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Land Reform as a Counterinsurgency Policy: Evidence from Colombia

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Abstract

Can targeted land reform reduce levels of civil war conflict by mitigating the factors that contribute to rural rebellion? This article uses new micro-level data on land reform and insurgency at the municipal level from Colombia from 1988 to 2000, a country with high rates of land inequality and informal land ownership, to test whether land reform undercut subsequent guerrilla activity. The reform had two distinct aspects. Politically powerful large landholders blocked most large-scale reform, which resulted primarily in an enduring, low-intensity, and geographically dispersed reform that spurred low levels of insurgent activity. Larger-scale reforms were only implemented in areas that threatened serious violence and had the potential to harm elite interests, and in these limited areas reform reduced guerrilla activity. This suggests that while land reform can be an effective counterinsurgency policy, it may be politically difficult to implement at a sufficient scale because it threatens the status quo.

Keywords

civil war, counterinsurgency, land reform, inequality, grievance

How do targeted distributive programs during civil conflict impact the intensity of local insurgency? Recent decades have seen high rates of stubbornly persistent civil

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conflicts and rebellions, often with roots in rural areas. Scholars have traced rebellion to diverse causes such as economic inequality (Muller and Seligson 1987; Russett 1964), the erosion of subsistence security associated with economic growth and the transition to capitalist labor relations (Huntington 1968; Scott 1976), and citizen grievances over unfulfilled expectations or perceived unfairness in the distribution of benefits from modernization (Gurr 1971). More recent opportunity structure explanations of rebellion have emphasized conditions that favor insurgency such as weak states and rough terrain (Fearon and Laitin 2003) and “greed” explanations have highlighted that the presence of primary commodity exports can provide rebels the motivation and opportunity to support themselves (Collier and Hoeffler 2004).

Given the importance of the rural sector in many modernizing or underdeveloped states where rebellion occurs, land reform has long been cited as a potential remedy for unrest (Huntington 1968; Mason 1986, 1998; Paige 1975; Wood 2003). The question of how peasant support can be shifted from rebel groups to the government as a consequence of land reform is particularly salient, given the widespread persistence of land inequality and informal ownership as well as because most insurgencies are based in rural areas (Kalyvas 2006). While existing scholarship has considered how well factors such as grievances, opportunity, or “greed” explain rebellion, this article asks a related but distinct question: *can targeted land reform reduce levels of civil war conflict by undercutting the conditions that favor insurgency in rural areas?*

We explore this question in the context of Colombia, where insurgencies were motivated by issues of land inequality (e.g., Marulanda 1973). We find that the impact of land reform on insurgent attacks varies with the intensity of reform. Land titling (i.e., the granting of formal property rights to those with no land or with insecure tenure) was national in scope and lasted decades. However, it was implemented piecewise and incompletely, blocked in most areas by politically powerful large landholders. The low-intensity and geographically dispersed land reform created spillover effects that spurred low levels of insurgent activity not sufficiently severe to threaten elite interests. In most areas, therefore, greater levels of land reform were linked to *increased* guerrilla activity. In contrast to this broad pattern, land reform *was* vigorously and continuously implemented in a limited number of areas: poor frontier regions far from the reach of the state with high likelihoods of civil conflict that could potentially harm elite interests (Zamosc 1986).¹ The more intense reforms in these areas tended to *reduce* the future intensity of guerrilla activity. This suggests what we call a “Paradox of Partial Reform.” Land reform can be an effective counterinsurgency policy but may be politically difficult to implement at a sufficient scale because it threatens the status quo.

To address the question of how land reform affected guerrilla activity in Colombia, this article employs a unique, comprehensive micro-level data set of the universe of nearly half a million plots titled by the Colombian Land Reform Institute (INCORA) from 1960 to 2000. These data are combined with a variety of sub-national conflict variables that capture insurgent activities at the municipal level.

Not only is Colombia a prominent case due to its long, variable history of guerrilla conflict, but it has also experienced significant variation in land reform across time and space, enabling a careful analysis of the dynamic relationship between land reform and insurgency. Furthermore, the neglected peripheries, vague property rights, and frontier regions populated with subsistence sharecroppers that characterize Colombia are shared by many other developing and conflict-ridden countries, where land reform is hypothesized to play an important role in remedying social unrest (Huntington 1968; Mason 1998; Paige 1975). The mechanisms linking land reform with insurgency that are detailed here are consequently likely to operate in other contexts where states seek to use land reform to redress rural grievances.

In what follows, we first discuss the existing literature on land and civil conflict as well as the hypothesized causal mechanisms linking land tenure patterns and land reform to peasant support for insurgents. Next we provide a brief history of land reform and conflict in Colombia. We then detail the research design and data, and subsequently present the empirical results of the effects of land reform on guerrilla activity as well as a series of robustness tests that address possible endogeneity in the targeting of land reform and data biases. We conclude with a summary of the findings as well as their implications beyond the Colombian context.

Land, Land Reform, and Insurgency

Scholars have linked land and land reform to rebellion in cases as diverse as El Salvador (Mason 1986; Wood 2003), Nepal (Joshi and Mason 2008), Peru (Mason 1998), the Philippines (Galula 2006), Vietnam (Mitchell 1968), and Zimbabwe and South Africa (Moyo and Yeros 2005). Much of this literature examines how peasant support for either insurgents or the government is conditioned by land tenure patterns and the distribution of ownership. Land reform policies are therefore seen as a potential tool to diminish the intensity of guerrilla activity by restructuring land tenure patterns that are conducive to peasant support for rebels. For example, Wood (2003) observes that land reform during the civil war in El Salvador, and particularly the Phase III land-to-the-tiller titling program implemented by FINATA under the 1980 reform, was explicitly viewed as a preemptive counterinsurgency policy that could deflate guerrilla support. The outcomes of these reform efforts are nonetheless mixed: Moore, Lindström, and O'Regan (1996) point to contrasting findings on the effects of reforms on violence across countries.

We argue that the net effect of land reform on insurgent support ultimately depends on the extent of reform, an insight that helps reconcile some of the disparate findings on how land reform impacts insurgency. Toward this end, we develop a two-part theory that links individual-level incentives for land beneficiaries to support insurgents with the aggregate effects land reform can have on the intensity of insurgent activities as policies are implemented across broader populations. The theory therefore highlights an important yet understudied characteristic of land reform: not only does it have direct effects on individuals who receive formal title to

property, but it also affects individuals nearby that *do not* receive land. While in previous studies peasant support for insurgents would decrease monotonically as land reform intensity increases, our argument anticipates threshold effects. Whereas land titling at low levels may inflame the grievances of the large pool of nonbeneficiaries and thereby increase peasant support for guerrillas, titling at a sufficiently large scale more broadly addresses grievances and diminishes the size of the unreformed sector in a district that could otherwise turn its support to insurgents.²

The Effect of Land Reform on Individual Beneficiary Support for Insurgents

Grievance accounts of peasant support for insurgents suggest that receiving land should *reduce* an individual's support for rebels. Two prominent arguments about the sources of peasant grievances provide complementary explanations for this. First, rural guerrillas can exacerbate peasant grievances over the unjustness of landholding inequality and promise to institute reforms when they take power. Huntington (1968, 375), for example, argues that where landownership conditions "are inequitable and where the peasant lives in poverty and suffering, revolution is likely, if not inevitable." But, if the state can address a peasant's grievances by meeting the demand for land with land grants, it can potentially "buy" that peasant away from supporting guerrillas, turning him or her into a conservative force that supports the status quo over political movements that threaten individual property rights (Huntington 1968; Paige 1975). Converting the landless or those with precarious tenure into smallholders can reduce disparities between these beneficiaries and existing landholders and also increase their upward mobility, engendering economic competition that limits collective political organization (Paige 1975).

Absolute poverty can also contribute to peasant grievances, resulting in increased support for insurgents through several recruitment mechanisms. In rural settings, land productivity and ownership are key determinants of poverty. In addition to the direct impact of land on well-being through the ability to produce, market, and consume goods, formal property rights enable an owner to more easily obtain credit to make capital investments using their property and the value of crops they are cultivating as collateral. Rural poverty and a greater landless population are indicative of lower opportunity costs for joining armed groups (Paige 1975), greater susceptibility to selective benefits (Lichbach 1995), and greater potential gains from revolution (Joshi and Mason 2008; Mason 1986), all of which can induce civilians to support or join insurgent groups.

Enhancing the well-being of the rural poor and landless through land titling can decrease their support for insurgents by increasing the opportunity cost of rebellion.³ Failure to become a beneficiary, by contrast, may strengthen a peasant's grievances over unfulfilled expectations of improved circumstances or over perceived injustice. This can be exploited by guerrillas who promise remedies when they seize power or act as intermediaries with the state (Gurr 1971).

The Aggregate Effect of Land Reform on Support for Insurgents

Just as land reform is often hypothesized to reduce peasant support for guerrillas, there are also strong reasons to believe this effect is conditional on the scale of reform in a district. Land titling may generally help reduce disputes over property rights by clarifying ownership on a given set of plots and eliminating the grievances of peasants that become beneficiaries. But titling could also spur additional grievances and conflict. In particular, spillover effects from piecemeal reforms may overwhelm any beneficial effects of reform on rebel activity. Land titling may create antagonisms between nonbeneficiaries and beneficiaries (Tobón 1976), motivating local-level conflicts that guerrilla groups can use to make inroads. Kalyvas (2006), for instance, notes how in the Greek civil war and several other conflicts, guerrillas can gain footholds in communities by appropriating and settling disputes over issues such as landholding as armed local authorities. Land titling can also lead to conflict in adjacent untitled areas if peasants begin to view protesting as a way to get land. Under these circumstances, incomplete land reform can generate additional motives for supporting guerrilla activity if guerrillas assist in pressuring the state for additional reform. Directly relevant to the current study, Zamosc (1986) argues in the Colombian case that the experience of the peasants with land reform councils formed during the 1960s and 1970s demonstrated that it was necessary to protest and stage land invasions to get INCORA to focus attention on a region.

The possibility of spillover effects suggests a threshold effect where half measures in land titling may not sufficiently address grievances but instead inflame the majority of untitled peasants by demonstrating that real reform is not forthcoming. Evidence from Brazil provides support for this hypothesized *positive* effect of land reform on insurgency when implemented at low levels. Alston, Libecap, and Mueller (2000, 183) conclude that low-level land reform in Brazil encouraged invasions by squatters and accelerated attempts at evictions by landowners: “ironically, greater policy emphasis on land reform through the use of expropriations and settlements appears to have the unanticipated impact of *increasing* land conflicts.”

However, if land reform is sufficiently extensive, it can overcome these spillover effects and reduce insurgent activity. Beyond a certain threshold, the number of beneficiaries relative to nonbeneficiaries in a region will be sufficiently high that rural grievances are reduced and will not increase support for guerrilla groups.

Theoretical Predictions: Conditional Effects of Land Reform on Insurgency

Considering the possible countervailing effects of land reform on support for insurgents, we argue that whether land reform is ultimately successful depends largely on the extent of reform in a region. When land reform targets only a few individuals, it can actually generate antagonisms between beneficiaries and nonbeneficiaries, and can also lead to conflict in adjacent areas if peasants begin to view protesting as a

way to get land, can overcome their collective action problems (see, e.g., Paige 1975), and see insurgents as capable of addressing their grievances and defeating the state (Lichbach 1995). This suggests the following hypothesis:

Hypothesis 1: Low levels of land titling should be associated with increased guerrilla activity.

Whereas land titling at low levels may create spillover effects that encourage peasant support for guerrillas, titling at a sufficiently large scale more broadly addresses grievances and diminishes the size of the unreformed sector in a region that could otherwise experience inflammatory spillover effects. In this way, land reform policies may function to lessen the intensity of guerrilla activity by severing the linkages between land and conflict detailed above. As Mason (1986, 488) indicates, the government's pursuit of land reform is ideally a "middle ground" between repression and revolution, "building direct popular support for the regime in such a way as to 'inoculate' the rural population against appeals for revolutionary change." This suggests an important corollary of Hypothesis 1:

Hypothesis 2: Land reform above a certain threshold may adequately address peasant demands and undercut support for guerrillas, reversing the positive effect of reform on insurgency at low levels.

Land Reform in the Colombian Context

Rural poverty, the inequitable distribution of land, and informal land ownership have been salient issues in Latin America since the first *encomiendas* were established in the 1500s. LeGrand's (1986) study of early Colombian land reform policies in the 1800s describes modest titling as the government used colonization to populate frontier zones and alleviate land pressure. These migrations to remote areas with available land (*baldíos*) created conditions ripe for insurgency. Poverty, informality of land ownership, land conflicts, partisan strife, and the weak presence of state institutions all precipitated the La Violencia conflict that swept the country from 1946 to 1960, causing over 200,000 deaths (see, e.g., Sánchez Torres 2007).

In the aftermath of La Violencia and the Cuban revolution, National Front leaders faced the threat of insurgency with the formation of peasant "independent republics" and new communist guerrilla groups such as the Revolutionary Armed Forces of Colombia (FARC) and National Liberation Army (ELN) in the mid-1960s. Embedded in these groups' platforms were demands for land reform. Concerned about these demands for reform, the government passed the Social Agrarian Reform Act of 1961 (Law 135), which also established the land reform agency INCORA. The three primary motivations for enacting reforms were pacification of the countryside, economic necessity to increase food production, and foreign pressure from international institutions and the US-initiated Alliance for Progress (Tai 1974).

The Agrarian Reform Committee board made high-level policy decisions. Land reform was implemented through regional project zones created by this governing board (Tobón 1976; Zamosc 1986). The selection of zones was guided by factors such as demographic pressures, landholding patterns, the potential for land reclamation, and proximity to markets, but was also strongly shaped by landed elite resistance (Duff 1968). The board included members of the Federation of Colombian Cattle Ranchers (FEDEGAN), a precursor to right-wing paramilitary groups, as well as elected congressmen, agricultural interests, the church, and the armed forces. Peasant representation never exceeded two of the fifteen total members. As a result, landed elites were able to shape the land reform process by (1) influencing the intensity of reform; (2) shifting its emphasis from expropriation and redistribution of *latifundios* to legalization and titling; and (3) determining the geographical targeting of reform.

The measured pace of land reform in the early 1960s led to the government-supported formation of peasant land reform councils through the National Peasant Association (ANUC) to help express grievances and create an enduring constituency for land reform (Zamosc 1986). Beginning in the early 1970s, the ANUC councils initiated a spate of land invasions where reform demands had not been met. These invasions caught INCORA's attention, but conservative presidents acted to weaken ANUC and stall redistribution (Zamosc 1986). Only then did more harshly repressive policies emerge. The 1980s and 1990s saw some spikes in land titling as a result of the National Reconciliation Plan and Law 30 of 1988, but without an organized rural constituency INCORA was less active (Giugale et al. 2003). An effort to spur peasant-led reform by subsidizing market-based land purchases in the 1990s was tepidly received and has had little impact on landholding patterns (Machado 1998), leaving many peasants still impoverished while large areas of cultivable land are underutilized (Giugale et al. 2003).

Did Colombia's land reform program ultimately serve to reduce grievances and support for insurgent groups—one key motivation for the reform—or did its partial nature actually exacerbate conflict? The long and varied history of land reform and insurgency in Colombia and available data enable a research design to address this question that incorporates both the land reforms of the 1960s and the more recent period of reform for which micro-level data on insurgency and additional covariates are available.

Research Design and Data

To test the effects of land reform on conflict in Colombia, we analyze data on guerrilla activity and land reform from 1988 to 2000.⁴ Beyond reasons of data availability, this is a particularly relevant period given the expansion of violence and guerrilla activity that came to affect much of the country, as well as waves of land reform that occurred prior to and during these years. We use the municipality-year as the unit of analysis.⁵

Dependent Variable: Guerrilla Actions

Our dependent variable of guerrilla activity is the combined number of armed actions by the FARC and ELN rebel groups against government forces or installations as well as battles with paramilitary groups recorded in a given municipality-year. Data to construct this variable and other conflict-related variables detailed are taken from the Colombian Vice Presidency's Human Rights Observatory (OVP) and Sánchez in the following (2007).⁶ Offensive armed actions include armed contact initiated by the group, confrontations, ambushes, attacks on installations, terrorist explosive attacks, terrorist arson, other terrorist acts, incursions on populations, assaults on private property, roadblocks, harassment of armed forces, and highway robberies. These activities plausibly reflect initiative and control through the ability to project force.

Activities aimed more at civilians than enemy armed groups (incursions, robberies, and roadblocks) are relatively few (9 percent of total events).⁷ The most commonly observed action in the data set is armed contact with government forces (37 percent), in which rebels attack public security forces such as the military or national police. Also common are explosive attacks (16 percent) that terrorize through the use of explosive devices, and harassment (14 percent), low-intensity (hit-and-run) surprise attacks on military units. Detailed definitions of remaining actions (24 percent) are available in the Appendix. The guerrilla activity measure *excludes* activities against civilians such as homicides, political homicides, displacement, massacres, and kidnappings. These data on violence against civilians are classified separately by the National Police and the Presidency's Social Action agency.⁸

The standardization of data collection and reporting procedures add confidence to the measures of armed group action. The categorization of guerrilla activities limits the possibility of these activities being conflated with activities of other armed groups since actions by the armed forces or paramilitaries are coded in separate categories. Furthermore, the criteria and coding used by the Colombian Military to record data are standard across the country (Human Rights Observatory 2006). First, tactical units send radio reports of incidents to the Brigade. The Brigade reviews information for accuracy and sends information to the Division, which verifies information daily and enters events into a national database while simultaneously providing paper reports to the General Command, ensuring there are no duplicate reports. Officers at the General Command review information for accuracy and combine reports from different branches of the armed forces. Data are then given to civilian officials in the Ministry of Defense.

Although coding criteria for what constitutes guerrilla activity are clear and reporting procedures are standard across the country, there remain concerns with potential data biases such as misreporting since governments may not want to release particularly politically sensitive information or may want to leverage information for other purposes (see Davenport 2007, 6). Because incomplete or biased data may threaten inferences, potential biases and proposed solutions to address them are discussed in detail in the following.

Figure 1a shows the national trend in reported guerrilla armed actions over time from 1988 to 2000. Guerrilla activity varied significantly over time, and there is a clear increasing trend from 1995 to 2000. The geographic distribution of guerrilla activity is displayed in Figure 1b. Armed guerrilla actions were most heavily concentrated in the departments of Guaviare in the southwest, and Arauca and Norte Santander in the northwest near the border with Venezuela.

Key Independent Variables: Prior and Current Land Reform

Variables on the number of plots reformed come from INCORA. We construct two variables from these data. First, we construct a stock of Prior Plots indicator, which is the cumulative total of plots titled by INCORA since 1960—after which land reform by INCORA began—through the previous year of observation $t - 1$. This measure intends to capture how much preventive reform was done prior to the year in which guerrilla activity is being measured. Second, we construct an annual Plots Reformed variable that indicates the number of land titles awarded in a given year t . Given a particular stock or history of reform in a municipality, annual plots reformed reflect the marginal effect of land reform. To ease interpretation, Prior Plots is divided by 1,000 in the analysis and the annual Plots Reformed variable is divided by 100.

Including both of these variables helps clarify the relative importance of timing and magnitude of land reform in limiting guerrilla activity. An increase in the land reform variables may reduce guerrilla activity if they successfully reduce land grievances as detailed earlier. Yet, at low levels, these variables will instead increase insurgency if strong within-municipality (i.e., cross-farm or cross-community) or cross-municipality spillover effects predominate. The positive effect of low-level land reform on insurgency will only be followed by a negative marginal effect of current reform on insurgency when a certain threshold of prior reform has been reached.

Figure 2a shows the trend in land reform over time from 1960 to 2000. Land reform intensity peaked during the late 1960s and 1970s, declining after the 1973 Chicoral Pact. Reform again increased in the late 1980s and early 1990s, followed by a drop in the mid-1990s. Figure 2b presents the geographical distribution of land reform. The regions receiving the greatest levels of land reform were low-lying areas such as the Middle Magdalena river region in the north central part of the country, the southeastern areas of Caquetá, parts of the eastern plains, Urabá, and areas along the Pacific.

Control Variables

We include a number of control variables to account for the propensity for guerrilla activity and contestation in a municipality. First, we control for actions by government forces (Government Attacks) and paramilitary groups (Paramilitary Attacks). Actions that are considered offensive in nature by paramilitary groups are the same as those for the dependent variable of guerrilla actions. Government actions are

comprised of deactivation of explosives, dismantling of drug labs, capture of armaments, rescue or freeing of kidnap victims, anti-narcotic operations, and raids. To the extent that government attacks affect civilians in addition to guerrillas, land reform's counterinsurgent benefits may be undercut (Mason 1998; Wood 2003). Indeed, "reform with repression" of this sort in El Salvador undercut the effects of land reform to a greater extent than did guerrilla actions (Mason 1998).

As the state titles land to local peasants, military suppression of insurgent-supported protest and elite-led resistance to peasants through paramilitary groups may also be more likely. Therefore, as the literatures on repression and dissent (e.g., Davenport 1995; Francisco 1995; Moore 1998) imply that insurgency can be an aftereffect of counterinsurgency (Fearon and Laitin 2003), the arrival of state forces or paramilitary groups can spur greater guerrilla activity since rebels may act on behalf of peasants pushing for land. For instance, in the mid-1970s President López in Colombia promoted assistance to frontier colonist areas, defending his policy by citing the need for an "escape valve" for conflictive and problematic areas with guerrilla presence (Zamosc 1986). His actions were vigorously opposed by FEDEGAN and the agricultural producers' association (SAC), who went on to form paramilitary groups as defensive measures, spurring further conflict. An increase in paramilitary or government attacks in year t is therefore expected to increase guerrilla attacks in year $t + 1$. Lower levels of rebellion are only likely to ensue after guerrillas have been routed by sustained government attacks, an elusive result in the Colombian case during this period. Because of potential reciprocity between repression and dissent, we also used instrumental variables to examine endogeneity between guerrilla/paramilitary actions and government actions, but do not include the results here given that land reform was nearly uncorrelated with government and paramilitary actions during the period.⁹

An index of basic unmet needs calculated from the census is used as measure of socioeconomic status and poverty. This composite measure includes indicators for inadequate housing and public services, low income, and low rates of school attendance. Poverty can translate grievances into insurgent support through recruitment, given the lower opportunity costs for joining armed groups (Collier and Hoeffler 2004; Paige 1975). We therefore anticipate poverty to be linked to greater guerrilla activity.

A variable named Other Tenancy is also calculated from the census according to the percentage of households that do not formally rent or own their properties in a given municipality. A property is owned under "other tenancy" if a respondent lists their ownership status as "no information," "other situation," or "no payment." This variable may proxy for a larger landless or displaced population that not only lacks property rights but is also more difficult to organize and mobilize all else equal (see, e.g., Paige 1975), producing a negative correlation with insurgency. A Percent Minorities variable is included for the proportion of residents that identify as Indigenous or Afro-Colombian in a given municipality, since these groups have different landholding patterns, legal rights, and petition procedures for obtaining land as stipulated in the 1991 Constitution and Law 160/94.

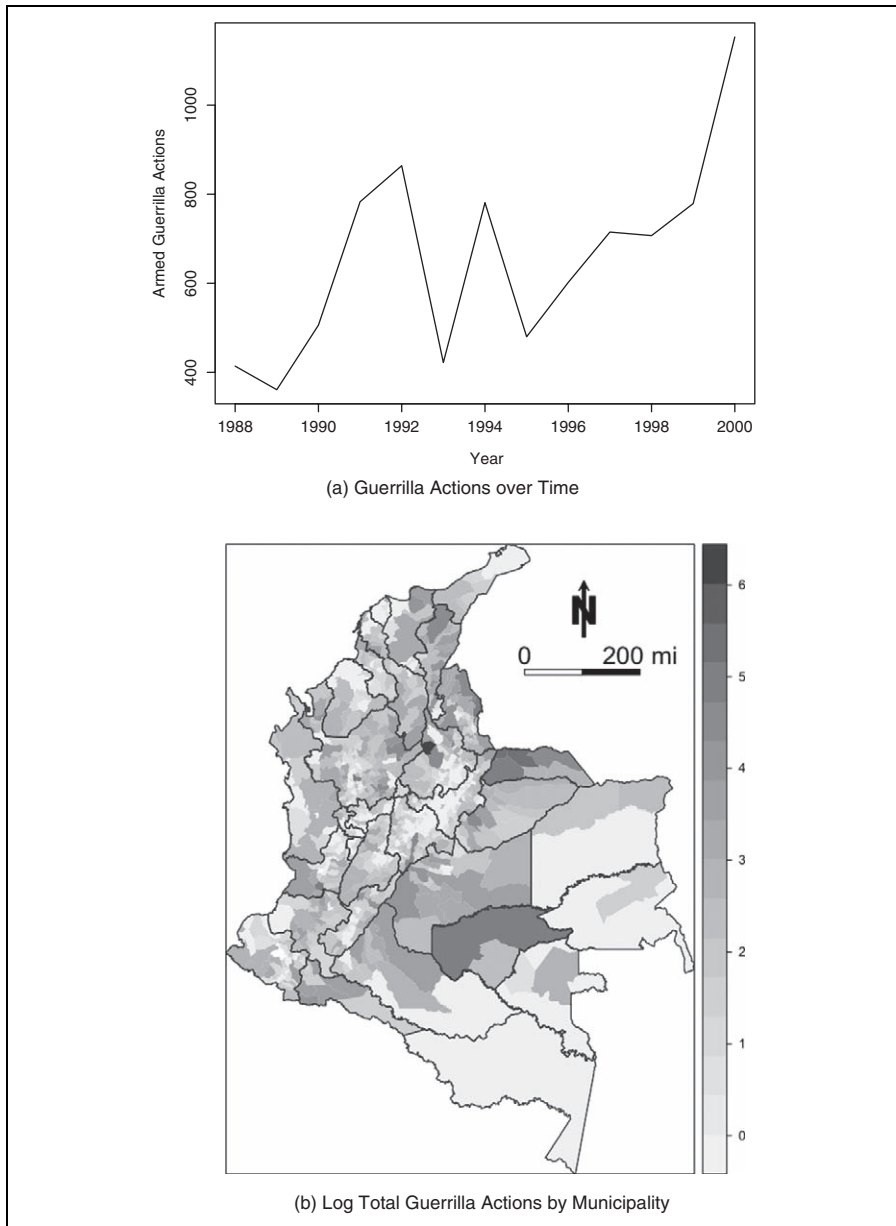


Figure I. Armed guerrilla actions in Colombia, 1988–2000.

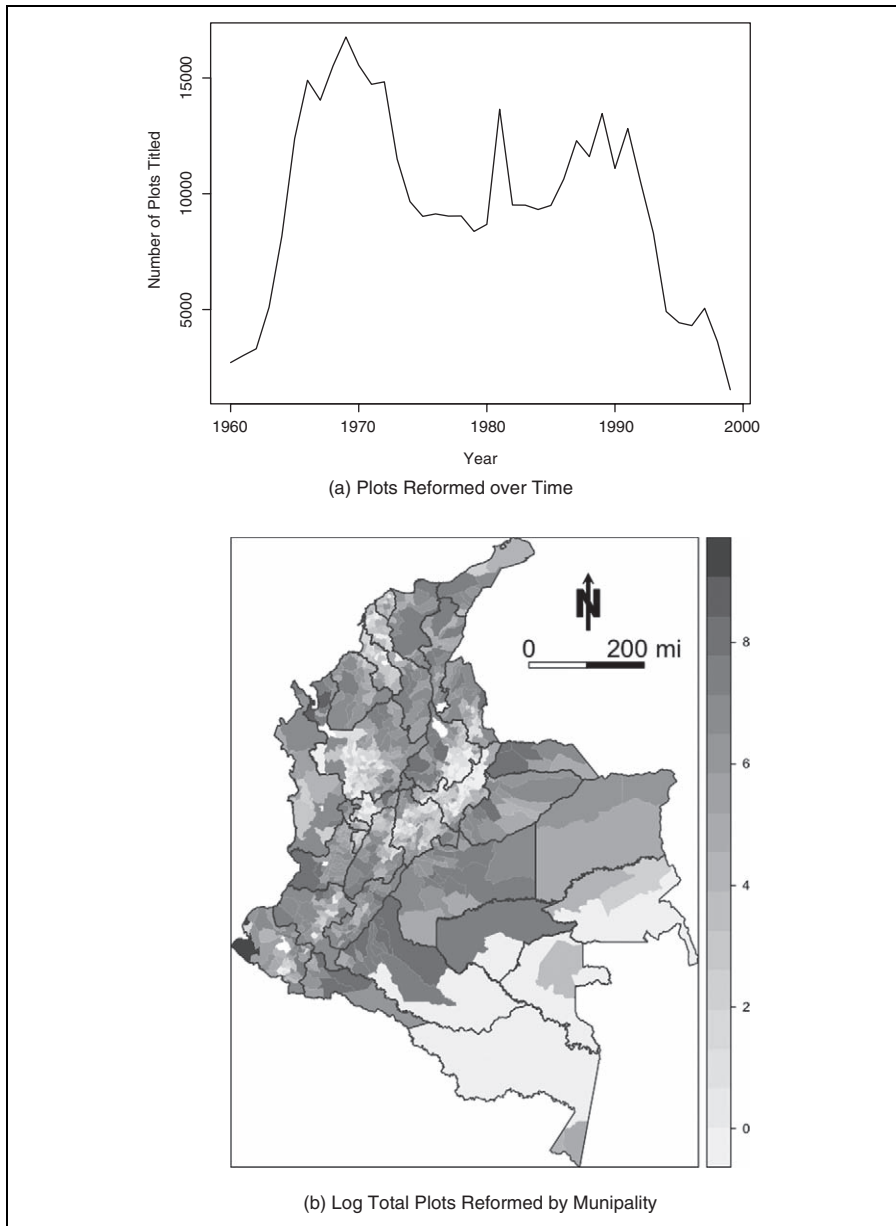


Figure 2. Land reform by INCORA in Colombia, 1960–2000.

The analyses include a measure of rough terrain since Fearon and Laitin (2003) find that rebel groups thrive where they can hide. Since Colombia is an extremely mountainous country, we measure rough terrain as the elevation above sea level of the county seat of each municipality (Departamento Administrativo Nacional de Estadística [DANE] 2000). However, because Colombia's mountainous regions were settled earlier and are under greater state control than low-lying jungle areas, we expect this variable to have a negative sign. Because lootable natural resources and illegal economies may provide greater motivation and opportunity for rebels to sustain themselves with less dependence on civilian support (Collier and Hoeffler 2004), we include a dummy variable that captures whether a municipality was a coca-growing region in 1994 (Echandía 1999) and expect a positive sign.

We also include a number of additional control variables. Population Density is included since higher population density may enable rebels to more easily hide among the population and present rebels with more targets.¹⁰ We therefore expect a positive sign. We also include the dummy variable New Colonized for whether or not a municipality was considered a colonization area since colonist centers are more likely to witness contestation for military control. In 1985, the Ministry of Agriculture designated these as "areas with directed or spontaneous settlements in the last 15 years (1970–85) without regard to property size" (Lorente, Salazar, and Gallo 1985). Finally, we control for guerrilla attacks in neighboring municipalities in some specifications, given potential spatial autocorrelation in insurgency that could arise from spillovers between municipalities. We expect greater insurgency in a municipality to be linked to increased insurgency in neighboring municipalities. Similar in construction to the Gleditsch and Ward's (2006) measure used to examine democratic diffusion, this variable is measured as the sum of guerrilla attacks in the previous period in neighboring municipalities whose seats are within a 100-kilometer radius of a municipality.

The summary statistics of the variables are presented in Table 1.¹¹

Empirical Results

To investigate the effect of land reform on guerrilla activity, we use a panel data set of the full set of 1,014 Colombian municipalities from 1988 to 2000. The analysis employs both negative binomial and tobit models to account for the censored nature of the data. Since guerrilla activity only takes on nonnegative values, using ordinary least squares (OLS) would result in biased coefficients.¹² The negative binomial models treat the number of guerrilla attacks in a municipality as a count variable. The tobit models use the log of guerrilla attacks as the dependent variable, which helps to normalize its distribution. For each set of models, we present results using department fixed-effects specifications.¹³ Year effects are included in all model specifications to account for time-varying trends in the data and common shocks.

There is reasonable concern that a positive land-conflict correlation could be observed as the result of endogeneity due to land reform targeting. If INCORA

Table 1. Summary Statistics.

Variable	Mean	SD	Min.	Max.	N
Guerrilla Actions	0.662	2.102	0	60	12,950
Plots Reformed	7.327	23.408	0	512	13,004
Plots Reformed 1960–85	265.401	558.249	0	6980	13,004
Cumulative Plots Reformed	329.721	657.033	0	8864	13,004
Paramilitary Attacks	0.013	0.168	0	8	13,002
Government Attacks	0.112	0.651	0	18	13,002
Poverty	0.544	0.196	0.017	0.999	12,304
Population Density	119.743	499.033	0.06	12280.529	12,637
Other Tenancy	0.21	0.119	0	0.99	12,221
Coca Region	0.106	0.307	0	1	12,688
New Colonized Region (1985)	0.078	0.268	0	1	13,056
Altitude (Ths. m.)	1.237	0.908	0	3.657	12,749
Percent Minorities (1985)	0.062	0.19	0	0.992	12,901
La Violencia	0.405	0.491	0	1	12,806
ANUC Raids 1971-78	0.897	2.885	0	38	12,857
Percent Land Overuse	0.34	0.264	0	1	12,829
Rehabilitation Zone (1985)	0.103	0.303	0	1	13,056
Area (Ths. km. sq.)	1.325	4.876	0	65.674	12,950
Land Inequality Gini (1985)	0.677	0.139	0	0.984	11,101

targets land reform to distant, isolated regions with high propensities for guerrilla activity or a history of prior challenging behavior, a positive effect of reform on insurgency may arise due to selection bias. Frontier colonist areas may be both targets of land reform and have coca cultivation, competition among armed groups, and banditry in the absence of the state justice system (e.g., Molano 1987). To account for potential endogeneity bias, all independent variables in the models presented are lagged by one period to ensure that they occur prior to the observed guerrilla activity. To further mitigate endogeneity concerns, the following analysis also employs alternative lags in the effect of reform on guerrilla activity, and subsequently uses propensity-score matching, a selection model that exploits data on the underlying propensity for violence, and an instrumental variables technique.

The Effect of Land Reform on Guerrilla Actions

The analysis shows a generally positive relationship between land reform and guerrilla activities. Table 2 presents both negative binomial and tobit models with department fixed effects. The standard errors in the negative binomial models are clustered at the municipal level to address heteroscedasticity and serial correlation. The first specification in each set of models includes the time-varying variables hypothesized in the literature to affect guerrilla activity. It also includes a fixed measure of the prior number of plots reformed in a municipality from 1960 to 1985. The second

Table 2. Land Reform and Guerrilla Actions in Colombia 1988–2000, Department Fixed Effects Models.

	Tobit							
	Negative binomial							
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Plots reformed		0.416*** (0.123)	0.384*** (0.122)	0.407*** (0.119)		0.228*** (0.066)	0.201*** (0.066)	0.226*** (0.065)
Prior plots	0.768*** (0.137)	0.665*** (0.112)	0.457*** (0.091)	0.466*** (0.090)	0.447*** (0.026)	0.370*** (0.024)	0.277*** (0.026)	0.291*** (0.025)
Paramilitary attacks	0.350** (0.145)	0.307** (0.130)	0.231*** (0.089)	0.247*** (0.078)	0.293*** (0.072)	0.279*** (0.071)	0.243*** (0.071)	0.285*** (0.070)
Government attacks	0.288*** (0.027)	0.280*** (0.025)	0.274*** (0.026)	0.266*** (0.025)	0.241*** (0.021)	0.236*** (0.020)	0.222*** (0.020)	0.219*** (0.020)
Poverty	0.737** (0.319)	0.647** (0.302)	0.070 (0.331)	0.200 (0.336)	0.653*** (0.108)	0.580*** (0.108)	0.207* (0.113)	0.333*** (0.114)
Population density	0.398** (0.194)	0.403** (0.182)	0.392** (0.184)	0.395** (0.187)	0.151*** (0.030)	0.154*** (0.030)	0.147*** (0.030)	0.135*** (0.029)
Other tenancy	-2.580*** (0.500)	-2.417*** (0.464)	-1.942*** (0.413)	-1.959*** (0.415)	-1.592*** (0.179)	-1.512*** (0.178)	-1.315*** (0.180)	-1.344*** (0.179)
Coca		0.356*** (0.117)	0.356*** (0.117)	0.397*** (0.119)		0.348*** (0.055)	0.348*** (0.055)	0.375*** (0.054)
New colonized		0.625*** (0.196)	0.628*** (0.196)	0.625*** (0.199)		0.467*** (0.062)	0.467*** (0.062)	0.447*** (0.062)
Altitude		-0.272*** (0.083)	-0.272*** (0.083)	-0.304*** (0.083)		-0.091*** (0.028)	-0.091*** (0.028)	-0.113*** (0.028)
Percent minorities		0.057 (0.528)	0.057 (0.528)	0.067 (0.499)		-0.044 (0.157)	-0.044 (0.157)	-0.013 (0.157)
Neighbor violence		0.878*** (0.137)	0.878*** (0.137)	0.878*** (0.137)		0.743*** (0.076)	0.743*** (0.076)	0.743*** (0.076)
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,709	11,709	11,640	11,640	11,709	11,709	11,640	11,640
Municipalities	964	962	955	955	964	962	955	955

Note. Tobit models use log attacks. Standard errors in parentheses, clustered by municipality for negative binomial models. Constants and time dummies are not shown. All independent variables lagged by one period.
 * $p < .10$. ** $p < .05$. *** $p < .01$ (two-tailed).

specification in each set includes a time-varying measure of current land reform and modifies the measure of prior plots to include all plots up to the current year so that it varies in municipalities that experienced land reform in the period 1988–2000. The third model in each set additionally includes a series of time-invariant variables related to guerrilla attacks. The final model in each set includes a measure for guerrilla attacks in neighboring municipalities to account for potential spatial autocorrelation in insurgent activities.

Across all of the Table 2 models, land reform is consistently positively related to higher levels of guerrilla activity, whether measured as the number of plots reformed in the previous year or as the sum of prior plots titled between 1960 and the year prior to the present. An increase of fifty plots reformed in the previous period (2 *SDs*) produces an estimated 21 percent increase in guerrilla attacks using the Model 3 coefficient. Moving across half the range of the plots reformed variable (increasing by 250 plots) results in slightly over a 100 percent increase in attacks. The stock of total prior plots reformed in a municipality is also positively linked to more guerrilla attacks. Using the Model 3 coefficient, a two standard deviation increase in prior plots reformed yields an estimated 66 percent increase in guerrilla attacks. In the tobit models, the estimated effects of land reform on rebellion are similar though slightly lower.

To investigate whether guerrilla activity in a municipality may be influenced by spillovers of land reform in surrounding municipalities in the previous period, we tested (but do not include in the results presented here) a spatial lag of land reform. The coefficient of the spatial lag of reform was generally negative, and since it was positively correlated with reform in a municipality its inclusion increased the magnitude of the coefficient for Plots Reformed. This suggests that local cross-municipality spillover effects of land reform on guerrilla activity are not driving the results. Spillover effects may therefore occur locally within municipalities or operate nationally rather than regionally, for instance, if expectations of reform are built up as peasants learn of other regions receiving reform while their own region is largely ignored. National or regional spillovers would suggest that guerrillas are not only connected through contiguity but also through other means such as communication or media and that the interactions of some especially important guerrilla cells with the state and peasants could more broadly resonate through the movement.

Paramilitary and government actions in Table 2 are both consistently associated with increased guerrilla activity. Paramilitary attacks against guerrilla groups lead to resistance on the part of these groups in the form of more attacks. Similarly, government actions elicit retaliation from guerrillas, at least in the short term when government forces cannot completely eliminate guerrillas in an area.

Guerrilla activities are consistently greater in more densely populated areas and where poverty rates are higher. Consistent with the literature on rebellion, densely populated areas may enable rebels to hide amongst the local population and may present more targets. The link between poverty and insurgency is consistent with grievance explanations of civilian support for guerrillas. A one standard deviation

increase in poverty is associated with an estimated 14 percent increase in guerrilla activities in Model 2. However, poverty loses its statistical significance in the negative binomial models that include Coca, a dummy for colonization zones, and percent minority population. These latter variables are all positively correlated with poverty. The proportion of residents classified as Other Tenancy consistently has a negative and statistically significant effect on guerrilla activity. Consistent with Paige (1975), this variable likely represents areas where there are larger landless populations that can be more difficult to organize than poor smallholders, leading to weaker guerrilla support.

Altitude is strongly negatively linked to guerrilla activities in Table 2. Mountainous areas in Colombia generally experience less guerrilla activity because of greater state presence. Guerrillas in Colombia have also long found security in lowland jungles outside of the Andean population centers. While this finding is consistent with the theoretical argument on rough terrain made by Fearon and Laitin (2003), it suggests that the measure of rough terrain used in some cross-national studies of civil war may be subject to considerable measurement error.

Coca-growing regions are strongly associated with higher rates of guerrilla activity, consistent with Collier and Hoeffler (2004). Revenues from illegal coca cultivation enable rebels to sustain operations with less dependence on civilians. Colonist centers are also more likely to suffer from guerrilla attacks and contestation for control between guerrillas and government forces. A greater presence of ethnic minorities has no measureable effect on guerrilla activity, consistent with null findings in the literature (e.g., Fearon and Laitin 2003). Finally, Models 4 and 8 find that guerrilla activities in a given municipality are strongly affected by insurgency in neighboring municipalities, suggesting spillovers in insurgency. Accounting for spillovers in insurgency increases the land reform coefficients.

In sum, land reform is consistently *positively* related to higher levels of guerrilla activity in Table 2, whether measured as the number of plots reformed in a municipality in the previous year or as the sum of prior plots titled between 1960 and the year prior to the present.

Robustness: Endogeneity Bias in Reform

Could the observed positive relationship between land reform and guerrilla activity in Table 2 be a result of INCORA targeting land reform at areas that have a higher underlying propensity for insurgency? If certain municipalities are more likely to experience insurgency, they may be chosen to receive reform to undercut increasing rebel strength, possibly introducing endogeneity running from guerrilla activity to land reform. Table 2 accounts for this both by estimating models in which the land reform measure is temporally prior to guerrilla activity and by lagging land reform at different intervals. Endogeneity bias may persist, however, if there is an upward trend in insurgency for reasons not related to reform that nonetheless lead INCORA to direct reform at these areas as a longer-term pacification strategy. This section

addresses potential endogeneity in three different ways: propensity-score matching, exploiting data on municipalities that the government identified as prone to violence, and instrumental variable estimation. The results of these three techniques for estimating the effect of land reform on insurgency all indicate that higher levels of active reform are still associated with greater rebellion.

Propensity-score matching can reduce selection bias in receiving reform using a set of observable covariates to estimate the likelihood that a given individual or unit is assigned to the “treated” or “untreated” group. Each “case” receives a propensity score for treatment, and cases in the treatment group (those receiving land reform) are then matched to cases in the control group (those not receiving land reform) based on the closeness of their propensity score. The treatment effect is estimated as the difference in mean values on the dependent variable between the treatment and control group matches. This technique helps overcome endogeneity problems by analyzing subgroups of observations that are strictly comparable. We apply it here to compare how land reform treatment affects the rates of guerrilla activity in municipalities with similar observable characteristics.

We employ propensity-score matching first using municipalities and then municipality-years as the unit of analysis. For both analyses, land reform “treatment” is indicated by at least 300 plots reformed from 1988 to 2000, and at least three years with fifty or more plots reformed. Municipalities with this level of reform (a total of 74) were actively receiving significant reform during this period.¹⁴ The cross-sectional matches of municipalities sum guerrilla attacks, prior plots, paramilitary actions, and government actions over the period and take other variables at their means.

The probit estimation of the propensity scores based on observable covariates indicated that land reform may indeed be targeted at areas with high underlying propensities for violence. A history of previous reform as measured by the prior number of plots reformed as well as government and paramilitary military operations were positively related to current land reform treatment.

The estimated treatment effects of land reform on guerrilla activity are presented in panel A of Table 3. The results are similar to those found in Table 2: land reform is positively and statistically significantly associated with guerrilla actions. The first row of panel A indicates that municipalities “treated” with land reform during this period experienced an estimated increase of 34.6 attacks over untreated municipalities with similar observable characteristics. Given an average of approximately nine guerrilla attacks per municipality from 1988 to 2000, this represents an estimated fourfold increase in attacks. The second row of panel A presents propensity-score matching results when matching on municipality-year units rather than municipalities. The estimated effect of land reform on guerrilla activity in row 2 of Table 3 is again positive and statistically significant. Municipalities receiving land reform treatment had an estimated 1.4 additional guerrilla attacks per year, a 210 percent increase over the average of .66 attacks per municipality-year. In sum, our matching analyses support the main Table 2 results that land reform is generally associated with higher levels of guerrilla activity.

Table 3. Land Reform and Guerrilla Actions, Propensity Score Matching on Municipality and Restricting to Rehabilitation Zones.

Panel A: Propensity Score Matching				
Unit of matching	ATT estimate	SE	Observations	Treated obs.
Municipality	34.574***	11.683	958	65
Municipality-year	1.396**	0.6084	11641	754
Panel B: Restricting to rehabilitation zones				
Unit of Matching	Estimate	SE	Observations	Controls
Municipality	1.6207***	0.3434	106	No
Municipality	0.6030**	0.2518	99	Yes
Municipality-year	1.6910***	0.4518	1,326	No
Municipality-year	0.6682**	0.2657	1,203	Yes

Note. Treatment indicates at least 300 plots reformed from 1988 to 2000 with at least three years of 50 plots or more reformed. Panel A estimates are based on propensity scores derived from probit estimates, restricting to common support. Standard errors are calculated using bootstrapping. Municipality matching is done with prior plots, paramilitary attacks, and government attacks summed over the period, and other variables (those remaining in Model 3 of Table 2) taken at their means. Panel B municipality analysis also uses these controls. Panel A municipality-year matching, and Panel B municipality-year analyses use controls as specified in Model 3 of Table 2.

* $p < .10$. ** $p < .05$. *** $p < .01$ (two-tailed).

One drawback to propensity-score matching is that it may not completely eliminate selection bias if there are unobserved factors that both influence the likelihood of treatment and are correlated with the dependent variable (Becker and Ichino 2002). Fortunately, the Colombian Ministry of Agriculture conducted a study of guerrilla activity in the mid-1980s to determine which regions were most subject to violence. One hundred twenty municipalities were designated as “rehabilitation zones” in need of additional resources and programs such as land reform (Lorente, Salazar, and Gallo 1985). Given similar underlying propensities for insurgency within these rehabilitation municipalities, we can compare outcome levels of insurgency in municipalities that received land reform “treatment” with those that did not.

Panel B of Table 3 presents a set of negative binomial models that estimate the effect of land reform treatment on guerrilla attacks among rehabilitation municipalities. The models use the “treatment” cutoff and units of analysis as in panel A. The results are consistent with the matching exercises above where land reform is positively associated with increased guerrilla attacks. Land reform “treatment” in row 4 is associated with slightly more than double the expected number of guerrilla attacks (the increase is .668), an effect similar to that in panel A.

Both propensity-score matching and the use of government designated rehabilitation zones help account for nonrandom selection into land reform “treatment.”

It is possible, however, that the relationship between land reform and conflict runs in both directions. Land reform may cause insurgency through one of the mechanisms discussed above, and insurgency or the propensity for insurgency may lead certain areas to be targeted with land reform. To account for this possibility, the final strategy to address endogeneity uses instrumental variables. We construct a two-stage simultaneous equations model with instruments for both violence and land reform. We estimate first-stage models of both of the endogenous regressors and calculate their predicted values. If the instruments are exogenous, then what remains are measures of the endogenous regressors that strip away the reverse causation and produce unbiased estimates in the second-stage equations.

Model 1 of Table 4 presents a simple OLS estimation of the effect of land reform on guerrilla activity without accounting for potential endogeneity. Both of these variables are logged in Table 4 to normalize their distribution. The prior plots measure included here is the fixed number of plots reformed from 1960 to 1985. The results are similar to those in Table 2. Models 2 and 3 display the first-stage estimation results in which first land reform and then guerrilla attacks are instrumented. We use both the land area of a municipality and the percentage of “overused” land according to the Colombian geography agency (IGAC) as instruments for land reform. A larger municipality indicates a larger number of landholders on average and therefore an increased likelihood of reform. There is no obvious reason why larger municipalities should have experienced more rebellion except through their varying likelihood of receiving land reform, and indeed, Figure 2 displays a heterogeneous mix of large and small municipalities that both witnessed guerrilla activity and were largely peaceful. Land overuse in some areas of a municipality can lead INCORA to target undeveloped or marginal adjacent areas for reform. However, land overuse does not appear to be linked to insurgency except through reform. Although there are high rates of overuse in both the Andes and the northern departments such as Santander and Norte de Santander, the latter have experienced relatively more guerrilla attacks, a difference that more likely resides in land reform policies than overuse itself.

As instruments for insurgent activity, we use a dummy variable for whether a municipality was affected by La Violencia in the 1950s (Guzmán, Borda, and Umaña 1963) as well as a count of land invasions by ANUC in the 1970s (Zamosc 1986). Previous violence and unrest as indicated by these variables are only likely to affect land reform in an area through the channel of an increased propensity for insurgency. That they would have directly affected land reform since 1988 seems implausible, given the time elapsed between these periods and the decline of ANUC. All of these instruments are added as exogenous regressors in the first-stage estimations of Models 2 and 3. La Violencia and ANUC raids are both strongly linked with subsequent guerrilla attacks in Model 2, and land area and land overuse strongly predict land reform in Model 3.¹⁵

Models 4 and 5 present the second-stage IV estimates of both guerrilla attacks and land reform. The coefficient for Plots Reformed in Model 4 is positive and

Table 4. Land Reform and Guerrilla Actions, IV Estimates with Department Fixed Effects.

	OLS				
	Model 1	Stage 1: Attacks Model 2	Stage 1: Land Model 3	IV: Attacks Model 4	IV: Land Model 5
Log Guerrilla actions					
Log plots reformed	0.025*** (0.007)			0.338*** (0.069)	0.189 (0.415)
Log prior plots (1960–85)	0.041*** (0.005)	0.041*** (0.005)	0.170*** (0.011)	-0.018 (0.013)	0.162*** (0.020)
Paramilitary attacks	0.206*** (0.048)	0.201*** (0.052)	0.037 (0.073)	0.184*** (0.061)	-0.001 (0.111)
Government attacks	0.175*** (0.023)	0.167*** (0.022)	-0.095*** (0.024)	0.199*** (0.021)	-0.124* (0.074)
Poverty	-0.092 (0.089)	-0.127 (0.084)	0.016 (0.152)	-0.126 (0.087)	0.012 (0.153)
Population density	0.066 (0.054)	0.070 (0.055)	-0.027 (0.018)	0.080 (0.054)	-0.038 (0.034)
Other tenancy	-0.256*** (0.077)	-0.268*** (0.075)	-0.629*** (0.177)	-0.040 (0.100)	-0.616*** (0.207)
Coca	0.143*** (0.053)	0.134*** (0.051)	0.233*** (0.087)	0.051 (0.060)	0.216*** (0.101)
New colonized	0.198*** (0.065)	0.194*** (0.065)	0.186** (0.086)	0.125* (0.073)	0.137 (0.124)
Altitude	-0.030* (0.018)	-0.019 (0.017)	-0.049 (0.034)	-0.005 (0.020)	-0.043 (0.036)
Percent minorities	-0.097 (0.126)	-0.198 (0.129)	-1.155*** (0.246)	0.206 (0.138)	-1.097*** (0.248)
La Violencia		0.095*** (0.023)	-0.061 (0.045)	0.118*** (0.027)	
ANUC raids 1970s		0.012** (0.006)	0.016 (0.011)	0.007 (0.006)	
Area		0.035*** (0.008)	0.112*** (0.017)		0.106*** (0.023)
Percent land overuse		0.077** (0.037)	0.187*** (0.071)		0.187*** (0.079)
Year Effects	Yes	Yes	Yes	Yes	Yes
Observations	10,694	10,646	10,646	10,646	10,646
Municipalities	955	951	951	951	951

Note: Standard errors clustered by municipality in parentheses. Constants and time dummies are not shown. All independent variables lagged by one period. * $p < .10$. ** $p < .05$. *** $p < .01$ (two-tailed).

highly statistically significant, indicating that land reform is positively associated with guerrilla attacks. This finding parallels the results in Tables 2 and 3. The coefficient for plots reformed is larger in the two-stage model than in Model 1, which indicates that accounting for endogeneity strengthens the main result. By contrast, the coefficient for prior plots in Model 4 is no longer significant, suggesting that it is not the stock of prior reform in a municipality that is linked to higher guerrilla activity but rather recent and ongoing reform. The Model 5 results present the IV estimates of plots reformed. The coefficient for guerrilla actions, as measured by the log number of guerrilla attacks, is positive but not significant, indicating that guerrilla activity is not robustly linked to increased land reform. The significance of prior plots in this model suggests that areas of previous reform are likely to receive additional reform in the future.

In short, a variety of techniques used to address concerns of endogeneity bias running from insurgency to land reform suggest that the main results presented in Tables 2 and 3 hold: ongoing land reform is linked to increased guerrilla activity.

“Fighting Fires”: Testing the Effect of Intense Reform on Guerrilla Activity

Is it possible that low levels of land reform engender insurgency, but that insurgency is dampened at higher levels when the government targets significant resources toward reform? Zamosc (1986, 36) argues that INCORA dedicated some of its scarce resources to “fighting fires” where there were signs of developing or sustained conflict. To address violence arising from land conflict and disputes over ownership in a region, INCORA may have had to dedicate resources over a significant period of time to suppress guerrilla expansion. The insignificant relationship between the stock of prior land reform in a municipality and rebellion in the Table 4 IV estimates suggests that while short-term reform may have a positive effect on guerrilla activity, sustained large-scale reform may reduce insurgency or at least have a neutral effect.

Table 5 explores this possible paradox of partial reform in greater detail, introducing an interaction between the sum of plots previously reformed in a municipality with the number of current plots reformed. This measure taps the marginal effect of an additional plot of land reformed in areas where land reform intensity has been high in the past. Models 1 through 4 of Table 5 follow Models 2 and 3 in Table 2, but also include the interaction term.¹⁶ Models 5 and 6 limit the sample to rehabilitation zones. Finally, Models 7 and 8 present the second-stage IV estimates that instrument land reform with land area and land overuse as discussed in the context of Table 4.

The interaction term between current and prior plots reformed is negative across all Table 5 models and statistically significant in all but Model 6, demonstrating that where INCORA dedicated significant, sustained resources to land titling and reform in a municipality, *additional reform had a negative impact on guerrilla activity*. Using the Model 4 coefficients, the marginal effect of fifty additional plots reformed in a municipality becomes negative when the number of prior plots reformed reaches

Table 5. Intense Land Reform and Guerrilla Actions in Colombia 1988–2000.

	RE NegBin models			Dept FE NegBin models			Rehabilitation zones			Stage 2		
	Dept FE NegBin models			Dept FE NegBin models			Dept FE NegBin models			Dept FE IV estimates		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9	Model 10	Model 11	
Plots reformed	0.323*** (0.074)	0.281*** (0.075)	0.957*** (0.167)	0.854*** (0.153)	0.941*** (0.338)	0.814** (0.354)	2.005*** (0.383)	1.532*** (0.340)				
Prior plots	0.408*** (0.060)	0.260*** (0.063)	0.750*** (0.114)	0.542*** (0.089)	0.674*** (0.117)	0.477*** (0.123)	0.049 (0.071)	0.058 (0.067)				
Plots × Prior plots	-0.069*** (0.019)	-0.069*** (0.020)	-0.367*** (0.057)	-0.309*** (0.042)	-0.414*** (0.170)	-0.344* (0.181)	-0.227*** (0.040)	-0.197*** (0.037)				
Paramilitary attacks	0.038 (0.042)	0.027 (0.042)	0.300** (0.127)	0.229** (0.093)	0.571*** (0.167)	0.443*** (0.169)	0.186*** (0.050)	0.208** (0.055)				
Government attacks	-0.003 (0.012)	-0.009 (0.012)	0.274*** (0.028)	0.269*** (0.030)	0.056 (0.042)	0.033 (0.042)	0.197*** (0.022)	0.200*** (0.024)				
Poverty	0.708*** (0.195)	0.262 (0.210)	0.622** (0.312)	0.061 (0.337)	-0.932 (0.796)	-0.873 (0.754)	-0.069 (0.077)	-0.157* (0.089)				
Population density	0.168** (0.074)	0.201** (0.083)	0.412** (0.194)	0.401** (0.196)	10.982** (4.952)	11.760*** (3.787)	0.070 (0.056)	0.066 (0.057)				
Other tenancy	-1.877*** (0.311)	-1.322*** (0.319)	-2.365*** (0.477)	-1.906*** (0.428)	-0.371 (0.801)	-0.017 (0.818)	-0.162** (0.082)	-0.158** (0.078)				
Coca		0.605*** (0.119)		0.358*** (0.121)		0.172 (0.326)		0.067 (0.057)				
New colonized		0.416*** (0.130)		0.596*** (0.192)		0.590** (0.268)		0.183*** (0.065)				
Altitude		-0.162*** (0.054)		-0.264*** (0.086)		-0.290 (0.207)		-0.029 (0.019)				
Percent minorities		-0.242 (0.246)		0.089 (0.582)		-5.659* (3.063)		0.062 (0.134)				
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes				
Observations	11709	11640	11709	11640	1229	1203	10720	10658				
Municipalities	962	955	962	955	101	99	959	952				

Note. Standard errors in parentheses, clustered by municipality in Models 3 through 8. Constants and time dummies are not shown. All independent variables lagged by one period.

* $p < .10$. ** $p < .05$. *** $p < .01$ (two-tailed).

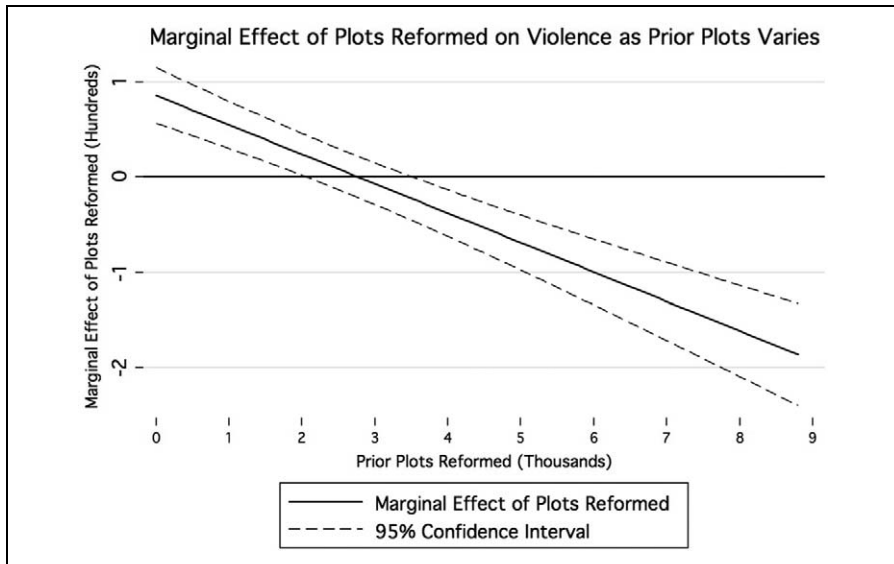


Figure 3. Marginal effect of plots reformed on guerrilla actions.

roughly 2,800 (or 2,300 in Models 5 and 6). Given an average of about 5,400 households per municipality during the period, this represents titling nearly half the plots in a municipality. At the same time, the marginal effect of 1,000 additional prior plots reformed in a district turns negative when the number of current plots reformed surpasses 175. However, areas above these thresholds where negative effects of land reform on guerrilla activity are found comprise roughly only 5 percent of all municipalities that experienced at least some land titling using the Model 4 coefficients (7.4 percent of municipalities in Models 5 and 6). Figure 3 graphs the marginal effect of current land reform on guerrilla activity as prior plots reformed varies.

The municipalities that reached the predicted threshold level of land reform after which additional reform diminishes insurgency are geographically dispersed in over half of Colombia’s departments. A number of them experienced significant amounts of guerrilla activity from 1988 to 2000, and most had higher than average poverty rates. For instance, some municipalities in the northeastern department of Arauca experienced sustained land reform sufficient to reduce guerrilla activity. INCORA’s 1964 Arauca Project #1 reformed over 200,000 hectares of land. In the municipality of Tame, 2,776 plots were reformed from 1960 to 1988. The project attracted colonists from other parts of Colombia who were given land titles by INCORA and then assisted with development and road-building to aid in transporting agricultural products to market.

The Arauca region nonetheless experienced guerrilla attacks by the left-wing FARC in the 1980s, who then met resistance by right-wing paramilitaries. INCORA

redoubled its land reform efforts, titling over 1,100 plots in Tame from 1988 to 2000. Guerrilla attacks in Tame decreased after heavy land reforms in the early 1990s. The reforms were sustained as a large latifundio was repartitioned to 350 squatting families when its owner died in 1997 (*El Tiempo* 1999). Over the decade, guerrilla actions in Tame rose considerably less than the near tripling of attacks at the national level and in the nearby municipality of Saravena, which, partly for bureaucratic reasons, experienced (relatively) little land reform in the 1990s. While Saravena was also initially colonized and substantially titled during the Arauca Project #1, titling of its Sarare subregion stalled during the 1990s because the departmental manager of INCORA could not get sufficient attention from the national government, frustrating ANUC and other peasant organizations (*El Tiempo* 1992).

The findings have several implications. First, a high intensity of land reform is required to reduce guerrilla activity. Prior land reform will do little if not sustained and new, incremental land reforms will have little immediate impact if guerrillas are already strong. If less reform was done in the past, it takes more reform in the present to reduce guerrilla activity, and vice versa. This lends some support to relative deprivation theory: whereas land reforms can help address grievances, symbolic gestures will not fix underlying problems. If expectations of grievance alleviation are built up (as by INCORA in the 1960s) with little material change, the result will be more rather than less rebellion.

Robustness: Considering Reporting Biases

Although Tables 2 through 5 link land reform to greater guerrilla activity, there are valid reasons to be concerned about potential reporting biases in the government data—both intentional (e.g., manipulation) and unintentional (e.g., poor reporting quality)—that could confound inferences. Indeed, Colombian government forces have been accused of human rights abuses and collusion with paramilitaries and drug traffickers. In addressing possible reporting biases, the most relevant questions are: (1) How severe are issues of discrepant reporting? (2) What type of bias would be problematic for our results? and (3) How sensitive are the results to different assumptions about reporting biases? We discuss each of these in turn.

One technique for estimating the true values of a phenomenon that possibly suffers from hidden cases or misreporting is multiple systems estimation (MSE; see, e.g., Lum et al. 2010). MSE ideally requires at least three independent data event-level sources. Unfortunately, complete event-level data on guerrilla activity from three independent collections spanning this period and with full country coverage are not available for this analysis. We are therefore limited in our ability to assess the true severity or pattern of potential reporting biases. However, we take several additional steps to shed further light on the quality of our data, the nature and degree of bias that would be problematic for our inferences, and the sensitivity of the results to different assumptions about possible reporting bias, as summarized in Table 6.

Table 6. Summary of Additional Robustness Tests and Evidence on Data Reporting.

Method/Evidence	Purpose	Findings/Conclusions	Source
Robustness tests of “publicly visible” guerrilla actions	Address potential reporting bias; more visible events less subject to misreporting	Main results robust to analysis of the more visible guerrilla actions	OVP data
Guerrilla presence figures from human rights monitors	Provide an independent estimate of the reference population of conflict events	Estimates of aggregate guerrilla presence with government data similar to that estimated by Census of Personerías	National Census of Personerías
Correlation of OVP and CINEP data	Provide a sense of overlap between data and compare closeness	OVP and CINEP data highly correlated	OVP data; CINEP data
Factor analysis of OVP and CINEP data	Determine whether OVP and CINEP data capture same underlying dimension of actions	OVP and CINEP capture same underlying concept; provides justification for using CINEP as alternative to OVP	OVP data; CINEP data
Overlap analysis	Determine municipality-years where OVP and CINEP diverge most	Divergence not linked to land reform variables; results robust to splitting by high and low divergence predictors	OVP data; CINEP data
Robustness tests using municipal diff-in-diff models and dept. × year fixed effects	Determine what type of bias in guerrilla actions would be problematic	Main results robust; bias must be municipal-year level, time-varying, deviate from dept.-level yearly trends, be correlated with yearly changes in land reform for it to be problematic ^a	OVP data
Sensitivity analyses based on OVP and CINEP overlap	Examine results’ sensitivity to dropping observations with least overlap that may suffer misreporting bias	Main results robust to excluding observations most subject to reporting bias based on poor overlap	OVP data; CINEP data
Regression calibration	Address the possibility of an omitted variable that predicts bias in reporting	Main results robust to including reporting bias variable measured with error	OVP data

(continued)

Table 6. (continued)

Method/Evidence	Purpose	Findings/Conclusions	Source
Interviews with government officials	Provide information on reporting from practitioners	Manipulation or failure to report was punished	Military field intel officers
Case study evidence from Arauca	Provide deeper discussion of case municipalities	Strong qualitative evidence consistent with proposed mechanisms in manuscript (see the Appendix)	Carroll (2011); <i>El Tiempo</i>

Note. OVP data are Colombian government data on guerrilla actions from the Office of the Vice Presidency and Sánchez Torres (2007). CINEP data are conflict data from the Jesuit think tank CINEP based in Bogotá. Further details on specific tests and evidence can be found in the Appendix.

^aThe type of bias that could be problematic for the finding of a positive effect of current and prior land titling on guerrilla actions is therefore quite restrictive: municipality yearly varying biases across both government and CINEP sources that result in increases in overreporting where yearly changes in land reform are positive (or more underreporting where yearly changes in land reform are negative).

Greater detail on reporting procedures and processes and additional robustness tests are found in the Appendix.

First, since some kinds of guerrilla activities are more publicly visible than others and therefore less likely to be susceptible to bias or manipulation (e.g., explosive attacks, incendiary attacks, attacks against installations, and illegal roadblocks), we conducted several robustness tests restricting the data to these visible events. The results were similar. Second, we obtained data from the 1993 National Census of Personerías, which provides an approximation of the true “reference population” of conflict events. Personerías are government employees present in every town that monitor conflict conditions and record and report cases of victimization. The government data on guerrilla presence used here coincide closely with the aggregate census results (see Echandía 1999). Third, we obtained comparable municipal-level conflict data compiled independently by CINEP, a Jesuit think tank based in Bogotá. CINEP codes news stories from twenty national and regional press sources, which are verified with local church officials. CINEP uses similar coding categories and definitions for guerrilla, government, and paramilitary actions to those of the government. Models specified similarly to those in Table 2 but that use CINEP data on guerrilla, paramilitary, and government actions yield similar results to those based on government data sources.

The “visible” events analyses, Personería data, and CINEP analyses help add confidence to our findings, but they provide few clues about possible reporting biases. To better understand these potential biases given the possibility of different reporting incentives and objectives across sources (Davenport and Ball 2002), we conduct several overlap analyses that compare the government and CINEP data. A total of 74 percent of municipality-years have perfect overlap, where both sources

report the same number of guerrilla actions. A full 88 percent of municipality-years differ by at most one guerrilla action, and 96 percent differ by three or fewer actions.

Overlap in the data sets could occur for two principal reasons. First, the two data sets could each partially capture the underlying true distribution of the data. This could occur for several reasons, such as CINEP media sources focusing on larger or more controversial events (threshold effects), or dropping coverage of events as interest wanes or costs rise (media fatigue effects; see McCarthy, McPhail, and Smith [1996] and Davenport [2007], respectively). At the same time, government data may have inaccuracies due to incentives to avoid politically sensitive issues or accomplish alternative aims (Davenport 2007). Second, both data sets could be biased for similar reasons such as restricted access to hard-to-reach regions, limited resources for data collection in violent areas, or government manipulation that is replicated in press reports (see, e.g., Davenport 2007; Lum et al. 2010). Common biases would cause the reported values of both data sets to diverge from the “true” event counts, either for a subset of observations or, in the worst case, for the full data set. We address these possibilities in turn.

To determine the characteristics of the municipality-years where these sources diverge most and may suffer either different biases or different degrees of the same bias, we use the land reform variables and covariates to predict *differences* in the reported number of guerrilla actions between the government and CINEP data. These models focus on municipality-years in which one of these sources reported non-zero amounts of guerrilla activity, since high overlap could in part be driven simply by guerrilla absence in certain regions.

The results in Table 7 indicate the differences between the government and CINEP data are not statistically significantly correlated with the land reform variables. This implies that any biases reflected by the differences between the sources would not affect the coefficients of the land reform variables or the inferences that we draw. Our results are also robust to splitting the sample by the predictor variables of poverty and government attacks that were found to explain divergence between the data sets.

There could still be a threat to inference, however, if the government and CINEP data share common biases in the same direction. We therefore conduct several analyses of the nature and degree of bias that would be problematic for our inferences. The models in the preceding analyses include department and year fixed effects to help account for any department- or time-specific reporting inconsistencies. Additional tests yield similar results using difference-in-difference models at the municipal level and including department \times year fixed effects (see the Appendix). The type of bias that could be problematic for the finding of a positive effect of current and prior land titling on guerrilla actions is therefore quite restrictive: municipality yearly varying biases across both government and CINEP sources that vary differently from department-level yearly trends and that result in increases in overreporting where yearly changes in land reform are positive (or more underreporting where yearly changes in land reform are negative).

Table 7. Overlap Analysis of OVP and CINEP Data.
(Dependent Variable: OVP-CINEP Guerrilla Actions).

	Model 1	Model 2
Plots reformed	-0.213 (0.313)	-0.139 (0.334)
Prior plots	0.346 (0.225)	0.267 (0.220)
Paramilitary attacks		0.610 (0.467)
Government attacks		0.643*** (0.151)
Poverty	-3.220** (1.375)	-3.482** (1.454)
Population density	0.345 (0.426)	0.290 (0.410)
Other tenancy	-1.491 (1.038)	-1.408 (0.975)
Coca	0.205 (0.346)	0.109 (0.347)
New colonized	1.665* (0.864)	1.628* (0.873)
Altitude	-0.648* (0.351)	-0.716* (0.370)
Percent minorities	-0.218 (0.578)	-0.241 (0.569)
Year effects	Yes	Yes
Dept. Fixed Effects	Yes	Yes
Observations	3,123	3,123
Municipalities	703	703

Note. Standard errors clustered by municipality in parentheses. Constants and time dummies are not shown.

* $p < .10$. ** $p < .05$. *** $p < .01$ (two-tailed).

We finally conducted several sensitivity analyses to examine how stable the results are to different assumptions about possible reporting bias. First, we estimated a series of regressions with the samples increasingly restricted to only those municipality-year observations with high levels of overlap between government and CINEP data—observations that may be less subject to bias. The principal results were robust even excluding observations where government and CINEP data differed by more than one reported guerrilla action. Second, we considered the possibility of an omitted variable (incentives or tendencies to misreport) that predicts bias in the reporting of guerrilla actions. We introduced proxies for these incentives or tendencies to misreport that reasonably suffer from measurement error and then used regression calibration (see Carroll et al. 1995) to estimate the resulting measurement error models. The land reform variables remained positive and significant predictors of guerrilla actions across the range of measurement error in our proxies for reporting bias.

Conclusion

Existing evidence and theory on the relationship between land and rebellion gives reason to believe that land reform may be an effective counterinsurgency measure. Our evidence from Colombia suggests a “Paradox of Partial Reform”: that the effect of land reform on insurgency is conditional on how reform is implemented. When

land reform is implemented at a sufficient scale, it can effectively reduce guerrilla activity. Our findings suggest that reductions in insurgency most likely occur through either the reduction of local (i.e., within-municipality) disputes over land, or the alleviation of peasant grievances at a large scale. More frequently, however, we find that land reform is positively linked to intensified insurgency. Even after controlling for alternative greed, grievance, and opportunity structure explanations of conflict, we find a positive effect of land reform on insurgency outside a select set of 5 to 7 percent of municipalities where the amount of reform surpassed the threshold necessary to undercut the conditions favoring rebellion. The negative marginal effect of reform on guerrilla activity in these select regions suggests that land reform needs to be done comprehensively to be effective.

The positive link between low-level land reform and insurgency can be attributed to land reform politics. In Colombia, as in other countries with high inequality, powerful landowners blocked extensive land reforms that harmed their interests. This limited the effectiveness of the government's policies intended to undercut future guerrilla expansion. Elites successfully lobbied for INCORA to focus its efforts at "those projects least disturbing to the existing land tenure and involving minimal cost" (Tai 1974, 294). Instead of promoting government policies that might have reduced guerrilla support, elites instead attempted to retain their land and hire private "self-defense" armies to counter the guerrilla threat. The reforms that did occur often provoked paramilitary retaliation and pressure for counterreform, which in turn fed more guerrilla activity. While municipal-level data may be too blunt to capture local spillover effects, historical accounts from Colombia also suggest that land reform may have created animosity between beneficiaries and nonbeneficiaries, leading some peasants who did not receive land to protest and turn to guerrillas for support. Finally, in line with Gurr's (1971) theory of "relative deprivation," it is possible that land grievances among neighbors or at a national scale were not addressed and fomented even greater peasant dissatisfaction and insurgent support.

There are at least two reasons why these findings from Colombia are applicable to other settings. First, Colombia shares similar characteristics with other developing and conflict-ridden countries in Latin America, Africa, and Asia, where land reform is hypothesized to play an important role in remedying social unrest. Many developing states in these regions have the capacity to implement moderate reforms, but suffer from neglected peripheries, clientelist rule by powerful elites who shape public policy, vague property rights, and frontier and colonist regions populated with subsistence sharecroppers.

Second, because our hypothesized causal mechanisms linking land reform with insurgency are derived from peasants' responses to both the state and the guerrilla groups, they are likely to operate in other contexts where the state seeks to redress rural grievances. While the potential for upward mobility and risk aversion to property loss may make smallholders more resistant than landless laborers to the appeals of guerrilla groups, this does not imply that a marginal increase in smallholders will necessarily reduce insurgency. Animosity between beneficiaries and

nonbeneficiaries and grievances generated by selective land distribution can increase net support for guerrillas even if support is reduced among beneficiaries. Only when reforms are implemented at a large scale, which depends on the degree of political resistance to reform, do they have the capacity to undercut insurgency.

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Notes

1. We address possible selection effects later.
2. The idea that half-measures and the appeasement of limited, selected groups may have negative consequences is also found in Walter (2006).
3. We focus on titling rather than on transfers in state-supported land markets or expropriation and redistribution. Most land reform in Colombia was titling of state-owned land. For a discussion of both land titling and redistribution, see Mason (1986).
4. Land data from INCORA ends in 2000 when it was liquidated and reborn as INCODER. Data on guerrilla attacks begins in 1988.
5. This is the lowest level at which data are available. Slightly less than 10 percent of municipalities split to create new jurisdictions during the period. We minimize the extent to which changing geography affects our results by merging the variable values for “mother” and “daughter” municipalities where possible (DANE 2000).
6. Sánchez Torres’s (2007) data come from information from both the Colombian government and the nonprofit organization Fundación Social.
7. Note that this variable is coded on “active presence” indicators. Although guerrillas typically operate locally, they may leave safe havens to carry out actions in more target-rich areas.
8. The effect of land reform on homicide rates is negative, differing from its effect on guerrilla activity. This suggests that the indicator for guerrilla activity is not simply a proxy for violence more generally.
9. The analyses indicated that accounting for reciprocity should slightly depress the estimated coefficients on government and paramilitary actions and leave their signs unchanged. However, since the number of plots reformed is correlated with both government and paramilitary actions at less than .02, the net effect of including the reciprocal relationship between guerrillas and paramilitaries/government forces for the plots reformed coefficient is negligible. For more on responses to repression, see, e.g. Mason and Krane (1989).
10. Population, poverty, and other tenancy were interpolated for years between censuses.

11. We also tested a Gini coefficient of land inequality for 1985 from Offstein (2005) since guerrillas in rural areas can seize on grievances over inequality to gain support. This variable was positive and statistically significant in most models, but is not reported because of missingness. A municipality's length of paved roads as a proxy for state strength was generally statistically insignificant.
11. The results of OLS models, however, are similar to those presented later. See Table 5.
12. Municipality random effects models yield similar results (see the Appendix). Although models with municipal fixed effects suffer drawbacks, given the data structure (e.g., $N \gg T$, and 20 percent of municipalities experienced no guerrilla attacks from 1988 to 2000), they too indicate the number of plots reformed in a municipality is positively associated with attacks ($p < .10$).
13. Measuring treatment as receiving any land reform during the period, or greater than 10 or 100 plots reformed all had similar results for municipality matching. Higher thresholds of 500 plots reformed with at least three years of 50 or more plots also yield similar results for both municipality and municipality-year matching.
14. F -tests also indicate the instruments are jointly significant.
15. The Table 7 results are similar using tobit models.

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