensity before the 1990s, until the emergence and consolidation of paramilitary groups across the country. These groups emerged and consolidated as a response to the guerrilla threat to big land owners. The official coalition of paramilitary forces, the United Self-Defense Forces of Colombia (AUC by its Spanish acronym) had over 15,000 fighters by the late 1990s and over 30,000 by the time it demobilized in 2007. The AUC increased the intensity of the conflict throughout the country, especially in places were guerrilla insurgents used to have a significant presence and

[9] The text of Law 70 was translated by Dr. Norma Lozano Jackson (Benedict College) and Dr. Peter Jackson (Benedict College).

[10] See appendix Fig.A5 for details.

[11] Of the 271 communities with requests for titles still pending, it is estimated that 53.5% (145) of these are in Caribbean communities (Arango, 2018, p. 5).

conservative elites intended to expand their political and economic power (Romero, 2003).

The two largest organizations, the paramilitary AUC and the guerrilla FARC have now signed peace deals with the government. The paramilitary demobilization took place during the government of Alvaro Uribe, beginning in 2003. The peace agreement with the FARC was signed in 2016 with the government of Juan Manuel Santos, staring a process of political transition in the country. However, illegal armed organizations continue to be present in different parts of the country, especially in the Pacific and Atlantic regions, where social and political leaders are now targets of criminal violence.

Our argument relies on the assumption that paramilitary organizations are an instrument of de facto elite power. According to this idea, paramilitary organizations work together with local political elites to maintain power through counter-reform violence. Local politicians and government officials made several military and political alliances with paramilitary groups in their fight against the guerrillas (Duncan, 2015). As opposed to the guerrillas, that intensified their attacks during elections in their areas of control, the paramilitaries were supported by local politicians during their expansion (Gallego, 2018). There is also extensive qualitative and quantitative evidence according to which paramilitary groups made alliances with local-level and national-level politicians (Romero and Valencia, 2007; Hernández and Martínez, 2010). These 'para-political' alliances, as they are known in Colombia, were more prominent between right-wing politicians and paramilitary organizations in the Pacific and Atlantic coasts. Consequently, we should expect an increase in violence in places where the state is captured by this type of coalition.

5 Data

The data on collective land titles come from the Observatory for Ethnic and Peasant Territories (OTEC, 2018), run by the Department of Environmental and Rural Studies at Universidad Javeriana in Bogotá, Colombia.[12] The research team at OTEC conducted extensive archival and field research to compile a novel dataset of collective land tenure in Colombia. As of the date that we accessed this data, OTEC had assembled records for all communities that had received land titles between 1996, the first year of collective titling, and 2018. Since the data from OTEC aren't georeferenced, we manually code information on the department and municipality of the land title by matching the names of the *consejo*

[12] This data was last accessed on July 23, 2019 through the OTEC GIS portal.

communitarios in the OTEC database with *consejos* officially registered with the the Office of Asuntos de Comunidades Negras at the Ministerio del Interior.[13] We supplement these records with data assembled by Albertus and Kaplan (2013) from the now defunct Colombian Land Reform Institution (INCORA) on the total stock of land plots allotted to rural peasant farmers to proxy for the magnitude of existing land informality. We use this data to construct a series of measures for the total number of plots reformed by INCORA to rule out the impact of prior reforms and test alternative hypotheses.

The main dependent variable in our analysis is armed actor violence. In particular, we want to understand the relationship between collective land titling and armed actor violence perpetrated by state, guerrilla, and paramilitary groups. The ¡Basta Ya! (Enough Already!) database, compiled by the Grupo de Memoria Histórica (2012), includes high-frequency, event-level data on violent attacks perpetrated by paramilitaries, guerrillas, state forces, and unknown actors from the 1980s until 2012. Following Acemoglu, Robinson, and Santos-Villagran (2013), we code guerrilla attacks and paramilitary attacks to include the number of kidnapping events, massacres, destruction of civilian property, terrorist attacks, territory occupations, targeted assassinations, and civilian fatality events in armed combat.[14] The records are disaggregated by armed actor and modality of violence and come from a variety of sources including state records, newspaper reports, and policy papers.[15] We georeference the event-level data manually using a roster of municipalities provided by DANE (2000). Out of 64,189 recorded incidents, we were able to locate 61,632 (96%).

In addition to the variables for land titles and conflict, we collect data from a range of sources to examine the mechanisms that would drive a statistical relationship between collective land titling and political violence. To measure state capacity, we use detailed data on state institutions and public employees compiled by the Colombian NGO *Fundacion Social*. The records from 1995 are disaggregated by national-level agencies, which include law enforcement (police posts and inspections), the judiciary (national courts), public hospitals and agricultural banks.[16] We use this data to estimate the heterogeneous effects of collective land titling by the type of public institutions present within each municipality.

For information on election results and political parties, we draw on data from Pachón and Sánchez (2014). We collect details on the share of votes for conservative and left-leaning candidates running for

[13] We accessed this data on May 29, 2019. The list is updated quarterly.

[14] Since the data for anti-personnel mines doesn't list the individuals or groups that were involved in the incident, we exclude it from our main analysis.

[15] Apart from anti-personnel mine data, the research team at GMH explicitly avoided using official government statistics.

[16] See articles 287-288 and 311-321 of Law 60 in the 1991 Constitution for further details on how these agencies were classified.

the 1994 presidential election. This race immediately precedes the start of land titling. We combine this data with electoral records on the vote shares for conservative, left-wing, and liberal parties contesting the 1994 mayoral elections to compare the impact of titling on armed actor attacks across federal and local elections in Colombia. Finally, to better understand how the land titles were allocated across municipalities, we compiled geographic, socioeconomic, and historical data on a range of variables. Details for these additional measures can be found in appendix C.[17]

6 Research Design

6.1 Benchmark Model

Our benchmark empirical strategy combines differences in the timing of land reforms with cross-sectional variation in municipal titling. The baseline model is given by:

$$v_{mdt} = \alpha + \beta\left(T_{md} \times \mathbf{I}^{Post}_{t\{t>1995\}}\right) + \tau_t + \pi_m + \delta_d t + \left(\mathbf{X}'_{md} \times \mathbf{I}^{Post}_{t\{t>1995\}}\right)\Gamma + \varepsilon_{mdt} \tag{1}$$

Here v_{md} represents the change overtime in violence for municipality **m** in department **d** for year **t**, where $t = 1980, 1981, \ldots, 2012$. T_{md} is our treatment indicator which is equal to one if a municipality had an Afro-descendant community that received a land title following the passage of Law 70, and zero otherwise. $\mathbf{I}^{Post}_{t\{t>1995\}}$ is the indicator for the post-reform period. π_m and τ_t are a full set of municipality and year fixed effects, and $\delta_d t$ is the department-specific time trend. Finally, ε_{mdt} is the idiosyncratic error term, clustered at the municipality level. Our coefficient of interest, β, captures the *difference-in-difference* effect of land titling on violence after titling was introduced.

Following Burbidge, Magee, and Robb (1988) and MacKinnon and Maggie (1990), we use the inverse hyperbolic sine (IHS) instead of a natural log to transform our dependent variables. This prevents exclusion of municipal-year observations with zero episodes of armed actor violence. We also allow for pre-treatment municipality-specific characteristics $\mathbf{X}'_{md}$ to vary flexibly in the pre- and post- reform periods. The vector of municipal controls includes physical features such as elevation (m), rainfall (mm/year), distance to the nearest river (km), municipal river density (m/ km^2), and dummies for the presence of oil, coca, and gold mines. We also include proxies for administrative capacity such as mu-

[17] We provide a summary of the descriptive statistics in appendix section A.1.

nicipality area (km^2), number of colonial *encomiendas*, distance to royal roads (km), department capitals (km), and nearest market towns (km), as well as a set of pre-treatment controls for demographic composition including municipal population in 1995 and the share of minority population in 1985 (%). Finally, we account for the persistent effects of historical conflict using data on the incidence of land conflict between 1901 and 1931, presence of armed combat during *La Violencia* (1948 - 1953), the frequency of land invasions by the National Peasant Association (ANUC) between 1971 - 1978, and the mean level of violence in neighboring municipalities (1988 - 1995). We also include controls for the total number of plots reformed between 1960 - 1985 to proxy for rural grievances.

6.2 Event-study Design

We supplement our benchmark empirical strategy with results from an event-study which exploits the phase-in of titling over time. Assuming conditional independence, equation 1 provides robust estimates for differences in violence before and after the start of land reforms. Since the timing of land titling in our panel varies at the municipal-year level, we can use the gradual roll-out of reforms to examine how political violence changed in the years immediately before and after each municipality in the treatment group was first titled. Specifically, we estimate the following regression:

$$v_{mdt} = \sum_{j=-10}^{-1} \lambda_j T_{md} \mathbb{1}(t - I_m = j) \ + \sum_{j=1,\, j \neq 0}^{10} \rho_j T_{md} \mathbb{1}(t - I_m = j) + \pi_m + \delta_{d(m)t} + \epsilon_{mdt} \tag{2}$$

Where π_m and $\delta_{d(m)t}$ are municipality and department-year fixed effects. T_{md}, the treatment indicator, is the same as the binary indicator defined in equation 1. We interact T_{md} with a set of event-year fixed effects $(t - \mathbf{I}_m = j)$ equal to one when the observation year is $j = -10, 9, \ldots, -1, 1, \ldots, 10$ years from I_m, the first year when municipality **m** was granted a land title ($j = 0$ is the omitted category). Conditional on the set of fixed effects, the point estimates λ_j represent the annual difference in mean violence between municipalities that eventually receive a land title and all other control municipalities j years *before* each treatment municipality received its first land title. We compare these estimates to ρ_j which capture any breaks in the trend of violence j years *after* land titling began in treated municipalities net of any changes in control municipalities.

Identification of event-study estimates relies on the assumption that the roll-out of land titling was orthogonal to municipal-specific characteristics after accounting for location and department-year fixed effects (i.e., $\mathbf{E}[T_{md} \cdot \epsilon_{mdt} \,|\, \pi_m,\, \delta_{d(m)t}] = 0$). This is a fairly strong assumption in the present context. It requires that communities selected to receive a land title in any given year be chosen in a random or *as-if* random way. Identification fails if, for instance, the allocation rule was a function of pre-existing factors such as community need or the potential benefits that accrue from being titled.

We address this challenge by including pre-treatment covariates from equation 1 interacted with a linear time trend to flexibly adjust for any year-specific changes correlated with the evolution of armed actor violence that may affect treatment and control municipalities differentially over time. The identification assumption can now be summarzied as $\mathbf{E}[T_{md} \cdot \epsilon_{mdt} \,|\, \mathbf{X'}_{mdt},\, \pi_m,\, \delta_{d(m)t}] = 0$, that is, conditional on linear trends in model covariates and fixed effects, unobserved factors are not systematically correlated with the titling treatment. This assumption will hold so long as any policy changes or economic shocks that affect violence do so linearly in $\mathbf{X'}_{mdt}$. If, however, unobserved factors correlated with land titling affect violence in a non-linear way over time, our identification assumption will fail. We account for potential nonlinearities by conducting a series of tests to estimate the *localized* effect of titling on violence using a regression discontinuity design. We discuss this strategy further in section 7.2.

7 Empirical Results

In the rest of the paper, we present estimates from the empirical strategies outlined above. We also provide evidence on potential mechanisms to confirm our main hypotheses by comparing state, paramilitary, and guerrilla group attacks. Our results indicate that the impact of communal land titling on political violence can be explained by greater state coercive capacity and the capture of national and local politics by counter-reform elites. As a further step, we show that violence increases when there is a credible threat to elite influence. This relationship is not explained by alternative mechanisms such as land inequality, lootable resources, extractive industries, or the presence of illicit markets.

7.1 Land Titling and Political Violence

Table 1 reports estimates from our benchmark specification in equation 1. We begin by pooling the sample to establish a baseline treatment effect, conditioning the OLS estimates on model covariates only. In column [1] we find that overall political violence increases by an average of about 35% in treatment (titled) municipalities compared to control municipalities in the post-reform period. The increase is

Table 1: Afro-descendant Land Titling and Political Violence

	All Attacks			
	[1]	[2]	[3]	[4]
Any Title (0/1) × Post (>1995)	0.351***	0.290***	0.205**	0.213**
	(0.109)	(0.083)	(0.102)	(0.098)
Any Title (0/1)	-0.173*			
	(0.092)			
Post (> 1995)	0.135***			
	(0.011)			
Adj. R-squared	0.112	0.273	0.280	0.285
Observations	31581	37026	31581	31581
Clusters	957	1122	957	957
Year F.E.	No	Yes	Yes	Yes
Municipality F.E.	No	Yes	Yes	Yes
Controls	Yes	No	Yes	Yes
Controls × Post	No	No	Yes	Yes
Department time trends	No	No	No	Yes
Dependent Variable Mean	0.926	0.870	0.926	0.926

Note: The unit of analysis is a municipality-year. We apply the inverse hyperbolic sine (IHS) transformation to all the outcomes to correct for the skewed distribution in the number of attacks. Robust standard errors clustered at the municipality level are reported in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

between 21% - 29% when we use the panel instead of a pooled sample. The estimated effects imply an escalation of about 3.9 to 5.4 additional armed attacks per week in titled municipalities following the roll-out of land reforms. Our results are robust to the inclusion of location and year fixed effects (column [2]), as well as a rich set of interacted pre-treatment covariates (column [3]) and department-specific time trends (column [4]). We also observe that municipalities which eventually received a land title experienced less violence on average prior to the start of land titling. The negative coefficient on the *any title* variable in column [1] implies a potential displacement of insurgency into predominantly Afro-descendant municipalities after titling began.

Fig. 2 provides further evidence to support our main finding that land reform under Law 70 caused an increase in violent attacks. The event-study estimates illustrate the *net* change in violence for municipalities that eventually received a land title in the years leading up to and after the first land title was granted. Three points are worth noting. First, we see no systematic pretrends in armed actor attacks before a municipality is allocated a title. We find no statistically significant differences in the magnitude

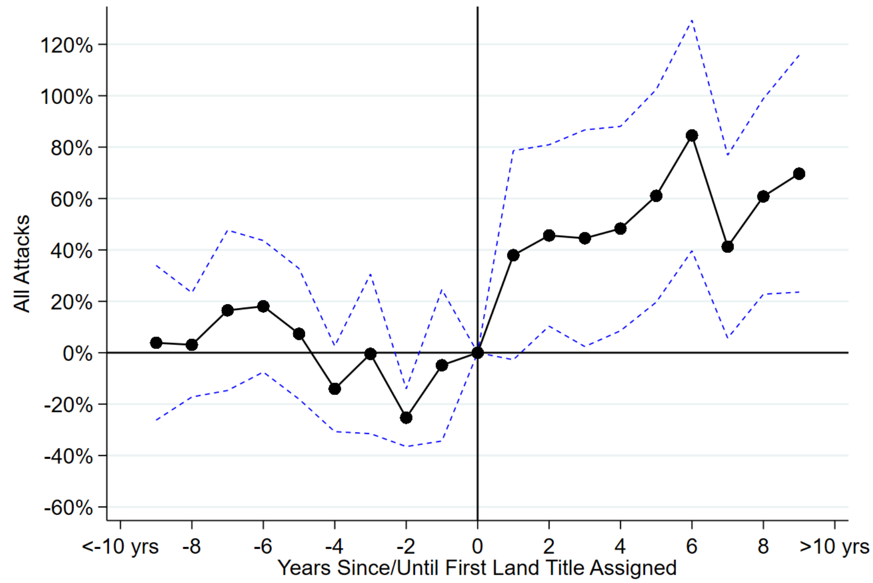


Figure 2: Political Violence Event-study Estimates

of pre-titling effects in the years leading up to a municipality receiving a land title. This implies that our empirical design fully accounts for any changes in the spatial distribution of the conflict before land titling was gradually phased-in. Second, we observe an immediate escalation in attacks once a land title is assigned. Compared to municipalities in the control group, treated municipalities experience nearly a 40% increase in attacks within the first year of receiving a land title ($\approx$ 7.5 additional attacks/week). This result is statistically significant and persists for more than a decade after municipalities received their first land title. Third, the lack of reversion to the pre-reform mean implies a shift in the landscape of the Colombian conflict toward predominantly black communities situated along the Pacific coast.

We perform several specification checks to assess the validity of our results. These include augmenting our baseline model and checking for violations of homogeneity assumptions. We provide details of these tests in appendix A.2. We find our estimates are robust to a variety of alternative models and samples. However, since we cannot completely rule out the possibility that our event-study estimates may be biased due to endogenous cross-cohort selection, we pool the sample and use a regression discontinuity in time (RDiT) design to estimate the impact of titling on violence within a narrow window of the threshold date when each new land title was assigned.[18] This allows us to establish a causal link between land titling and political violence.

[18] RDiT models are also referred to as interrupted time-series analysis in the literature (Mummolo, 2017). This makes our identification check a natural extension of the event-study design.

7.2 Identification

To the best of our knowledge, there are no applications of regression discontinuity designs using *time* as a forcing variable in the study of political violence.[19] We therefore adapt models commonly used in public policy.[20] In doing so, we make an important contribution to the literature on violence, combining a novel empirical design with highly disaggregated event-level data on the weekly frequency armed actor attacks. We begin by comparing the merits of different estimation strategies before explaining how the RDiT design works and how it helps us overcome the identification challenges mentioned earlier.

In order to estimate the treatment effect, both our benchmark *differences-in-differences* model (equation 1) as well as the event-study design (equation 2) require that we create a control group using municipalities that never received a land title. Given what we know about the titling process, communities in municipalities that never received a title may not be an appropriate counterfactual for titled communities. Land titling under Law 70 was conditional on, among other factors, Afro-descendence and proof of hereditary residence. By selecting a comparison group that didn't explicitly meet these criterion, we risk misspecifying the counterfactual and potentially biasing our point estimates.[21] In the ideal experiment, we would draw a sample of municipalities with Afro-descendent communities and then randomly assign the titling treatment. This would allow us to construct a comparable group of control municipalities. Though we cannot run such an experiment, we can however recover a well-identified local treatment effect using an RDiT model.

To assess the impact of titling on political violence, we estimate the following regression using both polynomial and local linear approaches:

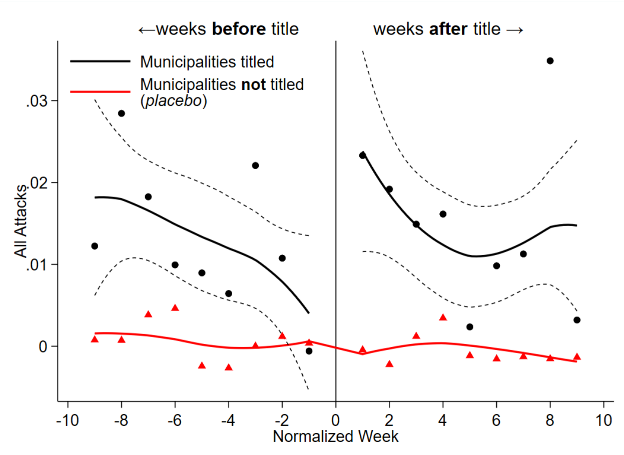
$$v_{mwt} = \psi_m \cdot \mathbb{1}[Date_{wt} \geq Date_{wt}^{title}] + f(Date_{wt}) + \tau_t + \mathbf{S'}_{wt}\Phi + \xi_{mwt} \tag{3}$$

In equation 3, the treatment indicator is determined by the cut-off $[Date_{wt} \geq Date_{wt}^{title}]$, which equals one for all weeks **w** of year **t** following the *first* date when a land title was granted, and zero otherwise. We use this cut-off rule because we are primarily interested in estimating the extensive margin of the

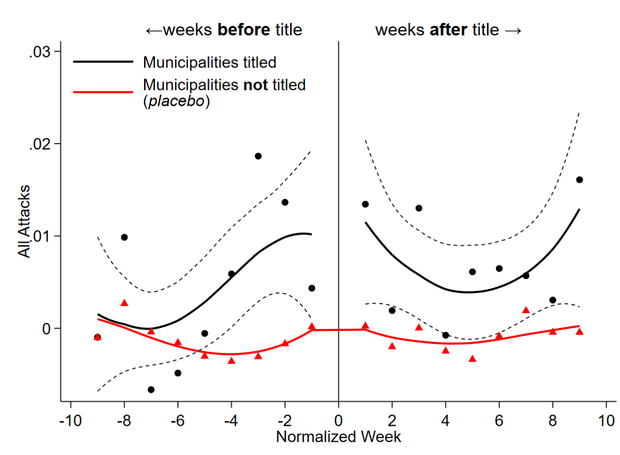
[19] A possible exception may be a recent study by Condra et al. (2018). However, their empirical model is closer in spirit to a standard event-study.

[20] See Hausman and Rapson (2018) for a detailed review.

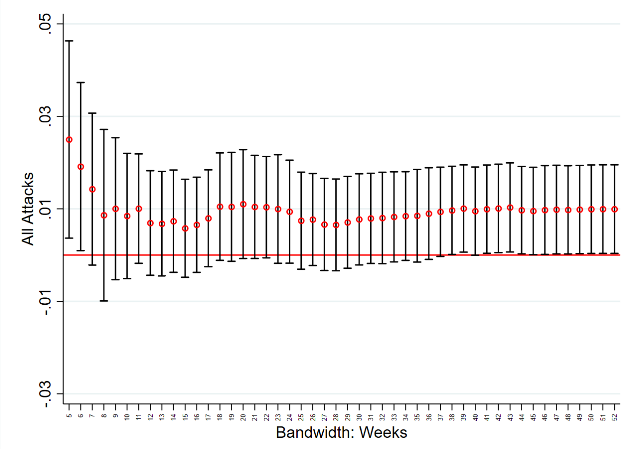
[21] This problem is further exacerbated by the fact that our benchmark *difference-in-difference* specifications make strict linearity assumptions regarding the effect of unobservables on the evolution of political violence. Model misspecification may introduce further bias unaccounted for by controls or fixed effects.



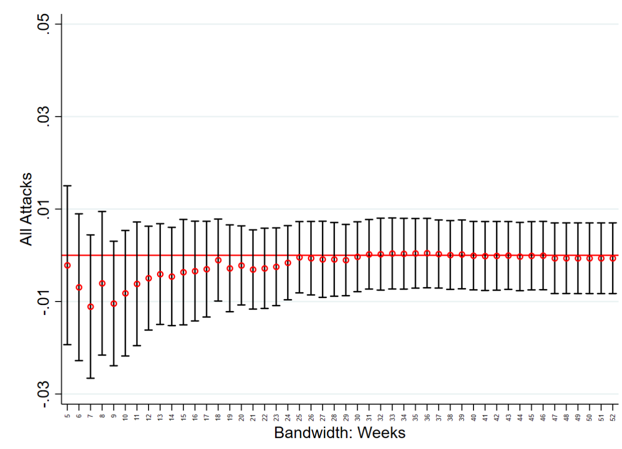
(a) RDiT Estimates



(b) RDiT Falsification Test



(c) RDiT Bandwidths Checks



(d) Falsification Test Bandwidths Checks

Figure 3: Afro-descendant Land Titling and Political Violence RDiT Analysis

treatment effect.[22] The forcing variable $f(Date_{wt})$ controls for smooth polynomial functions in time modeled separately on either side of the discontinuity. Since the week when the first land title was granted varies over time, we normalize $Date_{wt}$ to zero, allowing us to pool the sample and estimate separate treatment effects for each year.[23] We condition on a set of year dummies to identify the overall effect using within year variation only. We also include a set of season dummies $\mathbf{S}'_{wt}$ interacted with the forcing variable in all our specifications. Finally, we calculate standard errors adjusted for multi-way clustering using the method proposed by Cameron, Gelbach, and Miller (2011) to account for serial correlation in the error term both within municipalities and over time.

We present the main results graphically in this section and provide detailed estimates in the appendix. Fig. 3a plots the residuals from estimating equation 3 averaged within weekly bins against the

[22] We hypothesize that any additional titles granted in the weeks that follow likely increase the magnitude of the estimated effect, though we abstract away from predictions about how violence changes on the margin.

[23] This involves subtracting the threshold date from the forcing variable.

normalized dates when land titles were allocated. We then overlay a local linear regression fit separately on either side of the threshold date for a bandwidth of ten weeks. When we run the test using the sample of municipalities that received a land title following the passage of Law 70, we find a sharp, discontinuous increase in violence at the cut-off point. We test the robustness of this effect varying the bandwidths within the neighborhood of the cut-off in Fig. 3c and run falsification tests to examine whether the threshold had an independent effect on violence using the sample municipalities that were never titled under Law 70. Though the magnitude of the effect diminishes slightly as we increase the bandwidth size, we continue to detect a statistically significant increase in armed actor attacks. By contrast, we do not find a similar discontinuous jump in attacks for the placebo group.

We conduct a series of sensitivity tests to assess the validity of our RDiT results.[24] In Fig. 3b, we rerun the analysis using placebo thresholds. The placebo dates used for the falsification test are derived by deducting ten years from the official roll-out dates. This allows us to estimate the effects without significant overlap across different samples.[25] We find no evidence of any discontinuous effect of titling on violence using these alternative cut-offs. The null findings do not depend on the bandwidth choice. Based on this additional empirical evidence, we conclude that collective land titling caused an increase in armed attacks against black communities.

8 Mechanisms

8.1 Political Violence as Elite Counter-Reform

To examine the mechanisms underlying our main results, we disaggregate political violence by armed actor categories. This allows for a direct test of our hypotheses regarding elite investment in violence as a response to specific de jure reforms. We speculate that a racially targeted reform such as the titling of communal land belonging to Afro-descendant groups will foster greater elite cooperation compared to de jure reforms that accrue economic benefits to citizens more generally. Greater cooperation leads to more investment in de facto power, causing violence to increase. We provided evidence of this increase in the previous section. We will now examine whether it corresponds to specific elite investments in counter-reform violence.

[24] To conserve space, we provide details on several additional validation checks for the RDiT design in appendix section A.3.

[25] Any overlap will likely bias the estimates from the falsification test upwards. A null in this case would then imply that we find no confounding effects despite the upward bias.

Table 2: Political Violence by Armed Actor Type

	Policy & Army Attacks			Paramilitary Attacks			Guerrilla Attacks		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
Any Title (0/1) × Post (>1995)	0.028	-0.003	0.008	0.288***	0.205**	0.187**	0.133**	0.053	0.073
	(0.033)	(0.039)	(0.040)	(0.098)	(0.100)	(0.094)	(0.057)	(0.052)	(0.052)
Any Title (0/1)	-0.061			-0.060			-0.080*		
	(0.038)			(0.060)			(0.046)		
Post (> 1995)	0.002			0.140***			0.021***		
	(0.005)			(0.010)			(0.008)		
Adj. R-squared	0.0432	0.132	0.136	0.0865	0.230	0.237	0.0447	0.171	0.174
Observations	31581	31581	31581	31581	31581	31581	31581	31581	31581
Clusters	957	957	957	957	957	957	957	957	957
Year F.E.	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Municipality F.E.	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls × Post	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Department time trends	No	No	Yes	No	No	Yes	No	No	Yes
Dependent Variable Mean	0.106	0.106	0.106	0.378	0.378	0.378	0.442	0.442	0.442

Note: The unit of analysis is a municipality-year. We apply the inverse hyperbolic sine (IHS) transformation to all the outcomes to correct for the skewed distribution in the number of attacks. Robust standard errors clustered at the municipality level are reported in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

We start by manually coding the armed actors involved in each violent incident in our sample. These include official state actors such as the police and army as well as non-state armed actors such as right-wing paramilitary (AUC) and revolutionary guerrilla groups (FARC, ELN, EPL, etc.).[26] Given that elites in Colombia have historically relied on informal armies and militias to protect their interests, our theory would predict an increase in paramilitary attacks in response to collective land titling under Law 70 (Acemoglu, Robinson, and Santos-Villagran, 2013; Ch et al., 2018). Table 2 estimates the effect of collective land titling on political violence disaggregated by armed actor type. Columns [4] - [6] show that titling had a significant effect on attacks carried out by paramilitary groups. Paramilitary violence increases by approximately 20% following land reforms. By contrast, we find no effect of titling on government or guerrilla attacks.[27] This implies that the increase in paramilitary violence was not in response to the civil conflict. If titling had caused the restive insurgency to worsen, we would've observed an overall increase in violence across all armed actor categories.

To provide evidence on our second hypothesis, we run a set of regressions comparing land titling under Law 70 to a series of alternative land reforms. We analyze the impact of prior and concurrent

[26] We use the catalog in Daly (2016, p. 51 - 52) to classify the various armed actors involved in the Colombian conflict. See appendix A for further details on the different groups.

[27] Though the state was complicit in aiding paramilitary groups, the degree of its support varied considerably depending on the extent of local accountability (Fergusson et al., 2020). We explore this mechanism further when we evaluate the heterogeneous effects of state coercive capacity on police and army attacks following the roll-out of land titling.

Table 3: Effects of Racial/Non-Racial Reforms on Political Violence

	All Attacks		Police & Army Attacks		Paramilitary Attacks		Guerrilla Attacks	
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Panel A: Afro-descendent Land Titling								
Afro-descendent Area × Post (>1995)	0.007*		-0.000		0.007**		0.001	
	(0.003)		(0.001)		(0.003)		(0.002)	
Any Title (0/1) × Post (>1995)		0.209**		0.006		0.183**		0.072
		(0.098)		(0.039)		(0.093)		(0.051)
Panel B: Peasant Land Titling								
Prior Plots (1965 - 1980)/1000 × Post	-0.184	-0.195	-0.008	-0.008	-0.252	-0.263	-0.139	-0.142
	(0.261)	(0.267)	(0.104)	(0.105)	(0.210)	(0.215)	(0.147)	(0.147)
Rehabilitation zones × Post	-0.063	-0.061	-0.032	-0.031	-0.051	-0.050	-0.009	-0.007
	(0.055)	(0.054)	(0.021)	(0.021)	(0.047)	(0.046)	(0.035)	(0.035)
Plots Reformed (1988 - 2000)/1000 × Post	0.160	0.167	-0.011	-0.012	0.223	0.230	0.129	0.129
	(0.247)	(0.252)	(0.098)	(0.099)	(0.203)	(0.207)	(0.128)	(0.128)
Other tenancy × Post	-0.001	0.009	0.041	0.040	-0.164*	-0.154*	0.154**	0.154**
	(0.107)	(0.106)	(0.048)	(0.047)	(0.087)	(0.087)	(0.062)	(0.062)
Adj. R-squared	0.285	0.285	0.136	0.136	0.237	0.237	0.174	0.174
Observations	31581	31581	31581	31581	31581	31581	31581	31581
Clusters	957	957	957	957	957	957	957	957
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls × Post	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Department time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependent Variable Mean	0.322	0.322	0.0575	0.0575	0.146	0.146	0.161	0.161

Note: The unit of analysis is a municipality-year. We apply the inverse hyperbolic sine (IHS) transformation to all the outcomes as well as the Afro-descendant area variable to correct for skewed distributions. Robust standard errors clustered at the municipality level are reported in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

reforms using several measures of land titling for peasant farmers and landless agricultural workers. We use data from INCORA on rehabilitation zones[28] and the total number of prior plots reformed between 1965 and 1980, corresponding to the earlier periods of land titling, as well as the number of plots reformed annually between 1988 and 2000, which track the initial phase of land titling under Law 70. We also include measures for "other tenancy" which capture informal ownership of property. Crucially for our purposes, these prior and present-day land reforms (peasant land titling, henceforth) were not aimed at any one community in particular. Rather, peasant titling was a class-based reform meant to address the growing inequality and unrest in the countryside due to stalled or incomplete land reforms.

In Table 3, we compare estimates for titling under Law 70 (panel A) with peasant land titling (panel B). We run two sets of regressions. First, we run our standard benchmark model with the treatment indicator for titled communities. We then create a variable that captures the intensity of the land reforms using information on the total municipal area that was titled under Law 70. This allows us to examine

[28] These were the regional zones created by the Agrarian Reform Committee to implement land reform (Duff, 1966).

whether the scale of the reforms mattered for violence. Overall, we find that Afro-descendant land titling led to an increase in armed actor attacks in the post-reform period. Most of this increase can be explained by a significant escalation in paramilitary violence. When we compare these estimates to peasant land titling in panel B, we find no evidence that these reforms have any impact on paramilitary attacks. There is, however, a modest increase in guerrilla group activity when we analyze the point estimates for other tenancy. This may be because informal ownership is proxying for rural grievances.[29]

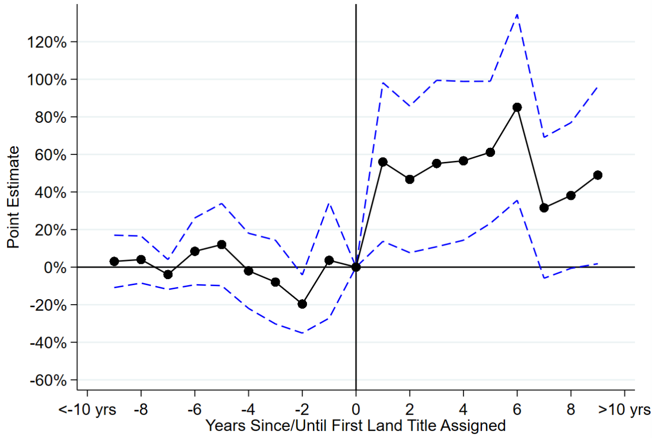
Next, we present time-varying estimates comparing the impact of racial and non-racial land reforms disaggregated by armed actor categories. Fig. 4 summarizes the effect of titling using equation 2.[30] The left column plots the estimates for racially targeted land titling under Law 70 on paramilitary, guerrilla, and state sponsored police and army attacks respectively. We run the same test and compare the results across armed groups for (non-racial) peasant land titling. We use the first year when municipalities were granted a title to identify the treatment timing for the municipal sample that received titles between 1988 and 2000.

Results from the event-study provide further evidence to support our hypothesis. We observe a significant increase in paramilitary attacks in municipalities with Afro-descendant communities that received a collective land title. We do not find similar effects for guerrilla or police and army attacks. We infer from these results that elites responded to de jure changes that targeted specific benefits to Afro-descendant communities by increasing investments in de facto power to counter the reforms using violence. This contrasts with the response to class-based reforms that accrue benefits more generally. Though elites may feel more threatened by a larger transfer of de jure power, they face wider social, economic, and political cross-cutting pressures from citizens not to invest in de facto power. We see evidence of this in the null effects for counter-reform violence in response to the roll-out of peasant land titles. Government and paramilitary attacks, in particular, do not register any change following the assignment of land titles to peasant farmers.

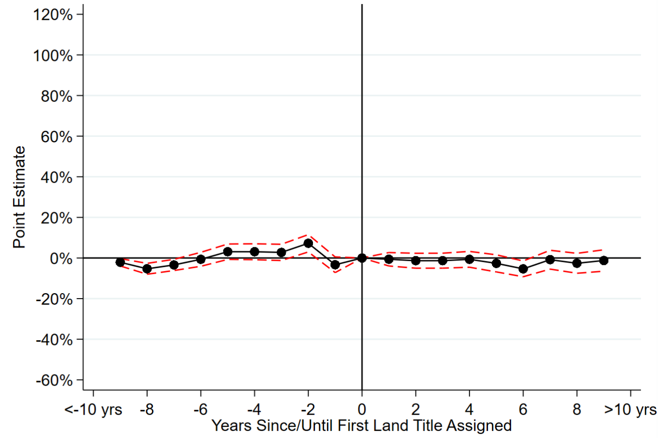
8.2 State Institutions and the Politics of Elite Capture

In this section, we provide empirical evidence to support our hypotheses regarding the impact of state coercive capacity and elite capture on counter-reform violence. Motivated by the literature on the

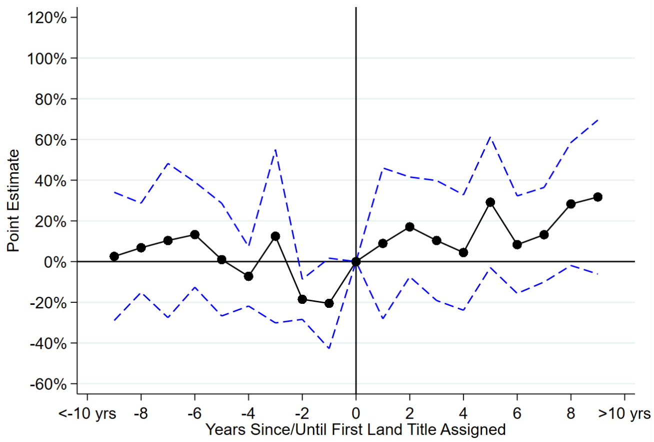
[29] Additional robustness checks are detailed in appendix section A.4.
[30] All specifications flexibly control for pre-treatment covariates.



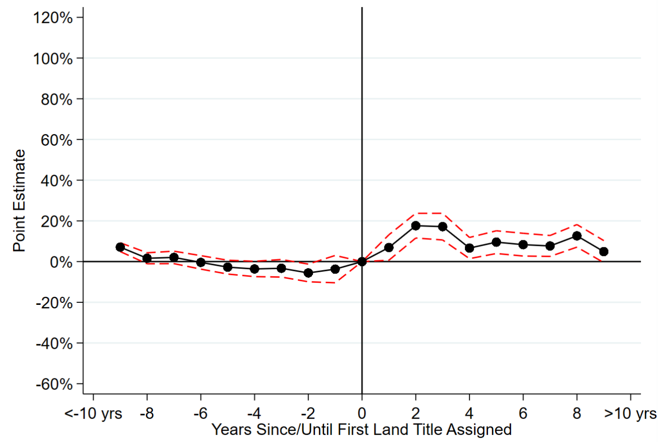
(a) Paramilitary Attacks (Afro-descendant titling)



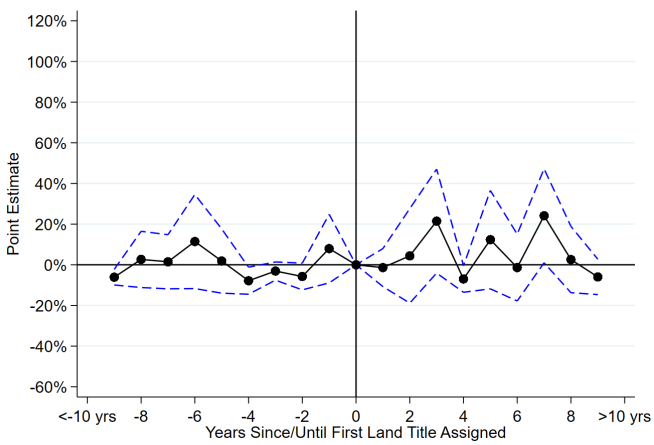
(b) Paramilitary Attacks (Peasant land titling)



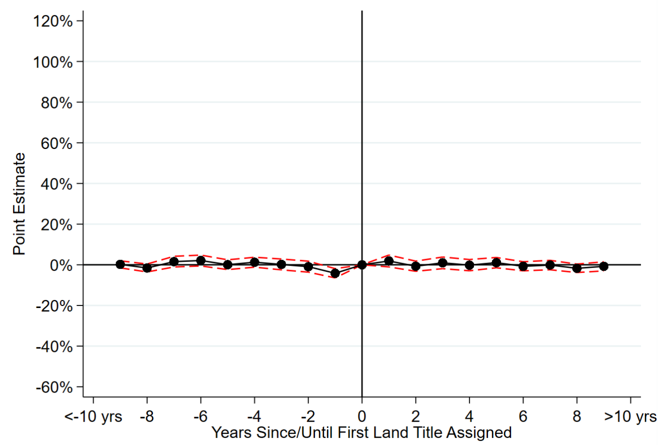
(c) Guerrilla Attacks (Afro-descendant titling)



(d) Guerrilla Attacks (Peasant land titling)



(e) Police and Army Attacks (Afro-descendant titling)



(f) Police and Army Attacks (Peasant land titling)

Figure 4: Afro-descendant and Peasant Land Titling Event-study Estimates by Armed Actor Type

complementarity between de jure and de facto power, we focus on the supply and demand factors that contribute to an increase in political violence. On the supply side, we investigate how the costs of investment in de facto power impact political violence. We argue that violence will increase in response to de jure reforms when the state has little incentive to hold counter-reform elites accountable. A greater monopoly of violence without adequate checks and balances reduces the marginal cost of investment in counter-reform violence for elites with access to the state apparatus. By contrast, racial minorities historically excluded from the nation-building process face greater risks with limited state protection. We therefore expect armed attacks to increase against black communities in municipalities where elites have more influence over state institutions. Regarding demand side factors, we examine how changes in the perception of threat to elite influence affect the use of violence. If the likelihood of implementing reforms that pose a credible threat to elite interests increases, we expect an escalation in political violence.

We operationalize state coercive capacity in several ways. First, we use aggregate measures for the total number of public institutions operating within each municipality. This includes both national and local-level agencies. We then focus more closely on coercive capacity, with an emphasis on law enforcement and the judicial system. In panel A of Table 4, we report the heterogeneous effects of state institutions on political violence for municipalities that received a land title in the post-reform period. We disaggregate the estimates by armed actor type to consider the impact on state and non-state actor attacks separately. The results offer three main lessons. First, we observe a significant increase in political violence for titled communities located in municipalities with a greater concentration of public institutions. When we separate the effects by actor type, we find that most of the increase in violence can be explained by a significant escalation in government and paramilitary attacks. We interpret this as preliminary evidence on the complementarity between de jure and de facto power. Second, state capacity mitigates armed attacks across all categories for municipalities that were not assigned a collective land title. We infer from this that in the absence of de jure reforms that threaten elite interests, a greater monopoly of violence matters less for democratic capture. Third, land titling does not have a significant impact on violence where state presence is limited. Though guerrilla attacks increase slightly, government and paramilitary attacks diminish in response to titling. This lends further support to our hypothesis that elite access to a weakly institutionalized state apparatus is an important catalyst for counter-reform violence.

Table 4: Mechanisms – State Institutions and Electoral Politics

	All Attacks	Police & Army Attacks	Paramilitary Attacks	Guerrilla Attacks
	[1]	[2]	[3]	[4]
Panel A: Public Institutions				
# Total Public Institutions × Any title × Post	0.005**	0.001**	0.006***	-0.001
	(0.002)	(0.001)	(0.002)	(0.001)
# Total Public Institutions × Post	-0.002*	-0.001	-0.003	-0.001
	(0.001)	(0.001)	(0.002)	(0.001)
Any title × Post	0.066	-0.035	-0.020	0.114*
	(0.112)	(0.043)	(0.104)	(0.063)
Adj. R-squared	0.288	0.139	0.242	0.175
Observations	30657	30657	30657	30657
Clusters	929	929	929	929
Dependent Variable Mean	0.321	0.0573	0.145	0.161
Panel B: Presidential Elections				
Vote share conservative candidates ('94) × Any title × Post	2.848*	1.094*	2.855**	0.057
	(1.668)	(0.578)	(1.335)	(0.720)
Vote share left-leaning candidates ('94) × Any title × Post	1.423	-0.386	0.642	2.071*
	(2.201)	(1.239)	(3.097)	(1.120)
Adj. R-squared	0.269	0.114	0.223	0.169
Observations	28083	28083	28083	28083
Clusters	851	851	851	851
Dependent Variable Mean	0.306	0.0515	0.137	0.155
Panel C: Mayoral Elections				
Vote share conservative party (1994) × Any title × Post	0.437**	0.108*	0.378**	-0.021
	(0.194)	(0.064)	(0.174)	(0.083)
Vote share liberal party (1994) × Any title × Post	-0.136	-0.089	-0.067	0.095
	(0.183)	(0.063)	(0.186)	(0.071)
Vote share left party (1994) × Any title × Post	0.445	-0.228	0.421	0.317*
	(0.367)	(0.169)	(0.359)	(0.184)
Adj. R-squared	0.286	0.137	0.239	0.175
Observations	31284	31284	31284	31284
Clusters	948	948	948	948
Year F.E.	Yes	Yes	Yes	Yes
Municipality F.E.	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Controls × Post	Yes	Yes	Yes	Yes
Department time trends	Yes	Yes	Yes	Yes
Dependent Variable Mean	0.323	0.0577	0.147	0.161

Note: The unit of analysis is a municipality-year. We apply the inverse hyperbolic sine (IHS) transformation to all the outcomes to correct for the skewed distribution in the number of attacks. Robust standard errors clustered at the municipality level are reported in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

In panels B and C of Table 4, we examine aspects of institutional capture using electoral data. Greater influence over the legislature by political parties with links to militia groups allows elites to carry out counter-reform violence with impunity. We therefore expect the nexus of elite complicity with right-wing paramilitary groups to be far more pronounced where establishment parties hold power. We use presidential and mayoral voting records from 1994, the election immediately preceding the start of land titling.[31] This mitigates concerns regarding reverse causality. Both in the case of presidential elections (Table 4, panel B), as well as mayoral elections (panel C), titled municipalities with a larger vote share for Colombia's traditional Conservative party experience a significant escalation in political violence.

We run several robustness checks to confirm our main findings. In appendix Table A9, we show that our results are robust to alternative measures of state capacity. We find similar effects when we separate the total number of public institutions into national and local state agencies. We also use personnel data on national and municipal public employees. Overall, our results remain unchanged. To rule out the possibility that our findings can be explained by the quality of governance, we draw on audit reports from the National Planning Department (DNP). The reports combine audits into indicies that capture competence, effectiveness, efficiency, and management capacity. Estimates from these regressions are reported in appendix Table A10. We find no conclusive evidence that government performance matters for violence in titled municipalities.

8.3 The Role of State Coercive Capacity

Since our theory on the complementarity between de facto and de jure power focuses on transaction costs, we hypothesize that counter-reform violence will increase when elites face fewer legal consequences. We predict that this will mostly likely occur in places where a stratified state has greater coercive capacity. We provide evidence to support this claim in Table 5. We find that Afro-descendant communities that received a land title experienced a surge in government and paramilitary attacks in municipalities that had more national courts (panel A) and a larger police presence (panel B). We do not detect an increase in attacks in titled communities where there was less law enforcement. In addition, we observe that municipalities with greater coercive capacity tended to be much safer in the post-reform period provided they were not assigned a land title. These findings confirm our earlier results.

[31] We get similar estimates using an indicator for whether a candidate affiliated with a specific party won or lost local elections (appendix Table A11, panel C).

Table 5: Mechanisms – What Kind of State Institutions Explain the Increase Violence?

	All Attacks	Police & Army Attacks	Paramilitary Attacks	Guerrilla Attacks
	[1]	[2]	[3]	[4]
Panel A: Judiciary				
# National Courts × Any title × Post	0.031*	0.009*	0.038**	-0.001
	(0.018)	(0.005)	(0.016)	(0.012)
# National Courts × Post	-0.007	-0.007*	-0.001	-0.004
	(0.006)	(0.003)	(0.007)	(0.003)
Any title × Post	0.096	-0.021	0.038	0.081
	(0.102)	(0.038)	(0.100)	(0.063)
Adj. R-squared	0.287	0.138	0.239	0.175
Observations	31053	31053	31053	31053
Clusters	941	941	941	941
Dependent Variable Mean	0.320	0.0572	0.144	0.162
Panel B: Law Enforcement				
# Police posts & inspections × Any title × Post	0.091*	0.032**	0.080*	0.016
	(0.050)	(0.016)	(0.046)	(0.025)
# Police posts & inspections × Post	-0.035***	-0.002	-0.047***	-0.001
	(0.009)	(0.005)	(0.013)	(0.005)
Any title × Post	-0.046	-0.088	-0.036	0.027
	(0.183)	(0.068)	(0.167)	(0.089)
Adj. R-squared	0.288	0.138	0.243	0.175
Observations	31020	31020	31020	31020
Clusters	940	940	940	940
Dependent Variable Mean	0.321	0.0573	0.144	0.162
Panel C: Falsification (Hospitals & Ag. Banks)				
# National Hospitals × Any title × Post	-0.052	-0.020	-0.021	-0.004
	(0.156)	(0.073)	(0.132)	(0.060)
Adj. R-squared	0.287	0.137	0.240	0.175
Observations	30987	30987	30987	30987
Clusters	939	939	939	939
Dependent Variable Mean	0.321	0.0573	0.144	0.162
# Agriculture Bank Offices × Any title × Post	-0.050	-0.051	-0.111	-0.010
	(0.186)	(0.046)	(0.131)	(0.112)
Adj. R-squared	0.287	0.138	0.246	0.175
Observations	31053	31053	31053	31053
Clusters	941	941	941	941
Dependent Variable Mean	0.320	0.0572	0.144	0.162
Year F.E.	Yes	Yes	Yes	Yes
Municipality F.E.	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Controls × Post	Yes	Yes	Yes	Yes
Department time trends	Yes	Yes	Yes	Yes

Note: The unit of analysis is a municipality-year. We apply the inverse hyperbolic sine (IHS) transformation to all the outcomes to correct for the skewed distribution in the number of attacks. Robust standard errors clustered at the municipality level are reported in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

To provide further support for our argument, we conduct two falsification checks. First, we assess whether 'non-coercive' national state capacity moderates the impact of titling on violence. Our theory predicts that state capacity only matters for counter-reform insofar as it enables elites to engage in extra-legal violence. Aspects of state capacity that don't reflect these costs should have no discernible effect on armed attacks. We test this claim using information on public hospitals and agricultural bank offices. Together with national courts and the police force, these institutions fall under the jurisdiction of the state. Estimates from these regressions are reported in Table 5, panel C. As expected, we find no significant change in violence across all armed actor categories.

Our second falsification check is designed to test how state coercive capacity affects violence when elites have less incentive to invest in counter-reform. In appendix Table A12, we rerun our heterogeneity analysis using peasant land reforms instead of our collective titling indicator. Since the benefits from class-based land reforms were not targeted toward any specific ethnic group, our theory would predict that they face less direct opposition from elites. With less elite interference, we expect that a monopoly of violence provides better enforcement of de jure reforms, reducing armed attacks. Overall, we do observe a significant decline in violence for reformed municipalities with greater state capacity. The decline was driven mostly by reductions in government and paramilitary attacks. Based on this evidence, we infer that state coercive capacity causes violence to increase only when elites are actively engaged in counter-reform.

8.4 Threats to Elite Influence Engender Violence

We conclude with a discussion on how the election of political outsiders with reformist agendas may have contributed to the escalation of armed attacks in black communities. Reforms to the electoral system in 1988 allowed left-wing groups to run in Colombia's elections. Since these excluded groups were more likely to implement policies that would threaten elite interests, we expect an increase in political violence in municipalities that were granted land titles under Law 70 following the election of a left-leaning candidate.

We test our claim empirically in Table 6. We draw on data from Fergusson et al. (2020) on mayoral races between 1997 and 2011.[32] We then estimate the impact of collective land titling on violence for municipalities where a left-wing mayor narrowly won the election (panel A).[33] Since we are mainly in-

[32] The sample includes all mayoral races in which a candidate associated with a left-leaning party either won or came second.
[33] We adapt the empirical strategy outlined in Fergusson et al. (2020) to estimate a *difference-in-discontinuity* effect. The

Table 6: Mechanisms – Political Outsiders and Counter-Reform Violence

	Government Attacks		Paramilitary Attacks		Guerilla Attacks	
	[1]	[2]	[3]	[4]	[5]	[6]
Panel A: Left-wing Mayoral Race Panel (1997 - 2011)						
Any Title (0/1) × Left-wing Mayor Elected	2.457	3.306	2.790**	2.812**	3.142	2.629
	(2.201)	(2.707)	(0.952)	(0.910)	(3.539)	(1.718)
Adj. R-squared	0.644	0.753	0.930	0.941	0.633	0.782
Observations	151	151	151	151	151	151
Dependent Variable Mean	0.563	0.563	0.563	0.563	0.563	0.563
Panel B: Falsification – Left-wing Races (1997 - 2011)						
Plots Reformed '88 - '00 (0/1) × Left-wing Mayor Elected	0.569	0.393	-0.115	-1.291	1.662	1.039
	(1.554)	(2.611)	(1.087)	(1.179)	(1.617)	(1.808)
Adj. R-squared	0.474	0.643	0.770	0.818	0.667	0.688
Observations	143	143	143	143	143	143
Dependent Variable Mean	0.564	0.564	0.564	0.564	0.564	0.564
Panel C: Falsification – Right-wing Races (1997 - 2011)						
Any Title (0/1) × Right-wing Mayor Elected	-1.025	-1.524	0.420	0.334	-2.048**	-2.407*
	(1.218)	(1.553)	(1.049)	(1.228)	(0.955)	(1.233)
Adj. R-squared	0.512	0.533	0.388	0.378	0.611	0.618
Observations	480	480	480	480	480	480
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
All Controls	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial	Linear	Quad.	Linear	Quad.	Linear	Quad.
Dependent Variable Mean	0.267	0.212	0.212	0.212	0.267	0.212

Note: The unit of analysis is an election race-year. We apply the inverse hyperbolic sine (IHS) transformation to all the outcomes to correct for the skewed distribution in the number of attacks. Robust standard errors clustered at the municipality level are reported in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

terested in the impact of electing a political outsider on counter-reform violence, we focus our attention on paramilitary attacks (columns [3] and [4]). The estimates suggest that electing a left-wing mayor in a municipality that was granted a land title caused paramilitary violence to increase by about 2.8% annually during the mayor's term in office. When we run a falsification test using peasant land titling, we find a negative and statistically insignificant effect (panel B). As a final step, we examine what happens when right-wing parties narrowly win mayoral elections. Since right-wing parties have historically represented the establishment in Colombia, our theory would predict that their win should not threaten elite interests. As expected, we find no significant effect of titling on armed attacks when mayors affili-

regression equation is given by:

$$v_{mt} = \alpha_0 + \alpha_1(L_{mt} \times T_m) + \alpha_2 L_{mt} + \alpha_3 T_m + [\phi_0 + \phi_1(L_{mt} \times T_m) + \phi_2 L_{mt} + \phi_3 T_m] \times f(X_{mt}) + \tau_t + \varepsilon_{mt}$$

Where L_{mt} is a left-wing win/loss indicator; T_m represents the binary titling treatment; $f(X_{mt})$ is a flexible polynomial in the left-wing vote margin. α_1 is our parameter of interest. For consistency, we use the Restrepo, Spagat, and Vargas (2004) violence data from the study.

ated with right-wing parties win (panel C).[34] We infer from these results that political violence against Afro-descendant communities was more likely to occur when elites perceived a credible threat to their interests.

9 Alternative Mechanisms

This section provides further evidence to support our main argument. We rule out some of the leading alternative explanations in the literature that may account for our findings. In appendix Table A15, we show that neither land inequality (panel A) nor land value (panel B) moderate the effect of titling on violence in the post-reform period. Moreover, the quality of land has no significant impact on armed actor attacks in titled municipalities (Table A16). To study the effect of natural resource use and the extractive sector, we compile agricultural suitability data from OTEC. We combine this data with information on mining titles and oil and gas exploration. For land use, we collect data on cattle and poultry farming. Estimates from the heterogeneity analysis using these variables are presented in appendix Table A17. Overall, we find a modest increase in guerrilla attacks in titled municipalities. We detect no significant escalation in police and army or paramilitary attacks. Results remain unchanged when we use the first principal component as a moderator in the analysis (Table A18). In addition, we show that forest reserve zones and protected areas do not predict violence against titled communities (Table A19).

We also examine whether commodity markets moderated the influence of titling on violence. We focus on coffee cultivation (1000's ha), oil production (barrels/day), and presence of coal and precious metal mining. Estimates from these regressions are reported in appendix Table A20. We find no significant differential effect of these commodities on violence in titled communities. In appendix Table A21, we investigate whether income shocks offer an alternative explanation for the surge in armed attacks. We calculate revenues for each commodity using output and price information.[35] Apart from an increase in coffee revenues, we find no other mediating changes in income for titled municipalities in the post-reform period.[36]

In Fig. A18a we test whether fiscal decentralization can account for our findings. The 1991 Consti-

[34] Results are robust to using past peasant land reforms (appendix Table A14).
[35] We use indicators for the presence of silver and platinum mining since we do not have output data for these commodities.
[36] Dube and Vargas (2013) find that a collapse in coffee prices in the 1990s led to surge in guerrilla attacks in coffee producing municipalities (an opportunity cost effect). An income shock from a decline in coffee prices in titled municipalities does not, however, account for the rise in paramilitary violence.

tution mandated a gradual devolution of power to local authorities. This involved the transfer of state revenues to municipal governments. These transfers may have caused armed groups to invest more in violence to capture public rents. To assess this claim, we examine whether the size of the local government increased differentially in titled municipalities. We use information on fiscal transfers, royalties, and revenues. In appendix Table A22, we provide estimates for these potential mediators. We find no evidence that collective land titling led to a growth in the size of the public sector.

In Fig. A18b of the appendix, we show that violence was not a response to electoral corruption. Paramilitary groups may have been mobilized by political opponents in response to the election of corrupt officials (Fergusson et al., 2020). We do not observe any significant change in vote buying, citizen reports, or the salaries and sanctioning of elected mayors. We also rule out economic development as a potential mechanism (Fig. A18c). Titled communities were not targeted by armed groups because they were richer in the post-reform period.

Next, we explore the possibility that illicit markets were responsible for the increase in political violence. If land titling contributed to an expansion of domestic coca production, the additional revenues could have caused the insurgency to swell in coca producing regions. Moreover, the surge in armed attacks could have followed changes in narcotic trafficking caused by land titling. In appendix Table A23, we find that coca presence in titled municipalities does not predict political violence. We also observe no differential effects of titling on anti-narcotic operations (Fig. A18d). To assess the role of trafficking networks, we use data compiled by Millán-Quijano (2020). The routes are constructed by connecting coca producing origin municipalities to road accessible border destinations using the least cost path. Results are presented in Fig. A18e. We find no systematic changes in the wholesale price of cocaine for titled municipalities exporting to the U.S., E.U., or both.[37]

Finally, we examine whether foreign aid to support municipal authorities caused paramilitary attacks to increase in titled communities. Most of the U.S. aid in Colombia is allocated to the Colombian military. During the 1990s, this aid was primarily meant to help the government counter narcotics trade. However, since the military was also involved in the civil conflict, some of the aid may have been distributed to combat guerrilla and paramilitary groups in titled municipalities. To test this alternative, we use data from Dube and Naidu (2015) on the location of Colombian military bases and the annual sum of U.S. military expenditures and anti-narcotics aid. Appendix Table A25 shows no significant increase

[37] In appendix Table A24, we show that titling is not correlated with internal cocaine trafficking.

in military aid in titled municipalities. Though event-study estimates in Fig. A18f do reveal that collective titling caused U.S. assistance to spike, this occurred about seven years after municipalities received their first land titles.

10 Conclusion

In this study, we argue that elite response to targeted reforms could turn violent when the society is stratified along both racial and class cleavages. Groups with de facto power may use violence to maintain and secure economic power through alliances with armed groups that have the same interests. We explore the case of the Law of Black communities in Colombia and show how in places were land titles were granted to black communities, armed actor violence increased disproportionately. Most of this increase can be explained by an escalation in paramilitary attacks.

Further, we argue that counter-reform violence increases when the state has little incentive to hold elites accountable. Accordingly, we find that in municipalities with a greater concentration of public institutions and greater coercive capacity, paramilitary violence is higher after the reform. Finally, to complete the analysis of elite-capture an counter-reform violence, we show that titled municipalities with a larger vote share for right-wing coalitions experienced a significant increase in paramilitary attacks. Our findings cannot be explained by an escalation in the civil conflict or the endogenous selection of a particular set of communities.

We believe our study opens a set of questions that goes beyond the scope of this paper. First, to what extent can we extrapolate the results found in Colombia to other cases? If the Colombian civil war were unique, it would be hard to imagine that we can find the same pattern in other settings. However, even if the Colombian conflict can have many distinctive characteristics, we believe that the ethnicization of conflict, as explained here, is not one of them. We provide a general argument that explains under what circumstances we would expect an increase in the de facto use of power. Similar initiatives such as the law against discrimination in Peru (Artículo 323 Código Penal Peruano), the recognition of rights to Afro-descendent communities in Mexico (Reform to Artículo 2 of the Constitution), and the recognition of land to Afro-communities in Brazil (Decreto 4887 of 2003) could represent other cases that are worth studying.

Second, what are the implications of our study? An increase in counter-reform violence does not imply that targeted reforms should not be applied. We argue instead that the implementation of these reforms should be complemented with other processes that guarantee the execution of the reforms as intended, and prevent the reversal of the effects of the reforms by violent local actors. As in Colombia, the contexts in which these reforms take place usually involve widespread structural inequities and institutionalized racism.

Finally, we only explore some potential mechanisms of the main effect: state capacity and institutional capture. However, we think that future research could further explorer alternative channels though which land reform can affect violence, and clarify the way in which minorities can successfully be included in the nation-building process.

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A Land Titling, Race, and Political Violence: Theory and Evidence from Colombia (Online Appendix)

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A Appendix Tables & Figures

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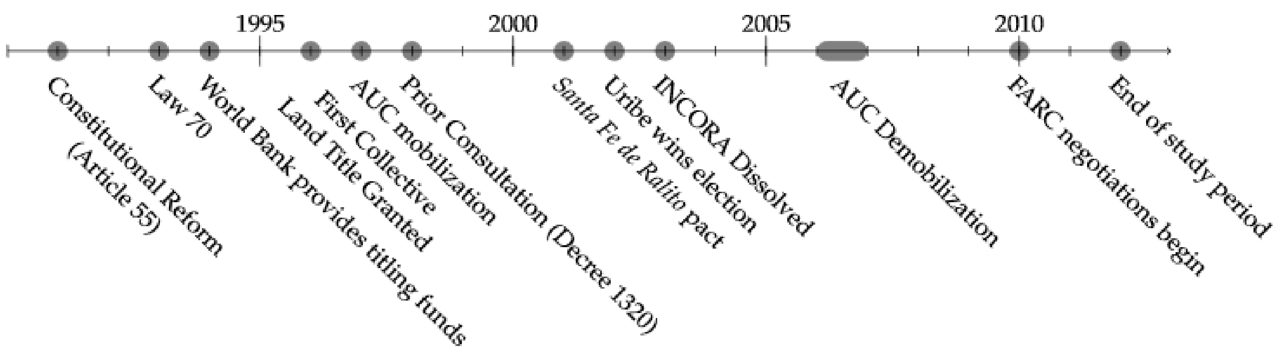


Figure A. 1: Timeline

Figure A. 2: Collective Territories Map

(a) Source: Geographic Information Systems of the Instituto Geográfico Agustín Codazzi, Instituto de Estudios Interculturales (Pontificia Universidad Javeriana – Cali) and OTEC (Pontificia Universidad Javeriana –Bogotá). Compiled by Elías Helo (Arango, 2018).

Armed Actors Involved in the Colombian Conflict[a]			
Group	*Affiliation*	*Details*	*Year Founded*
Fuerzas militares	*State*	*Ejército, Armada, Fuerza Aérea*	
Policía Nacional	*State*		
Fuerzas Armadas Revolucionarias de Colombia, Ejército del Pueblo, FARC-EP	*Guerrilla*		1964
Ejército de Liberación Nacional, ELN	*Guerrilla*	*National Liberation Army*	1965
Movimiento 19 de Abril, M-19	*Guerrilla*	*April 19 Movement*	1975
Ejército Popular de Liberación, EPL	*Guerrilla*	*Popular Liberation Army*	1967
Partido Revolucionario de los Trabajadores, PRT	*Guerrilla*	*Revolutionary Worker's Party*	1982
Movimiento Armado Quintín Lame, MAQL	*Guerrilla*	*Quintín Lame Armed Movement*	1984
Ejército Revolucionario del Pueblo, ERP	*Guerrilla*	*Revolutionary People's Army*	1985
Corriente de Renovación Socialista, CRS	*Guerrilla*	*Socialist Renewal Current*	1991
Ejército Revolucionario Guevarista, ERG	*Guerrilla*	*Guevarista Revolutionary Army*	1992
Bloque Cacique Nutibara	*Paramilitary*		
Autodefensas Campesinas de Ortega	*Paramilitary*		
Bloque Bananero	*Paramilitary*		
Autodefensas del Sur del Magdalena e Isla de San Fernando	*Paramilitary*	*Autodefensas de Cundinamarca*	
Bloque Catatumbo	*Paramilitary*		
Bloque Calima	*Paramilitary*		
Autodefensas de Córdoba	*Paramilitary*		
Frente Suroeste Antioqueño	*Paramilitary*		
Frente Mojana	*Paramilitary*		
Frente Héroes de Tolová	*Paramilitary*		
Bloque Montes de María	*Paramilitary*		
Bloque Libertadores del Sur	*Paramilitary*		
Bloque Héroes de Granada	*Paramilitary*		
Autodefensas de Meta y Vichada	*Paramilitary*		
Bloque Pacífico	*Paramilitary*		
Bloque Centauros	*Paramilitary*		
Bloque Noroccidente Antioqueño	*Paramilitary*		
Frente Vichada	*Paramilitary*		
Bloque Tolima	*Paramilitary*		
Frentes Nordeste Antioqueño, Bajo Cauca y Magdalena Medio	*Paramilitary*	*Frente Héroes y Mártires de Guática*	
Bloque Vencedores de Arauca	*Paramilitary*		
Bloque Mineros	*Paramilitary*		
Autodefensas Campesinas de Puerto Boyacá	*Paramilitary*		
Bloque Resistencia Tayrona	*Paramilitary*		
Autodefensas Campesinas de Magdalena Medio	*Paramilitary*		
Frentes Próceres del Caguán, Héroes de los Andaquíes y Héroes de Florencia	*Paramilitary*		
Frente Sur del Putumayo	*Paramilitary*		
Frente Julio Peinado Becerra	*Paramilitary*		
Bloque Norte	*Paramilitary*		
Frente Héroes del Llano	*Paramilitary*		
Frente Héroes del Guaviare	*Paramilitary*		
Bloque Élmer Cárdenas	*Paramilitary*		
Frente Cacique Pipintá	*Paramilitary*		
Autodefensas Campesinas del Casanare	*Paramilitary*		
Cartel de Medellín	*Cartel*	*Medellin Cartel*	1970s
Cartel de Cali	*Cartel*	*Cali Cartel)*	1980s
Cartel del Norte del Valle	*Cartel*	*North of Valle Cartel*	1990s
Microcarteles	*Cartel*		
Convivir	*Other*	*Cooperativas de Vigilancia y Seguridad Privada*	
Muerte a Secuestradores (MAS)	*Other*		

[a] See Daly (2016, p. 44 - 71) for further details.

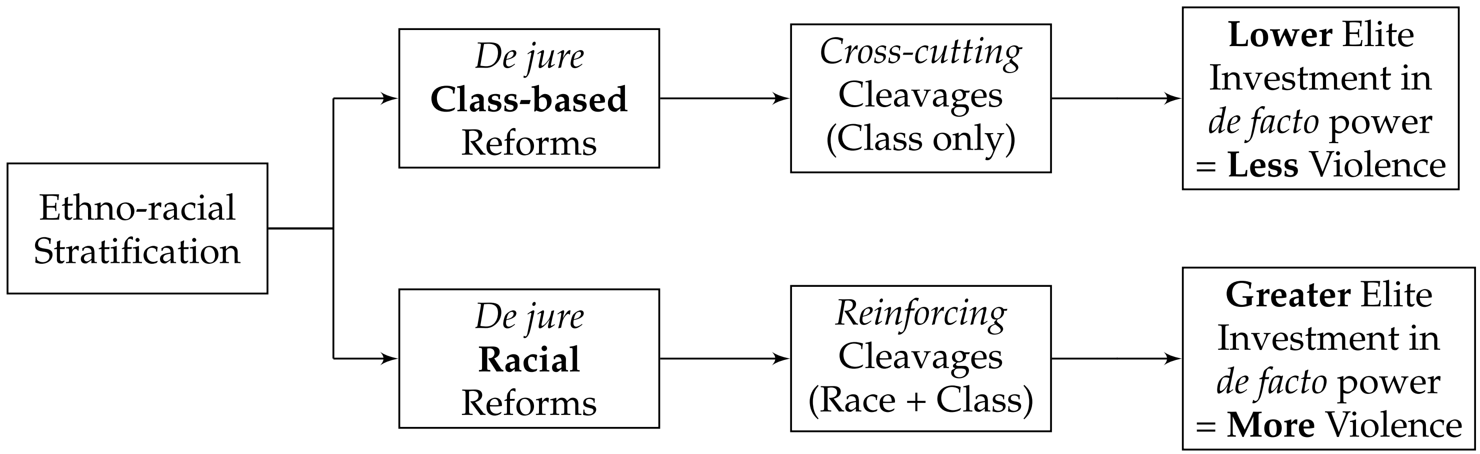


Figure A. 4: Main Argument

A.1 Descriptive Statistics

Panels A and B of Table A.0 present the summary statistics for the municipality-year sample which consists of a balanced panel of 1,122 municipalities $\times$ 33 years (1980 - 2012). We use the GMH data to construct the total armed actor attacks variable which represents the frequency of municipal-year attacks carried out by any state or non-state actor. There were approximately 976 violent events recorded against people and/or property in a given year, averaging to about 2.67 attacks per day over the sample period. We then disaggregate armed attacks into three separate variables for police and army attacks, paramilitary attacks, and guerrilla attacks. Guerrilla attacks made up the largest portion of armed attacks with about 1.26 attacks per day, followed by paramilitary (1.09/day), and police and army (0.31/day).

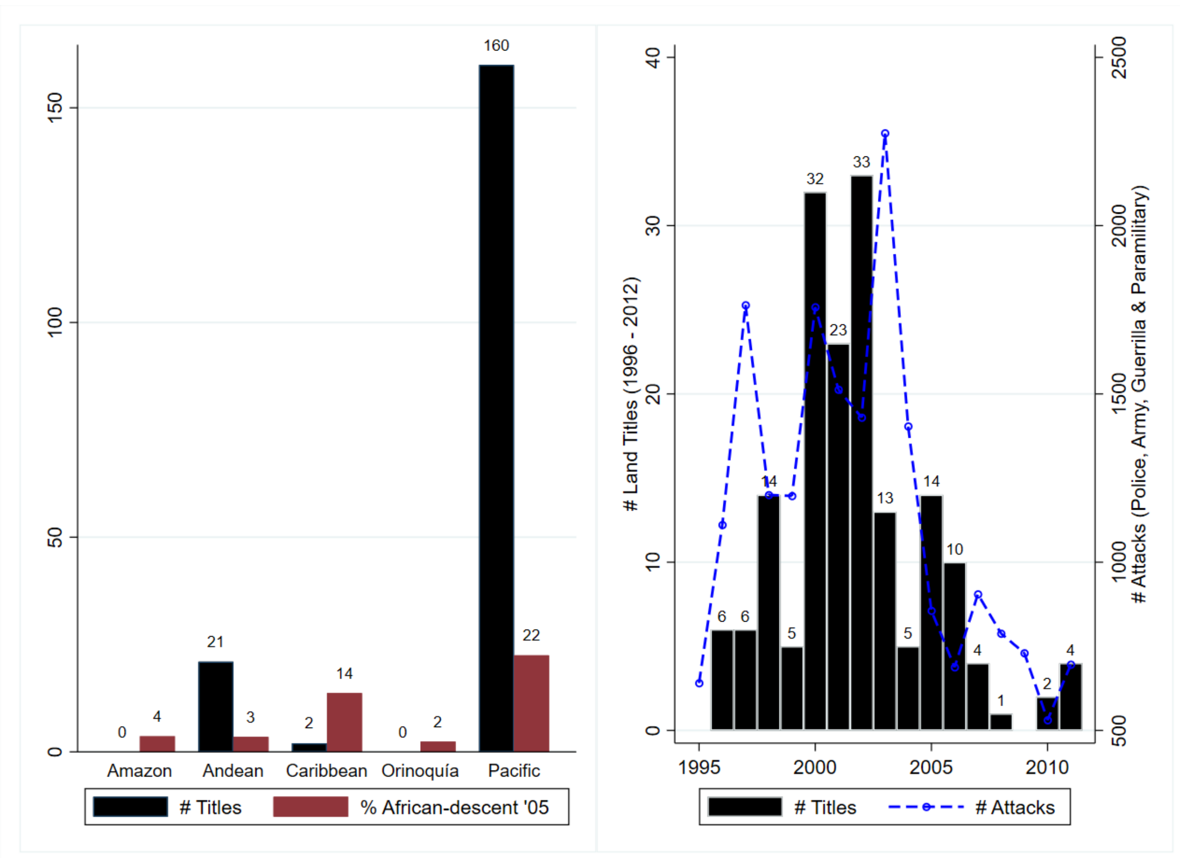


Figure A. 5: Land Titles (1996 - 2012)

For our independent variables in panel B, we use the OTEC data to construct several new measures of collective land titling at the municipal level. *Any Title* is a dummy variable that is coded as one if the municipality had an Afro-descendant community that received a land title between 1996 and 2012, and zero otherwise. We use this variable in our benchmark specification to estimate the difference in violent attacks for a particular municipality before and after the start of land titling. About 4.6% of municipalities received at least one collective land title following the passage of Law 70, with the majority allocated to the Pacific and Andean regions (Fig. A5). Total land titles by municipality-year captures the frequency of titles that a municipality received in any given year. Though the average municipality in the treatment group received at most one land title throughout the sample period, there were 4 Andean and 17 Pacific municipalities that were granted multiple land titles between 1996 and 2012 ($\approx$ 2 additional titles/year). We use total land titles to construct the new municipality-year title indicator variable, which equals one for each municipality-year in the sample when a new land title was

Table A.0: Summary Statistics

	[1] Mean	[2] Std. Dev.	[3] Min	[4] Max	[5] Obs.
Panel A: Dependent Variables					
All attacks	0.870	4.77	0.00	392.00	37,026
Police & Army attacks	0.101	0.95	0.00	112.00	37,026
Paramilitary attacks	0.357	2.75	0.00	232.00	37,026
Guerrilla attacks	0.411	3.51	0.00	380.00	37,026
Panel B: Afro-descendant Land Titling Variables					
Any Title (0/1)	0.046	0.21	0.00	1.00	37,026
Total land titles granted by municipality-year	0.005	0.12	0.00	10.00	37,026
New municipality-year land title	0.003	0.05	0.00	1.00	37,026
Cumulative municipality-year land titles	0.053	0.72	0.00	35.00	37,026
Panel C: Land Titling Seasons					
Winter (0/1)	0.459	0.50	0.00	1.00	183
Spring (0/1)	0.273	0.45	0.00	1.00	183
Summer (0/1)	0.087	0.28	0.00	1.00	183
Fall (0/1)	0.180	0.39	0.00	1.00	183
Panel D: RDiT Sample					
All attacks	0.052	0.47	0.00	33.00	41,600
Winter attacks	0.012	0.19	0.00	14.00	41,600
Spring attacks	0.016	0.29	0.00	23.00	41,600
Summer attacks	0.014	0.28	0.00	33.00	41,600
Fall attacks	0.011	0.17	0.00	12.00	41,600

Note: The unit of analysis for panels A and B is a municipality-year. For panels C and D, the unit is a municipality and a municipality-week, respectively. Seasons are coded as Spring (March - May), Summer (June - August), Fall (September - November), and Winter (December - February).

approved. This variable is used to calculate the first year when a municipality received a land title for the event-study design (Fig. A6). Finally, the cumulative municipality-year titles variable represents the total stock of collective land titles allocated to the municipality between 1996 (when the implementation of Law 70 begins) and any given year. The running tally allows us to test for linear or threshold effects for violence based on the number of existing titles.

Panels C and D provide summary statistics for the samples used in the RDiT analysis. Out of a total of 205 land titles granted between 1996 and 2018, we were able to geolocate 200 (97.6%). Of these, we only had information on the roll-out dates for 196 titles spread across 50 municipalities. Since our violence panel extends until 2012, we restrict the sample further to give us a total of 183 titles (89.2% of the original sample). Panel C shows that most of the land titles were assigned in the Winter and Spring (73.2%), however there is considerable variation over the sample period. Titling over Spring also corresponds with an escalation in political violence. Panel D presents estimates for the average number of weekly attacks for the sample of titled municipalities from the start of land titling in 1996 until 2012 (50 municipalities $\times$ 52 weeks $\times$ 16 years).[1] There were on average 2.6 violent attacks per week over the 16 year period with about 57.7% of all attacks taking place during the Spring and Summer months. We account for these trend differences using a vector of seasonal controls in our analysis.

[1] To reduce measurement error and attenuation bias due to enumeration error in the recorded timing of armed actor attacks, we collapse the data at the municipality-week level.

Next, we examine the determinants of land titling and political violence in our sample. Fig. A7a illustrates differences in geographic, socio-economic, and historic characteristics between treatment and control municipalities. Since the titles were not randomly assigned, we expect to see some baseline differences across groups. Land titles were on average more likely to be allocated to areas which were closer to the coast, historically poorer, more isolated and rural. Treated municipalities also had a substantial presence of gold mining and slavery during the colonial period and a larger share of Afro-Colombian population in the present day. We control for these time invariant observable differences in our analysis using both municipality fixed effects and a rich set of pre-treatment covariates.

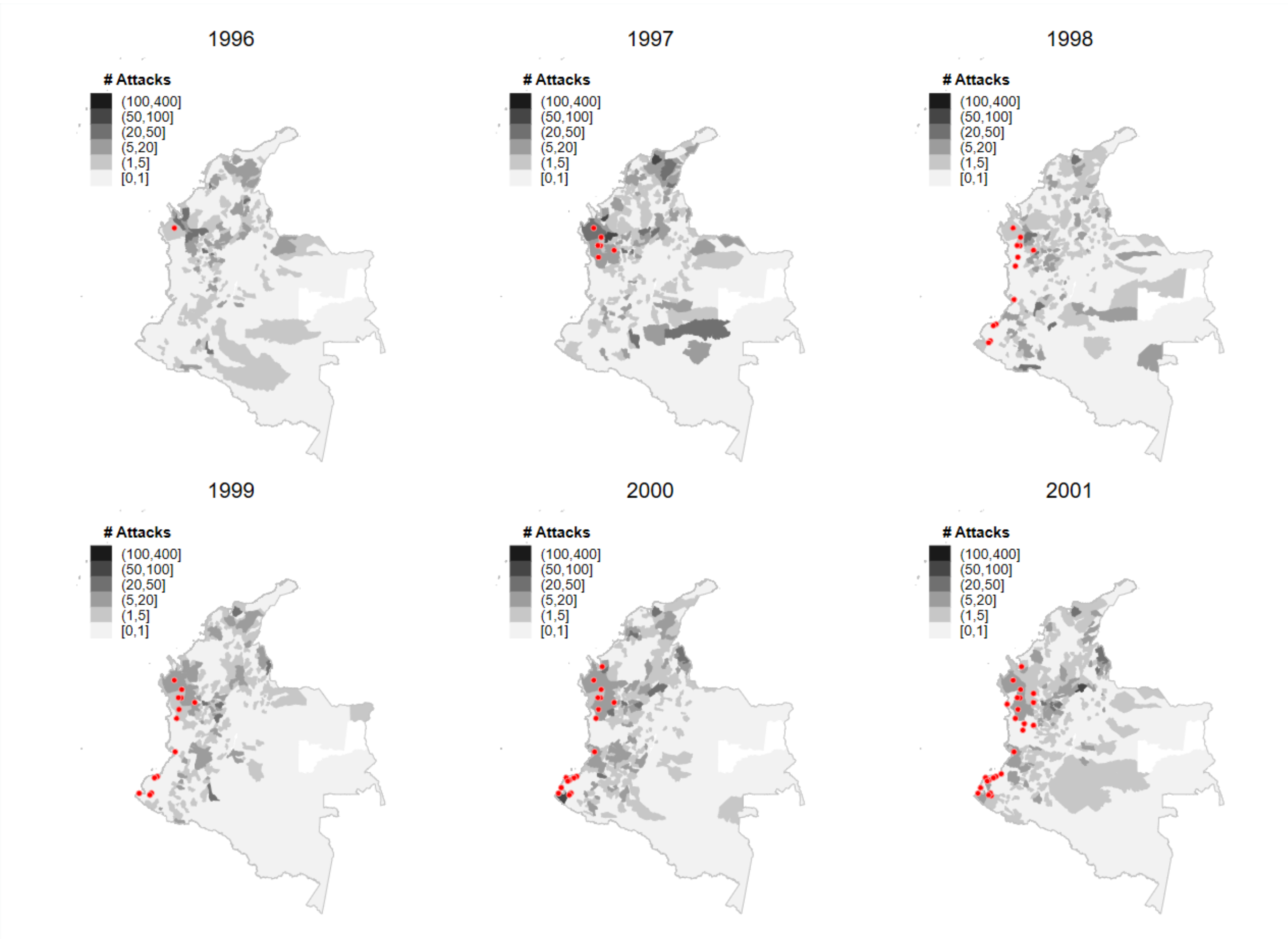
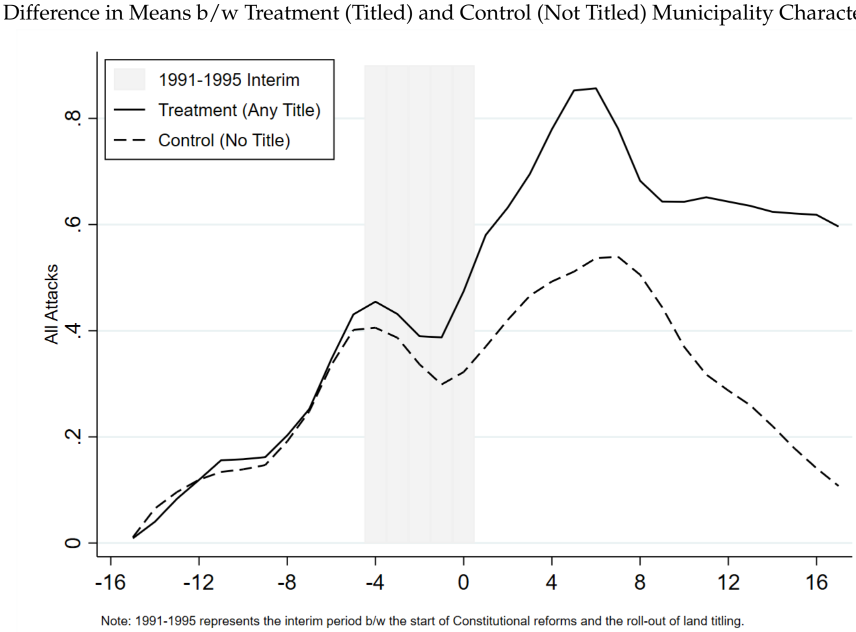


Figure A. 6: Armed Attacks, Yearly Totals (1996 - 2001). Dots represent municipal-year land titles.

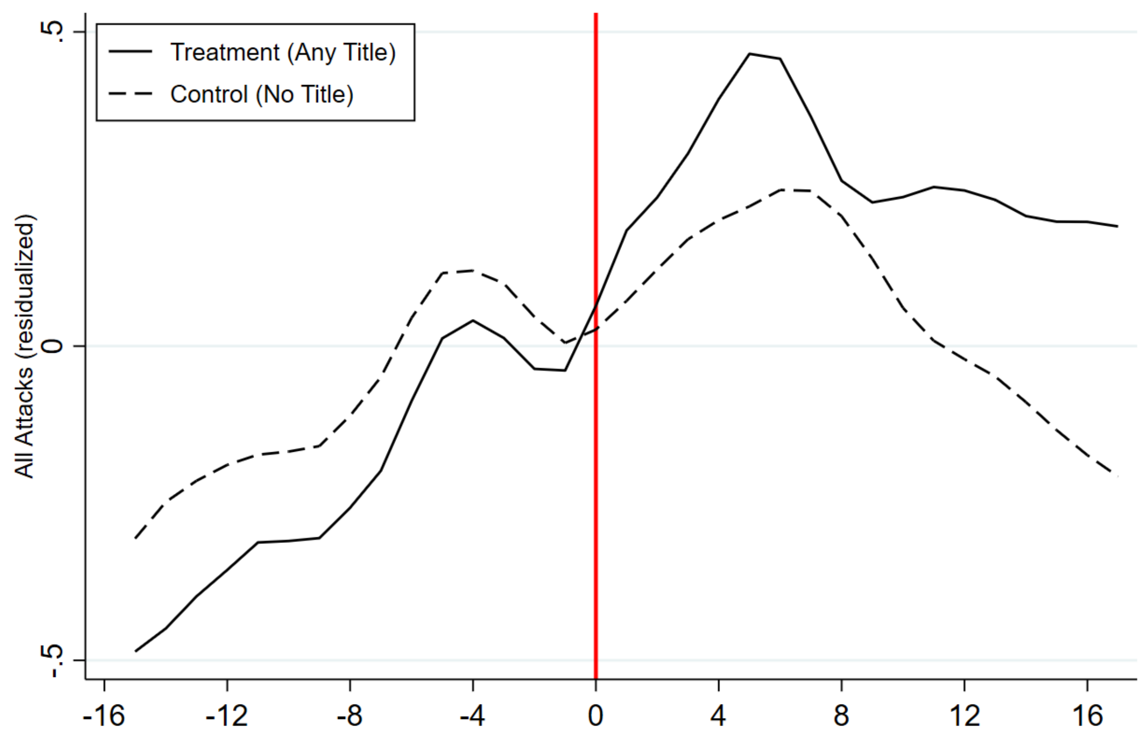
We also examine pre-reform trends in our outcomes of interest to check if the parallel trends assumption holds. Fig. A7b provides a visual illustration of annual changes in political violence. Prior to the constitutional reforms introduced in 1991, we see no significant differences between the treatment (any title) and control (no title) groups. Between 1991 and 1995, however, there is a break in the trend with a slight increase in violence after which we observe a substantial divergence in armed attacks in the treatment group compared to the control group. This could be because armed groups engaged in pre-emptive targeting of predominantly Afro-Colombian communities along the Pacific coast which were scheduled to receive land titles. We present further tests for parallel trends in section 7 to rule out the possibility of spurious results due to some artifact of the data generating process.

(a) Difference in Means b/w Treatment (Titled) and Control (Not Titled) Municipality Characteristics



(b) Trends in Political Violence 1982 - 2012 (*Year 0* = 1995)

Figure A. 7: Determinants of Land Titling and Political Violence



Note A: All attacks represent the residualized outcomes, partialing out geographic characterisitics such as altitude, area, river distance, density, and rainfall.

Figure A. 8: Evaluating Parallel Trends

A.2 Robustness Tests: *Difference-in-Difference*

In panel A of appendix Table A1, we calculate standard errors adjusted for spatial dependence of an unknown form (Conley, 1999), cluster the errors at the department instead of the municipality level, and correct for serial correlation across observations within the same department and year by clustering on each department-year combination (Bertrand, Duflo, and Mullainathan, 2004). Our estimates remain significant at conventional levels across all specifications. We include region-by-year fixed effects in panel B to account for systematic shocks at the regional level, drop urban areas from the sample, and use log attacks and log attacks per capita instead of the inverse hyperbolic sine to transform our outcomes of interest. Our main results are robust to these alternative specifications as well. Finally, we estimate the effects using negative binomial and tobit regressions to adjust for censoring in the dependent variable in appendix Table A2. We obtain comparable estimates to our benchmark OLS model.

Table A. 1: Baseline Model Specification Checks

	Dependent Variable: All Attacks				
	Spatial HAC	Dept.	Dept. × Yr		
	[1]	[2]	[3]		
Panel A: Clustering Checks					
Any Title (0/1) × Post (>1995)	0.19**	0.20**	0.20***		
	(0.06)	(0.09)	(0.07)		
	[0.08]				
Observations	2244	31581	31581		
Clusters		27	891		
Model	Long diff.	Panel	Panel		
	Region × Year F.E.	Log - All Attacks	Drop Urban Areas	All Attacks per capita	Log - Attacks per capita
	[1]	[2]	[3]	[4]	[5]
Panel B: Specification Checks					
Any Title (0/1) × Post (>1995)	0.195**	0.579**	0.343**	0.000**	0.005**
	(0.098)	(0.242)	(0.172)	(0.000)	(0.002)
Observations	31581	31581	10329	31581	31581
Clusters	957	957	313	957	957
Dependent Variable Mean	0.926	0.926	0.528	0.926	0.926

Note: The unit of analysis is a municipality-year. Unless indicated otherwise, we apply the inverse hyperbolic sine (IHS) transformation to all the outcomes to correct for the skewed distribution in the number of attacks. Robust clustered standard errors are reported in parentheses. Standard errors corrected for spatial correlation following the method described in Conley (1999) are reported in brackets. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

We conduct two robustness checks to assess the validity of our event-study estimates. First, we augment our baseline specification by including a linear trend in pre-treatment covariates. Assuming linearity in $\mathbf{X}'_{md}$, the revised model now fully accounts for municipal-specific factors systematically correlated with both the roll-out of land titling and the evolution of armed actor attacks. Fig. A9a in the appendix shows that net changes in violence in the years leading up to the assignment of the land title are indistinguishable from zero. By contrast, we observe that violence increases significantly in the years that follow, lending further support to our initial findings.

Next, we estimate treatment effects for early and late land titling "adopters". Municipalities exposed to the titling treatment in the first couple of years of the program are classified as early adopters. We compare these municipalities to those treated in later phases of the staggered roll-out. This allows us to examine how much of the variation in the treatment effect can be explained by endogenous selection

Table A. 2: Negative Binomial and Tobit Regressions (*All Attacks*)

	Negative Binomial Model			Tobit Model		
	[1]	[2]	[3]	[4]	[5]	[6]
Any Title (0/1) × Post (>1995)	1.180***	1.145***	0.787***	1.363***	1.336***	1.058***
	(0.380)	(0.274)	(0.279)	(0.209)	(0.215)	(0.264)
Any Title (0/1)	0.441	-0.635**	-0.522*	0.258	-0.920***	-0.778***
	(0.313)	(0.248)	(0.267)	(0.177)	(0.194)	(0.216)
Post (> 1995)	2.955***	2.813***	4.059***	4.213***	3.988***	5.538***
	(0.544)	(0.595)	(0.663)	(0.653)	(0.647)	(0.697)
Observations	37026	31581	31581	37026	31581	31581
Clusters	1122	957	957	.	.	.
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Department F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes
Controls × Post	No	Yes	Yes	No	Yes	Yes
Department time trends	No	No	Yes	No	No	Yes
Dependent Variable Mean	0.870	0.926	0.926	0.870	0.926	0.926

Note: The unit of analysis is a municipality-year. Robust standard errors clustered at the municipality level are reported in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

into the treatment group (Goodman-Bacon, 2019). Our event-study estimates will be unbiased if the pattern of the effects is the same across early and late adopters. However, if the estimates vary over the sample period, the timing of cross-cohort selection into the treatment group generates a differential trend, biasing the estimated effect away from the true effect.[2]

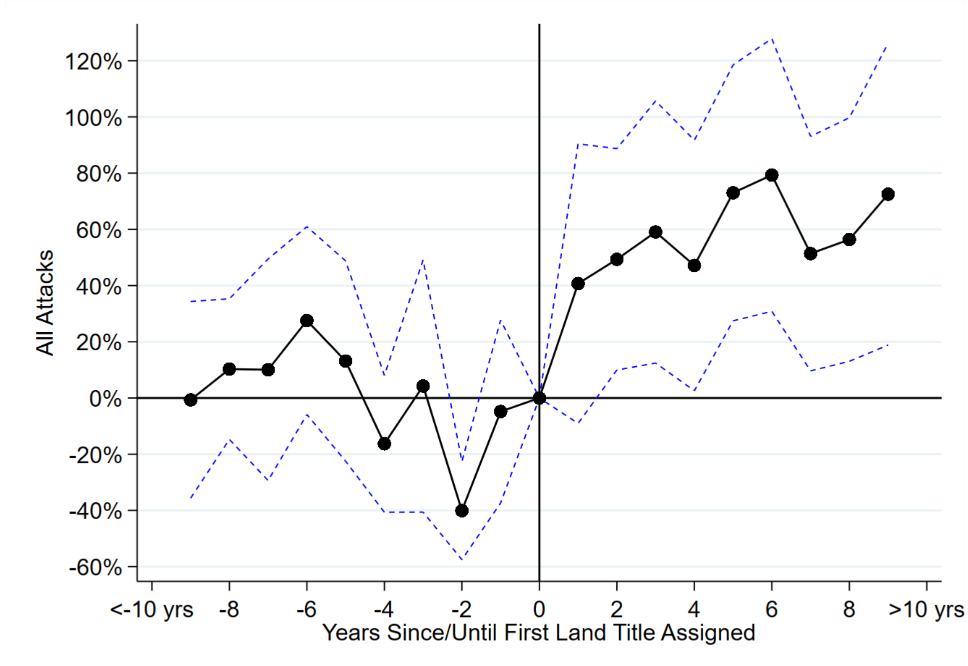
We identify early and late adopters based on the date when INCORA, the land agency responsible for granting land titles, was dissolved.[3] In 2003, following years of malfeasance and accusations of corruption, the Colombian government disbanded INCORA and replaced it with the Colombian Institute of Rural Development (INCODER).[4] We use this date to separate early and late adopters and calculate the treatment effects for each group using our baseline model. Fig. A9b in the appendix presents the event study estimates for the pre- and post-2003 samples. Our analysis indicates that the increase in violence following the assignment of a land title is driven primarily by early adopters. Though there is a break in the trend within the first year of receiving a land title for late adopters, a significant divergence does not occur until about four years into the program. This could be for several reasons. First, the titling of Afro-descendant lands declined substantially after 2003 (nearly 65% of the variation in land titling comes from INCORA's tenure). Second, there was a significant decline in armed actor attacks following a ceasefire negotiated by the Uribe government that subsequently led to the demobilization of paramilitary groups in 2006. This may have had a differential effect on early and late adopters of the program. Finally, the criteria used by INCODER to allocate land titles changed following pressure by the Constitutional Court and human rights groups to address the mismanagement of prior land titling efforts.[5]

[2] Formally, this violates the treatment effect homogeneity assumption, which occurs when "when different cohorts experience different paths of treatment effect" (Abraham and Sun, 2020).

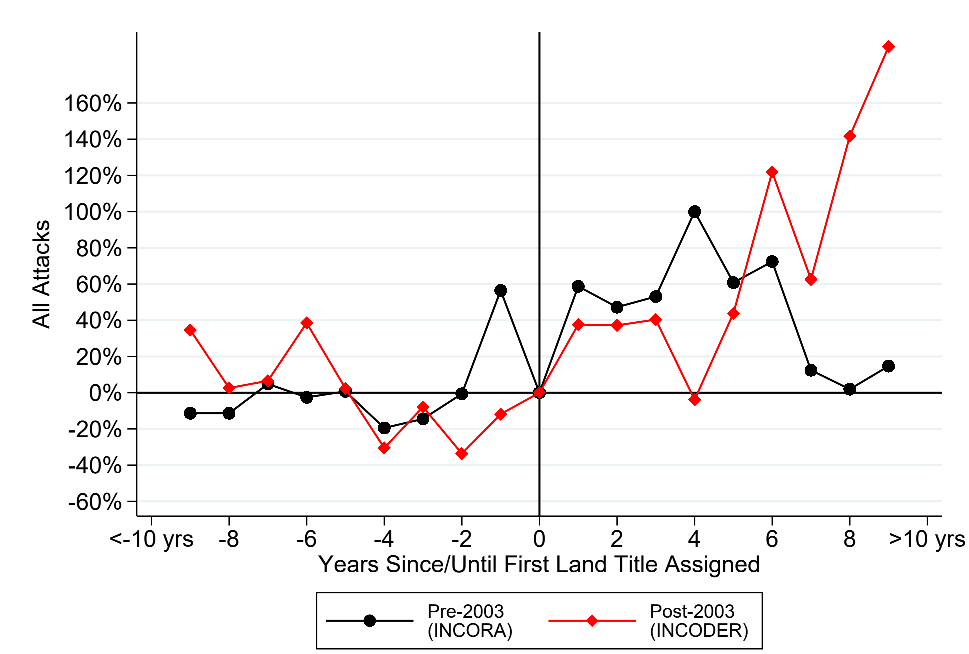
[3] Decree 2664 in 1994 designated INCORA as the agency responsible for overseeing the collective titling of Afro-descendant *baldíos* (Arango, 2018).

[4] In 2015, the government delegated the the management and regulation of collective land titling to National Land Agency (ANT).

[5] Following pressure from the Constitutional Court, the Uribe government passed the Statue of Rural Development in 2007 which "sought to reverse the direction of previous land reforms, notably Law 160, by promoting the market as the principal mechanism for distributing land" (Amnesty International, 2014).



(a) Including Linear Trend in Model Covariates



(b) Comparing Early (pre-2003 INCORA) & Late (post-2003 INCODER) Land Titling Adopters

Figure A. 9: Event-study Specification Checks

A.3 Robustness Tests: Regression Discontinuity in Time (RDiT)

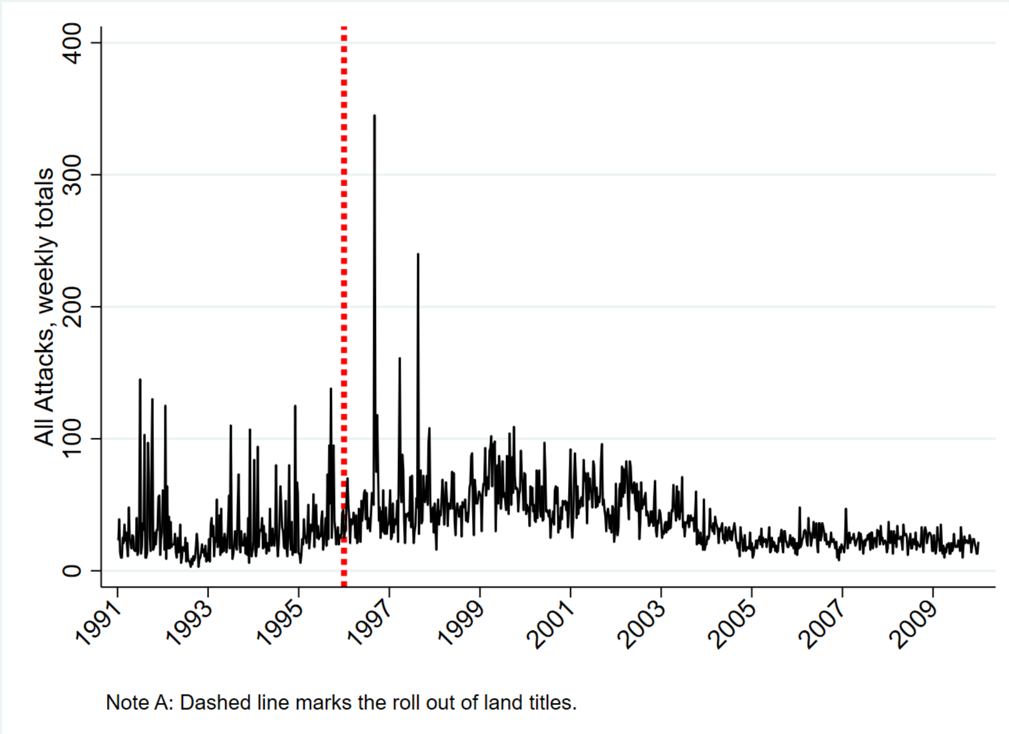


Figure A. 10: Weekly Attack Frequency (1991 - 2010)

Unlike standard cross-sectional RD designs, the RDiT model relies on asymptotics in time. Identification is based on exogeneity assumptions regarding the discontinuity at the threshold, rather than local randomization within a narrow bandwidth of the threshold (Hahn, Todd, and der Klaauw, 2001; Lee and Lemieux, 2010; Sekhon and Titiunik, 2017).[6] This has two specific advantages. First, it allows us to conduct within sample comparisons without making arbitrary modeling decisions regarding the counterfactual.[7] Second, it permits considerable flexibility in the way in which unobservable factors are allowed to vary, avoiding some of the pitfalls associated with making strong parametric assumptions about the underlying data generating process (Hausman and Rapson, 2018).

To estimate a local linear RDiT regression, we follow Imbens and Lemieux (2008) and specify the model as follows:

$$\begin{aligned} v_{mwt} = \; & \kappa_{m_1} \cdot \mathbb{1}[Date_{wt} \geq Date_{wt}^{title}] + \kappa_{m2} f(Date_{wt}) + \kappa_{m3} \Big(f(Date_{wt}) \times \mathbb{1}[Date_{wt} \geq Date_{wt}^{title}] \Big) \\ & + \tau_t + \mathbf{S'}_{wt} \Phi + \varphi_{mwt} \end{aligned} \tag{A.1}$$

In equation A.1, the treatment indicator is determined by the cut-off $[Date_{wt} \geq Date_{wt}^{title}]$, which equals one for all weeks **w** of year **t** following the first date when a land title was granted, and zero otherwise. We estimate κ_{m_1} for only the *first* date of a given year when a land title was granted. The forcing variable $f(Date_{wt})$ controls for smooth polynomial functions in time. We normalize $Date_{wt}$ to zero by subtracting the threshold date from the forcing variable.

[6] This is because the forcing variable in the standard RDiT framework is time, which cannot be randomly assigned.

[7] Since we compare the same group before and after the date a title is granted, we don't need to separate the cross-sectional sample into treated and untreated units (Mummolo, 2017). This also implies that the analysis is restricted to only those municipalities where Law 70 was applied, mitigating endogeneity concerns due to systematic regional differences in the sample (Albertus, 2020).

While most RD designs use controls to reduce statistical noise and improve precision, standard RDiT models require controls to account for any time varying unobservables correlated with the forcing variable that may have a discontinuous effect on the outcome of interest at the threshold (Auffhammer and Kellogg, 2011). Though this concern is substantially mitigated by the granular level at which our data is aggregated, we cannot rule out the possibility of counfounding due to underlying seasonal trends.If land titles were approved during particular seasons when armed conflict increased due to systematic changes in the labor market, trade, or rainfall shocks, our treatment effect will be biased upward (Miguel, Satyanath, and Sergenti, 2004; Dell, Jones, and Olken, 2012; Dell, Feigenberg, and Teshima, 2019). To control for this, we include a set of season dummies $\mathbf{S'}_{wt}$ interacted with the forcing variable in all our specifications.

In appendix Tables A3 and A4, we estimate the effects using both local-linear and polynomial approaches across a variety of bandwidths and functional forms. We present these estimates graphically in appendix Fig. A11. Though the standard errors are slightly larger within the neighborhood of the threshold when we use more flexible estimators, we continue to detect a positive and statistically significant effect of land titling on violence. We perform two validation checks to ensure that our results are not being driven by short-term changes or spillovers from other land reforms. First, we show in Fig. A12 in the appendix that our estimates are robust to using a flexible global polynomial estimator for entire sample period.[8] This helps allay concerns that our main findings only hold for the short-run.[9]

Second, we test whether spillovers from current or past land reforms had any impact on violence in Afro-descendant communities. Fig. A13 in the appendix illustrates the regional share of land reforms carried out by INCORA between 1965 and 2000 that were unrelated to Law 70. Though most of these other land titling efforts were concentrated in the Andean region, we do find that municipalities titled under Law 70 were also more likely to be selected for other (non-indigenous) land reforms.[10] Our RDiT estimates will be biased by concurrent land titling if there was a significant overlap between the date and location of these reforms and land titling under Law 70.

Since we only have information on the year when these other land reforms were carried out, we cannot directly test for the degree of overlap. We can, however, indirectly test for spillovers by examining whether the normalized threshold date for collective land titling had any effect on violence for municipalities that were selected for other types of land reforms. We draw municipality samples with (a) indigenous reservations; (b) peasant land titling between 1988 - 2000 and; (c) peasant land titling between 1965 and 1985, and rerun our main analysis on these alternative samples. If there is a discontinuous jump in armed attacks for any of these alternative samples, then spillovers are more likely. Conversely, if we do not find a significant increase, then our RDiT estimates are unbiased. Appendix Fig. A14 displays the reduced-form results. We find no impact of the threshold date on violence for any of the alternative samples. When we instead sample municipalities with Afro-descendant communities and estimate the effect,[11] we observe a discontinuous jump in attacks.[12] Based on these additional empirical results, we conclude that collective land titling does indeed cause armed actor attacks to increase in municipalities with Afro-descendant communities.

[8] The estimated coefficient using a second order polynomial is approximately 0.01 or 1% with a t-stat of 2.03.

[9] We eschew using global estimators in our main analysis since the effect decays sharply following treatment assignment. Depending on the decay process, a global estimator may produce biased estimates under a time-varying treatment effect if there is polynomial overfitting due to the longer sampling window (Hausman and Rapson, 2018; Gelman and Imbens, 2019).

[10] Regarding indigenous communities, appendix Fig. A15 shows that most of the indigenous reservation land resolutions were disproportionately concentrated in the control group municipalities.

[11] This includes *all* Afro-descendent communities, i.e., those communities that had been allocated a land title and those that had not.

[12] The magnitude of the estimated effect is smaller since we use the universe of municipalities with Afro-descendant communities (i.e., both titled and not yet titled).

Table A. 3: Regression Discontinuity in Time (RDiT) Estimates (*Local Linear*)

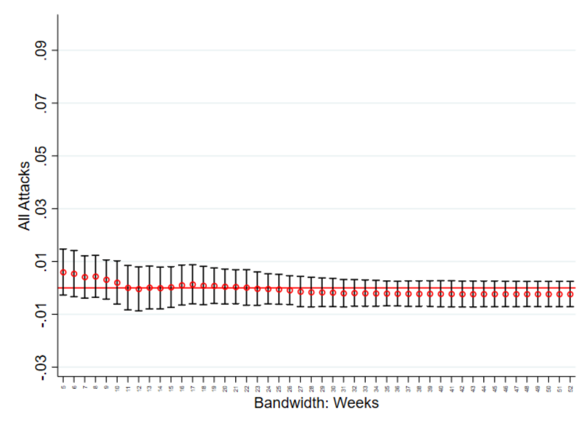
	All Attacks				Policy & Army Attacks				Paramilitary Attacks				Guerrilla Attacks			
	[1] 2 weeks	[2] 3 weeks	[3] 4 weeks	[4] 5 weeks	[5] 2 weeks	[6] 3 weeks	[7] 4 weeks	[8] 5 weeks	[9] 2 weeks	[10] 3 weeks	[11] 4 weeks	[12] 5 weeks	[12] 2 weeks	[12] 3 weeks	[12] 4 weeks	[12] 5 weeks
Panel A: Titled Municipalities																
Week Titled (0/1)	0.036***	0.045***	0.027*	0.025**	0.020*	0.013*	0.008*	0.007*	0.002	0.014*	0.005	0.004	-0.015***	0.001	0.001	0.003
	(0.013)	(0.016)	(0.014)	(0.011)	(0.011)	(0.007)	(0.005)	(0.004)	(0.012)	(0.009)	(0.006)	(0.006)	(0.004)	(0.005)	(0.006)	(0.005)
Adj. R-squared	0.0129	0.0120	0.0104	0.00986	0.0102	0.00813	0.00609	0.00588	0.0103	0.00967	0.00698	0.00652	0.0144	0.0120	0.00886	0.00840
Observations	3900	5400	6900	8350	2262	3132	4002	4843	3510	4860	6210	7515	3666	5076	6486	7849
Week Clusters	35	40	45	48	35	40	45	48	35	40	45	48	35	40	45	48
Municipality Clusters	50	50	50	50	29	29	29	29	45	45	45	45	47	47	47	47
Dependent Variable Mean	0.0369	0.0384	0.0375	0.0361	0.00400	0.00403	0.00404	0.00352	0.0123	0.0138	0.0140	0.0133	0.00917	0.00858	0.00847	0.00909
Panel B: Municipalities *not* titled (placebo)																
Week Titled (0/1)	0.001	-0.003	-0.006*	-0.003*	0.000	0.001	0.000	-0.001	-0.001	-0.001	0.000	0.001	0.002	-0.001	-0.005*	-0.001
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.003)	(0.001)
Adj. R-squared	0.00487	0.00483	0.00472	0.00451	0.00406	0.00329	0.00330	0.00279	0.00313	0.00327	0.00290	0.00294	0.00420	0.00333	0.00422	0.00327
Observations	77922	107892	137862	166833	36114	50004	63894	77321	50934	70524	90114	109051	61620	85320	109020	131930
Week Clusters	35	40	45	48	35	40	45	48	35	40	45	48	35	40	45	48
Municipality Clusters	999	999	999	999	463	463	463	463	653	653	653	653	790	790	790	790
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Season Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependent Variable Mean	0.0232	0.0234	0.0234	0.0231	0.00209	0.00210	0.00216	0.00213	0.00791	0.00810	0.00799	0.00770	0.00600	0.00585	0.00615	0.00596

Note: The unit of analysis is a municipality-week. We apply the inverse hyperbolic sine (IHS) transformation to all the outcomes to correct for the skewed distribution in the number of attacks. Standard errors are two-way clustered at the week and municipality level using the multi-way clustering method proposed by Cameron, Gelbach, and Miller (2011) to account for both the temporal and spatial dimensions of the estimation framework. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

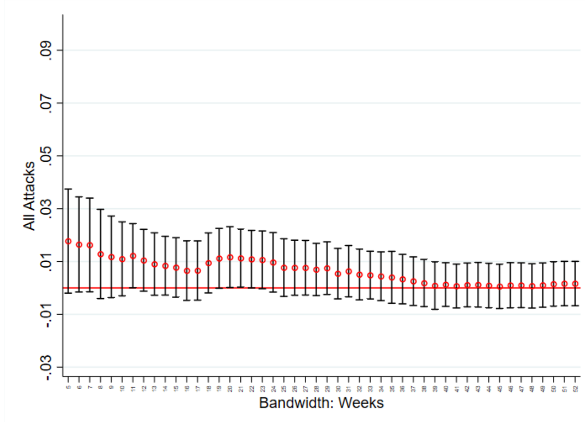
Table A. 4: Regression Discontinuity in Time (RDiT) Estimates (2^{nd} *Order Polynomial*)

	All Attacks				Policy & Army Attacks				Paramilitary Attacks				Guerrilla Attacks			
	[1] 2 weeks	[2] 3 weeks	[3] 4 weeks	[4] 5 weeks	[5] 2 weeks	[6] 3 weeks	[7] 4 weeks	[8] 5 weeks	[9] 2 weeks	[10] 3 weeks	[11] 4 weeks	[12] 5 weeks	[12] 2 weeks	[12] 3 weeks	[12] 4 weeks	[12] 5 weeks
Panel A: Titled Municipalities																
Week Titled (0/1)	0.025*	0.030**	0.016	0.016*	0.010	0.008	0.006	0.005	0.007	0.014**	0.004	0.000	-0.006	-0.003	-0.000	0.003
	(0.013)	(0.013)	(0.011)	(0.009)	(0.008)	(0.006)	(0.005)	(0.005)	(0.007)	(0.006)	(0.005)	(0.005)	(0.006)	(0.005)	(0.006)	(0.004)
Adj. R-squared	0.0128	0.0117	0.0101	0.00955	0.00959	0.00788	0.00599	0.00575	0.0102	0.00967	0.00697	0.00637	0.0141	0.0119	0.00885	0.00840
Observations	3900	5400	6900	8350	2262	3132	4002	4843	3510	4860	6210	7515	3666	5076	6486	7849
Week Clusters	35	40	45	48	35	40	45	48	35	40	45	48	35	40	45	48
Municipality Clusters	50	50	50	50	29	29	29	29	45	45	45	45	47	47	47	47
Dependent Variable Mean	0.0369	0.0384	0.0375	0.0361	0.00400	0.00403	0.00404	0.00352	0.0123	0.0138	0.0140	0.0133	0.00917	0.00858	0.00847	0.00909
Panel B: Municipalities *not* titled (placebo)																
Week Titled (0/1)	0.001	-0.001	-0.004**	-0.003**	0.000	-0.000	-0.000	-0.001*	0.000	0.001	0.000	0.000	-0.000	-0.000	-0.002	-0.001
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Adj. R-squared	0.00487	0.00482	0.00470	0.00451	0.00406	0.00325	0.00328	0.00278	0.00311	0.00324	0.00290	0.00293	0.00418	0.00333	0.00412	0.00326
Observations	77922	107892	137862	166833	36114	50004	63894	77321	50934	70524	90114	109051	61620	85320	109020	131930
Week Clusters	35	40	45	48	35	40	45	48	35	40	45	48	35	40	45	48
Municipality Clusters	999	999	999	999	463	463	463	463	653	653	653	653	790	790	790	790
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Season Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependent Variable Mean	0.0232	0.0234	0.0234	0.0231	0.00209	0.00210	0.00216	0.00213	0.00791	0.00810	0.00799	0.00770	0.00600	0.00585	0.00615	0.00596

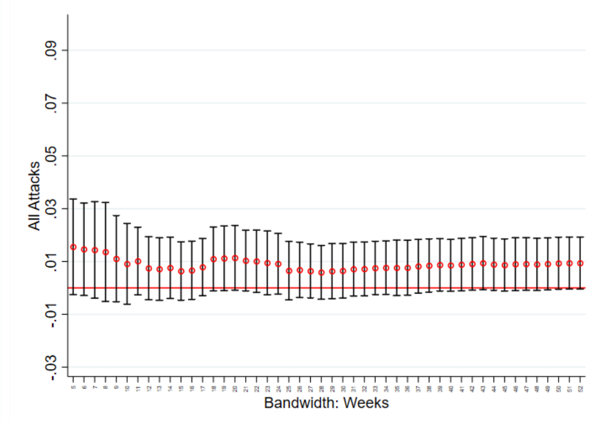
Note: The unit of analysis is a municipality-week. We apply the inverse hyperbolic sine (IHS) transformation to all the outcomes to correct for the skewed distribution in the number of attacks. Standard errors are two-way clustered at the week and municipality level using the multi-way clustering method proposed by Cameron, Gelbach, and Miller (2011) to account for both the temporal and spatial dimensions of the estimation framework. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.



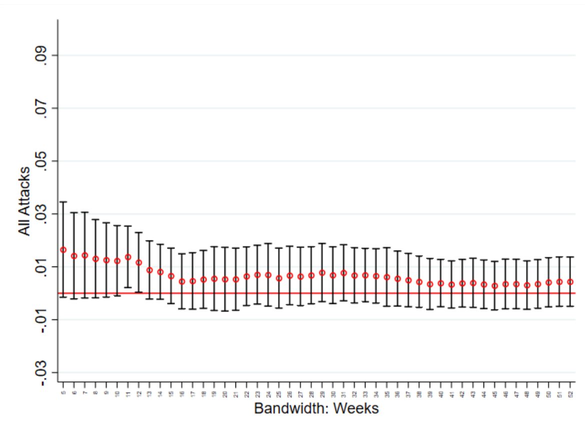
(a) Difference in Means



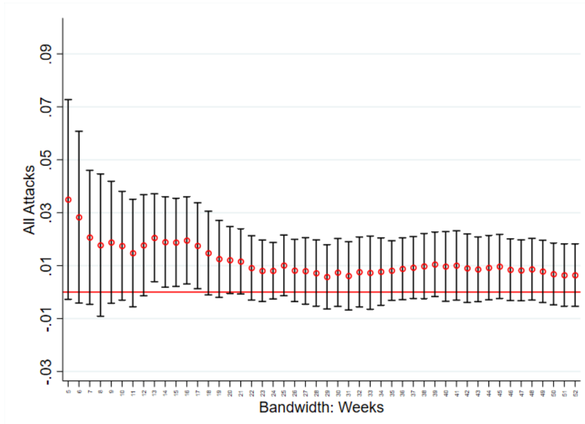
(b) Local-linear



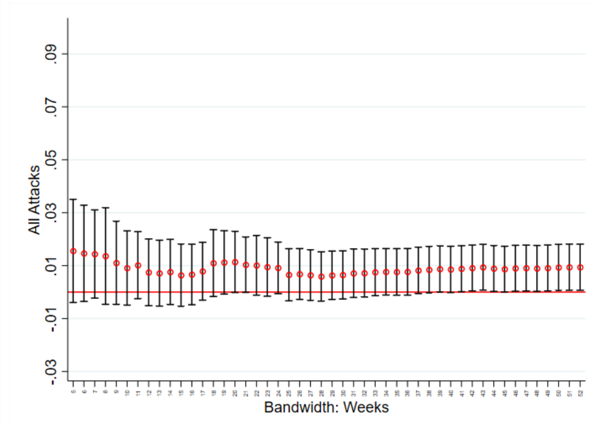
(c) Quadratic



(d) 1st Order Polynomial

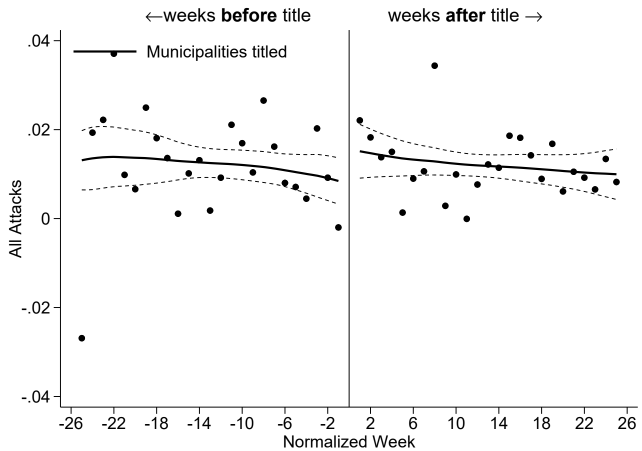


(e) 2nd Order Polynomial

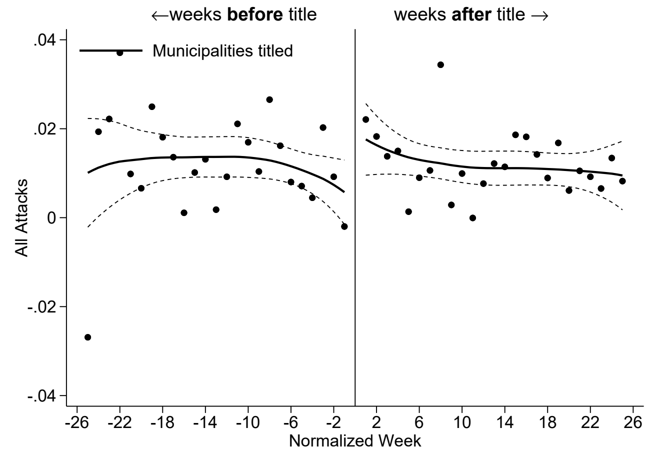


(f) 2nd Order Polynomial + Municipality F.E.

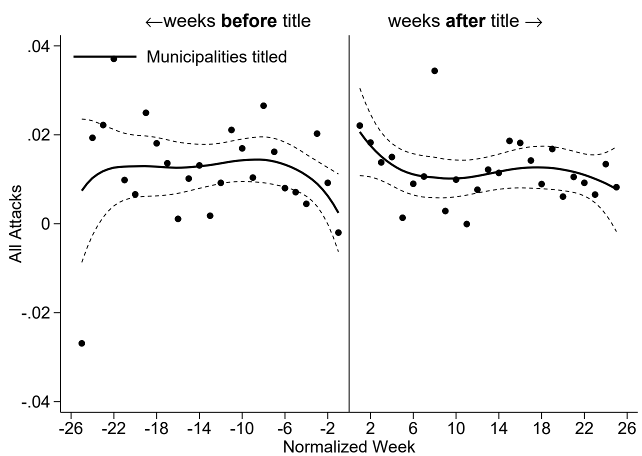
Figure A. 11: RDiT Specification and Bandwidth Robustness Tests



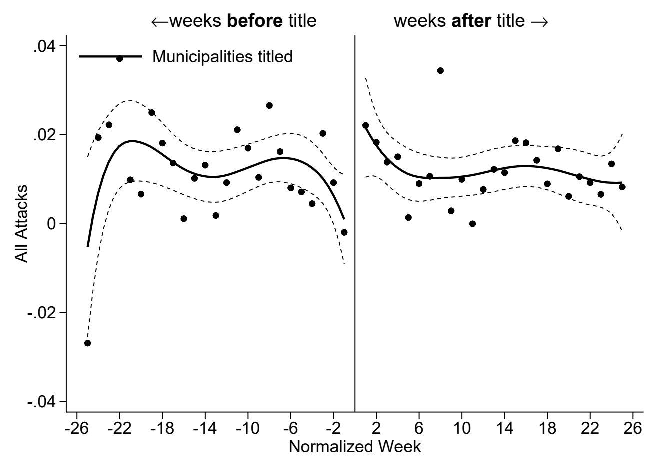
(a) 1^{st} order polynomial



(b) 2^{nd} order polynomial

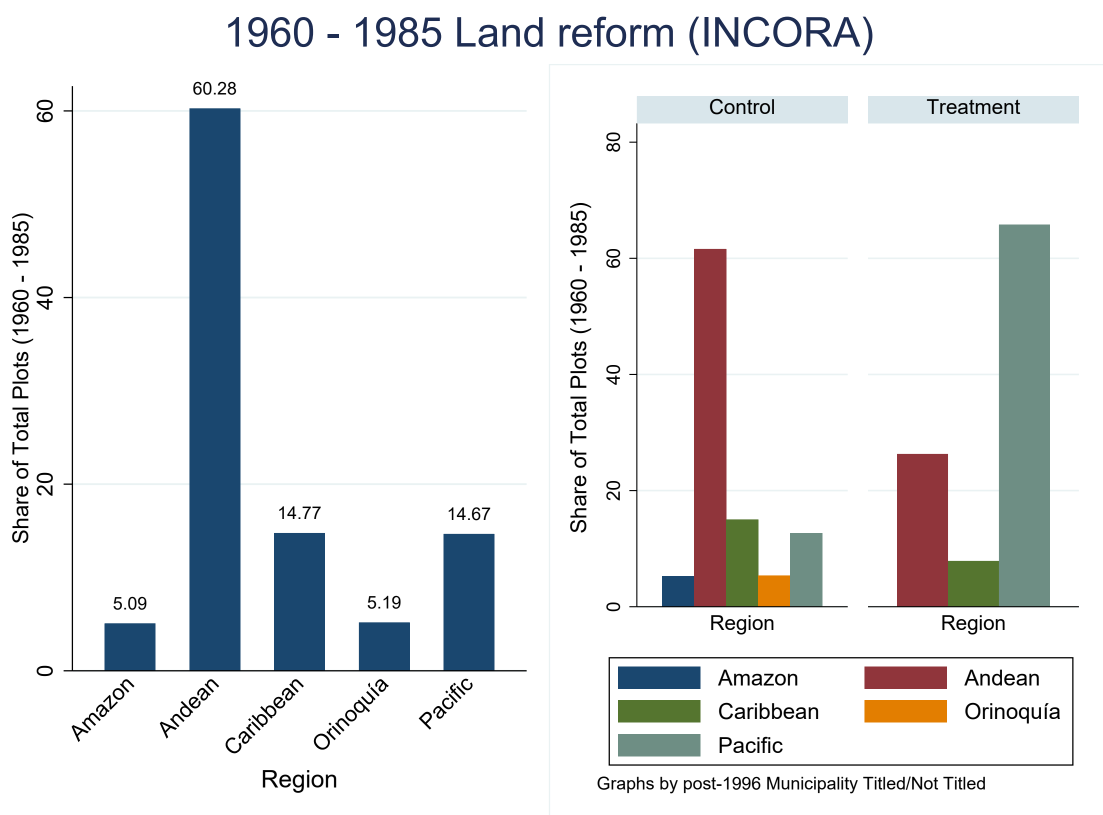


(c) 3^{rd} order polynomial

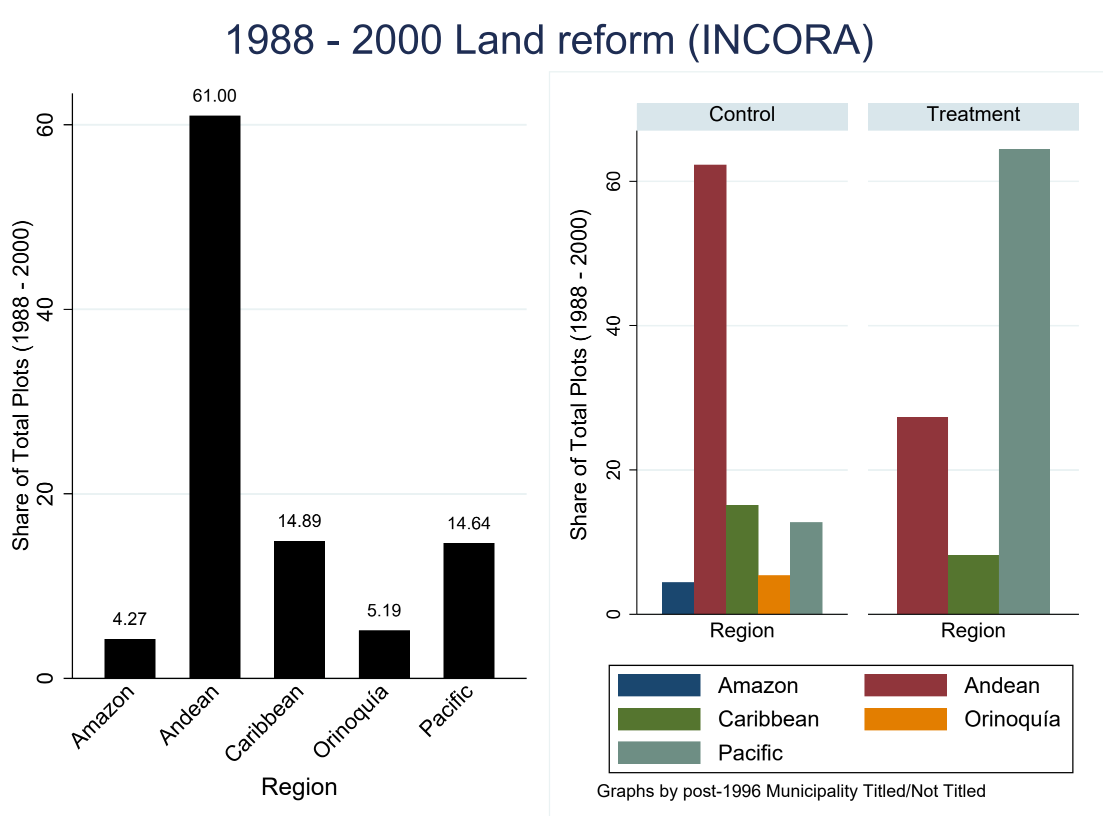


(d) 4^{th} order polynomial

Figure A. 12: RDiT Global Polynomial



(a) Plots Reformed (1965 - 1985)



(b) Plots Reformed (1988 - 2000)

Figure A. 13: INCORA Land Reforms not related to Law 70. *Authors' Calculations.*

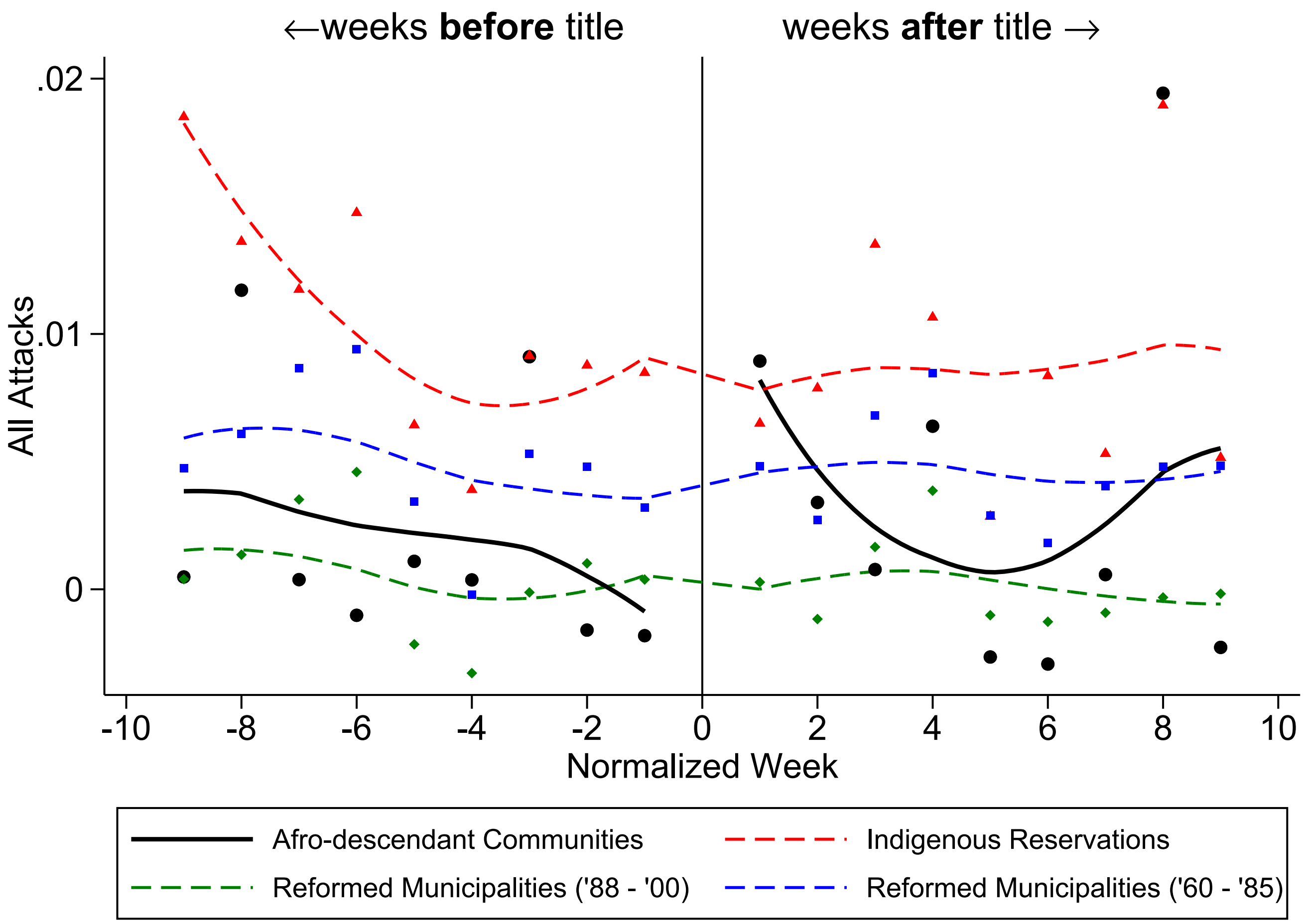


Figure A. 14: RDiT Alternative Samples

Table A. 5: Constitutional Reforms and Land Titling

	Policy & Army Attacks			Paramilitary Attacks			Guerrilla Attacks		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
Panel A: Constitutional Reforms									
Any Title (0/1) × Post (>1991)	-0.026	-0.026	-0.018	0.031	0.031	0.006	0.148*	0.148*	0.155*
	(0.023)	(0.023)	(0.024)	(0.031)	(0.032)	(0.033)	(0.083)	(0.084)	(0.085)
Post (> 1991)	0.022***	0.040***	0.038**	-0.003	0.047***	0.030	0.103***	0.192***	0.189***
	(0.006)	(0.009)	(0.015)	(0.007)	(0.011)	(0.025)	(0.012)	(0.019)	(0.024)
Any title (0/1)	-0.055			-0.067			-0.117***		
	(0.040)			(0.062)			(0.044)		
Panel B: Land Titling									
Any Title (0/1) × Post (>1995)	0.048	0.016	0.022	0.265***	0.182*	0.182*	0.022	-0.058	-0.043
	(0.036)	(0.040)	(0.040)	(0.099)	(0.103)	(0.099)	(0.086)	(0.079)	(0.078)
Post (> 1995)	-0.015**			0.142***			-0.056***		
	(0.007)			(0.010)			(0.012)		
Adj. R-squared	0.0436	0.132	0.136	0.0865	0.230	0.237	0.0481	0.172	0.174
Observations	31581	31581	31581	31581	31581	31581	31581	31581	31581
Clusters	957	957	957	957	957	957	957	957	957
Year F.E.	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Municipality F.E.	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls × Post	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Department time trends	No	No	Yes	No	No	Yes	No	No	Yes
Dependent Variable Mean	0.106	0.106	0.106	0.378	0.378	0.378	0.442	0.442	0.442

Note: The unit of analysis is a municipality-year. Robust standard errors clustered at the municipality level are reported in parentheses. We apply the inverse hyperbolic sine (IHS) transformation to all the outcomes to correct for the skewed distribution in the number of attacks. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

A.4 Robustness Tests: Mechanisms

We run two additional checks to address potential concerns regarding collinearity and threshold effects. Since land titling measures in our study are highly correlated, including them simultaneously in a regression may result in null effects due to multicollinearity. Rehabilitation zones, for instance, were originally used by INCORA to plan the implementation of most land reform programs. In appendix Table A6, we rerun our baseline model with each peasant land titling measure included separately across different specifications. We find no evidence to support multicollinearity. In addition, joint significance tests for paramilitary attacks reject the null that the coefficients on our titling indicator and the peasant land titling variables are zero at conventional levels.

Next, we examine whether the scope of the reforms had non-monotonic effects on violence. It could be argued that violence only increased in response to Afro-descendant land tiling on the extensive margin. If we were however to take into account the full scale of the reforms and measure the marginal effects, we would observe a decline after a certain threshold is reached (Albertus, 2020). We have already shown that the intensity of Afro-descendant land titling proxied for by the total area reformed led to an increase in political violence. As a further step, we estimate the impact of an additional land title conditional on the total stock of land reforms using three separate methods. First, we run a set of regressions using the total number of collective land titles assigned to each municipality between 1996 and 2012. Second, we use the total number of municipal-year titles that were granted. Third, we interact the municipal-year titles with a running tally of land titles granted for each year in the sample period.[13] Results for these regressions are provided in appendix Table A7. We find that attacks escalate in response to reforms when we use total land titles or municipal-year land titles as shown in panels A and B. Most of the increase can be explained by a surge in paramilitary attacks. This implies that the direction of the effect does not depend on the scale of land titling under Law 70. When we examine the effect of additional titles in panel C, we do find modest evidence of a decline in violence on the margin, however these effects are indistinguishable from zero. Overall, we do not find strong evidence to support the claim that a sufficiently large scale of collective land tiling would have led to a significant reduction in armed attacks.

As a final step, we examine whether the collective land tenure explains our main findings. Unlike other types of non-ethnic land reforms, land titling for Afro-descendant groups was based on non-transferable communal landholding. This raises three potential concerns for our interpretation. First, land reforms which permitted both private ownership and land trade may not be an appropriate counterfactual for collective land titling. Second, private ownership could have allowed third parties to trade directly with titled farmers, reducing the need for violence. Third, left-wing insurgent groups like FARC and ELN generally favored collectivized property arrangements (Steele, 2017). This may explain why we see no increase in guerrilla attacks against Afro-descendant groups.

To address these concerns, we rerun the analysis using titling data from indigenous reservations. The land resolutions that granted titles to indigenous groups were based on the same principles of collective land tenure reserved for Colombia's other ethnic minorities, including Afro-descendant communities (Arango, 2018). This allows us to isolate the effect of collective tenure on political violence. Though both Afro-descendant and indigenous groups hold land collectively, only Afro-descendent communities rely on the state for public goods. Each Indigenous Territorial Entity (ETI) is a self-governing institution with the right to set its own taxes and spending. Our theory predicts that racially targeted reforms will lead to greater counter-reform violence when elite interests are threatened. Since decision-making in indigenous territories would only apply to individuals living on reservations (and consequently have no direct impact on local or national policies), we do not expect these communities to be affected by counter-reform violence following new land resolutions that grant greater territorial sovereignty. Any

[13] This last measure is similar to the interaction of current and prior plots reformed in Albertus and Kaplan (2013). However, unlike their study, we do not have a "total stock" of prior land reforms since collective land titling began in 1996. We instead rely on a running tally of reforms to proxy for the total stock in any given year.

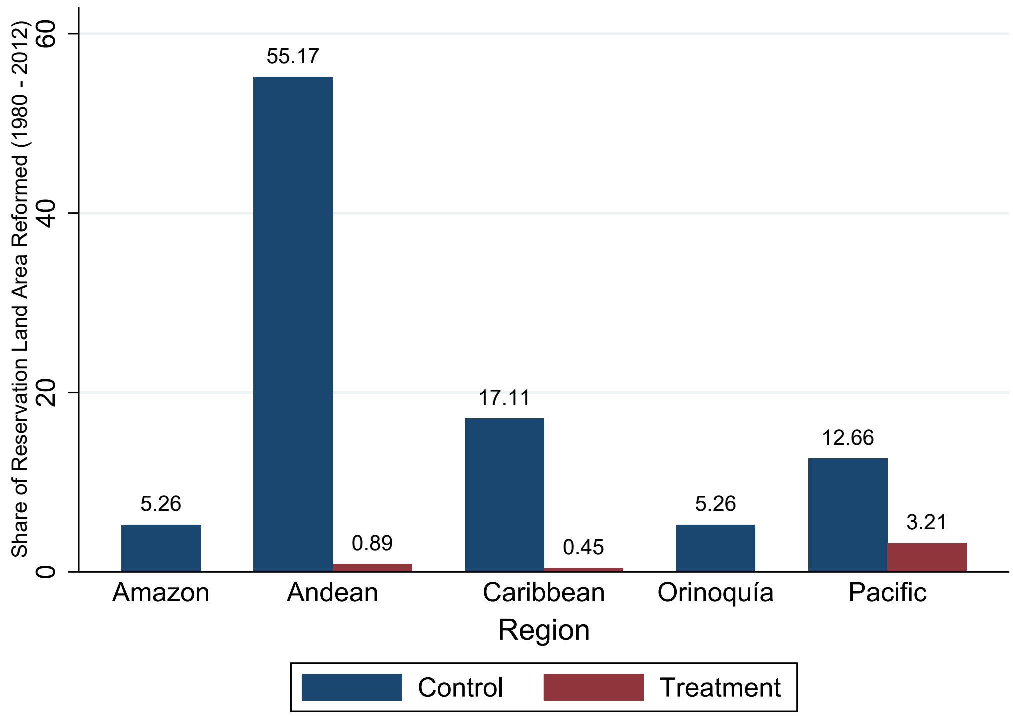


Figure A. 15: Indigenous Reservation Land Reform Resolutions (1980 -2012). *Authors' Calculations.*

increase in violence against indigenous groups can therefore be attributed to collective land tenure.

We test our argument in appendix table Table A8. We calculate the total indigenous territorial entity land area using OTEC data on the universe of indigenous land resolutions administered between 1953 and 2018. We also use the information on the timing of each new land resolution to construct a municipal-year indicator for the gradual phase-in of indigenous titles. We find no evidence of an increase in armed attacks against indigenous communities in the post-reform period. Comparing the coefficients on the indigenous reservation area variable to Afro-descendant titling, we observe little change in the magnitude or significance of either estimate. When we plot the residuals from our event-study, we again find no effect of indigenous titling on armed actor attacks. Fig. A16 in the appendix shows little change in government, paramilitary, or guerrilla attacks following assignment. Based on this evidence, we conclude that collective land tenure alone is an unlikely explanation for the increase in political violence against Afro-descendant communities.

Table A. 6: Prior INCORA Land Reforms

	All Attacks				Police & Army Attacks				Paramilitary Attacks				Guerrilla Attacks			
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]	[13]	[14]	[15]	[16]
Panel A: Intensive Margin																
Any Title (0/1) × Post (>1995)	0.213**	0.206*	0.211**	0.208*	0.008	-0.005	0.008	-0.004	0.187**	0.184*	0.184**	0.184*	0.073	0.074	0.071	0.074
	(0.098)	(0.107)	(0.096)	(0.107)	(0.040)	(0.044)	(0.039)	(0.044)	(0.094)	(0.100)	(0.093)	(0.099)	(0.052)	(0.052)	(0.052)	(0.052)
Prior Plots ('65 - '85)/1000 × Post	-0.012				-0.024				-0.004				0.001			
	(0.064)				(0.021)				(0.050)				(0.033)			
Rehabilitation zones × Post		-0.061				-0.037*				-0.051				-0.002		
		(0.054)				(0.021)				(0.045)				(0.037)		
Plots Reformed ('88 -'00)/1000 × Post			-0.006				-0.021				0.003				0.004	
			(0.057)				(0.019)				(0.046)				(0.028)	
Other tenancy × Post				0.004				0.042				-0.159*				0.152**
				(0.104)				(0.046)				(0.085)				(0.061)
Adj. R-squared	0.285	0.285	0.285	0.285	0.136	0.135	0.136	0.135	0.237	0.237	0.237	0.237	0.174	0.174	0.174	0.174
F-stat joint sig. (p-value)	.038	.03	.035	.175	.425	.533	.45	.477	.06	.034	.062	.009	.268	.234	.283	.317
Observations	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581
Clusters	957	957	957	957	957	957	957	957	957	957	957	957	957	957	957	957
Dependent Variable Mean	0.322	0.322	0.322	0.322	0.0575	0.0575	0.0575	0.0575	0.146	0.146	0.146	0.146	0.161	0.161	0.161	0.161
Panel B: Extensive Margin																
Any Title (0/1) × Post (>1995)	0.208*	0.206*	0.208*	0.208*	-0.004	-0.005	-0.004	-0.004	0.185*	0.184*	0.185*	0.185*	0.074	0.074	0.074	0.074
	(0.107)	(0.107)	(0.107)	(0.107)	(0.044)	(0.044)	(0.044)	(0.044)	(0.099)	(0.100)	(0.099)	(0.099)	(0.052)	(0.052)	(0.052)	(0.052)
Prior Plots'65 - '85 (0/1) × Post	-0.008				-0.007				-0.002				-0.001			
	(0.023)				(0.008)				(0.018)				(0.016)			
Rehabilitation zones (0/1) × Post		-0.061				-0.037*				-0.051				-0.002		
		(0.054)				(0.021)				(0.045)				(0.037)		
Plots Reformed (0/1)'88 -'00 × Post			-0.037				-0.010				0.000				-0.029	
			(0.025)				(0.009)				(0.017)				(0.019)	
Other tenancy (0/1) × Post				0.000				0.000				0.000				0.000
				(.)				(.)				(.)				(.)
Adj. R-squared	0.285	0.285	0.285	0.285	0.135	0.135	0.135	0.135	0.237	0.237	0.237	0.237	0.174	0.174	0.174	0.174
F-stat joint sig. (p-value)	.053	.03	.027	.053	.946	.533	.886	.929	.068	.034	.068	.063	.186	.234	.07	.158
Observations	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581
Clusters	957	957	957	957	957	957	957	957	957	957	957	957	957	957	957	957
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls × Post	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Department time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependent Variable Mean	0.322	0.322	0.322	0.322	0.0575	0.0575	0.0575	0.0575	0.146	0.146	0.146	0.146	0.161	0.161	0.161	0.161

Note: The unit of analysis is a municipality-year. Robust standard errors clustered at the municipality level are reported in parentheses. We apply the inverse hyperbolic sine (IHS) transformation to all the outcomes to correct for the skewed distribution in the number of attacks. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A. 7: Intensity of Afro-Descendant Land Titling Reforms

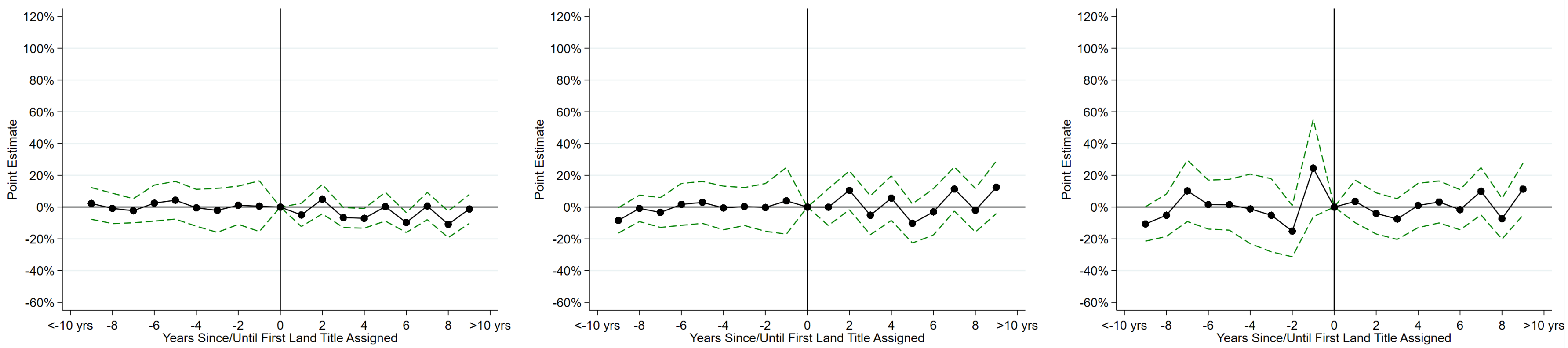
	All Attacks			Police & Army Attacks			Paramilitary Attacks			Guerrilla Attacks		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11	[12]
Panel A												
Total Collective Land Titles$_m$ $\times$ Post	0.048**	0.029*	0.033**	0.006**	0.005	0.007**	0.051***	0.039***	0.039***	0.008	-0.001	0.002
	(0.019)	(0.016)	(0.015)	(0.002)	(0.003)	(0.003)	(0.011)	(0.010)	(0.009)	(0.016)	(0.012)	(0.013)
Adj. R-squared	0.112	0.280	0.285	0.0446	0.132	0.136	0.0876	0.231	0.238	0.0443	0.171	0.174
Observations	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581
Clusters	957	957	957	957	957	957	957	957	957	957	957	957
Dependent Variable Mean	0.926	0.926	0.926	0.106	0.106	0.106	0.378	0.378	0.378	0.442	0.442	0.442
Panel B												
# Municipality-Year Land Titles	0.125**	0.078	0.077	-0.025	0.014	0.016	0.191***	0.119***	0.115***	0.006	-0.019	-0.017
	(0.061)	(0.055)	(0.056)	(0.018)	(0.014)	(0.014)	(0.043)	(0.042)	(0.044)	(0.037)	(0.032)	(0.033)
Adj. R-squared	0.101	0.278	0.283	0.0428	0.132	0.136	0.0656	0.225	0.233	0.0437	0.168	0.170
Observations	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581
Clusters	957	957	957	957	957	957	957	957	957	957	957	957
Department time trends	0.926	0.926	0.926	0.106	0.106	0.106	0.378	0.378	0.378	0.442	0.442	0.442
Panel C												
# Municipality-Year Land Titles $\times$ Land Title Tally$_t$	-0.005	-0.005	-0.004	0.005*	0.003*	0.003*	-0.014*	-0.009	-0.008	0.003	0.003	0.004
	(0.008)	(0.008)	(0.007)	(0.002)	(0.002)	(0.002)	(0.007)	(0.006)	(0.005)	(0.004)	(0.004)	(0.004)
# Municipality-Year Land Titles	0.085	0.063	0.056	-0.053	-0.020	-0.020	0.201**	0.143**	0.137*	-0.027	-0.058	-0.059
	(0.115)	(0.096)	(0.095)	(0.038)	(0.026)	(0.025)	(0.084)	(0.072)	(0.072)	(0.056)	(0.049)	(0.049)
Land Title Tally$_t$	0.038**	0.050**	0.051**	-0.009**	0.002	0.004	0.060***	0.054***	0.053***	-0.001	0.004	0.006
	(0.015)	(0.025)	(0.022)	(0.004)	(0.004)	(0.004)	(0.016)	(0.017)	(0.015)	(0.011)	(0.018)	(0.017)
Adj. R-squared	0.102	0.279	0.284	0.0430	0.132	0.136	0.0694	0.227	0.235	0.0437	0.168	0.170
Observations	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581
Clusters	957	957	957	957	957	957	957	957	957	957	957	957
Year F.E.	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Municipality F.E.	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls $\times$ Post(/Year)	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Department time trends	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Dependent Variable Mean	0.926	0.926	0.926	0.106	0.106	0.106	0.378	0.378	0.378	0.442	0.442	0.442

Note: The unit of analysis is a municipality-year. We apply the inverse hyperbolic sine (IHS) transformation to all the outcomes to correct for the skewed distribution in the number of attacks. Robust standard errors clustered at the municipality level are reported in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A. 8: Indigenous Reservations

	All Attacks			Police & Army Attacks			Paramilitary Attacks			Guerrilla Attacks		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
Panel A: Indigenous Reservations												
Indigenous Reservation Area × Post (>1995)	0.008	0.008	0.008	0.001	0.001	0.001	0.005	0.005	0.005	0.005	0.005	0.005
	(0.005)	(0.005)	(0.005)	(0.002)	(0.002)	(0.002)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
Panel B: Afro-descendent Communities												
Any Title (0/1) × Post (>1995)		0.210**			0.006			0.182**			0.074	
		(0.097)			(0.040)			(0.092)			(0.051)	
Afro-descendent Area × Post (>1995)			0.006*			-0.000			0.007**			0.001
			(0.003)			(0.001)			(0.003)			(0.002)
Adj. R-squared	0.284	0.285	0.285	0.136	0.136	0.136	0.237	0.237	0.237	0.174	0.174	0.174
Observations	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581
Clusters	957	957	957	957	957	957	957	957	957	957	957	957
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls × Post	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Department time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependent Variable Mean	0.322	0.322	0.322	0.0575	0.0575	0.0575	0.146	0.146	0.146	0.161	0.161	0.161

Note: The unit of analysis is a municipality-year. We apply the inverse hyperbolic sine (IHS) transformation to all the outcomes to correct for the skewed distribution in the number of attacks. Robust standard errors clustered at the municipality level are reported in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.



(a) Police and Army Attacks

(b) Paramilitary Attacks

(c) Guerrilla Attacks

Figure A. 16: Event-study Estimates – Indigenous Land Resolutions

Table A. 9: Alternative Measures of State Capacity

	All Attacks	Police & Army Attacks	Paramilitary Attacks	Guerrilla Attacks
	[1]	[2]	[3]	[4]
Panel A: National Public Institutions				
# National-level state agencies × Any title × Post	2.6163*	0.7347**	3.0549**	-0.0290
	(1.4360)	(0.3710)	(1.3322)	(1.0072)
# National state agencies × Post	-0.8203***	-0.3261**	-0.6625**	-0.1633
	(0.2698)	(0.1275)	(0.2835)	(0.1336)
Any title × Post	4.2151	-3.9010	-1.6384	7.7124
	(12.4472)	(4.4168)	(11.8306)	(7.8539)
Adj. R-squared	0.285	0.136	0.239	0.174
Observations	31581	31581	31581	31581
Clusters	957	957	957	957
Dependent Variable Mean	0.322	0.0575	0.146	0.161
Panel B: Local Public Institutions				
# Local-level state agencies × Any title × Post	0.4886**	0.1453**	0.7217***	-0.1765
	(0.2259)	(0.0612)	(0.2022)	(0.1213)
# Local state agencies × Post	-0.1929**	-0.0918*	-0.1498	-0.0565
	(0.0931)	(0.0551)	(0.1238)	(0.0417)
Any title × Post	11.3325	-2.0692	3.4869	11.3452*
	(10.2614)	(4.0439)	(9.5759)	(5.7906)
Adj. R-squared	0.285	0.136	0.238	0.174
Observations	31581	31581	31581	31581
Clusters	957	957	957	957
Dependent Variable Mean	0.322	0.0575	0.146	0.161
Panel C: National Public Employees				
# National Public Employees$_{\times 100}$ × Any title × Post	0.011*	0.002*	0.014***	-0.001
	(0.006)	(0.001)	(0.005)	(0.004)
# National Public Employees$_{\times 100}$ × Post	-0.000	0.001**	-0.002	0.000
	(0.001)	(0.001)	(0.002)	(0.000)
Any title × Post	10.411	-1.812	4.655	8.396
	(10.190)	(3.792)	(9.922)	(5.905)
Adj. R-squared	0.285	0.136	0.239	0.174
Observations	31581	31581	31581	31581
Clusters	957	957	957	957
Dependent Variable Mean	0.322	0.0575	0.146	0.161
Panel D: Municipal Employees				
# Municipal Public Employees$_{\times 100}$ × Any title × Post	0.045*	0.004	0.088***	-0.029*
	(0.027)	(0.009)	(0.025)	(0.016)
# Municipal Public Employees$_{\times 100}$ × Post	-0.020**	0.011*	-0.038***	0.002
	(0.009)	(0.006)	(0.012)	(0.004)
Any title × Post	17.443*	-0.153	11.103	10.333*
	(9.423)	(3.791)	(9.129)	(5.406)
Adj. R-squared	0.285	0.136	0.240	0.174
Observations	31581	31581	31581	31581
Clusters	957	957	957	957
Year F.E.	Yes	Yes	Yes	Yes
Municipality F.E.	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Controls × Post	Yes	Yes	Yes	Yes
Department time trends	Yes	Yes	Yes	Yes
Dependent Variable Mean	0.322	0.0575	0.146	0.161

Note: The unit of analysis is a municipality-year. We apply the inverse hyperbolic sine (IHS) transformation to all the outcomes to correct for the skewed distribution in the number of attacks. National-level employees includes judges, judicial employees, and police officials. Local-level employees includes notary, telecom, health, school, library, fire station, jail, deed registry, and tax collection officials. Robust standard errors clustered at the municipality level are reported in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A. 10: Quality of Governance

	All Attacks				Police & Army Attacks				Paramilitary Attacks				Guerrilla Attacks			
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]	[13]	[14]	[15]	[16]
Panel A: Governance Indicators																
Competence Index × Any title × Post	0.9579				0.4698				0.6321				0.3022			
	(0.8454)				(0.4079)				(0.7908)				(0.4255)			
Effectiveness Index × Any title × Post		-0.2694				0.1183				-0.5679*				0.0468		
		(0.3187)				(0.1127)				(0.3048)				(0.2066)		
Efficiency Index × Any title × Post			0.2642				0.3507				-0.0698				0.2048	
			(0.7040)				(0.3096)				(0.7232)				(0.4036)	
Management Index × Any title × Post				1.9838				0.2137				1.5040				1.2775
				(1.3272)				(0.4179)				(1.2967)				(0.8426)
Adj. R-squared	0.285	0.285	0.285	0.285	0.136	0.136	0.136	0.136	0.237	0.238	0.237	0.237	0.174	0.174	0.174	0.174
Observations	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581
Clusters	957	957	957	957	957	957	957	957	957	957	957	957	957	957	957	957
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls × Post	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Department time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependent Variable Mean	0.323	0.323	0.323	0.323	0.0575	0.0575	0.0575	0.0575	0.146	0.146	0.146	0.146	0.161	0.161	0.161	0.161

Note: The unit of analysis is a municipality-year. We apply the inverse hyperbolic sine (IHS) transformation to all the outcomes to correct for the skewed distribution in the number of attacks. Robust standard errors clustered at the municipality level are reported in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A. 11: Land Titling and Elections

	All Attacks	Police & Army Attacks	Paramilitary Attacks	Guerrilla Attacks
	[1]	[2]	[3]	[4]
Panel A: Presidential Elections				
Vote share conservative candidates ('94) × Any title × Post	2.848*	1.094*	2.855**	0.058
	(1.668)	(0.578)	(1.335)	(0.721)
Vote share conservative candidates ('94) × Post (>1995)	0.053	0.015	0.008	0.045
	(0.051)	(0.020)	(0.036)	(0.037)
Any Title (0/1) × Post (>1995)	-0.668	-0.295	-0.625	-0.055
	(0.550)	(0.207)	(0.443)	(0.236)
Vote share left-leaning candidates ('94) × Any title × Post	1.423	-0.386	0.642	2.069*
	(2.201)	(1.239)	(3.097)	(1.119)
Vote share left-leaning candidates ('94) × Post (>1995)	-0.431	-0.149	-0.252	-0.101
	(0.285)	(0.139)	(0.213)	(0.183)
Adjusted R^2	0.269	0.114	0.223	0.169
Panel B: Mayoral Elections (Vote Share)				
Vote share conservative party (1994) × Any title × Post	0.437**	0.108*	0.378**	-0.021
	(0.194)	(0.064)	(0.174)	(0.083)
Vote share conservative party (1994) × Post (>1995)	-0.028	-0.003	-0.028	-0.002
	(0.030)	(0.011)	(0.023)	(0.020)
Any Title (0/1) × Post (>1995)	-0.098	0.001	-0.119	-0.020
	(0.135)	(0.045)	(0.092)	(0.078)
Vote share liberal party (1994) × Any title × Post	-0.136	-0.089	-0.067	0.095
	(0.183)	(0.063)	(0.186)	(0.071)
Vote share liberal party (1994) × Post (>1995)	-0.011	-0.007	0.016	-0.027*
	(0.023)	(0.009)	(0.018)	(0.016)
Vote share left party (1994) × Any title × Post	0.445	-0.228	0.421	0.317*
	(0.367)	(0.169)	(0.359)	(0.184)
Vote share left party (1994) × Post (>1995)	0.277	0.178**	0.173	0.067
	(0.182)	(0.090)	(0.172)	(0.096)
Adj. R-squared	0.286	0.137	0.239	0.175
Observations	31284	31284	31284	31284
Clusters	948	948	948	948
Dependent Variable Mean	0.323	0.0577	0.147	0.161
Panel B: Mayoral Elections (Win/Loss)				
Conservative party win/loss (1994) × Any title × Post	0.520***	0.139**	0.416**	0.030
	(0.185)	(0.061)	(0.170)	(0.075)
Conservative party win/loss (1994) × Post (>1995)	-0.026	-0.000	-0.027	-0.003
	(0.030)	(0.012)	(0.023)	(0.020)
Any Title (0/1) × Post (>1995)	-0.182	-0.033	-0.159*	-0.070
	(0.128)	(0.039)	(0.089)	(0.072)
Liberal party win/loss (1994) × Any title × Post	-0.145	-0.094	-0.084	0.103
	(0.186)	(0.068)	(0.189)	(0.070)
Liberal party win/loss (1994) × Post (>1995)	-0.010	-0.010	0.015	-0.022
	(0.023)	(0.009)	(0.018)	(0.016)
Left party win/loss (1994) × Any title × Post	0.666*	-0.095	0.569	0.349*
	(0.346)	(0.145)	(0.352)	(0.194)
Left party win/loss (1994) × Post (>1995)	0.255	0.162**	0.186	0.040
	(0.191)	(0.081)	(0.204)	(0.088)
Adj. R-squared	0.286	0.137	0.239	0.175
Observations	31284	31284	31284	31284
Clusters	948	948	948	948
Year F.E.	Yes	Yes	Yes	Yes
Municipality F.E.	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Controls × Post	Yes	Yes	Yes	Yes
Department time trends	Yes	Yes	Yes	Yes
Dependent Variable Mean	0.323	0.0577	0.147	0.161

Note: The unit of analysis is a municipality-year. We apply the inverse hyperbolic sine (IHS) transformation to all the outcomes to correct for the skewed distribution in the number of attacks. Robust standard errors clustered at the municipality level are reported in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A. 12: State Institutions and Prior Peasant Land Titling Reforms

	All Attacks	Police & Army Attacks	Paramilitary Attacks	Guerrilla Attacks
	[1]	[2]	[3]	[4]
Panel A: Judiciary				
# Courts × Plots Reformed (0/1) × Post	-0.011	-0.007***	-0.015*	0.004
	(0.008)	(0.003)	(0.009)	(0.004)
# National Courts × Post	0.004	0.000	0.014*	-0.008**
	(0.008)	(0.002)	(0.008)	(0.003)
Plots Reformed (0/1) × Post	0.000	0.005	0.021	-0.016
	(0.027)	(0.009)	(0.023)	(0.017)
Adj. R-squared	0.286	0.138	0.238	0.175
Observations	31053	31053	31053	31053
Clusters	941	941	941	941
Dependent Variable Mean	0.320	0.0572	0.144	0.162
Panel B: Law Enforcement				
# Police posts & inspections × Prior Plots Reformed (0/1) × Post	-0.020*	-0.002	-0.053***	0.029***
	(0.012)	(0.007)	(0.012)	(0.009)
# Police posts & inspections × Post	-0.013	0.000	0.007	-0.029***
	(0.014)	(0.007)	(0.015)	(0.010)
Plots Reformed (0/1) × Post	0.020	-0.002	0.087***	-0.055**
	(0.032)	(0.014)	(0.028)	(0.022)
Adj. R-squared	0.287	0.137	0.242	0.175
Observations	31020	31020	31020	31020
Clusters	940	940	940	940
Dependent Variable Mean	0.321	0.0573	0.144	0.162
Panel C: Hospitals & Ag. Banks				
# National Hospitals × Prior Plots Reformed (0/1) × Post	-0.120***	-0.008	-0.162***	-0.001
	(0.036)	(0.015)	(0.029)	(0.029)
Adj. R-squared	0.286	0.137	0.241	0.174
Observations	30987	30987	30987	30987
Clusters	939	939	939	939
Dependent Variable Mean	0.321	0.0573	0.144	0.162
# Agriculture Bank Offices × Plots Reformed (0/1) × Post	-0.090***	0.002	-0.083***	-0.038*
	(0.030)	(0.013)	(0.024)	(0.021)
Adj. R-squared	0.287	0.138	0.246	0.175
Observations	31053	31053	31053	31053
Clusters	941	941	941	941
Dependent Variable Mean	0.320	0.0572	0.144	0.162
Year F.E.	Yes	Yes	Yes	Yes
Municipality F.E.	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Controls × Post	Yes	Yes	Yes	Yes
Department time trends	Yes	Yes	Yes	Yes

Note: The unit of analysis is a municipality-year. We apply the inverse hyperbolic sine (IHS) transformation to all the outcomes to correct for the skewed distribution in the number of attacks. Robust standard errors clustered at the municipality level are reported in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A. 13: Party Institutionalization, Reforms, and Political Violence

	All Attacks			Police & Army Attacks			Paramilitary Attacks			Guerrilla Attacks		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
Panel A: Afro-descendant Land Titles												
# of Parties (NP)'94 × Title × Post	0.191			0.079**			0.184*			0.013		
	(0.129)			(0.034)			(0.102)			(0.081)		
Effective # of Parties (N)'94 × Title × Post		0.802***			0.194**			0.721***			0.207	
		(0.158)			(0.083)			(0.142)			(0.130)	
Hyperfractionalization (I)'94 × Title × Post			0.180			0.064**			0.182*			0.011
			(0.128)			(0.029)			(0.100)			(0.079)
Adj. R-squared	0.286	0.288	0.287	0.137	0.137	0.137	0.242	0.244	0.243	0.174	0.174	0.174
Observations	31284	31284	31284	31284	31284	31284	31284	31284	31284	31284	31284	31284
Clusters	948	948	948	948	948	948	948	948	948	948	948	948
Dependent Variable Mean	0.323	0.323	0.323	0.0577	0.0577	0.0577	0.147	0.147	0.147	0.161	0.161	0.161
Panel B: Plots Reformed (1960 - 1985)												
# of Parties (NP)'94 × Plots Reformed (0/1) × Post	-0.017			-0.121***			0.036			-0.030		
	(0.145)			(0.040)			(0.140)			(0.088)		
Effective # of Parties (N)'94 × Plots Reformed (0/1) × Post		-0.115			-0.187***			0.010			-0.086	
		(0.158)			(0.059)			(0.195)			(0.116)	
Hyperfractionalization (I)'94 × Plots Reformed (0/1) × Post			-0.050			-0.096***			0.010			-0.081
			(0.085)			(0.027)			(0.083)			(0.080)
Adj. R-squared	0.286	0.286	0.286	0.137	0.137	0.137	0.241	0.241	0.242	0.174	0.174	0.174
Observations	31284	31284	31284	31284	31284	31284	31284	31284	31284	31284	31284	31284
Clusters	948	948	948	948	948	948	948	948	948	948	948	948
Dependent Variable Mean	0.323	0.323	0.323	0.0577	0.0577	0.0577	0.147	0.147	0.147	0.161	0.161	0.161
Panel C: Plots Reformed (1988 - 2000)												
# of Parties (NP)'94 × Plots Reformed (0/1) × Post	-0.103			-0.102***			0.028			-0.021		
	(0.072)			(0.037)			(0.078)			(0.044)		
Effective # of Parties (N)'94 × Plots Reformed (0/1) × Post		-0.010			-0.143**			0.174*			-0.030	
		(0.098)			(0.055)			(0.102)			(0.053)	
Hyperfractionalization (I)'94 × Plots Reformed (0/1) × Post			-0.039			-0.077***			0.071			-0.013
			(0.066)			(0.030)			(0.072)			(0.038)
Adj. R-squared	0.286	0.286	0.286	0.137	0.137	0.137	0.241	0.242	0.242	0.174	0.174	0.174
Observations	31284	31284	31284	31284	31284	31284	31284	31284	31284	31284	31284	31284
Clusters	948	948	948	948	948	948	948	948	948	948	948	948
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls × Post	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Department time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependent Variable Mean	0.323	0.323	0.323	0.0577	0.0577	0.0577	0.147	0.147	0.147	0.161	0.161	0.161

Note: The unit of analysis is an election race-year. We apply the inverse hyperbolic sine (IHS) transformation to all the outcomes to correct for the skewed distribution in the number of attacks. Robust standard errors clustered at the municipality level are reported in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A. 14: Prior Land Reforms and Counter-reform Violence

	Government Attacks		Paramilitary Attacks		Guerilla Attacks	
	[1]	[2]	[3]	[4]	[5]	[6]
Panel A: Left-wing races (1997 - 2011)						
Plot Reformed 1965 -1980 (0/1) × Left-wing Mayor Elected	-0.127	-0.532	-0.573	-1.905	1.242	0.899
	(2.020)	(2.848)	(0.816)	(1.173)	(1.910)	(1.523)
Adj. R-squared	0.449	0.644	0.790	0.864	0.575	0.681
Observations	143	143	143	143	143	143
Dependent Variable Mean	0.564	0.564	0.564	0.564	0.564	0.564
Panel B: Right-wing Races (1997 - 2011)						
Plots Reformed ’88 - ’00 (0/1) × Right-wing Mayor Win	-0.268	-0.262	0.062	0.063	-0.032	-0.029
	(0.701)	(0.641)	(0.527)	(0.513)	(0.898)	(0.907)
Adj. R-squared	0.507	0.515	0.373	0.362	0.582	0.577
Observations	456	456	456	456	456	456
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
All Controls	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial	Linear	Quad.	Linear	Quad.	Linear	Quad.
Dependent Variable Mean	0.265	0.212	0.212	0.212	0.265	0.212

Note: The unit of analysis is an election race-year. We apply the inverse hyperbolic sine (IHS) transformation to all the outcomes to correct for the skewed distribution in the number of attacks. Robust standard errors clustered at the municipality level are reported in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A. 15: Land Inequality

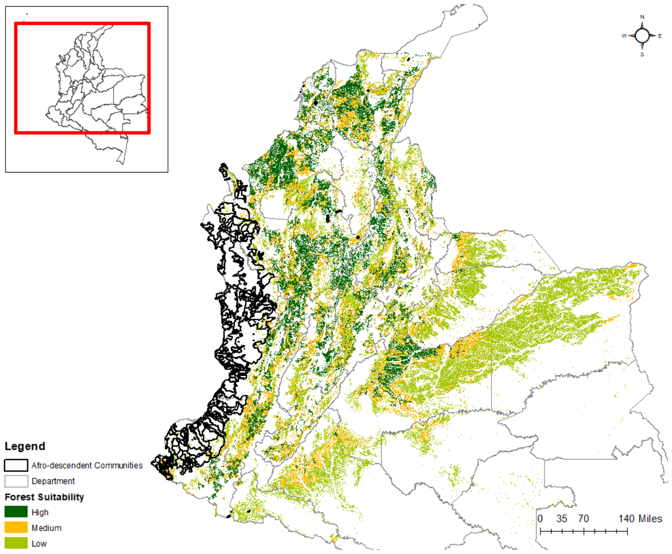
	All Attacks			Police & Army Attacks			Paramilitary Attacks			Guerrilla Attacks		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
Panel A: Land Gini												
Land gini (1993) × Post (>1995)	0.236	0.184	0.182	-0.095	-0.097	-0.087	-0.247	-0.295*	-0.294*	0.551***	0.536***	0.524***
	(0.214)	(0.210)	(0.209)	(0.095)	(0.095)	(0.094)	(0.173)	(0.167)	(0.165)	(0.174)	(0.174)	(0.174)
Any Title (0/1) × Post (>1995)		0.209**	0.185		0.010	0.193**		0.194**	0.216		0.061	-0.154
		(0.097)	(0.263)		(0.039)	(0.092)		(0.093)	(0.258)		(0.051)	(0.178)
Any title × Land gini (1993) × Post (>1995)			0.056			-0.431*			-0.051			0.505
			(0.702)			(0.254)			(0.664)			(0.426)
Adj. R-squared	0.284	0.285	0.285	0.136	0.136	0.136	0.236	0.237	0.237	0.174	0.174	0.174
Observations	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581
Clusters	957	957	957	957	957	957	957	957	957	957	957	957
Dependent Variable Mean	0.322	0.322	0.322	0.0575	0.0575	0.0575	0.146	0.146	0.146	0.161	0.161	0.161
Panel B: Land Value												
Land value (2000) × Post (>1995)	-0.003	-0.003*	-0.005**	-0.000	-0.000	-0.000	-0.002*	-0.003**	-0.002*	-0.001	-0.001	-0.003***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
Any Title (0/1) × Post (>1995)		0.298***	0.274***		0.059**	0.061**		0.267***	0.272***		0.036	0.010
		(0.096)	(0.100)		(0.030)	(0.030)		(0.097)	(0.101)		(0.054)	(0.054)
Any title × Land Value (2000) × Post (>1995)			0.011***			-0.001			-0.002			0.012***
			(0.004)			(0.001)			(0.004)			(0.002)
Adj. R-squared	0.275	0.276	0.276	0.127	0.127	0.127	0.238	0.239	0.239	0.164	0.164	0.164
Observations	27423	27423	27423	27423	27423	27423	27423	27423	27423	27423	27423	27423
Clusters	831	831	831	831	831	831	831	831	831	831	831	831
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls × Post	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Department time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependent Variable Mean	0.287	0.287	0.287	0.0525	0.0525	0.0525	0.125	0.125	0.125	0.144	0.144	0.144

Note: The unit of analysis is a municipality-year. We apply the inverse hyperbolic sine (IHS) transformation to all the outcomes to correct for the skewed distribution in the number of attacks. Robust standard errors clustered at the municipality level are reported in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

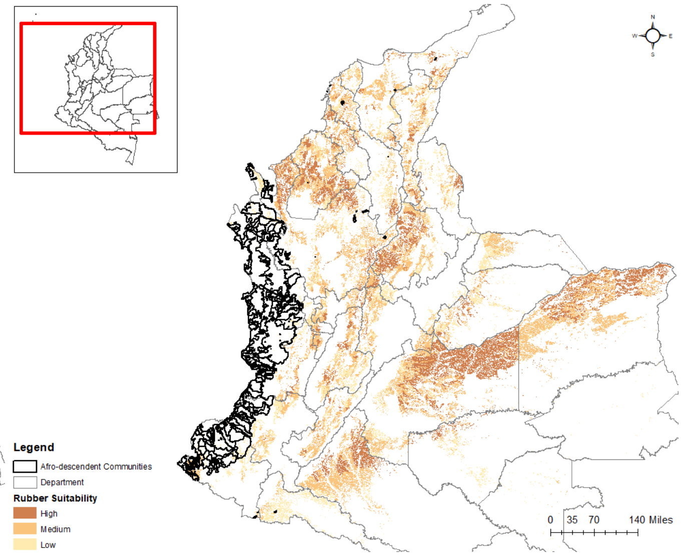
Table A. 16: Quality of Municipal Land

	All Attacks				Police & Army Attacks				Paramilitary Attacks				Guerrilla Attacks			
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]	[13]	[14]	[15]	[16]
Panel A: Share of high quality land																
Share Quality 1 × Title × Post	0.000				0.000				0.000				0.000			
	(.)				(.)				(.)				(.)			
Share Quality 2 × Title × Post		1.151				7.068***				-8.130**				3.079		
		(3.303)				(1.137)				(3.174)				(1.894)		
Share Quality 3 × Title × Post			-1.438*				-0.653				-0.377				-0.601*	
			(0.772)				(0.462)				(0.645)				(0.361)	
Share Quality 4 × Title × Post				0.034				-0.246				0.257				0.199
				(0.395)				(0.176)				(0.458)				(0.283)
Adj. R-squared	0.286	0.286	0.286	0.286	0.136	0.137	0.137	0.137	0.239	0.239	0.239	0.239	0.174	0.174	0.174	0.174
Observations	31218	31218	31218	31218	31218	31218	31218	31218	31218	31218	31218	31218	31218	31218	31218	31218
Clusters	946	946	946	946	946	946	946	946	946	946	946	946	946	946	946	946
Dependent Variable Mean	0.320	0.320	0.320	0.320	0.0574	0.0574	0.0574	0.0574	0.145	0.145	0.145	0.145	0.160	0.160	0.160	0.160
Panel B: Share of low quality land																
Share Quality 5 × Title × Post	-0.040				-0.087				-0.367				0.345			
	(0.556)				(0.165)				(0.490)				(0.314)			
Share Quality 6 × Title × Post		-1.702				-0.640				-1.178				-0.185		
		(1.041)				(0.477)				(0.957)				(0.337)		
ShareQuality 7 × Title × Post			0.464				0.202				0.378				-0.056	
			(0.400)				(0.141)				(0.373)				(0.191)	
Share Quality 8 × Title × Post				-0.009				0.099				-0.131				-0.010
				(0.273)				(0.086)				(0.246)				(0.185)
Adj. R-squared	0.286	0.287	0.286	0.286	0.136	0.137	0.137	0.136	0.239	0.240	0.239	0.239	0.174	0.174	0.174	0.174
Observations	31218	31218	31218	31218	31218	31218	31218	31218	31218	31218	31218	31218	31218	31218	31218	31218
Clusters	946	946	946	946	946	946	946	946	946	946	946	946	946	946	946	946
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls × Post	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Department time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependent Variable Mean	0.320	0.320	0.320	0.320	0.0574	0.0574	0.0574	0.0574	0.145	0.145	0.145	0.145	0.160	0.160	0.160	0.160

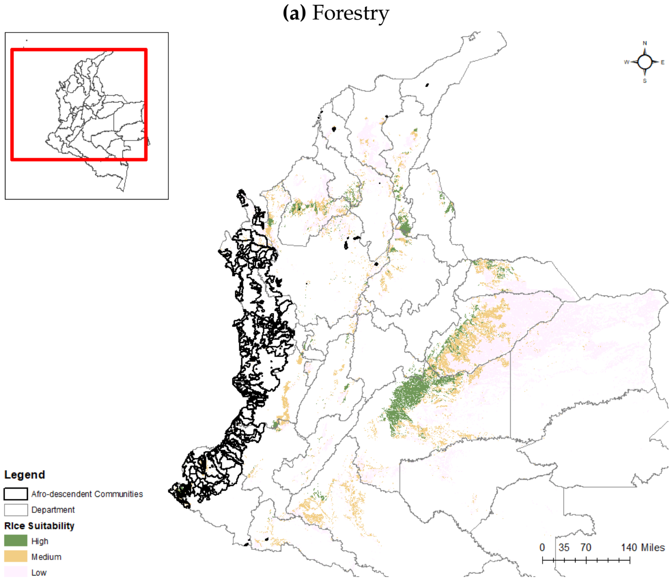
Note: The unit of analysis is a municipality-year. We apply the inverse hyperbolic sine (IHS) transformation to all the outcomes to correct for the skewed distribution in the number of attacks. Robust standard errors clustered at the municipality level are reported in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.



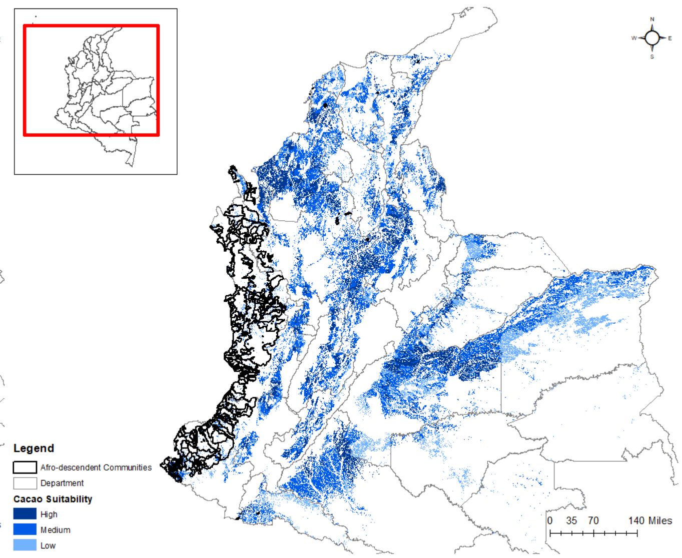
(a) Forestry



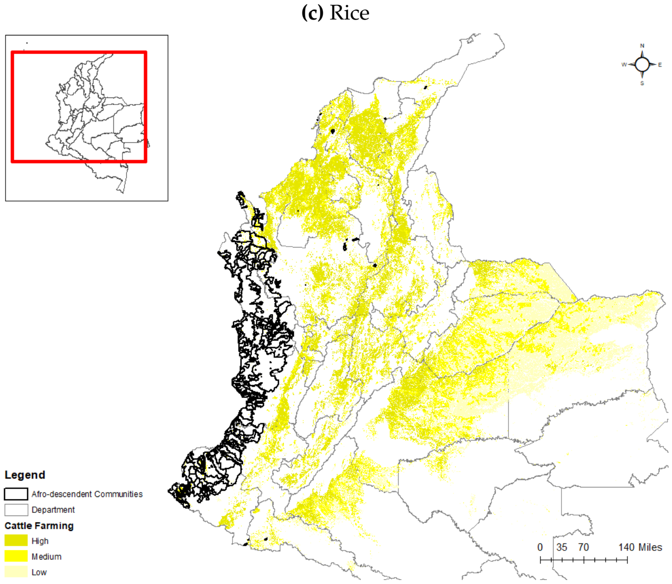
(b) Rubber



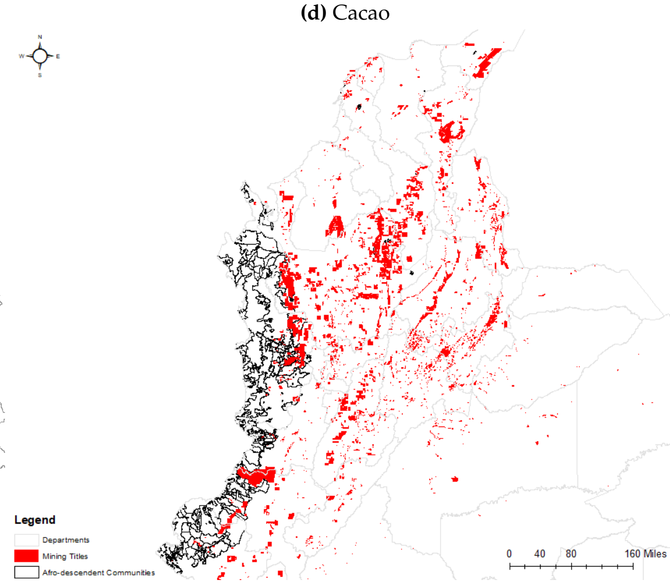
(c) Rice



(d) Cacao



(e) Cattle



(f) Mining Titles

Figure A. 17: Lootable Resources and Extractive Industry Maps (OTEC)

Table A. 17: Lootable Resources, Extractive Industries, and Land Use

	All Attacks	Police & Army Attacks	Paramilitary Attacks	Guerrilla Attacks
	[1]	[2]	[3]	[4]
Panel A: Lootable Resources				
Rubber Suitable Area × Any title × Post	0.436***	0.084	0.276	0.206***
	(0.158)	(0.052)	(0.179)	(0.073)
Forest Suitable Area × Any title × Post	-0.016	-0.020	-0.035	0.011
	(0.175)	(0.066)	(0.170)	(0.097)
Cacao Suitable Area × Any title × Post	-0.240	-0.118*	-0.258	0.042
	(0.174)	(0.064)	(0.165)	(0.092)
Rice Suitable Area × Any title × Post	-0.015	-0.083***	0.014	0.018
	(0.060)	(0.028)	(0.049)	(0.026)
Coffee Cultivated Area × Any title × Post	0.066	0.141	-0.050	0.011
	(0.341)	(0.125)	(0.301)	(0.207)
Coca Cultivated Area × Any title × Post	-0.039	0.011	-0.051	-0.012
	(0.045)	(0.015)	(0.038)	(0.027)
Panel B: Extractive Industries				
Mining Title Area × Any title × Post	-0.058***	0.005	-0.041**	-0.026*
	(0.021)	(0.009)	(0.018)	(0.015)
Oil & Gas Exploration Area × Any title × Post	-0.045*	0.009	-0.037*	-0.027
	(0.024)	(0.012)	(0.021)	(0.017)
Panel C: Land Use				
Cattle Farming Area × Any title × Post	-0.067	-0.019	-0.029	-0.048
	(0.140)	(0.048)	(0.114)	(0.086)
Poultry Farming Area × Any title × Post	-0.217	0.081	-0.161	-0.243***
	(0.144)	(0.067)	(0.153)	(0.081)
Adj. R-squared	0.297	0.133	0.239	0.205
F-stat joint sig.	.0013	.0001	.2792	0
Observations	20757	20757	20757	20757
Clusters	903	903	903	903
Year F.E.	Yes	Yes	Yes	Yes
Municipality F.E.	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Controls × Post	Yes	Yes	Yes	Yes
Department time trends	Yes	Yes	Yes	Yes
Dependent Variable Mean	0.383	0.0561	0.161	0.213

Note: The unit of analysis is a municipality-year. We apply the inverse hyperbolic sine (IHS) transformation to all the outcomes to correct for the skewed distribution in the number of attacks. Robust standard errors clustered at the municipality level are reported in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A. 18: Resources, Extractive Industries, & Land Use (*Principal Component Analysis*)

	All Attacks	Police & Army Attacks	Paramilitary Attacks	Guerrilla Attacks
	[1]	[2]	[3]	[4]
Panel A: Lootable Resources				
Component 1 × Any title × Post	0.116***	-0.001	0.023	0.090***
	(0.042)	(0.011)	(0.034)	(0.026)
Adj. R-squared	0.294	0.130	0.234	0.203
Observations	20757	20757	20757	20757
Clusters	903	903	903	903
Dependent Variable Mean	0.383	0.0561	0.161	0.213
Panel B: Extractive Industries				
Component 1 × Any title × Post	0.017	0.001	-0.003	0.025
	(0.023)	(0.011)	(0.030)	(0.018)
Adj. R-squared	0.285	0.136	0.237	0.174
Observations	31581	31581	31581	31581
Clusters	957	957	957	957
Dependent Variable Mean	0.322	0.0575	0.146	0.161
Panel C: Land Use				
Component 1 × Any title × Post	-0.203	-0.045	-0.374**	-0.012
	(0.223)	(0.062)	(0.172)	(0.151)
Adj. R-squared	0.285	0.136	0.238	0.174
Observations	31581	31581	31581	31581
Clusters	957	957	957	957
Year F.E.	Yes	Yes	Yes	Yes
Municipality F.E.	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Controls × Post	Yes	Yes	Yes	Yes
Department time trends	Yes	Yes	Yes	Yes
Dependent Variable Mean	0.322	0.0575	0.146	0.161

Note: The unit of analysis is a municipality-year. We apply the inverse hyperbolic sine (IHS) transformation to all the outcomes to correct for the skewed distribution in the number of attacks. Robust standard errors clustered at the municipality level are reported in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A. 19: Forest Reserve Zones

	All Attacks	Police & Army Attacks	Paramilitary Attacks	Guerrilla Attacks
	[1]	[2]	[3]	[4]
Panel A: Pacific ZRF				
Pacific Forest Reserve Area × Any title × Post	0.259	0.052	0.103	0.106
	(0.197)	(0.087)	(0.186)	(0.112)
Adj. R-squared	0.285	0.136	0.237	0.174
Observations	31581	31581	31581	31581
Clusters	957	957	957	957
Dependent Variable Mean	0.322	0.0575	0.146	0.161
Panel A: Amazon ZRF				
Amazon Forest Reserve Area × Any title × Post	-0.145	0.083	0.030	-0.267**
	(0.186)	(0.075)	(0.166)	(0.108)
Adj. R-squared	0.285	0.136	0.237	0.174
Observations	31581	31581	31581	31581
Clusters	957	957	957	957
Dependent Variable Mean	0.322	0.0575	0.146	0.161
Panel C: Pacific & Amazon ZRF				
Pacific Forest Reserve Area × Any title × Post	0.281	0.072	0.107	0.102
	(0.204)	(0.091)	(0.196)	(0.118)
Amazon Forest Reserve Area × Any title × Post	0.055	0.128	0.116	-0.197
	(0.213)	(0.104)	(0.196)	(0.133)
Adj. R-squared	0.285	0.136	0.237	0.174
Observations	31581	31581	31581	31581
Clusters	957	957	957	957
Dependent Variable Mean	0.322	0.0575	0.146	0.161
Panel D: Reserve Zones & Protected Areas				
Pacific Forest Reserve Area × Any title × Post	0.329*	0.082	0.111	0.141
	(0.192)	(0.082)	(0.174)	(0.120)
Amazon Forest Reserve Area × Any title × Post	0.134	0.131	0.116	-0.136
	(0.255)	(0.103)	(0.230)	(0.161)
Protected Area × Any title × Post	-0.012	-0.002	-0.001	-0.009
	(0.016)	(0.005)	(0.014)	(0.009)
Adj. R-squared	0.285	0.136	0.237	0.174
Observations	31581	31581	31581	31581
Clusters	957	957	957	957
Year F.E.	Yes	Yes	Yes	Yes
Municipality F.E.	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Controls × Post	Yes	Yes	Yes	Yes
Department time trends	Yes	Yes	Yes	Yes
Dependent Variable Mean	0.322	0.0575	0.146	0.161

Note: The unit of analysis is a municipality-year. We apply the inverse hyperbolic sine (IHS) transformation to all the outcomes to correct for the skewed distribution in the number of attacks. Robust standard errors clustered at the municipality level are reported in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A. 20: Extractive and Commercial Industries: Oil, Coal, Gold, Precious Metal Mining, and Coffee

	All Attacks					Police & Army Attacks					Paramilitary Attacks					Guerrilla Attacks				
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]	[13]	[14]	[15]	[16]	[17]	[18]	[19]	[20]
Panel A																				
Oil'88 × Title × Post	3.046					-1.021					3.959					1.846				
	(2.201)					(1.489)					(2.749)					(1.903)				
Coal'78 × Title × Post		-0.593					-0.307					-0.357					-0.193*			
		(0.588)					(0.218)					(0.597)					(0.104)			
Gold'04 × Title × Post			-0.016***					-0.002					-0.012*					-0.001		
			(0.006)					(0.003)					(0.006)					(0.005)		
Mining'78 × Title × Post				0.000					0.000					0.000					-0.000	
				(0.000)					(0.000)					(0.000)					(0.000)	
Coffee'97 × Title × Post					-0.040					0.103*					-0.094					-0.027
					(0.119)					(0.053)					(0.115)					(0.067)
Adj. R-squared	0.264	0.265	0.264	0.264	0.266	0.114	0.114	0.114	0.114	0.111	0.214	0.214	0.214	0.215	0.217	0.168	0.168	0.168	0.168	0.169
Observations	30426	30426	30426	30426	29799	30426	30426	30426	30426	29799	30426	30426	30426	30426	29799	30426	30426	30426	30426	29799
Clusters	922	922	922	922	903	922	922	922	922	903	922	922	922	922	903	922	922	922	922	903
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls × Post	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Department time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependent Variable Mean	0.303	0.303	0.303	0.303	0.299	0.0515	0.0515	0.0515	0.0515	0.0487	0.133	0.133	0.133	0.133	0.133	0.156	0.156	0.156	0.156	0.154

Note: The unit of analysis is a municipality-year. We apply the inverse hyperbolic sine (IHS) transformation to all the outcomes to correct for the skewed distribution in the number of attacks. Robust standard errors clustered at the municipality level are reported in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A. 21: Titling and Commodity Price Shocks

	Coffee'97 × log price	Oil'88 × log price	Gold'04 × log price	Coal'04 × log price	Mining'78 × log gold price	Mining'78 × log silver price	Mining'78 × log plat. price
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Panel A							
Any Title (0/1) × Post (>1995)	0.209***	0.003	-0.262	-0.091	129.370	42.492	-36.684
	(0.059)	(0.002)	(0.612)	(0.340)	(717.947)	(241.633)	(51.105)
Adj. R-squared	0.904	0.992	0.144	0.986	0.166	0.997	0.544
Observations	16254	16596	16596	16596	16596	16596	16596
Clusters	903	922	922	922	922	922	922
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls × Post	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Department time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependent Variable Mean	0.572	0.0139	0.0359	-4.379	54.65	-2373.3	217.4

Note: The unit of analysis is a municipality-year. Robust standard errors clustered at the municipality level are reported in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A. 22: Property Taxes and Revenues

	Total Revenue	Current Revenue	Tax Revenue	Property tax Rev. p.c.	Total Royalties	Total Trasnfers
	[1]	[2]	[3]	[4]	[5]	[6]
Panel A						
Any Title (0/1) × Post (>1995)	-0.017	-0.013	-0.155	0.162	0.147	-0.257
	(0.066)	(0.086)	(0.242)	(0.183)	(0.405)	(0.224)
Adj. R-squared	0.940	0.910	0.729	0.736	0.553	0.321
Observations	22730	22729	23917	26594	23917	23543
Clusters	957	957	957	957	957	957
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Municipality F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Controls × Post	Yes	Yes	Yes	Yes	Yes	Yes
Department time trends	Yes	Yes	Yes	Yes	Yes	Yes
Dependent Variable Mean	18694.0	9172.8	5829.1	1711.1	666.5	1471.3

Note: The unit of analysis is a municipality-year. We apply the inverse hyperbolic sine (IHS) transformation to all the outcomes to correct for the skewed distribution in the dependent variables. Robust standard errors clustered at the municipality level are reported in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A. 23: Coca Presence

	All Attacks			Policy & Army Attacks			Paramilitary Attacks			Guerrilla Attacks		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
Panel A: Titling & Coca Presence												
Coca (0/1) × Post	0.073	0.077	0.092*	0.030	0.030	0.036	0.118***	0.122***	0.136***	0.015	0.017	0.023
	(0.052)	(0.052)	(0.052)	(0.023)	(0.023)	(0.024)	(0.044)	(0.044)	(0.045)	(0.035)	(0.035)	(0.036)
Any Title (0/1) × Post (>1995)		0.213**	0.265***		0.008	0.029		0.187**	0.236***		0.073	0.094
		(0.098)	(0.087)		(0.040)	(0.037)		(0.094)	(0.091)		(0.052)	(0.058)
Coca (0/1) × Any Title (0/1) × Post (>1995)			-0.254			-0.102			-0.240			-0.104
			(0.326)			(0.117)			(0.267)			(0.124)
Adj. R-squared	0.284	0.285	0.285	0.136	0.136	0.136	0.236	0.237	0.237	0.174	0.174	0.174
Observations	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581	31581
Clusters	957	957	957	957	957	957	957	957	957	957	957	957
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls × Post	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Department time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependent Variable Mean	0.926	0.926	0.322	0.106	0.106	0.0575	0.378	0.378	0.146	0.442	0.442	0.161

Note: The unit of analysis is a municipality-year. We apply the inverse hyperbolic sine (IHS) transformation to all the outcomes to correct for the skewed distribution in the number of attacks. Robust standard errors clustered at the municipality level are reported in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A. 24: Internal Cocaine Trafficking

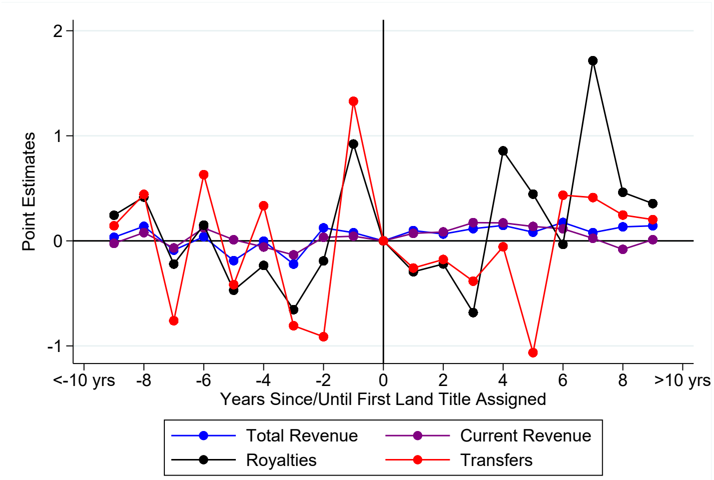
	Ln(PUS) $\times$ U.S. only	Ln(PEU) $\times$ EU only	Ln(PUS) $\times$ (U.S. & EU)	Ln(PEU) $\times$ (U.S. & EU)	Ln(PEU)/ Ln(PUS) $\times$ U.S. only	Ln(PEU)/ Ln(PUS) $\times$ EU only	Ln(PEU)/ Ln(PUS) $\times$ (U.S. & EU)
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Panel A							
Any Title (0/1) $\times$ Post (>1995)	0.003	0.001	0.004	0.000	-0.002	0.002	-0.004
	(0.008)	(0.004)	(0.007)	(0.019)	(0.012)	(0.003)	(0.014)
Adj. R-squared	0.998	0.999	0.998	0.999	0.961	0.961	0.962
Observations	15280	15280	15280	15280	15280	15280	15280
Clusters	955	955	955	955	955	955	955
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls $\times$ Post	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Department time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependent Variable Mean	0.557	0.140	1.534	1.582	0.167	0.0409	0.461

Note: The unit of analysis is a municipality-year. Robust standard errors clustered at the municipality level are reported in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

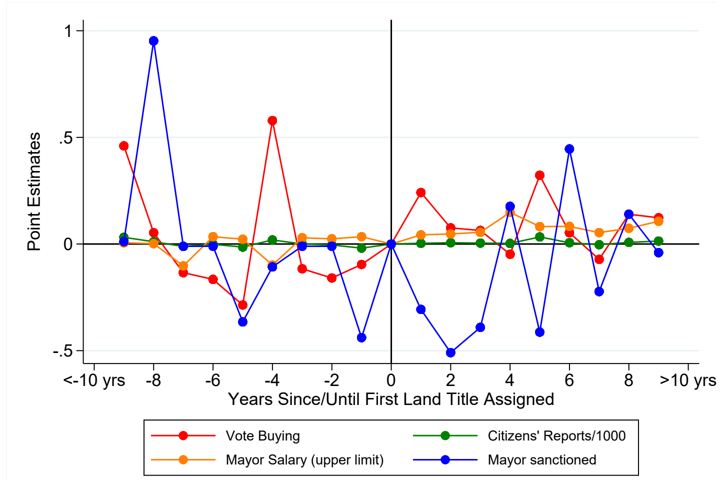
Table A. 25: Anti-Narcotics Operations & Military Aid

	Anti-Narcotic Operations	Drug Properties Seized	Bases × Log Mil. Aid (Non-LAC)	Bases × Log Anti-narco. Aid	Bases × Log US mil. & narco. aid (Col.)	Bases × Log US mil. aid (Col.)	Bases × Log US narco. aid (Col.)	Bases × Log US military aid × Election
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Panel A								
Any Title (0/1) × Post (>1995)	0.161*	0.054	0.002	0.003	0.001	0.004	0.002	0.007
	(0.095)	(0.039)	(0.003)	(0.047)	(0.052)	(0.005)	(0.082)	(0.027)
Adj. R-squared	0.402	0.164	0.986	0.881	0.801	0.898	0.809	0.369
Observations	15309	31581	15102	15102	15102	15102	15102	15102
Clusters	957	957	839	839	839	839	839	839
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls × Post	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Department time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependent Variable Mean	0.464	0.155	0.0514	-0.0739	-0.0734	-0.133	-0.0947	-0.0353

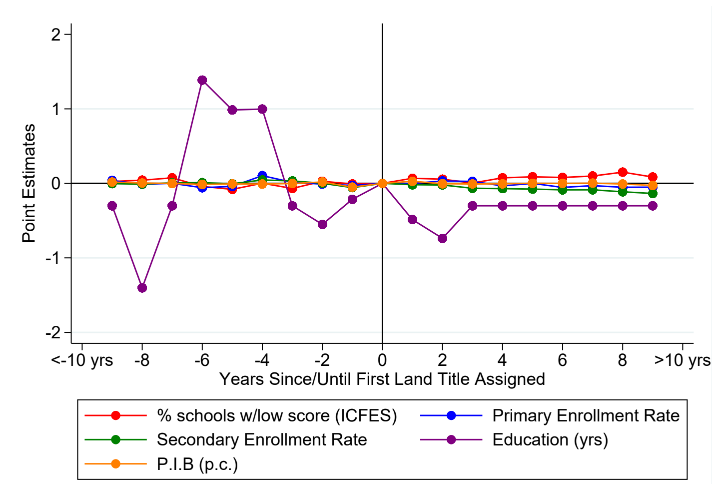
Note: The unit of analysis is a municipality-year. Robust standard errors clustered at the municipality level are reported in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.



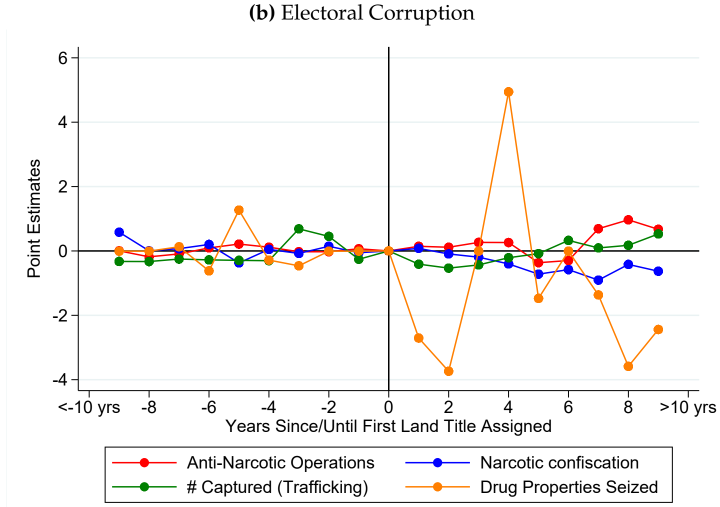
(a) Revenues and Transfers



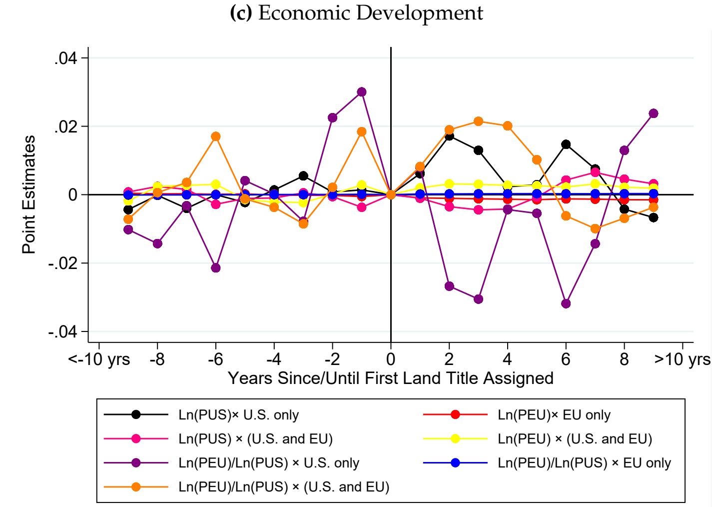
(b) Electoral Corruption



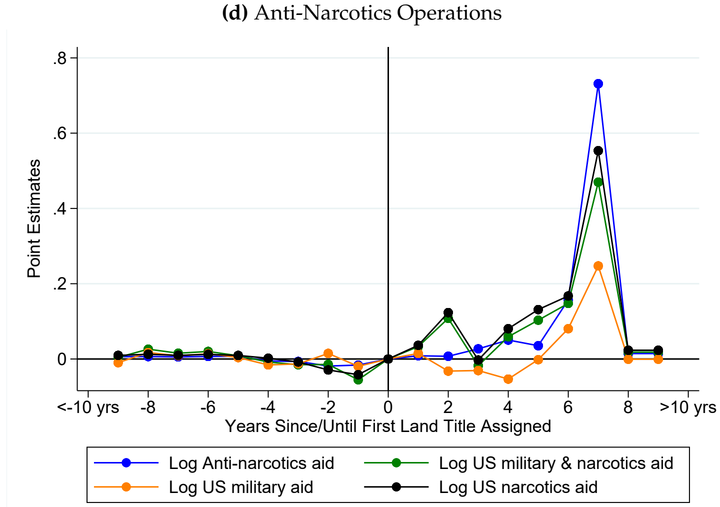
(c) Economic Development



(d) Anti-Narcotics Operations



(e) Internal Cocaine Trafficking



(f) U.S. Military Aid

Figure A. 18: Alternative Mechanisms

B Law 70

B.1 Law 70 and the Social Structure of Racial Inequality in Colombia

Colombia is currently the second country with the largest number of afro-descendant population in Latin America after Brazil. Afro-descendants represent around 10% of the Colombian population according to the 2005 Census. Black population was mostly located on both coasts (Pacific departments and Caribbean coast), but in the last years a large sector of the population has settled in the main cities of the country.

Transitory Article 55 in the 1991 Colombian Constitution promised that congress would pass a law within 2 years that would codify black rights to multicultural citizenship, and among these rights, the right to the collective occupation of public lands in the Pacific region. This was an important step in the recognition of rights for afro-descendants in Colombia. The suite of articles, legislation and decrees that culminated in Law 70 marked a substantial change to interest intermediation for rural black peasants across Colombia, but especially in the predominately black departments on the Pacific. Black social movements demanded institutional spaces to use to ensure that their autonomous control over land use in their traditional territories (Escobar, 2008; Paschel, 2016). Institutional boundaries between black people and the majority *mestizo* (mixed-race) population were erased with the end of slavery in the Republic of Colombia in 1852. Black people—former slaves and those born free—were incorporated into political, social, civic citizenship rights essentially without qualification. But, abolishing racial difference at the end of slavery effectively preserved patterns of political, economic, and social stratification by race (Nunn, 2008b; Wade, 1993). By the end of slavery, the Pacific region came to constitute "the black region par excellence" in the Colombian imagination.

Law 70 is a distributional land reform. Black communities were granted the right to title empty public lands, lands that had no other owner except the state. This land reform could then formally be considered "positive-sum", as it created no net losses to land that was formally owned by other actors.[14]

In the same way, while the Pacific Basin was largely recognized as empty public land, the territory was crisscrossed with private and state interests in economic and infrastructural development. Law 2 of 1959, declared large portions of public lands on the Pacific Coast as a protected forest reserve.[15] The creation of the Pacific Forestry Reserve Zone was principally a development project motivated by the desire of the state to first order and then exploit the rich natural resource wealth on the reserved land.[16] The push for extractive industry in the region reached full tilt by the beginning of the 1980s, when industries—primarily in logging, palm oil cultivation, and aquaculture—received titles from the state, displacing and coercing labor from the peasants that occupied the land informally (Cárdenas, 2012; Vélez-Torres, 2014). In the wake of this reform, black communities gained the right to access, extract, manage and exclude on their territory (Vélez-Torres, 2014).[17] Black Communities did not acquire alienation rights through Law 70, meaning collective lands must remain under the collective ownership of that community and under no circumstances will those titles be taken away or can they be transferred or sold to someone that is not a member of the community. This parchment protection would become important in the context of increasing violence and conflict in the Pacific Basin. The law provides the right to "first consult" with communities (a policy known as *consulta previa*) before a development contract is awarded to use part of the land (Fuentes, 2019).

Law 70 significantly reorganized the racial and territorial organization of Colombian society. It is the prototypical case of incorporating Afro-Latin communities into the multicultural citizenship rights

[14] Borras (2006) argues that transfers of public land, should actually be considered cases of redistribution (zero-sum) rather than distribution (positive-sum), where existing practices of informal landownership are entwined with private interests and relationships between elites and peasants.

[15] See Article 1, Section "a" of Law 2 of 1959.

[16] See Article 2 ibid.

[17] While the title itself is inalienable, the land itself is subject to easements and outside development projects.

that had previously been reserved for indigenous groups (Hooker, 2005). Recent evidence suggests that Law 70 led to improvements in household income, poverty reduction, and educational attendance in rural areas that received collective titles (Peña et al., 2017). Law 70 has also contributed to significant reductions in deforestation rates in titled territories in parts of the Colombian Pacific (Vélez et al., 2020). Still, Law 70 was not a revolutionary reorganization of the economic, political or social structure of racial inequality. There is significant evidence to suggest that the Colombian state and powerful economic interests circumscribed the scope of the reform, recognizing that the demands linked to landed and racial reform, on the one hand, would create significant challenges to economic modernization and development on the other.[18]

The political reform created an institutionalized mechanism, the right to first consult, which made community councils a formidable opponent to commercial and economic elites. During the late 1980s and the 1990s, Afro-Colombian communities attempted to resist logging efforts with relatively little success. Their fortunes changed post-Law 70 once they received de jure rights to organize and resist the entry of extractive industry in their territory (Vélez et al., 2020). The de jure power of collectively titled black communities reduced the range of legal and quasi-legal tactics that elite interests had at their disposal to gain control over this territory.

B.2 Details of Land Titling Process

Law 70 codified the right to collective ownership of *terrenos baldíos*. Terrenos baldíos is the name given to the territory that does not formally belong to anyone and, thus, are property that belongs to the Colombian government. In order to obtain the legal right over a Baldío, Law 70 created the figure of community councils (*consejos comunitarios*). These community councils would operate as the legal authority over land usage rights within the collective territory (Arango, 2018). The law required that titled communities and communities in the process of being titled had to be formally informed and consulted in licensing decisions for projects and activities on their land .[19]

In order to receive a land title, the community council must provide documentation to the regional office of the National Land Authority: 1) affirming their collective occupation of public lands, 2) documenting the distinctive ethnic history, and 3) the maintenance of distinctive cultural practices in the community (Article 20 of Decree 1745, 1995).

Furthermore, Article 7 of Law 70 decrees that once granted collective titles are "non-transferable, imprescriptible, and non-mortgageable". In other words, once granted to a black community, collective lands must remain under the collective ownership of that community and under no circumstances will those titles be taken away or can they be transferred or sold to someone that is not a member of the community. While the title itself is inalienable, the land itself is subject to easements and outside development projects. The law provides the right to first consult with communities (a policy known as *consulta previa*) before a development contract is awarded to use part of the land (Fuentes, 2019).

Fig. 1 illustrates the limited geographic and temporal implementation of Law 70. It shows the location and timing of titling decisions under Law 70 from 1996-2018.

[18] Charles Hale used the Guatemalan case to theorize the "menace" behind multicultural reforms like Law 70. Hale argues that multicultural citizenship regimes, at least as commonly implemented across Latin America, are constituent elements of elite projects designed to manage the radical and threatening (read as redistributive) edges of subaltern mobilization (Hale, 2002, p. 508). He defines "neoliberal multiculturalism" as the recognition of a minimal set of cultural rights, "and an equally vigorous rejection of the rest" (p. 485). The multiculturalist reforms that states enacted alongside neoliberal reforms are a form of controlled or managed inclusion that both creates political opportunities for marginalized groups and positions the state as the gatekeeper for which claims are acceptable and which go too far.

[19] Cárdenas (2012) argues that Law 70 made black communities "legible to the state" by transforming them into "political actors who must be contended with, as participants who cannot be simply swept aside" (pg. 320). In contrast to the pre-Law 70 status quo, this reform marked a substantial redistribution of de jure political, economic and social power from economic elites to organized, black peasants.

C Data Appendix

Main Analysis Data Description and Sources		
Variable	*Description*	*Source*
Municipalities	Unique 5-digit code for each municipality	DANE (2000)
All attacks	Sum of attacks perpetrated by paramilitaries, guerrillas state forces and unknown actors in year t (1980-2012)	Grupo de Memoria Histórica (2012)[a]
Police and army attacks	Sum of attacks perpetrated by police and army (state) forces in year t (1980-2012)	Grupo de Memoria Histórica (2012)
Paramilitary attacks	Sum of attacks perpetrated by paramilitary groups in year t (1980-2012)	Grupo de Memoria Histórica (2012)
Guerrilla attacks	Sum of attacks perpetrated by guerrilla groups in year t (1980-2012)	Grupo de Memoria Histórica (2012)
Any title	Dummy variable coded as "1" if a municipality received at least one black collective land title between 1996 and 2012	OTEC (2018)
Cumulative land titles	The cumulative sum of all black collective land titles in a municipality in year t	OTEC (2018)
New land title	Dummy variable coded as "1" if a municipality received at least one new black collective title in year t	OTEC (2018)
Afro-descendent area	Cross-section of total land area in the municipality that is titled to collective black communities (post-1996)	OTEC (2018)
Indigenous reservation area	Cross-section of total land area of indigenous reservations in the municipality (post-1996)	OTEC (2018)
Altitude	Altitutde of municipality (1000s of meters)	CEDE (2016)
Municipal Area	Surface area of municipality (1000s of meters)	CEDE (2016)
River distance	Distance of municipality to main rivers (Magdalena/Cauca)	CEDE (2016)
River density	Density of primary rivers (mts./sq. kms.) in each municipality	CEDE (2016)
Rainfall	Rainfall in millimeters per year	CEDE (2016)
Department capital distance	Linear distance between in the municipality and the capital of the department in kilometers	CEDE (2016)
Market distance	Linear distance to principal food market	CEDE (2016)
Population	Population from 2005 census. Estimations before 2005, projections after 2005	CEDE (2016)
% Afro-descent	Afro-Colombian population share based on the 2005 census	CEDE (2016)
% Indigenous	Indigenous population share based on the 2005 census	CEDE (2016)
Foundation date	Date in which the municipality was officially founded	Durán y Díaz (1794)[b]
Colonial presence	Number of colonial state agencies	Durán y Díaz (1794)
Crown Employees	Municipal-level number of Crown Employees	Durán y Díaz (1794)
Spanish occupation	Dummy of Spanish occupation of territory 1510-1561	Durán y Díaz (1794)
Royal Rd. dist.	Distance between centroid of municipality and closest royal road	Durán y Díaz (1794)
Gold mines (1560)	Dummy variable coded as "1" if the municipality had gold mines during the colonial era	Colmenares (1973)[c]
Encomienda	Dummy variable coded as "1" if encomiendas were present in the municipality during the colonial era	Colmenares (1973)
Indigenous pop. (1535-'40)	Dummy variable coded as "1" if indigenous populations were present in the municipality between 1535 and 1540	Colmenares (1973)
Coca	Dummy variable coded as "1" if there is presence of coca crops in the municipality	Echandía (1999)
Oil	Dummy variable coded as "1" for oil producing municipalities	IGAC (2014)
Slave ratio	Proportion of slaves in the municipality in 1843	del Interior Secretaría (1843)
Land conflict	Dummy variable coded as "1" if land conflicts occurred in the municipality between 1901 and 1931	Restrepo, Spagat, and Vargas (2004)[d]
La Violencia '48 -'53	Dummy variable that takes a value of "1" if a conflict related to La Violencia occurred in the municipality between 1943 and 1953	Restrepo, Spagat, and Vargas (2004)
ANUC Raids '71-'78	Dummy variable coded as "1" if the peasant union perpetrated a land raid in the municipality between 1971 and 1978	Zamosc (1986)
Neighbor violence	Sum of attacks in year t-1 in neighboring municipalities whose capitals are within a 100 kilometers of the municipality	Albertus and Kaplan (2013)
State land'05	Number of hectares of State land (hm2.)	CEDE (2016)
Plots reformed '60-'85	Cumulative sum of land plots titled to peasants from 1960 to 1985 (not racially targeted)	INCORA (2002)[e]
Total plots reformed	Cumulative sum of land plots titled to peasants from 1988 to 2001 (not racially targeted)	INCORA (2002)
Rehabilitation zones	Dummy variable coded as "1" if the municipality contained a rehabilitation zone (Decree 2002 of 2002)	Lorente, Salazar, and Gallo (1985)
textitPrior plots'95	Cross-section of the total number of land plots titled to peasants ($t \leq 1995$)	INCORA (2002)
Other tenancy	Proportion of households that do not formally rent or own their land	Albertus and Kaplan (2013)
Public institutions	Cross-section of the number of national judicial institutions (courts), law enforcement institutions (police posts), and local public goods institutions (hospitals and agricultural banks) (t = 1995)	Fundacion Social (1998)[f]
National Courts	Cross-section of the number of national courts in the municipality (t = 1995)	Fundacion Social (1998)
National Police	Cross-section of the number of national police stations and inspection stations in the municipality (t = 1995)	Fundacion Social (1998)
Public Hospitals	Cross-section of the number of public hospitals in the municipality (t = 1995)	Fundacion Social (1998)
Agricultural Banks	Cross-section of the number of agricultural banks in the municipality (t = 1995)	Fundacion Social (1998)
Conservative candidate share	Percent of votes for conservative presidential candidates in the 1994 presidential election	Pachón and Sánchez (2014)[g]
Left-leaning candidate share	Percent of votes for left-leaning presidential candidates in the 1994 presidential election	Pachón and Sánchez (2014)
Conservative party share	Percent of votes for Conservative party in the 1994 presidential election	Pachón and Sánchez (2014)
Liberal party share	Percent of votes for Liberal party in the 1994 presidential election	Pachón and Sánchez (2014)
Left party share	Percent of votes for left party in the 1994 presidential election	Pachón and Sánchez (2014)
Left-wing mayor	Dummy coded as "1" if left-wing candidate got elected as mayor	Pachón and Sánchez (2014)
Right-wing mayor	Dummy coded as "1" if right-wing candidate got elected as mayor	Pachón and Sánchez (2014)

[a] See paper for details on authors' calculations for all Grupo de Memoria Histórica (2012) references.
[b] See also Acemoglu, García-Jimeno, and Robinson (2015).
[c] For all Colmenares (1973) references, see also Acemoglu, García-Jimeno, and Robinson (2012) & del Interior Secretaría (1843) for further details.
[d] Updated by Universidad del Rosario. See also Guzmán et al. (1963) & Albertus and Kaplan (2013).
[e] For all INCORA (2002) references, see also Albertus and Kaplan (2013) for further details.
[f] For all Fundacion Social (1998) references, see also Acemoglu, García-Jimeno, and Robinson (2015) for data access.
[g] See also Fergusson et al. (2020) & Ch et al. (2018) for further details for all Pachón and Sánchez (2014) references.

Supplementary Data Description and Sources		
Variable	*Description*	*Source*
Urban areas	Percentage of urban areas in the municipality	CEDE (2016)
Total Land titling	Total stock of collective municipal land titles	OTEC (2018)
# Land titles	Number of black collective land titles established in year t (1996-2012)	OTEC (2018)
Indigenous reservation area	Panel : indigenous reservation area in municipality	OTEC (2018)
Afro-descendant area titled	Total Afro-descendant municipal land area titled ('96 - '12)	OTEC (2018)
National agencies	Cross-section of the total number of courts and national police offices and inspection posts (t = 1995)	Fundacion Social (1998)
Local agencies	Cross-section of the total number of local public hospitals and agricultural banks (t = 1995)	Fundacion Social (1998)
National public employees	Cross-section of the total number of national public employees in the municipality (t = 1995)	Fundacion Social (1998)
Municipal public employees	Cross-section of the total number of municipal public employees in the municipality (t = 1995)	Fundacion Social (1998)
Land gini'93 & '00	Land inequality gini in the municipality in 1993 & 2000	IGAC (2014)[a]
Land quality	Share of land quality coded by land type	IGAC (2014)
Land value	Per capita land value in the municipality	IGAC (2014)
Land quality	Cross-section of the percent of municipal land within 8 different levels of land quality	IGAC (2014)
Rubber suitable area	Cross-section of the degree of suitability for rubber production in the municipality	OTEC (2018)
Forest suitable area	Cross-section of the degree of suitability for forestry in the municipality	OTEC (2018)
Cacao suitable area	Cross-section of the degree of suitability for cacao cultivation in the municipality	OTEC (2018)
Rice suitable area	Cross-section of the degree of suitability for rice cultivation in the municipality	OTEC (2018)
Coffee cultivated area	Cross-section of the degree of suitability for coffee cultivation in the municipality	OTEC (2018)
Coca cultivated area	Cross-section of the degree of suitability for coca cultivation in the municipality	OTEC (2018)
Mining title	Cross-section of the municipal land area titled for mining	OTEC (2018)
Oil & gas area	Cross-section of the municipal land area titled for oil and gas exploration	OTEC (2018)
Cattle farming	Cross-section of the municipal land area titled for cattle farming	OTEC (2018)
Poultry farming	Cross-section of the municipal land area titled for poultry farming	OTEC (2018)
Forest reserve zone	Cross-section of the municipal land area titled for forestry reserves	OTEC (2018)
Oil'88	Municipal daily Oil production (100,000 barrels/day) in 1988	Ministry of Mines and Energy (MME)[b]
Coal'78	Dummy variable coded as "1" if the municipality had coal reserves in 1978	Ministry of Mines and Energy (MME)
Mining'78	Hectares reserved for precious metal mining in the municipality in 1978	Ministry of Mines and Energy (MME)
Coffee'97	Hectares of coffee cultivation in municipality in 1997 (1,000s of hectares)	NFCG, Ag. Ministry
Coffee price	log internal coffee price	NFCG, Ag. Ministry
Oil price	log oil price	International Financial Statistics (IFS)
Gold price	log gold price	Ingeominas; Global Financial Data (GFD)
Silver price	log silver price	Ingeominas; Global Financial Data (GFD)
Platinum price	log platinum price	International Financial Statistics (IFS)
Coal price	log coal price	International Monetary Fund (IMF)
Total Revenue	Total annual municipal revenue (1998-2012)	Nuñez (2005)
Current revenue	Current municipal revenue (1998-2012)	Nuñez (2005)
Tax revenue	Total annual municipal tax revenue (1998-2012)	Nuñez (2005)
Property tax rev.	Annual property tax revenue in a municipality (1998-2012)	Nuñez (2005)
Royalties	Annual municipal royalties (1998-2012)	Nuñez (2005)
Transfers	Annual municipal transfer (1998-2012)	Nuñez (2005)
Anti-narcotic operations	Number of anti-narcotic operations in the municipality (1988-2005)	Dube and Naidu (2015)
Drug properties seized	Number of drug properties seized in municipality (DNE haciendas 1980-2009)	Dube and Naidu (2015)
Bases	Number of global security bases	Dube and Naidu (2015)
Military aid (Non-LAC)	U.S. military aid to non-Latin American countries (billions of 2000 U.S. dollars)	Dube and Naidu (2015)
Anti-narcotic aid	U.S. anti-narcotic aid to non-Latin American countries (billions of 2000 U.S. dollars)	Dube and Naidu (2015)
U.S. military aid	U.S. military aid to Colombia (billions of 2000 U.S. dollars)	Dube and Naidu (2015)
Anti-narcotic aid (Col.)	U.S. Anti-narcotics aid to Colombia (billions of 2000 U.S. dollars)	Dube and Naidu (2015)
Cocaine prices	International price of cocaine in U.S. (PUS) and European Union (PEU) - (USD pre gram)	Millán-Quijano (2020)
Cocaine trafficking routes	Dummy = 1 if municipality belongs to a drug trafficking route linked to U.S. or EU markets	Millán-Quijano (2020)
Enrollment rate	Secondary enrollment rate in the municipality	CEDE (2016)
Education (yrs)	Mean educational attainment in the municipality (in years)	CEDE (2016)
P.I.B per capita	Municipal gross domestic product per capita	CEDE (2016)
% School w/low score	Percentage of schools in the municipality with low ICFES standardized testing scores	CEDE (2016)
Vote buying	Vote buying citizens' reports	Office of the Inspector General[c]
Citizens' Reports	citizens' reports of vote buying per 1000 people	Office of the Inspector General
Mayor Salary	Salary mayors upper limit in min wages	Office of the Inspector General
Mayor Sanctioned	Mayor was sanctioned before incumbency period	Office of the Inspector General

[a] For all IGAC (2014) references, see also Acemoglu, García-Jimeno, and Robinson (2015) and Ch et al. (2018) for further details.

[b] For all natural resource references, see also Jacome (1978) and Dube and Vargas (2013) for further details.

[c] For all Office of the Inspector General references, see also Jacome (1978) and Rueda and Ruiz (2020) for further details.

Data Note

All databases can be found through the Base de Datos ¡Basta Ya!

File	Description from centrodememoriahistorica.gov	Original Sources
SecuestrosColombia1970-2010	Situación ocurrida en territorio colombiano en la cual una persona es privada ilegalmente de su libertad en contra de su voluntad para obtener algún provecho de la ella o de un tercero a cambio de su liberación. Criterios de inclusión - Secuestro • Trata de personas en territorio colombiano. • Pesca milagrosa. • Toma de rehenes. Criterios de exclusión - Secuestro • Violación. • Disputas de patria potestad. (as I understand it disputes over parental authority) • Paseo millonario. (this is when a person is abducted, taken to an ATM and forced/threatened to pay for immediate release: “express kidnapping”) • Trata de personas internacional. • Desaparición forzada. • Reclutamiento forzado. • Menos de 24 horas, salvo que haya rescate, fuga o proceso de negociación y pago.	Bases de datos Cifras y Conceptos para el Informe General.
Masacres1980-2012	Se entiende como el homicidio intencional de 4 o más personas en estado de indefensión y en iguales circunstancias de modo, tiempo y lugar, y que se distingue por la exposición pública de la violencia. Es perpetrada en presencia de otros o se visibiliza ante otros como espectáculo de horror. Es producto del encuentro brutal entre el poder absoluto del actor armado y la impotencia absoluta de las víctimas. Criterios de inclusión - Masacres • Se incluyen los casos perpetrados por grupos armados identificados o cuando y donde haya indicios de que fueron perpetrados por estos (porte de prendas de uso privativo de las fuerzas militares, armas largas o miembros de un grupo armado) • Se incluyen los casos de víctimas con militancias sociales y políticas por su vulnerabilidad como objetivos militares en el marco del conflicto armado Criterios de exclusión - Masacres • Se excluyen los homicidios de cuatro o más víctimas en hechos diferentes. • Se excluyen las desapariciones forzadas con cuatro o más víctimas. • Se excluyen las víctimas de desaparición forzada en las masacres mientras no se esclarezca si el desenlace es o no homicidio y las circunstancias de modo, tiempo y lugar de su ocurrencia. • Se excluyen los homicidios de cuatro o más víctimas como consecuencia del desarrollo de acciones bélicas (combates, bombardeos, emboscadas, incursiones y ataques a objetivos militares) • Se excluyen los homicidios de cuatro o más víctimas como consecuencia del ataque a bienes civiles • Se excluyen los homicidios de cuatro o más víctimas como consecuencia de atentados terroristas, entendidos como ataques indiscriminados perpetrados con artefactos explosivos contra objetivos civiles.	Sources: Various including news sources, policy reports, NGOs

	• Se excluyen los homicidios de cuatro o más víctimas en operaciones de intervención legal por parte de la fuerza pública. • Se excluyen los homicidios de cuatro o más victimas por campos minados. • Se excluyen los homicidios de cuatro o más víctimas inhumadas en fosas comunes. • Se excluyen los homicidios de cuatro o más víctimas perpetrados por grupos de limpieza social, individuos en un ataque de locura, atentados terroristas de estructuras organizadas del narcotráfico, reacciones de miembros de la Fuerza Pública ante ataques de estructuras organizadas del narcotráfico, guerras entre estructuras criminales organizadas, delincuencia común, venganzas personales y conflictos interpersonales.	
MAP1982-2013 (MAP refers to Minas Antipersonales)	Por "mina antipersonal" se entiende toda mina concebida para que explosione por la presencia, la proximidad o el contacto de una persona, y que en caso de explosionar tenga la potencialidad de incapacitar, herir y/o matar a una o más personas. Las minas diseñadas para detonar por la presencia, la proximidad o el contacto de un vehículo, y no de una persona que estén provistas de un dispositivo antimanipulación, no son consideradas minas antipersonal por estar así equipadas. Artículo 1 de la Ley 759 de 2002. Definición del PAIMA. Note: This is a Month/Year database of APM events and a separate Month/Year database of victims per municipality. The problem with the latter is that there is no event id # to know whether individual victims are victims of the same event.	Sources: bases de datos tomadas del Programa Presidencial de Acción Integral contra Minas Antipersonal. PAIMA.
DanoBienesCiviles1988-2012	Se refiere a los daños causados a bienes materiales que no son objetivos militares y que no deben ser objeto de ataque o represalia. Criterios de inclusión - Daño a bienes civiles • Se incluyen todos los casos en los cuales haya destrucción parcial o total de bienes materiales como consecuencia del accionar de los actores armados. • Se incluyen entre los bienes civiles las afectaciones a entidades públicas, infraestructura eléctrica, energética, vial y de comunicaciones, viviendas, propiedad rural, entidad bancaria, establecimiento comercial, infraestructura empresarial, medios de transporte, infraestructura empresarial, bienes culturales y lugares de culto, infraestructura educativa y médica, sedes de partidos o movimientos políticos, gremios, sindicatos y organizaciones no gubernamentales, mercancías, semovientes y objetos materiales. • Se incluyen civiles muertos cuando su victimización es consecuencia del ataque intencional contra los bienes civiles. Por ejemplo: un civil muerto como consecuencia de la voladura de un oleoducto o una torre de energía. Criterios de exclusión - Daño a bienes civiles • Se excluyen los civiles muertos en acciones bélicas, atentados terroristas, asesinatos selectivos y masacres que tengan daño a bienes civiles como hechos simultáneos para evitar duplicidad en el registro de víctimas con otras bases de datos.	Sources: various, but what appear to mainly be policy reports from NGOs
AtaquesPoblaciones1988-2012	Se entiende como una incursión que implica la ocupación transitoria de un territorio y una acción militar continuada dirigida hacia el arrasamiento de un objetivo militar dentro de un casco urbano. Criterios de inclusión - Ataques a poblaciones	Sources: various, but what appear to mainly be policy reports from NGOs

	- Se incluye los ataques con explosivos siempre y cuando hagan parte de una incursión en la que se atacan objetivos militares. - Se incluye todo ataque a objetivos militares dentro del casco urbano siempre y cuando haya incursión en el territorio por parte de un contingente armado. Criterios de exclusión - Ataques a poblaciones - Se excluyen los hostigamientos por su baja intensidad y su carácter esporádico. - Se excluyen los ataques a objetivos militares que no impliquen incursión en el territorio ni confrontación directa entre combatientes.	
AsesinatosSelectivos1981-2012	Es el homicidio intencional de 3 o menos personas en estado de indefensión en iguales circunstancias de modo, tiempo y lugar, perpetrados por los actores del conflicto armado. Criterios de inclusión - Asesinatos selectivos - Se incluyen los casos de cuatro o más víctimas fatales siempre cuando sean hechos diferentes agrupados por las fuentes de información en un caso. - Se incluyen los casos perpetrados por grupos armados identificados o cuando y donde haya indicios de que fueron perpetrados por estos (porte de prendas de uso privativo de las fuerzas militares, armas largas o miembros de un grupo armado). - Se incluyen los casos de víctimas con militancias sociales y políticas por su vulnerabilidad como objetivos militares en el marco del conflicto armado aun cuando el perpetrador no haya sido identificado. Criterios de exclusión - Asesinatos selectivos - Se excluyen los casos de víctimas de masacres, minas, acciones bélicas y atentados terroristas. - Se excluyen los casos perpetrados por grupos de limpieza social, delincuencia común y organizada, narcotráfico, miembros de la fuerza pública en circunstancias no relacionadas con el conflicto armado, conflictos entre particulares y ataques individuales.	Sources: Various
CivilesMuertosAcciones Belicas1988-2012	Es el homicidio de civiles en el desarrollo de acciones bélicas propias del conflicto armado como consecuencia de la violación del principio de proporcionalidad en el uso de la fuerza, el recurso a métodos y medios ilícitos, y la prevalencia del imperativo militar sobre el principio humanitario de protección de la población civil Criterios de inclusión - Civiles muertos en acciones bélicas - Se incluyen los civiles muertos en combates, emboscadas, incursiones, ataques a objetivos militares y bombardeos Criterios de exclusión - Civiles muertos en acciones bélicas - Se excluyen los casos en los cuales la población civil haya sido usada como escudo humano por parte de los actores armados. - Se excluyen los atentados terroristas. - Se excluyen las minas antipersona y munición sin explotar. - Se excluye el sabotaje.	Sources: Various
AtentadosTerroristas1988-2012	Se entiende como todo ataque indiscriminado perpetrado con explosivos contra objetivos civiles en lugares públicos con un alto potencial de devastación o letalidad. Criterios de inclusión - Atentados terroristas	Sources: Various

	• Se incluyen los casos perpetrados por grupos armados identificados o cuando y donde haya indicios de que fueron perpetrados por estos (porte de prendas de uso privativo de las fuerzas militares, armas largas o miembros de un grupo armado) Criterios de exclusión - Atentados terroristas • Se excluyen los ataques con explosivos contra objetivos militares. • Se excluyen los ataques con explosivos contra objetivos civiles específicos siempre y cuando no haya afectación masiva e indiscriminada en el entorno. • Se excluyen los ataques con explosivos contra entidades bancarias • Se excluyen los sabotajes a la infraestructura energética, eléctrica, vial o de comunicaciones. • Se excluyen los casos perpetrados por el narcotráfico, la criminalidad organizada, la delincuencia común o que responden a ataques individuales.	