# Land Titles and Violent Conflict in Rural Mexico*

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**Abstract**

Better enforcement of property rights should reduce the incentives to engage in violent competition over resources. However, improving tenure security disrupts a key mechanism for political and social control, the discretionary allocation of land by local authorities, potentially affecting the level of violence. We investigate the effect of a land certification program, which produced exogenous variation in tenure security, on violent deaths in Mexico's rural municipalities from 1993-2007. We find that land titles significantly decrease violent deaths on average. However, this reduction is present only in municipalities where the dominant political party has never lost an election. If all ejidos had been certified instantaneously in 1993, our estimates give a 12.8% reduction in homicides, pointing to a large social cost of having land as a political tool.
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# 1 Introduction

From East Timor to Eastern Kentucky, disputes over land rights have been a prominent source of violent conflict.[1] Economists[2] as well as anthropologists, geographers[3], and political scientists[4], have documented how insecure property rights invite violence in the competition for resources. When property rights are not well-defined, nothing guarantees that Coasean bargaining will suffice to resolve conflicting claims over resources and the threat of violence constitutes one of the primary alternative means to secure ones interest in a resource. Besides this direct efficiency loss, violent conflict associated with tenure insecurity can have important indirect effects. For example, de Soto (2000) describes a typical household response to tenure insecurity is to leave the strongest at home and send the less able-bodied individuals, such as children, to work outside the home. For these reasons, policymakers, such as the World Bank, have placed great attention on policies that increase tenure security. In particular, land titling programs have been championed to reduce the violent competition over uncertain claims to resources (Feder & Feeny 1991).[5]

According to the standard logic, land certification clarifies claims to land, reducing disputes and discouraging violence as a strategy to enforce ones claims to land. This characterization of the relationship between tenure insecurity and violence is not wholly satisfactory due to the fact that the allocation of land is often used as a tool for political and social control. By exercising their discretionary control over the distribution of land, local authorities can influence household behavior for personal, political or social gain. Improvements in tenure security will then also affect social outcomes through this second mechanism of diminished political control. This suggests that the effect of land titles on conflict may depend upon political factors

[1] East Timor is a well-known example of violence over land disputes. The home of Randall McCoy and site of one of the final Hatfield-McCoy feuds was located in Hardy, Kentucky, near the border with West Virginia.

[2] See Andre & Platteau (1998) for one of the earlier empirical studies on land rights and conflict by economists.

[3] Fields (2012) and Blomley (2003) discuss enforcement problems in property law and its relation to conflict over land.

[4] Fearon & Laitin (2011) argue that territorial attachment to land explains civil conflict.

[5] Of course, land titling programs may not always increase tenure security (Shipton 1988).

and its overall effects on the level of violence depend on the objectives and capacity of those in power.

To formalize these arguments, we modify a standard conflict model to incorporate uncertain claims to land. We show that improved tenure security to land indeed reduces the equilibrium level of conflict, which is simply the standard tenure security effect and operates independently of political factors. Improved tenure security can also reduce extra-legal competing claims to land by limiting the local authorities' discretionary control over the allocation of land. In the presence of this discretionary control, the tenure security effect can be decomposed into the standard tenure security effect and an *entitlement* effect. The latter capturing the reduction in the discretionary control of those in power.

We investigate the relationship between tenure security and violent conflict empirically in Mexico's ejido sector, which underwent a large-scale land certification program (PROCEDE) during 1993-2007. The institution of the ejido governs half of Mexico's agricultural land and impacts the livelihoods of most of Mexico's rural population. This reform is also attractive because it changes tenure security without regularizing traditional land rights. Typically, land titling programs formalize informally held land rights, which could change the nature of land tenure and lead to greater social conflict.[6] To date, the literature on the agrarian reform of the ejido sector in Mexico has primarily focused on how changes in property rights affected agricultural production (Bouquet 2009, Castañeda Dower & Pfutze 2013, de Janvry, Emerick, Gonzalez-Navarro & Sadoulet 2015) and ours is the first paper to econometrically investigate how these changes affected conflict. In this context,Escalante-Gonzalbo (2009) has argued that land certification played a major role in the dramatic national decline, from 20 to 8, in the homicide rate during this period, but this hypothesis has never been rigorously tested.

In addition to the standard tenure security effect that Escalante-Gonzalbo (2009) and Villarreal (2004) and others have identified, the ejido sector should also exhibit the entitlement effect. A number of case studies present evidence of the extensive

[6]Regularizing land rights may not lead to increased conflict as Ali, Deininger & Goldstein (2014) show.

social and political control that the ejido authorities exercised (Schryer 1986, Torres-Mazuera 2013, Castellanos 2010). For example, Castellanos (2010) gives the account of an *ejidatario*, identified as Don Teo, who had had land taken away for his drunken and disorderly behavior. Castañeda Dower & Pfutze (2015) show that land certification limited the political control of ejido authorities by leading to lower levels of vote suppression. In order to disentangle the standard tenure security effect from the entitlement effect, we take advantage of Mexico's democratic transition, which occurred in parallel to PROCEDE. Since municipal authorities in Mexico do not have any legal authority over ejidos, their discretionary power over land allocation stemmed from informal power structures, cultivated over the course of over 70 years of one-party rule. Once a democratic transition takes place in a municipality, these informal structures will break down. By this reasoning, the entitlement effect will also disappear.

We employ panel data methods to reveal the effect of land titles on violent deaths in roughly 1850 municipalities over a decade and a half. While the nearly universal participation in the land certification program was voluntary, the timing of certification had been mostly supply-driven and the main determinants of program rollout were time-invariant. Hence, once these factors are accounted for, land certification can be reasonably assumed to be exogenous to violent conflict. Our estimation strategy differences away any unobservable time-invariant factors and can accommodate municipal-specific linear time trends, which should alleviate concerns about omitted heterogeneity in pre-reform trends in the homicide rate. To bolster the credibility of our exogeneity assumption, we show that the speed of certification in a municipality is not correlated with the 1990-1992 trend in the homicide rate or the 1990-2007 change in other economic factors (representative data are only available for these two years) that may influence the homicide rate.

Estimating in first-differences, we find that land certification lowers violent deaths per capita in the average municipality. One standard deviation increase in land certification results in a reduction of the violent deaths per capita by 1.7 to 2.8, approximately 10% of the standard deviation in violent deaths per capita. For the median municipality, for example, San Gabriel in the state of Jalisco with 20 ejidos

and 14,280 inhabitants in 1990, the pre-reform homicide rate would have fallen by 17% if all the ejidos has been certified at once, saving the lives of 7 or 8 inhabitants from 1993 to 2007.[7] When disentangling the respective contributions of the two hypothesized effects, we find that the entire reduction in homicides can be attributed to the entitlement effect: In municipalities that have never witnessed a democratic change of their local government, the estimated effect of PROCEDE almost doubles to 4.8-5.9 fewer violent deaths for a one standard deviation increase in land certification.

These results are confirmed by a qualitative variable that only uses the difference in the direction of change of the homicide rate. Our results improve if we exclude observations from smaller municipalities, for which homicides would be a very rare event, and from the later years of the reform when the rollout slowed considerably. Our results change little if we include the lagged homicide rate, suggesting that non-contemporaneous reverse causality is unlikely. In addition, as falsification tests, we investigate the effect of land certification on nonviolent deaths, such as accidents and suicides, and we find no effect on these deaths.

Next, we exploit the temporal and spatial nature of the reform to strengthen our claim that the contemporary effect is a causal response to changes in land certification and not some other response to the reform. The first concern is that the initial information meeting about the reform essentially opened up a window in which competing claims had to be resolved, possibly through violent means. If homicides decrease when the window closes from the reform-induced elevated levels, then the relationship between certification and homicides would be spurious. The second concern is that a change in observable variables in a neighboring municipality may affect land certification and the violent deaths per capita in the municipality in question. Our results are robust to specifications that address both of these concerns.

Due to the quasi-experimental nature of our approach, we entertain the possibility that confounding factors, correlated with program rollout and the homicide rate,

[7]Since we only have three years of pre-reform homicide data, to calculate the pre-reform homicide rate for San Gabriel, we take the average homicide rate over 1990-1992 of the 30 closest municipalities to the median to get a rate of 20.56. In San Gabriel, for the years 1990-1992, there were in fact zero homicides.

explain our results. We focus on factors discussed in the literatures on homicide and the agrarian reform in Mexico. The literature on homicides in Mexico is fairly recent and almost exclusively focused on the drug related violence starting in 2008 (BenYishay & Pearlman 2013, Blanco 2012, Dell 2011). For the period 1990-2007, a study by Villarreal (2002) shows how more competitive municipal elections in Mexico increased the number of violent deaths. The reform could have led to more migration (de Janvry et al. 2015), removing the demographic group that potentially commit most violent acts. We show several results, based on migration intensity and the demographic composition that rule out migration as the main driver behind our results.

These findings contribute to the growing empirical literature on property rights and conflict(Clay 2006, Alston, Libecap & Mueller 1999, Deininger & Castagnini 2006, Ali et al. 2014, Anderson & Genicot 2015). Both Clay (2006) and Alston et al. (1999) argue that the legal inconsistencies in and incompleteness of property rights open the door for violent conflict as a means to legitimate claims. Our paper gives strong empirical evidence that land titles and improvements in tenure security reduce the incentive to engage in violent conflict. These improvements, however, reflect the changes in competition over resources between the local authorities and citizens in addition to citizens themselves. Anderson & Genicot (2015) focus on intra-household conflict, and show that improved tenure security for women increased such conflicts. This resulted in increased suicide rates for both sexes, but particularly for women. Though not the focus of our study, we show the reform had no effect on suicide rates.

The rest of the paper proceeds as follows. Section 2 presents the model. Section 3 provides a description of the Mexican context. Section 4 describes the data and empirical strategy. Section 5 discusses our results, and section 6 concludes.

# 2 The Model

To better understand how we would expect conflict to respond to an improvement in property rights when land is a political tool, we develop a standard conflict model for which output is contestable because land assets face tenure insecurity. In the

standard conflict model, agents are given an endowment of labor, which can be used to produce output (Hirshleifer 2001). While the labor input is perfectly protected from predation, the output of these efforts is not. Building on the model in Gonzalez (2012), we introduce a second factor of production, land, for which property rights are insecure as well. To simplify the model and its presentation and maintain consistency with the standard model, we choose to work in the static context and treat any insecurity of inputs as if it were contestable output.[8]

## 2.1 The tenure security effect

Each agent is endowed with one unit of labor time and one unit of land. The agent can allocate the labor endowment towards predatory ($v$), productive ($l$), and protective ($e$) activities whereas land ($\ell$) can only be allocated to productive activities. As in any conflict model, property rights over output are not perfectly secure and depend upon the agents' predatory and protective activities and the enforcement technology. Following Dixit (2004), we model protection and predation as a competition between the agent and the community-wide average. In these models, each agent successfully defends a fraction, $p(e_i, \bar{v})$, of output and successfully appropriates the fraction, $1 - p(\bar{e}, v_i)$ of the community's average output, where

$$p(e_i, \bar{v}) = \frac{\pi e_i}{\pi e_i + \bar{v}}, \text{ and } p(\bar{e}, v_i) = \frac{\pi \bar{e}}{\pi \bar{e} + v_i}$$

, where $\pi$ is the relative advantage the enforcement technology grants the possessor, $\bar{a}$ representing the average for $a = l, e, v$ and $p(0, 0) \equiv p_0$.

In our model, the degree to which output is contestable depends upon tenure security. Land tenure insecurity has been commonly represented in the literature as some probability that the agent could lose his/her land. To focus on the effect of tenure security, we assume that tenure security governs whether land and its agricultural output is contestable or not. That is, under perfectly secure land tenure, output is never contestable and, under fully insecure tenure, output is always con-

[8] One could alternatively build a two-period model with contestable inputs that would deliver qualitatively similar results.

testable. We denote the probability that governs whether output is contestable or not by the parameter $\psi$ and we refer to the response to a change in the parameter $\psi$ as the standard tenure security effect.

The agent is endowed with an agricultural production function that depends on labor and the amount of land over which the agent has individual control, $\ell$, described by the function $f(l, \ell) = Al\ell$. Since land can only be used for productive purposes, the agent always employs the full land endowment and $f(l, \ell)$ reduces to $Al$.

Taking the community-wide labor allocation as given, the agent's problem is to choose an allocation of labor time that maximizes the following payoff:

$$U_i(l_i, e_i, v_i) = (1 - \psi)Al_i + \psi(p(e_i, \bar{v})Al_i + (1 - p(\bar{e}, v_i))A\bar{l})$$

.

The allocation of labor time is subject to nonnegativity and resource constraints,

$$l_i + e_i + v_i \leq 1$$

.

An additional constraint is the community-wide adding-up condition. Since we will focus on a symmetric equilibrium, this constraint will be automatically satisfy the adding-up condition.

For an interior solution, the marginal return of each labor time activity must be equalized. Equalizing the marginal return to protection and the marginal return to predation at an interior optimum, together with symmetry and by canceling terms, we can say that:

$$\frac{\partial p(e_i, \bar{v})}{\partial e_i} = -\frac{\partial p(\bar{e}, v_i)}{\partial v_i}.$$

Thus, the optimal allocation of protection and predation is at exactly the same level and we have $e^* = v^*$. This equality insures that $p(e^*, v^*)$ simplifies to $\frac{\pi}{\pi+1}$ at the optimum.

Notice that tenure security over the asset, an input of the production function,

has no effect on the relative level of protection vs. predation. However, tenure security does affect the relative attractiveness of productive labor.

To find, the optimal level of productive labor, we equalize the return to production and predation to get:

$$(1-\psi)A + \psi\frac{\pi}{1+\pi}A = \frac{\pi}{(\pi+1)^2}A\psi\frac{l}{e}$$

which reduces to,

$$l^* = (\pi+1)Ke^*,$$

where $K = \frac{(1-\psi+\pi)}{\psi\pi}$.

The resource constraint implies that $l^* = 1 - 2e^*$ and we substitute for $l^*$, giving

$$e^* = \frac{1}{(\pi+1)K+2}.$$

*Proposition 1* (Tenure security): The equilibrium level of conflictive behavior is increasing in $\psi$, the probability that output is contestable.

Assuming an interior solution, the equilibrium level of conflictive behavior is given by $e^*$, which is increasing in $\psi$ *iff* $K$ is decreasing in $\psi$.
The partial derivative of $K$ with respect to $\psi$ is

$$-\frac{\pi(1+\pi)}{(\psi\pi)^2},$$

which is always negative for $\pi \geq 1$ and $\psi \in (0, 1]$.

The standard tenure security effect is purely economic and the gain in efficiency from having clearly-defined property rights is a direct result of limiting the violent competition over resources.

## 2.2 Land as a political tool

We argue that this direct effect does not fully capture the effect of tenure security on conflict since, in most institutional environments and in Mexico, in particular, there is a political dimension to property rights that could have a first order influence on labor and land allocation.

We model the political dimension by introducing asymmetric power bestowed upon local authorities by granting them discretionary control over the allocation of land allotments (and possibly other jointly held assets). Discretionary control amplifies tenure insecurity due to ambiguous claims to land because it opens up a new arena over which resources can be contested. This amplification is governed by the parameter $\theta$, where $\theta \in (0, 1)$, and the effective tenure security is then $(1-\theta)(1-\psi)$ yielding the probability that resources are contestable as $\psi + \theta - \psi\theta$. For simplicity, we take a partial equilibrium approach and abstract from strategic considerations by the local authority, assuming that their behavior is determined by parameters exogenously chosen. In our context, this assumption is plausible given the hegemonic and centralized nature of the governing party during this time. The local authorities could commit to exercising discretionary control, provided that the local political machine's grip on power is credible. If it is not credible, then the local authorities cannot exercise this control and $\theta$ is assumed to be zero.

Moreover, conceptually, it is important to distinguish between the local authority having the ability to extract rents, and its willingness to do so. For this reason, we furthermore introduce the parameter $\alpha$, which captures this distinction:

$$U_i(l_i, e_i, v_i) = (1-\theta)(1-\psi)Al_i + \psi(p(e_i, \bar{v})Al_i + (1-p(\bar{e}, v_i))A\bar{l}) + \theta(1-\psi)\alpha Al_i$$

.

Here, if either $\theta = 0$ (no ability to extract resources) or $\alpha = 1$ (no willingness/incentive to extract resources), the model collapses to the previous one without any political inference. We can think about a situation with a high $\theta$ and a high $\alpha$ as one in which the local government would have the ability to extract rents, but

is prevented from doing so by, for example, strong local democratic institutions or traditions. In the Mexican context, discussed in more detail below, we believe that a democratization process at the local level primarily affected $\theta$ (by moving it towards zero).

Community-wide adding-up condition still holds because we again focus on a symmetric equilibrium and the political diversion of resources accounts for the remaining share of contestable output.

For an interior solution, the marginal return of each labor time activity must be equalized. Equalizing the marginal return to protection and the marginal return to predation at an interior optimum, together with symmetry and by canceling terms, implies:

$$\frac{\partial p(e_i, \bar{v})}{\partial e_i} = -\frac{\partial p(\bar{e}, v_i)}{\partial v_i}.$$

Thus, the optimal allocation of protection and predation is at exactly the same level and we have $e^* = v^*$. This equality insures that $p(e^*, v^*)$ simplifies to $\frac{\pi}{\pi+1}$ at the optimum. Again, tenure security over the asset has no effect on the relative level of protection vs. predation.

To find, the optimal level of productive labor, we equalize the return to production and predation to get:

$$(1-\theta)(1-\psi)A + \psi\frac{\pi}{1+\pi}A + \theta(1-\psi)\alpha A = \frac{\pi}{(\pi+1)^2}A\psi\frac{l}{e}$$

which reduces to

$$l^* = (\pi+1)K_P e^*,$$

where $K_P = \frac{1-\psi+\pi-\theta(1-\psi)(1+\pi)(1-\alpha)}{\psi\pi}$.

Since the resource constraint implies that $l^* = 1-2e^*$, we substitute for $l^*$, giving

$$e^* = \frac{1}{(\pi+1)K_P + 2}.$$

While $K$ and $K_P$ share a similar form, they have very different implications. First, $K_P$ now depends upon the political factors embodied in the parameters $\theta$ and $\alpha$. Second, the effect of tenure security on conflict, which could be interpreted as either a change in $\psi$ or $\theta$ can be decomposed into the standard effect, which, as above, results in a decrease in conflict, and a countervailing *entitlement* effect, which produces a relative increase in conflict.

*Proposition 2* (Conditional tenure insecurity): The equilibrium level of conflictive behavior is increasing in $\psi$, but the magnitude of the effect is decreasing in $\theta$.

Assuming an interior solution, the equilibrium level of conflictive behavior is given by $e^*$, which is increasing in $\psi$ *iff* $K_P$ is decreasing in $\psi$. $K_P$ is decreasing in $\psi$ whenever $-\pi(1+\pi)(1-\theta(1-\alpha)) \geq 0$ which holds because $\theta, \alpha \in [0,1]$.

We can decompose the partial derivative into a negative component, given by $-\frac{\pi(1+\pi)}{(\psi\pi)^2}$, the standard tenure security effect described above, and a positive component, $\frac{\theta\pi(1+\pi)(1-\alpha)}{(\psi\pi)^2}$. Since $\theta$ only appears in the positive component, it is easy to show that the crosspartial derivative is positive:

$$(\psi\pi)^2(\pi(1+\pi)(1-\alpha)) > 0$$

.

Nevertheless, the effect of a change in $\theta$ on conflict is not positive. This is due to the fact that when the authorities exercise discretionary control the probability that the individual farmer retains control over his/her land allocation decreases.

*Proposition 3 (Entitlement effect)* : The equilibrium level of conflictive behavior is increasing in $\theta$, the degree of discretionary control.

Assuming an interior solution, the equilibrium level of conflictive behavior is given by $e^*$, which is increasing in $\theta$ *iff* $K_P$ is decreasing in $\theta$. $K_P$ is decreasing in $\theta$ whenever

$$-(\psi\pi)((1-\psi)(1+\pi)(1-\alpha)) \leq 0$$

which holds for all $\alpha, \psi \in [0,1]$ and $\pi \geq 1$.

More generally, the entitlement effect could make the effect of tenure security potentially ambiguous. In our model, the entitlement effect never dominates, but there is at least one channel through which the entitlement effect operates that we abstract from. We restrict attention to agricultural production. For productive activities that don't require land as an input, discretionary control could still be used to discipline behavior but the standard tenure security effect should not apply.

# 3 The Mexican Context

Escalante-Gonzalbo (2009) provides a rich descriptive analysis on the evolution of Mexico's homicide rate over the period 1990-2007, based on the same administrative records that we use. His main points are reflected in figure 1: After a peak in the early 1990s, violent deaths declined steadily until 2007. This decline was most pronounced in smaller, more rural municipalities, leading to some convergence in municipalities of different sizes. It was strongest between 1992 and 2000, after which it somewhat leveled off. Escalante-Gonzalbo (2009) speculates that the steeper decline in rural areas is the result of fewer conflicts over land following certification.

Mexico's agricultural sector is divided into two different property regimes: Private and comunal property, the latter consisting mainly of ejidos. Private farms tend to consist of larger and more productive units, while the ejido farms operate on communally held land. The ejido sector is administered by a separate ministry at the federal level and comprises of more than 50% of Mexico's national territory (albeit mostly unproductive, arid land) and contains roughly 30,000 *ejidos*[9] Each ejido represents an agricultural community, consisting of mostly household-farm units, centered on a rural locality to which land (in proximity of up to a 7km radius) has been granted by the government.[10]

In most ejidos, some of the land, such as for grazing or forestry, was tended to by the entire community but most of the land was allocated to individual households

[9]plus some 2,000, mostly indigenous *comunidades agrarias* with a slightly different regime.

[10]See Sanderson (1984) for an excellent and detailed description of the pre-reform ejido sector and its regulatory framework

for agricultural production. These plots, however, could be taken away if an ejido member (called *ejidatario*) violated the rules. The rules written into the constitution were that an ejido member must farm the land allocated to him and could not rent out the land nor hire external labor. Ejidos could also have a set of internal rules. In the 2001 Ejidal Census, roughly two-thirds of the ejidos report having a set of internal rules. de Janvry, Gordillo & Sadoulet (1997), using a 1994 ejido-level survey of 255 ejidos, find that over 50% of ejidos report having their own set of internal rules; however, in only one-third of these ejidos do the rules deal with agricultural activities.

Local ejido authorities, the *Comisariado Ejidal*, held considerable sway over allocation of individual plots and access to communal lands. Of particular importance is the role of ejido authorities in maintaining order and resolving disputes within the community. In a very detailed study on the social relations within the ejido sector, McKinley-Grohmann (2011) describes how the ejido authorities "often constituted themselves as parallel powers"[11], and how the the formal political powers delegated parts of their functions to the PRI-linked local strongmen under the condition that they maintained the "social peace"[12].

Little direct evidence exists on the ejido authorities' reliance on discretionary control other than what was indicated in the constitution. The best documented evidence that ejido authorities used their power to influence behavior for social and political purposes comes from the connection between the ejido and local strongmen (*caciques*) who acted as local power brokers for the *Partido Revolutionario Institutional* (PRI), the dominant political party in Mexico (Roniger (1987), Holzner (2003), Paré (1975)). Torres-Mazuera (2013) documents in Central Mexico how the control over resources empowered the ejido and gave it considerable influence on political and economic development in rural areas. However, she also argues that agrarian re-

[11] "A menudo, los órganos directivos de los ejidos se constituyeron como poderes paralelos a las instancias políticas locales como los municipios, otorgando a dichos órganos particularmente al Comisariado Ejidal una influencia y autoridad dentro de sus lmites territoriales más allá de lo estrictamente agrario [...]" (pg.10)

[12] "[...] el poder político delegaba parte de la función del control político a particulares vinculados con el partido los cacicazgos priistas con la condición de que mantuvieran en paz social a sus gremios o ámbitos de influencia." (pg.15)

forms of the 1990s initiated a transformation of the ejido as a political institution by weakening it. Unfortunately, there is no feasible way to test this hypothesis directly with currently available data.

In the early 1990s the Mexican government, under president Carlos Salinas, decided to radically reform the ejido sector by a 1992 constitutional amendment.[13] For one, after the last available land had been given away in the 1970s, it put an end to further land redistribution. In addition, it lifted many of the restrictions on land usage, such that land could now be left unused, rented or tended by hired labor. Lastly, it opened the possibility for ejidos to decide whether they wanted to convert their land into private property.

In order to make these new regulations workable the government also realized that it should improve the property registry that documents ejidos' external and internal boundaries. Up to that point, the registry of communal land holdings had been more than deficient. If documentation existed at all, it was often too vague to determine exact boundaries, or, in some cases, even contradictory. Individual plots were commonly allocated within the ejido without bothering to maintain proper record keeping.

This certification process (PROCEDE), explained in more detail below, took place against the background of a democratization process at all levels of government. The formerly omnipotent PRI lost its first governorship in 1989, and went on to lose the majority in the national Congress in 1997, followed in 2000 by the majority in the Senate and the presidency. At the municipal level, it was still in charge in over 90% of municipalities at the beginning of our period of analysis. This dropped to 70% in 2000, and below 50% in later years. We use this demise of the party machine to disentangle the tenure security and entitlement effects. The latter should only be present in municipalities where the PRI maintains hegemonic control, and is, therefore, able to exercise its power through the ejido authorities. In places where the opposition was able to prevail in a democratic election, this political dimension of the ejido is expected to have ceased to exist.

To summarize, before land certification, the existing land cadastre archives were

[13]See de Janvry et al. (1997) for an overview of the reform and its effects.

in poor condition and subject to corruption by parties involved in land disputes. Seeking redress for tenure rights' violations from the federal government was mostly infeasible or prohibitively costly. Land certification changed all of this to a large extent. Not only was the local land cadastre updated and completed but the physical land certification document could easily be employed by third-party enforcement. Land certification, thus, limited ejido authorities' control over land allocation and inhibited a key mechanism of social control. Households with land certificates are less subject to the social control of the ejido, entitling them to greater individual determination of their actions, for better or worse, thus our label, the *entitlement effect*. At the same time, an increasing number of municipalities elected mayors from opposition parties. Since the PRI dominated local governments exercised their power through informal party-linked channels, the newly elected authorities were not able to engage in the same rent-seeking activities. The bottom line is that land certification only had an effect of on discretionary political control in continuously PRI-ruled places.

## 3.1 PROCEDE: The land certification program

After passing the amendment of Article 27, the government set in motion the program of properly measuring out external and internal ejido boundaries, and to give individual ejido members a title over their individual plots, making third party enforcement of land rights possible. This program, called PROCEDE (standing for *Programa de Certificación de Derechos Ejidales y Titulación de Solares Urbanos*), started in 1992 and proceeded in several stages. In the first stage, the *Procuraduría Agraria* (PA, a body of the federal administration) contacted the ejido authorities to set up a first informational meeting (the *Asamblea de Información y Anuencia* (AIA)) with all ejido members, the general assembly. After that meeting, the general assembly had to take a vote, with a quorum requirement of 50%, whether or not to initiate the certification process. In case of a positive vote, the ejido formed a commission (*Comisión Auxiliar*) to set up a rough draft for a map of the ejido, showing its external and internal boundaries. This draft had then to be approved by

the general assembly, again with a 50% quorum. In the next step, Mexico's National Institute of Statistics and Geography (INEGI by its Spanish acronym) would start, jointly with the ejido members, a detailed measurement, producing detailed maps for the land registry. These maps would be publicly presented for two weeks, during which complaints could be filed. At the last stage, all the ejido's external neighbors had to agree to the maps, and the general assembly, this time with a 75% quorum, had to approve them. Subsequently, the maps were sent to the National Agrarian Registry (RAN by its Spanish acronym), and titles were issued. Thus, this process minimized arbitrary redrawing of internal and external boundaries, which could lead to greater land conflict.

Initially, the government thought that PROCEDE would not take longer than its remaining two years in office. It became soon clear, however, that the whole process would take much longer. The next administration, of Ernesto Zedillo, continued the program, but significantly reduced its budget, not least in response to the economic crisis starting a few weeks after taking office. In the end, PROCEDE took a full 15 years, and ended only in 2007 with more than 90% of all ejidos certified. Figure 2 shows the percentage of ejidos certified in each year. The ejidos not certified by 2007 are either generally suspicious of the government, or have have as yet unresolved internal or external land disputes that make them ineligible.

The certification process was essentially carried out at the state level, with teams from the different federal agencies involved working from the state capitals. The PA contacted ejidos for the first informational meeting starting with ejidos in close proximity to the state capital, and moving gradually further away from it. Responding to political pressure and the budgetary restrictions, INEGI followed a strategy of certifying as many ejidos as quickly as possible. This means that once an ejido had approved its participation in the process, the time it had to wait for actual certification was determined by how easy it was to be measured out. These strategies have been confirmed by our personal conversations with officials from the PA and INEGI who were involved in the process. For INEGI, the characteristics that made an ejido easy to certify were primarily a small land area and level terrain. Shared boundaries with other ejidos also mattered. This strategy is also apparent in figure

2: Most ejidos were certified over the 1990s, but as INEGI had to move into the more difficult ones, the speed of certification declined substantially in the 2000s.

# 4 Data and Empirical Strategy

Our data come mainly from Mexico's administrative archives and the country's 1990 census. The outcome of interest, violent deaths, can be found in the administrative data section on the Mexican National Statistical Agency's webpage (INEGI by its Spanish acronym). Violent deaths are listed within a broader data set on mortality, compiled based on the death certificates issued by the civil registry. In case a death is declared not to be due to natural causes (either violent or the result of an accident), the *Ministerio Público* (roughly comparable to a state-level attorney's office) is required to open an investigation. The information found in the dataset is based on the latter's documentation(*Cuaderno para defunciones accidentales y violentas*). It is important to understand that at this point deaths that are not due to natural causes are only declared to be either accidents, suicides or homicides. Our figures for violent deaths come from this homicide classification and, therefore, comprise all murders and manslaughters committed (premeditated and spontaneous, intentional, as well as, unintentional, or even in legitimate self defense), since their precise nature can only be determined by bringing them to trial. This broad measure reflects our intention to use the number of violent deaths as a measure for the level of violent conflict in a municipality.

For each death, the data show the year and month of occurrence, as well as, the year and month of its registration. We restrict our measure to deaths registered in the year of their occurrence or of the one thereafter. While in some cases homicides may only be registered years after they happened (for example, when the corpse is finally discovered), such cases are very rare. By restricting ourselves to two years of registration we capture close to 99% of all homicides (based on those that occurred in the early 1990s on which we have close to 20 potential years of registration). Some deaths that occurred during the very last days of each year, however, are always registered during the first days of the next year, which is why we include

registrations from the following year.

Our treatment variable of interest, ejido certification, is taken from data made available to us by the Mexican National Agrarian Registry (RAN by its Spanish acronym). We are able to observe the name of the ejido, its municipality, and the date its land titles were issued. This allows us to know the number of ejidos certified in each municipality per year. Our ideal treatment variable would be the number of beneficiaries (i.e. individuals living in households directly affected by PROCEDE) as a fraction of the total population. However, we are unable to observe the number of members (plus their dependents) at the ejido level. Using the number of certified ejidos per capita as a close proxy for the ideal measure will introduce some measurement error to the extent that ejidos' size (in terms of membership) differs across municipalities. But given that we will estimate linear in parameters models, this will only bias our estimates towards zero. The total population of a municipality is taken directly from the 1990 census, carried out by INEGI and available on its webpage. For some of the additional specifications and robustness checks, we used data from the 1992 agricultural census, which has been made available to us for producers on ejido lands aggregated at the municipal level, and electoral data taken from the data set on municipal elections compiled by, and freely available from, the Mexico City think tank CIDAC.

Information on our most important variables, violent deaths and ejido certifications, are available on a yearly basis. This allows us to construct a panel data set over 15 years (the duration of the PROCEDE program) for all municipalities with at least one ejido within their territory. Other information at the municipal level is, however, much less frequent. Mexico conducts a full census every five years, but information on most characteristics of interest is only gathered in years ending in zero. In years ending in five, a much smaller questionnaire is administered, collecting mostly information on the country's demographic structure. The agricultural census has only been conducted in 1992 and 2007, making data even scarcer. Electoral data are, of course, available for all election years (at the municipal level every three, actual years differ by state, however).

We conduct our empirical analysis in per-capita terms. Our dependent variable is

a municipality's yearly violent deaths per capita, that is the number of deaths ruled homicides per 100,000 inhabitants. In order to arrive at comparable and easy to interpret results, we put all other per-capita variables on the same scale. Our treatment variable is, therefore, the number of certified ejidos per 100,000 inhabitants. Since population data are available only once every five years, a constant growth rate is assumed during the four intervening years to construct the time varying per capita variables.

Table 1 presents summary statistics for the set of variables included in our analysis. We observe a total of 1,851 municipalities (those with at least one ejido) over 15 years, yielding a total of 27,765 observations. The first two, *Homicides per 100k population* and *Procede per 100k population*, capture the number of violent deaths and certified ejidos per 100,000 inhabitants, respectively. For the sake of consistency, we also normalized the population data to units in terms of 100,000 people. Hence the average municipality in our sample had 36,870 inhabitants. The average homicide rate by municipality over the 1993-2007 period was around 13, which is in line with the data presented in figure 1. *Only PRI* is a dummy equal to one if the municipality has been ruled continuously by the former state-party PRI (it is still coded as one in the election year it changes) and is the variable we will be using to proxy for institutional entrenchment. As for the other electoral variables discussed below, we are not able to observe results for all municipalities in all years, given that some municipalities do not hold party-based elections, that some elections have been annulled or due to data coding errors. Generally speaking, around 60% of municipality/year observations were continuously ruled by the PRI. Over time, this proportion dropped from 83% of municipalities in 1993 to 13% in 2007. This decline has been fairly steady, dropping by a few percentage points each year (the highest drop is by 10 percentage points in 1995).

The other variables in the table will be used to test for alternative causal mechanisms. *Election Year* is a dummy variable equal to one in a year with a municipal election, and the *Election Margin* is the difference in votes between the winner in that election and the runner-up (municipal elections in Mexico are practically winner takes all). The binary variable *PRI Incumbent* is equal to one if the municipality

is currently ruled by the PRI (always referring to the beginning of the year). In addition, we have two variables coding the presence of an illegal drug industry in the municipality. These variables are time-invariant and will be interacted with the Procede variable. INEGI publishes administrative data on indicted suspects, always listing their most severe crime. Based on this data we were able to construct the variable *Narco Crimes p.c.*, which denotes the number of such suspects, based on the year the crime occurred, per 100,000 inhabitants. Unfortunately, this data is only available from 1997 onwards. For that reason, we construct this measure based on the average over the 1997-1999 period. *Dummy Top 100 Marijuana Producer* is a dummy equal to one if the municipality was in a list of the biggest producers of marijuana, measures as hectares per capita, according to Resa-Nestares (2005). Lastly, in order to control for the intensity of international migration we include the proportion of return migrants (from anywhere outside Mexico) over the 1985-90 period in the total population according to the 1990 census. We use this, slightly imperfect variable, because Mexico only started to collect more detailed data on international migration with the year 2000 census. However, it can be expected to be closely correlated with the intensity of migration. Related to the migration variable, we also directly control for the proportion of young males (aged 15-29) relative to the total population in the municipality. Lastly, we control for rainfall shocks by separately measuring yearly positive and negative deviations from the long term rainfall average. These data come directly from Mexico's meteorological service (*Servicio Meteorológico Nacional*) as measurements at the level of meteorological stations. Since not every municipality has its own measurements, we matched it to the the closest stations within 500 meters of elevation to the municipal seat.

The drawn out process of the certification program is at the heart of our identification strategy. First, it allows us to construct a panel dataset over 15 years at the municipal level, providing us with a lot of statistical power. Secondly, we know that for each ejido the moment of certification is partly determined by a number of time invariant characteristics (distance to the state capital, size, geography, adjacency to other ejidos). Applying panel data techniques that difference these fixed effects away, we identify the effect of Procede as long as there are no time-specific factors that

affect violent deaths and influence the timing of Procede in any given ejido[14] . This idea will be further tested in various different specifications.

We estimate the model in first differences. Though we will present a large number of different specifications, the principal model for municipality i in year t is:

$$\Delta H_{i,t} = \beta_0 + \beta_1 \Delta Proc_{i,t} + \lambda_t + \mu_i + e_{i,t}, \tag{1}$$

However, the comparative statics of our theoretical model suggest that changes in $\psi$ and $\theta$ will lead to changes in conflict. Since PROCEDE affects both of these parameters, and these parameters themselves may be interdependent, the reduced form representation in 1 would not allow us to distinguish between the two mechanisms. In light of the discussion above, since we cannot observe discretionary control directly, we proxy for it with a binary variable indicating continuous PRI rule. Since the entitlement effect can be assumed to be present only in municipalities in which the PRI is entrenched, the treatment is then interacted with this dummy variable:

$$\Delta H_{i,t} = \beta_0 + \beta_1 \Delta Proc_{i,t} + \beta_2 (\Delta Proc_{i,t} * Only\ PRI) + \beta_3 Only\ PRI + \mu_t + \theta_i + e_{i,t}, \tag{2}$$

where $\Delta H_{i,t}$ denotes the change in the violent deaths per 100,000 inhabitants, and $\Delta Proc_{i,t}$ the change in the number of certified ejidos per 100,000 inhabitants. $\lambda_t$ denotes the year specific error term, and $\mu_i$ the municipality specific one. The idiosyncratic error $e_{i,t}$ will be clustered at the municipal level. We allow for a particular structure of the first differenced error term, consisting of $\lambda_t$, $\mu_i$, and $e_{i,t}$. The first error term captures year specific shocks common to all municipalities, and will be controlled for with *year fixed effects*. Likewise, the second term, $\mu_i$, can be controlled for with municipality specific fixed effects, which are to be interpreted as

[14]We also conducted the whole gamut of stationarity tests for panel data on the first differences of our two variables of interest, homicides and certification by Procede. In all cases the null hypothesis of a unit root could be reject well below the 1% level of confidence.

*municipality specific linear time trends* given that expressions 1 and 2 are already in first differences. More broadly, not finding any big differences in our estimates after adding municipal-specific linear time trends is consistent with the parallel trends assumption required for our underlying estimation strategy. These fixed effects are operationalized through demeaning, and, where applicable, subsequently interacted with *PRI Only* to yield a similar expression to that in 2.

We stick to this fairly parsimonious specification for two reasons. Firstly, as already discussed, information on most other municipality characteristics of interest is only available every ten years. While we could assume constant growth rates for these variables, any variation in them will be mainly between municipalities and be differenced away. Secondly, some of these characteristics may be endogenous in the sense of being affected by the violent deaths per capita in previous periods. Our preferred course of action is, therefore, to restrict ourselves to the inclusion of interaction terms of baseline characteristics from the early 1990s with our treatment variable.

One problem we face with the data is that violent deaths are a rare event. Working on the municipal level, most smaller municipalities will not have a single violent death in most years. Given that almost 10% of municipalities had 3,000 inhabitants or less, and over 20% less than 5,000 in 1990, a single murder would increase the dependent variable from zero to over 33 or over 20, respectively. This compares to an average of 12.6 across all years under study (see table 1). The bottom line is that smaller municipalities have a very high variance in the outcome variable, substantially increasing the standard error in all estimations. We will, therefore, present results first for the whole sample, and then drop the bottom decile and quintile. As will become clear below, this exercise suggest dropping the bottom decile from the sample.

Our identification strategy relies crucially on the assumption that once municipality fixed effects are differenced away, the rollout of Procede is uncorrelated with any omitted variables from the model (i.e. can be treated as quasi-random). In the analysis below, we will indirectly test for this assumption by comparing results with and without municipality-specific time trends (fixed effects). Here, in table 2,

we present a more formal treatment on the implementation of Procede. The dependent variable is the municipality-level average of our treatment over the 15 years of analysis, capturing the average speed of rollout. In the first column we regress this variable only on a constant, state dummy variables, and the trend in the homicide rate over the three years prior to the start of the program [15]. This pre-trend is clearly statistically insignificant and does not change in the next two columns either. In column 2 we add a number of differenced observable variables[16] that may indicate broader socioeconomic changes over the 1990-2005 period [17]. The only highly significant variable here is the difference in the illiteracy rate. However, as becomes clear in the next column, this variable mainly proxies for a municipality's remoteness. All other variables are insignificant with the exception of the difference in the mean wage, which is at the 10% level (but also turns insignificant in column 3). Finally, in column 3 we add a number of time invariant variables that have been discussed before as determining Procede rollout, all taken from the agricultural and ejido census 1991. As expected, the total number of ejidatarios has a negative effect on the speed of implementation, while the total number of ejidos in a municipality enters positively. The effect of both, distance from the closest big city and ruggedness of the terrain also have a negative effect. The only unexpectedly insignificant variable is the total land area of ejidos. Most likely, this is because the largest ejidos by land area can be found in the infertile north of the country. As they mostly consist of communal grazing land their certification may have been fairly easy. In order to sum up the results from this table, we added the F-statistic of a joint insignificance test on all time variant variables (that is excluding the constant, state dummies and the time invariant variables in column 3) to the bottom of the table. This confirms their

[15] Ideally, we would like to have a longer pre-trend, but the data available only starts in 1990.

[16] These variable are: The proportions of the population that is indigenous, illiterate, unemployed, or economically active; the average self-reported wage reported (in terms of minimum wages) and its standard deviation; the proportions of the labor force working in each of the three most important sectors (agriculture, manufacturing, and construction), with services acting as the baseline category; the proportion of the population that returned from abroad in the previous five years, and the total population

[17] these variables are observable every five years only, so the 1990-2005 period is the one that most closely matches our time window.

lack of significance in the last column.

# 5 Results

We present our principal results in tables 3 and 4. In subsection 5.2, we strengthen these main results using a number of additional specifications, testing for the robustness of our estimates and potential alternative causal channels. All tables, in addition to the variables listed in each table, include a full set of year dummies. In tables 3, 4 and 5 we also show results for categorical outcomes (explained below), while in all other tables that follow the outcome is always the homicide rate. In most tables, we present results with and without municipality specific time trends (i.e. additional fixed effects). In tables 6 and 7, we restrict the analysis to the specifications with such trends to keep tables to a manageable number.

## 5.1 Main results

Table 3 presents the core results. We show results for the full sample of all municipalities, and under the exclusion of the bottom decile and quintile of municipalities by to their population in 1990. As explained above, our principal concern here is that, due to the rare event nature of homicides, the presence of small municipalities will increase the variance in our estimates. The results in the table show that this concern is indeed warranted. When the lowest population decile is excluded, our point estimates become somewhat smaller, yet statistically much more significant by virtue of their much smaller standard errors. The results in column 1 are only borderline significant at the 5% level. However, moving from the exclusion of the first decile, to excluding the entire first quintile barely changes the results. We conclude that the smallest municipalities push our estimates away from zero, but also add a lot of noise due to their low precision. The municipalities in the second decile (which have a size of roughly 3,000-5,000 inhabitants in 1990) are already large enough not to introduce a lot of additional variance. For this reason, in the tables that follow

we will present results excluding the first decile[18].

Our point estimates in the second two columns are negative throughout with a point estimate of 0.025 (without municipality specific time trends) and 0.039 (with such time trends). These have the interpretation as the predicted drop in the homicide rate due to the certification of one additional ejido per 100,000 inhabitants. If we were to scale up this effect to the entire country (roughly 30,000 ejidos and a population of 100m in the year 2000), total Procede certification would have resulted in a drop in the homicide rate of about 1.17. Taking into account that only 90% of ejidos were certified by 2007 we would be taking about a drop of roughly 1.05. While important, this drop is far from explaining the total reduction in the rural homicide rate during the time period under study (as has been hypothesized by Escalante-Gonzalbo (2009)).

A different way to address the high variance in the homicide rate in small municipalities is to simply look at the direction of the change. This is done in the bottom panel of table 3. The outcome and treatment variables are based on absolute numbers, not relative to population size. Here the dependent variable is coded as one if the homicide rate between two years increased, minus one if it decreased and zero if it stayed constant. The treatment variable is the change in the number of certified ejidos. As expected, excluding the smallest municipalities has no important effect on our estimates, which stay essentially the same. The interpretation of the point estimates is not very illuminating, but their importance here resides in the observation that they are similar to the ones in the top panel in terms of sign and statistical significance.

The results thus far suggest that, using the terminology of our model, the magnitude of the tenure security effect is much larger than that of the entitlement effect. We now want to assess whether the second exists at all. In order to do so, we take advantage of Mexico's democratic transition that took place over the same period of time at the municipal level. Above, we provided an extensive discussion on the links between the local PRI and the ejido authorities. We thus use the interaction of

[18]In the appendix we show results for a number of additional restrictions on the sample, these a very consistent with the results shown here.

Procede with an indicator for entrenched PRI rule (defined as the PRI having never been out of power at the municipal level) to shed light on this question. In terms of our model, the assumption is that $\theta$ is equal to zero (that the entitlement effect is absent) in municipalities where the PRI has lost a democratic election. The parameter estimate on Procede will then capture only the (unconditional) tenure security effect, which, since it does not depend on political institutions, can be assumed to be similar in both kinds of municipalities. The interaction term captures the entitlement effect, which, by the same reasoning, is only present in municipalities with an entrenched PRI government.

Table 4 shows results for this exercise. The basic structure is the same as in the previous table. The important insight is that the entire previously identified negative partial effect of Procede is only present, and almost twice the size as the estimates in table 3, when PRI is entrenched in a municipality. The implications are that the standard tenure security effect played no role. Furthermore, given the effect's size and level of significance, we can also conclude that the local PRI was indeed strongly rent-seeking (i.e. $\alpha$ must have been low). Consequently, its discretionary power over land allocation was used in such a way as to increase violent conflict. Procede, by curtailing this power, reduced violence. Subsequent PRI governments did either not have the discretionary power over land allocations, since the link with the ejido authorities was severed, or the new democratic accountability effectively dissuaded them from using it (a high $\alpha$ in terms of our model). In municipalities that already had democratized, and where the link between the local government and the ejido authorities was severed, Procede had no effect at all on the level of conflict. In terms of our model, this means that Procede did not effectively alter the value of $\psi$. The bottom line is that the main source of violence related to insecure land titles was to be found in the arbitrary exercise of political power, rather than the attempted appropriations by other members of the community. These results are further confirmed in the bottom panel of the table that uses the categorical outcome.

## 5.2 Additional specifications

We now further strengthen our results first by conducting a number of falsification tests using other forms violent deaths (accidents and suicides) as our dependent variable. We then show that our results are robust to additional potential threats to exogeneity and that they are not driven by land certification acting through other mediating variables.

In table 5 we present results for the first exercise. The dependent variables here are the number of deaths that are ruled accidents or suicides, respectively, per 100,000 inhabitants. Other than that, the estimations are identical those in columns 3 and 4 in the top panel of tables 3 and 4. It can easily been seen that both Procede variables are statistically insignificant with t-statistics mostly below one. Moreover, the point estimates have the opposite sign than before. The magnitude of the effects is also smaller than in the case of homicides when put in relation to their respective means. On average, there are 37.62 accidental deaths per 100,000 inhabitants in our data (i.e. three times the level of homicides), but only 3.35 suicides (less than one-third of homicides). In light of the aforementioned work by Anderson & Genicot (2015), the absence of an effect on suicides is worth pointing out. Following these authors' argument, we would conclude that Procede had no discernible effect on intra-household conflict.

In tables 6 and 7 we test for various different threats to exogeneity. In the interest of space, we restrict ourselves to the specification in column 4 of table 4. One concern with panel data of this kind is the possibility non-contemporaneous reverse causation, perhaps because a high level of violent deaths may interfere with the certification process. In order to rule this out, we control for the lagged dependent variable. Table 6, in column 1, reproduces our principal results with the addition of a lagged dependent variable (i.e. the homicide rate in the previous year). Additionally, the inclusion of the lagged dependent variable also controls for possible effects of lagged independent variables that may act as omitted variables (and the effect of which may be picked up by their contemporaneous values). We stress that our aim is to merely show that the inclusion of the lagged dependent variable does not change our

previous results in any important way, implying that the change in certification is uncorrelated with prior changes in the homicide rate. Given the panel structure of our data, lagged dependent variables are necessarily endogenous given their correlation with the lagged error term. Their parameter estimate will, therefore, be biased by construction. However, if they are uncorrelated with the independent variables, they will not bias any of other parameters. The bottom line is that the inclusion of a lagged dependent variable in our model provides a valid test for non-contemporaneous reverse causation and/or omission of lagged independent variables. Since we are not interested in the parameter estimate on the lagged dependent variable itself, there is no need to employ Arellano-Bond type estimation techniques (which would in addition rely on very ad-hoc assumption on the autoregressive process). Table 6 confirms that estimates are indeed almost identical to our principal results.

In the next columns of table 6, we follow a similar logic. We control for additional spatial and temporal autoregressive processes that could invalidate our results. In columns 2 and 3 we include the population weighted contemporaneous homicide rate and treatment variable for a municipality's direct neighbors (defined as sharing a boundary). The concern being addressed is that spatial spillovers may pose threats to exogeneity of our treatment variable. For one, Procede may be spatially correlated. Moreover, if in addition land certification in an adjacent municipality has an effect on homicides it would act as an omitted variable in our model. Secondly, changes in homicides in neighboring municipalities could affect Procede roll-out, as well as, homicides. Columns 2 and 3 show that their respective inclusion has no significant effect on our estimates. Homicides in neighboring municipalities enter significantly, but with the same caveat regarding endogeneity as the lagged dependent variables, as does Procede roll-out. Neither changes the point estimates on the variables of interest in any important way.

In columns 4-6 of table 6 we test for the existence of lead and lag effects of Procede. Given that our data spans the 1993-2007 period, by including leads and lags we lose the observations corresponding to 2007 and 1993, respectively. When lags are included, we also add an additional lag of the dependent variable for the sake of consistency. The results show that there is no significant effect of either on the

current violent deaths per capita (t-statistics are consistently close to or less than one), nor does their inclusion change our previous point estimate in any important way. The last two columns in table 6 control for one and two period lags of the changes in the number of ejidos that have finished their first information meeting, as explained in section 3. These variables ($AIA$) are constructed in the same way as the treatment of interest, Procede. With this exercise we want to show that the negative significant point estimate on Procede is not driven by a regression to mean effect after a temporary increase in violent deaths during the certification process. This could be the case if the prospect of land certification after the AIA would lead to an increase in conflicts. The results show that this concern is unwarranted.

Moving to table 7 we take a closer look at the role of potential mediating outcomes. Of particular interest here are electoral outcomes and the role of narcotics related crimes. Villarreal (2002) shows that over a similar time frame, more competitive municipal elections resulted in more violent deaths. While this effect, in light of the results in Castañeda Dower & Pfutze (2015) works against the average effect we find, it is nonetheless of interest to asses its importance. In columns 1-3, we control for the upcoming election's electoral margin (*Election Margin*); if the municipality is currently ruled by the PRI (*PRI Incumbent*); and whether an observation corresponds to a year with a municipal election, *Election Year*. Neither of these additional variables has any significant effect on the outcome. More importantly, their inclusion does not alter our prior results on Procede and continuous PRI rule. However, they may warrant some additional discussion. One important insight is that the electoral margin becomes statistically insignificant if continuous PRI rule is included. Another one is that the PRI being a mere incumbent, after having been out of power at least once, has no bearing on homicides. This supports our assumption that continuous PRI rule is a good measure of political entrenchment.

Another important determinant of violent deaths, even before the renewed increase in violence in the late 2000s, is the prevalence of the illegal drug industry. Land certification, by precipitating a loss of social control by local authorities, could result in an increase in the cultivation of illegal drugs, and hence in more narcotics related murders. As with the political outcomes, if it exists, this effect would work

against the effects found thus far. In columns 4 and 5 we include interaction terms of Procede with two time invariant measures of the degree of narcotic crimes. The first one is a binary variable indicating whether a municipality was among the top 100 marijuana producers over the 1994-2003 period, the second measures the average drug related indictments over 1997-1999 per 100,00 inhabitants. None of these terms is statistically significant, nor does their inclusion alter our other results.

Next, in columns 6 and 7 of the table we add controls for international migration and its effects on the demographic composition. Previous work (de Janvry et al. 2015, Valsecchi 2011) has shown that Procede had a positive effect on outmigration from the ejido locality. In particular, international migration to the United States is of interest here. Since most such migrants tend to be male and relatively young, a reduction of that particular demographic could on its own reduce violent deaths. Unfortunately, detailed data on international migration at the municipal level is only available for the years 2000 and 2010. The importance of social networks makes migration patterns highly persistent over time, and can also be expected to be an important factor in mediating the effect of land certification on international migration. For that reason, in column 8, we use the proportion of the population in 1990 that has returned from abroad since 1985 as a proxy for migration intensity in a municipality and interact it with the treatment variable. As in most of the other specifications, it is statistically insignificant and does not alter any other result. In column 7, we control directly for the demographic effects. Detailed demographic data is available every five years (1990, 1995, 2000, 2005, 2010). We constructed the difference in share of males aged 15-29 in the total population for these five year periods and assigned the change to each of the intervening years (i.e. the variable is constant in each municipality for five year periods). We interact this variable with the change in Procede in order to introduce more yearly variance (this interaction has the effect of putting a larger weight of the demographic variable on years with more certifications). All the additional variables are statistically insignificant, and their inclusion changes our point estimate on Procede only negligibly. Lastly, in column 11 we control for rainfall. Based on a municipality's closest weather station[19] we

[19]For this exercise, we used data from all Mexican weather stations made available to us by

constructed one variable equal to the absolute value of negative deviations from the municipality long-term yearly rainfall averages and zero in case of no such deviation; and the corresponding variable for positive deviations. Their inclusion, once more, does not change our results.

# 6 Conclusion

Property rights and violence have a storied association. The conventional view is that disputes arising from ambiguous claims to land or dysfunctional legal order lead to violence. The empirical evidence presented in this paper supports this view, albeit with a twist. We find that a substantial reduction in violent deaths in rural areas can be attributed to Mexico's most recent and largest land certification program, PROCEDE. However, land certification's curtailing of local political authority's discretionary power over land allocations explains the reduction rather than the standard apolitical mechanism. The magnitude of the effects are large. Scaled up to the entire country, and assuming that all municipalities were under continuous PRI rule, our estimates would imply a reduction of 2.4 in the total homicide rate. This would correspond to a 12.8% reduction in homicides for the year 1993 if all ejidos had been certified instantaneously.

This paper's main contribution is to demonstrate the importance of the political dimension of property rights to land for the literature on the political economy of conflict. A lack of well-defined and enforceable property rights will not only increase the risk of violent appropriation by other private agents, but also open the door to interference by political actors. These actors are most likely local authorities, who not only control local law enforcement, but also possess the local knowledge

the country's National Meteorological Service. Even though there are currently close to 3,000 such stations in service, for many of them data is missing from several years. We imposed as a conditions for inclusions that a weather station i) has complete data between the years 1993-2007 (i.e. the time under analysis), and ii) has data on at least eight other years since 1980. We then matched each municipality (based on the coordinates of its seat) to the closest remaining weather station under the condition that the difference in elevation is less than 500m. In total, we matched to 639 different stations. For each year under study, we then calculated the difference in total yearly rainfall between that year and the yearly average since 1980.

necessary to target individual agents. We have shown this to have been the case in Mexico, whenever municipal authorities were entrenched in power under the banner of the long-time state party PRI. Once democratic practices started to prevail, this effect disappeared. We did not find any evidence for a reduction in conflict due to a lower risk of appropriation by private individuals. While this result is important, it may be due to the specific context of Mexico's ejidos, where such appropriation would have only been feasible by a reduced group of members.

We believe that our results point toward a clear direction for future research. The political economy literature often models insecure property rights as an equilibrium outcome of a game played by political agents (Acemoglu & Robinson 2012, Baland & Robinson 2008, Conning & Robinson 2007). The strategic use of ambiguous property rights, or of a lack of enforcement thereof, by local political actors needs greater attention. In many settings, such as in ours, local political authorities (be they formal or informal) may take advantage of their position of power and engage in rent-seeking activities in violation of norms set at a higher level of authority. The reverse could equally hold. Insecure property rights could enable rent-seeking by higher-up elite, while providing a means for local elite to improve welfare on the ground. More research is needed to understand how the entitlement effect operates empirically and theoretically.

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Figure 1: Violent deaths per 100,000 inhabitants by year.

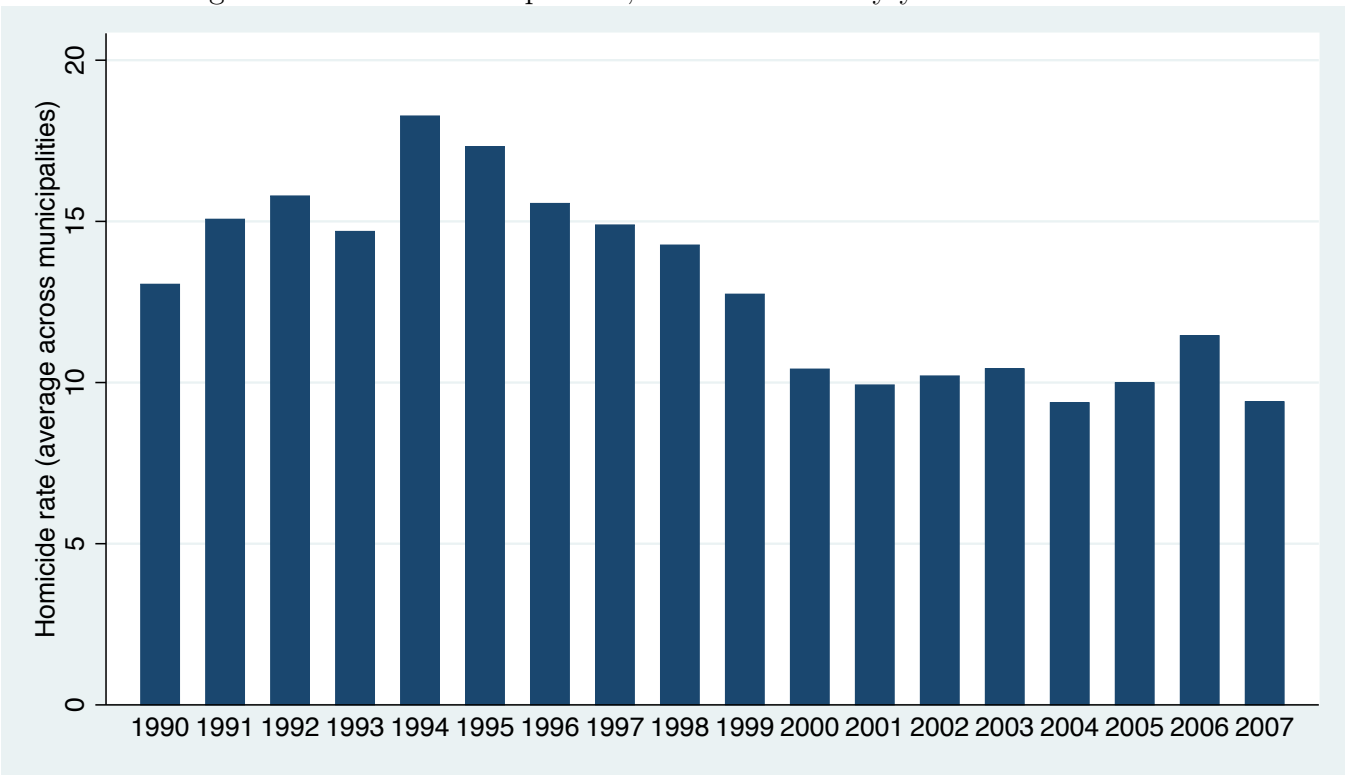
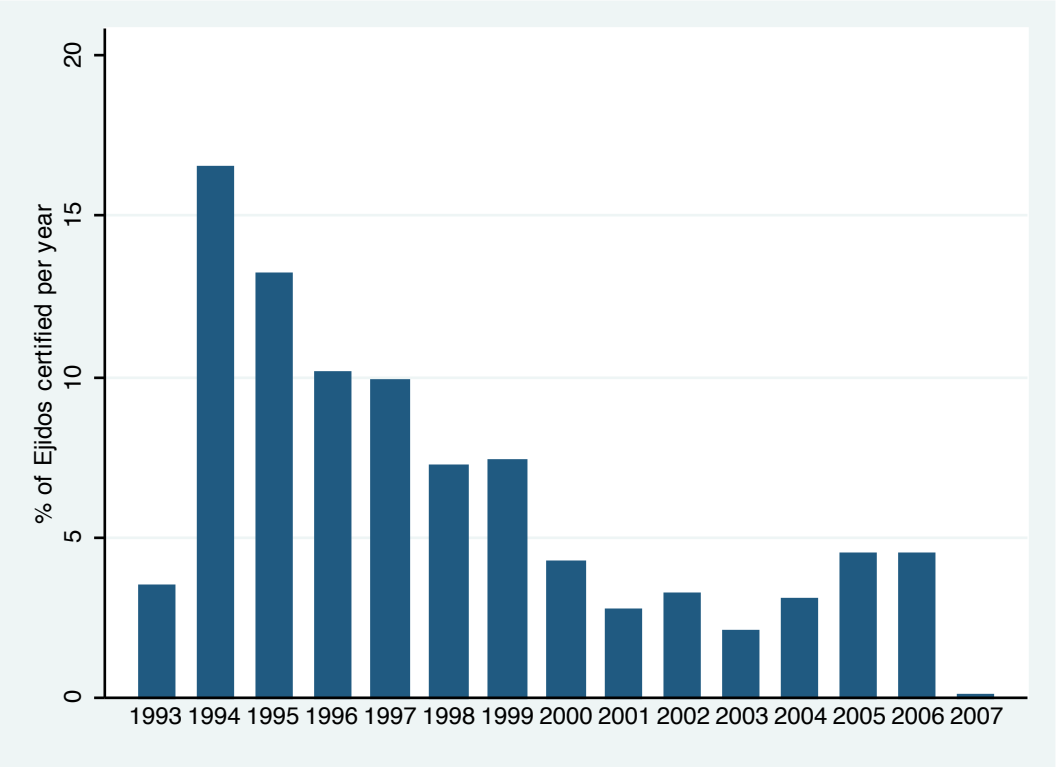


Figure 2: Percentage of Ejidos certified by year.




Notes: Graph shows percentage of Ejidos certified in each year. The bars sum to 92.25%.

Table 1: Summary Statistics

| Variable | Mean | Std. Dev. | Min. | Max. | N |
|---|---|---|---|---|---|
| Homicides per 100k pop. | 12.6 | 22.57 | 0 | 735.79 | 27,765 |
| Procede per 100k pop. | 50.99 | 67.4 | 0 | 875 | 27,765 |
| Population 1990 | 0.3687 | 0.9397 | 0.0038 | 16.5 | 27,765 |
| AIA per 100k pop. | 60.91 | 71.27 | 0 | 875 | 27,765 |
| Only PRI | 0.6066 | 0.4885 | 0 | 1 | 27,708 |
| Election Year | 0.3248 | 0.4683 | 0 | 1 | 27,765 |
| Election Margin | 0.2492 | 5.88 | 0 | 454.64 | 25,561 |
| PRI Incumbent | 0.7462 | 0.4352 | 0 | 1 | 27,708 |
| Dummy Top100 Marijuana Producer | 0.0443 | 0.2058 | 0 | 1 | 27,765 |
| Narco Crimes p.c. | 31.14 | 79.72 | 0 | 1,175.13 | 27,765 |
| Return Migration | 0.0022 | 0.0039 | 0 | 0.0546 | 27,765 |
| Young Males | 0.1279 | 0.0166 | 0.0431 | 0.2258 | 27,653 |
| Rain Below Yearly Average | 98.69 | 155.65 | 0 | 1,991.82 | 27,765 |
| Rain Above Yearly Average | 81.66 | 179.96 | 0 | 5,060.83 | 27,765 |

Table 2: Results for exogeneity of treatment: Average speed of roll-out 1993-2007

| | (1) | (2) | (3) |
|---|---|---|---|
| HomiTrend9092 | -.006 | -.005 | -.004 |
| | (.006) | (.005) | (.005) |
| Diff Indigenous | | .001 | .002 |
| | | (.006) | (.005) |
| Diff Illiterate | | .028*** | .008 |
| | | (.005) | (.005) |
| Diff Unemployed | | .003 | .001 |
| | | (.004) | (.004) |
| Diff Econ Active | | .0009 | -.003 |
| | | (.006) | (.005) |
| Diff Mean Wage | | -.011* | -.006 |
| | | (.007) | (.007) |
| Diff Wage StDev | | .008 | .008 |
| | | (.006) | (.006) |
| Diff Agriculture | | -.005 | -.002 |
| | | (.006) | (.006) |
| Diff Manufacturing | | -.003 | -.002 |
| | | (.005) | (.005) |
| Diff Construction | | -.003 | .008 |
| | | (.005) | (.005) |
| Diff Return | | -.007 | -.007 |
| | | (.005) | (.005) |
| Diff Population | | .005 | .001 |
| | | (.004) | (.005) |
| Area Ejidos 91 | | | -.002 |
| | | | (.002) |
| Ejidatarios 91 | | | -.038*** |
| | | | (.005) |
| Ejidos 91 | | | .028*** |
| | | | (.005) |
| Distance City | | | -.012*** |
| | | | (.005) |
| Ruggedness | | | -.043*** |
| | | | (.005) |
| Obs. | 1850 | 1850 | 1850 |
| $F$ statistic | 1.02 | 3.87 | 0.86 |
| P-Value | 0.31 | 0.00 | 0.58 |

Notes: ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors, in parentheses, are robust. The dependent variable is the average number of ejidos certified per 100,000 inhabitants over the 1993-2007 time period. All specifications control for state level fixed effects which are omitted from the table. The additional F-statistic test for joint insignificance of all time variant variables included.

Table 3: Main results

| | All Municipalities | | 1st Decile Excluded | | 1st Quintile Excluded | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **Continuous:** | | | | | | |
| Procede | -.034** | -.048** | -.025** | -.039*** | -.027** | -.042*** |
| | (.017) | (.021) | (.012) | (.015) | (.011) | (.015) |
| $F$ statistic | 12.011 | 12.035 | 13.106 | 13.078 | 14.962 | 14.963 |
| **Categorical:** | | | | | | |
| Procede | -.036*** | -.032** | -.035*** | -.031** | -.035*** | -.032** |
| | (.010) | (.013) | (.011) | (.013) | (.011) | (.014) |
| $F$ statistic | 14.563 | 14.094 | 14.183 | 13.821 | 14.113 | 13.818 |
| Obs. | 27,765 | 27,765 | 24,975 | 24,975 | 22,200 | 22,200 |
| Fixed Effects | No | Yes | No | Yes | No | Yes |

Notes: All models are first-differenced linear regressions; ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors , in parentheses, are clustered at the municipal level. The dependent variable is the municipal-level homicide rate in the top panel, and the corresponding categorical outcome in the bottom panel. Also, in the top panel all per-capita and population variables are scaled in terms of 100,000 inhabitants. The first two columns run the estimation on the whole sample, the second two omit the lowest population quintile, the last two are restricted to years 1993-1999. All include year fixed effects.

Table 4: Interaction effects with potential for political violence

| | All Municipalities | | 1st Decile Excluded | | 1st Quintile Excluded | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **Continuous:** | | | | | | |
| Procede | .014 | 004 | .034 | .026 | .020 | .016 |
| | (.022) | (.028) | (.023) | (.029) | (.022) | (.028) |
| Procede*PRI Only | -.066** | -.072** | -.072*** | -.080** | -.070*** | -.088*** |
| | (.027) | (.034) | (.027) | (.034) | (.026) | (.033) |
| PRI Only | .493*** | .011 | .436** | .007 | .522*** | .144 |
| | (.186) | (.121) | (.172) | (.123) | (.169) | (.109) |
| $F$ statistic | 10.122 | 10.138 | 11.805 | 11.895 | 13.222 | 13.322 |
| **Categorical:** | | | | | | |
| Procede | -.014 | -.002 | -.011 | .001 | -.011 | -.0008 |
| | (.014) | (.018) | (.015) | (.019) | (.016) | (.020) |
| Procede*PRI Only | -.042** | -.062** | -.047** | -.067*** | -.056** | -.071*** |
| | (.020) | (.024) | (.021) | (.025) | (.023) | (.027) |
| PRI Only | .034*** | .012** | .037*** | .013** | .047*** | .017** |
| | (.010) | (.006) | (.011) | (.006) | (.013) | (.007) |
| $F$ statistic | 12.736 | 12.251 | 12.617 | 12.245 | 12.667 | 12.234 |
| Obs. | 25,740 | 25,740 | 23,685 | 23,685 | 21,270 | 21,270 |
| Fixed Effects | No | Yes | No | Yes | No | Yes |

Notes: All models are first-differenced linear regressions; ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors , in parentheses, are clustered at the municipal level. The dependent variable is the municipal-level homicide rate in the top panel, and the corresponding categorical outcome in the bottom panel. Also, in the top panel all per-capita and population variables are scaled in terms of 100,000 inhabitants. All estimations exclude the lowest population decile. All include year fixed effects.

Table 5: Results for falsification tests: accidents and suicides

| | Accidents | | | | Suicides | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Procede | .006 | -.006 | -.008 | -.049 | -.002 | -.003 | -.008 | -.012 |
| | (.020) | (.026) | (.037) | (.043) | (.005) | (.007) | (.008) | (.010) |
| Procede*PRI Only | | | .025 | .070 | | | .007 | .012 |
| | | | (.045) | (.054) | | | (.011) | (.013) |
| PRI Only | | | -.037 | .052 | | | .059 | .082* |
| | | | (.288) | (.202) | | | (.066) | (.045) |
| $F$ statistic | 6.228 | 6.195 | 5.488 | 5.48 | 2.018 | 2.027 | 2.124 | 2.198 |
| Obs. | 24,975 | 24,975 | 23,685 | 23,685 | 24,975 | 24,975 | 23,685 | 23,685 |
| Fixed Effects | No | Yes | No | Yes | No | Yes | No | Yes |

Notes: All models are first-differenced linear regressions; ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors , in parentheses, are clustered at the municipal level. The dependent variable is the municipal-level homicide rate in the top panel, and the corresponding categorical outcome in the bottom panel. Also, in the top panel all per-capita and population variables are scaled in terms of 100,000 inhabitants. All estimations exclude the lowest population decile. All include year fixed effects.

Table 6: Spatial and time leads and lags

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Procede | .026 | .020 | .015 | .026 | .028 | .031 | .025 | .024 |
| | (.030) | (.028) | (.030) | (.030) | (.028) | (.029) | (.031) | (.031) |
| Procede*PRI Only | -.071** | -.073** | -.079** | -.071** | -.084** | -.080** | -.080** | -.080** |
| | (.034) | (.033) | (.033) | (.034) | (.033) | (.033) | (.033) | (.033) |
| PRI Only | -.004 | -.009 | .008 | -.004 | .006 | .032 | .007 | .006 |
| | (.143) | (.122) | (.123) | (.143) | (.124) | (.147) | (.123) | (.124) |
| Lag Procede | | | | .002 | | .008 | | |
| | | | | (.013) | | (.013) | | |
| Lead Procede | | | | | .014 | .010 | | |
| | | | | | (.016) | (.014) | | |
| Homicides Neighbors | | .278*** | | | | | | |
| | | (.045) | | | | | | |
| Procede Neighbors | | | .073** | | | | | |
| | | | (.031) | | | | | |
| Lag AIA | | | | | | | .0009 | .0008 |
| | | | | | | | (.019) | (.019) |
| Lag 2 AIA | | | | | | | | .008 |
| | | | | | | | | (.017) |
| Lag Homicide Rate | -.464*** | | | -.464*** | | -.452*** | | |
| | (.012) | | | (.012) | | (.013) | | |
| $F$ statistic | 99.046 | 12.645 | 11.467 | 94.323 | 11.435 | 91.04 | 11.241 | 10.648 |
| Obs. | 23,685 | 23,670 | 23,670 | 23,685 | 22,106 | 22,106 | 23,685 | 23,685 |

Notes: All models are first-differenced linear regressions; ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors, in parentheses, are clustered at the municipal level. The dependent variable is the municipal-level homicide rate, all per-capita and population variables are scaled in terms of 100,000 inhabitants. All specifications control for municipality-specific time trends (fixed effects) and year fixed effects. All estimations exclude the lowest population decile.

Table 7: Additional controls

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Procede | .034 | .021 | .018 | .019 | .025 | .021 | .027 | .026 |
| | (.028) | (.028) | (.030) | (.026) | (.029) | (.029) | (.029) | (.029) |
| Procede*PRI Only | -.089*** | -.084** | -.078** | -.075** | -.081** | -.077** | -.082** | -.079** |
| | (.034) | (.035) | (.034) | (.032) | (.034) | (.034) | (.034) | (.034) |
| PRI Only | .053 | .0009 | -.023 | .005 | .007 | .006 | .006 | -.003 |
| | (.122) | (.126) | (.126) | (.123) | (.123) | (.123) | (.125) | (.123) |
| Procede*Election Margin | -.025* | | | | | | | |
| | (.015) | | | | | | | |
| Election Margin | .473 | | | | | | | |
| | (.337) | | | | | | | |
| Procede*PRI Incumbent | | .009 | | | | | | |
| | | (.009) | | | | | | |
| PRI Incumbent | | -.386 | | | | | | |
| | | (.442) | | | | | | |
| Procede*Election Year | | | .023 | | | | | |
| | | | (.035) | | | | | |
| Election Year | | | -.436 | | | | | |
| | | | (.305) | | | | | |
| Procede*Marihuana | | | | .034 | | | | |
| | | | | (.024) | | | | |
| Procede*Narco Crimes | | | | | .008 | | | |
| | | | | | (.008) | | | |
| Procede*Return Migration | | | | | | .017 | | |
| | | | | | | (.013) | | |
| Procede*Young Males | | | | | | | .168 | |
| | | | | | | | (.170) | |
| Young Males | | | | | | | -8.034 | |
| | | | | | | | (12.364) | |
| Rain Below Yearly Average | | | | | | | | .0008 |
| | | | | | | | | (.0006) |
| Rain Above Yearly Average | | | | | | | | .002** |
| | | | | | | | | (.0007) |
| $F$ statistic | 10.695 | 10.648 | 10.694 | 11.1 | 11.225 | 11.387 | 10.883 | 11.064 |
| Obs. | 22,806 | 23,685 | 23,685 | 23,685 | 23,685 | 23,685 | 23,581 | 23,685 |

Notes: All models are first-differenced linear regressions; ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors, in parentheses, are clustered at the municipal level. The dependent variable is the municipal-level homicide rate, all per-capita and population variables are scaled in terms of 100,000 inhabitants. All specifications control for municipality-specific time trends (fixed effects) and year fixed effects. All estimations exclude the lowest population decile.

# Appendices

Table A.1 shows results for a few further restrictions on the sample that we omitted from the main text in the interest of space. For each specification, we show results with and without municipal specific time trends (fixed effects) As indicated at the bottom of the table. Columns 1 and 2 show results for municipalities in the lowest population decile only. The next two columns show results for the bottom quintile. These results, though very far from any statistical significance, are fairly similar to those in table 3 in terms of sign. This, once more, goes to show that by excluding the smallest municipalities we are not 'cherry picking' results, but merely reducing the variance in the outcome variable. Columns 5-6 excludes the top and bottom deciles, and columns 7-8 exclude the top and bottom quintiles. Here we want to show that our results in the last four columns in table 3 are not driven by the largest municipalities. It is evident that their exclusion does not alter our results. Next, in columns 9-10, we reduce the years under study to the period 1993-99; i.e. the time period with the largest amount of certifications and biggest drop in homicides. For comparisons sake, we use our preferred sample, excluding the bottom decile. Losing half our observations results in larger standard errors and slightly lower statistical significance. However our point estimates are very close to those in table 3. Lastly, columns 11-12 restricts the sample to the municipalities that had no ejidos without Procede certification by 2007. The reasoning behind this exercise is to make sure that incomplete certification does not act as an omitted variable biasing our results. We are naturally excluding municipalities with with more ejidos overall, reducing our sample by about one-third (we again exclude the bottom decile of municipalities). This smaller sample again increases our standard errors, resulting in lower levels of statistical significance. However, the point estimates are identical to the corresponding results from table 3.

Table A.1: Results for additional restrictions on sample

| | First Decile | | First Quintile | | Deciles 2-9 | | Quintiles 2-4 | | Years 1993-99 | | Finished Procede | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Procede | -.026 | -.039 | .003 | -.007 | .034 | .025 | .017 | .012 | .033 | .037 | .049 | .038 |
| | (.052) | (.064) | (.040) | (.049) | (.023) | (.029) | (.025) | (.030) | (.034) | (.047) | (.032) | (.042) |
| Procede*PRI Only | -.046 | -.043 | -.053 | -.054 | -.074*** | -.081** | -.076*** | -.093*** | -.065* | -.087* | -.087** | -.091* |
| | (.061) | (.073) | (.047) | (.056) | (.028) | (.034) | (.029) | (.036) | (.038) | (.052) | (.038) | (.048) |
| PRI Only | .560 | -.279 | -.024 | -.876* | .512*** | .030 | .720*** | .212 | .542* | .241 | .324 | -.149 |
| | (.986) | (.700) | (.591) | (.469) | (.193) | (.137) | (.217) | (.140) | (.318) | (.251) | (.222) | (.168) |
| $F$ statistic | 1.085 | 1.093 | 1.719 | 1.76 | 10.41 | 10.466 | 9.418 | 9.484 | 8.047 | 8.231 | 6.784 | 6.809 |
| Obs. | 2,055 | 2,055 | 4,470 | 4,470 | 20,940 | 20,940 | 15,825 | 15,825 | 11,053 | 11,053 | 14,415 | 14,415 |
| Fixed Effects | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |

Notes: All models are first-differenced linear regressions; ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors, in parentheses, are clustered at the municipal level. The dependent variable is the municipal-level violent deaths per capita, all per-capita and population variables are scaled in terms of 100,000 inhabitants. Columns 1-2 show results for the bottom decile of municipalities in terms of population. Columns 3-4 do the same for the bottom quintile. Columns 5-6 exclude the top and bottom deciles, and columns 7-8 exclude the top and bottom quintiles. Columns 9-10 restrict the sample to the years 1993-99, and columns 11-12 exclude municipalities that still had ejidos without Procede certification by 2007. The last four columns exlude the bottom decile. As indicated at the bottom of the table, the first column of each sample shows results in first differences, whereas the second columns adds municipality-specific fixed effects (linear trends).