Land Titles and Violent Conflict in Rural Mexico*

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Abstract

Insecure property rights are commonly thought to incite violence; however, discretionary control over land allocation can also serve to maintain social order. Using a conflict model, we demonstrate how these opposing forces alter the level of violence in a community. We then investigate the effect of a land certification program, which produced exogenous variation in tenure security over time and municipality, on violent deaths in the rural municipalities of Mexico from 1993-2007. We find that land titles significantly decrease violent deaths on average, but, as the model predicts, municipalities above a threshold on population size experience an increase. The rich panel structure of our data allows us to show that these results are robust to potential threats to exogeneity. Our findings illuminate a tension between local and national institutions in securing an environment that is conducive to economic growth.

JEL Classification: K42, O17, Q15

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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 the presence of insecure property rights inevitably involves 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 violence or the threat of violence constitutes one of the primary alternative means to secure ones interest in a resource.

Besides this direct efficiency loss, the threat of violence 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, leaving that less-able bodied individuals, such as children, to work away from 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 because they increase tenure security and, as a consequence, reduce the violent competition over uncertain claims to resources (Feder & Feeny 1991).[5] We argue that the tenure security effect does not fully characterize the relationship between property rights and violence. It is well-documented how communities use alternative institutions to provide social order when public order institutions have failed (McMillan & Woodruff 2000). Insecure property rights make it possible for a local power holder who has an interest in maintaining social order to punish individuals for violent behavior by exercising discretionary control over the distribution of land. In this case, which we refer to as an *entitlement* effect, land titles may actually increase the level of violence because they restrict the control of local power holders.

[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).

We build a standard conflict model that incorporates these two opposing effects to understand the nature of violence in a community with uncertain claims to land. We show that the entitlement effect dominates the tenure security effect when the population size is large relative to the size of the community over which the local power holder has control. The intuition is that tenure security can only reduce conflicts over resources with uncertain claims whereas the social order effect can extend to generic production, which is more important in larger populations.

We investigate this hypothesis empirically in the context of the ejido reform in Mexico, which instituted a large-scale land certification program during 1992-2007. It is a little known fact about Mexico that the country's national homicide rate fell dramatically from roughly 20 to 8 during this period. Some (e.g. Escalante-Gonzalbo (2009)) have argued that the land certification played a major role in this decrease but this hypothesis has never been rigorously tested.

We employ panel data methods to uncover the effect of land titles on violent deaths in roughly 2000 municipalities over this 15 year period. While participation in the land certification program was voluntary, we argue that the adoption of land titles can be treated as exogenous to violent conflict given our estimation strategy. In particular, we estimate the effect of the program in first differences so that any unobservable time invariant factors, such as persistent conflicts or violent culture, would be differenced away. We also present specifications that allow for spatial dependence, ruling out, for example, the possibility that the results are driven by the spatial redistribution of violent conflict, and for lead and lag effects of the land certification program, which would address a potentially serious concern that land conflicts had to be resolved before land certification could occur. While this latter concern is a potential threat to exogeneity, it also has a silver lining since it allows us to rule out a general criticism of land titling programs, that they ignore preexisting claims to land and consequently often cause conflict. Finally, our results are robust to the inclusion of municipal-specific linear time trends, which should address any concerns about differences in pre-trends.

We find that land certification lowers the violent deaths per capita on average. One standard deviation increase in land certification results in a reduction of the violent deaths per capita by 1.6 to 2.6, approximately 10% of the standard deviation in the violent deaths per capita. However, the effect of the reform is very heterogenous and corresponds to a decreasing effect in population size as the model predicts. A one standard deviation in land certification would reduce the violent deaths per capita by between 1.98 and 3 in a municipality with 10,000 inhabitants, but raise it by between 2.43 and 4.18 in a municipality with 100,000.

We perform two types of robustness checks to strengthen our case that the land certification reform had an impact on the number of violent deaths through these two channels. First, we incorporate data on elections, the illegal drug trade and migration to test for alternative mediating effects. Second, we rerun our results on restricted samples to exclude observations from municipalities below population thresholds, from municipalities that failed to achieve full certification by the end of Procede, and from years in which the program rollout slowed considerably. Beyond explaining why violent deaths per capita fell and fell more sharply for smaller municipalities, our results and model illustrate an important point concerning institutional change in the development process. The beneficial effects of improvements in institutions such as increases in tenure security in land may be undermined when other market-supporting institutions do not function well. In our case, some municipalities that adopted well-defined property rights bear a cost which they would not have had to bear had there been better public order institutions.

The rest of the paper proceeds as follows. Section 2 situates our paper in the previous literature. Section 4 presents the model. Section 3 provides a description of the Mexican context. Section 5 describes the data and empirical strategy and section 6 discusses the results. Section 7 concludes.

2 Previous Literature

The first contribution of this paper is to the empirical literature on property rights and conflict. There is a small but growing empirical literature on tracing the effects of insecure property rights on conflict (Clay 2006, Alston, Libecap & Mueller 1999, Deininger & Castagnini 2006). 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. However, the main contribution of our paper is that we also demonstrate the heterogenous effects of such changes. The literature on the agrarian reform of the ejido sector in Mexico has focused mostly on how changes in property rights affected agricultural production (Bouquet 2009), although there are a number of papers that have explored alternative outcomes. (de Janvry, Gonzalez-Navarro & Sadoulet forthcoming) investigate how the reform affected electoral outcomes and (de Janvry, Emerick, Gonzalez-Navarro & Sadoulet 2011) show that the reform had an impact on migration. We are the first paper to rigorously investigate how changes in tenure security in Mexico's ejido sector affect conflict.

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, the little literature that exists is mostly descriptive. For example, Escalante-Gonzalbo (2009) presents a very detailed analysis of the evolution of the homicide rate for different geographical aggregates, some of which will be replicated in this paper. One of the few exercises that establishes determinants of violent deaths is the study by Villarreal (2002) that shows how more competitive municipal elections in Mexico increased the number of violent deaths and Procede could have made municipal elections more competitive if, for example, PRI had involved ejido authorities to use the threat of land expropriation to marshal votes. In contrast, this paper argues that the land certification program, by increasing third party enforcement, decreased violent conflict related to land but this third party enforcement came at the expense of weakening the control of the ejido authorities, who regulated a wide range of activities, including deviant behavior. Fortunately, we will be able to distinguish the two mechanisms we have in mind from the violence associated with competitive elections as well as other explanations for increases and decreases of violence in rural Mexico such as the effects of the reform on migration (de Janvry et al. 2011). The second contribution of this paper is to models of resource allocation when property rights are insecure, also known as conflict models. 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. Hafer (2006) also studies a contest over an input, an indivisible asset. In her set-up, ownership of the asset is an all or nothing proposition. In contrast, we model a situation in which there is tenure insecurity over land that is conditional on the discretion of a local authority and the unilateral action of the agent. We feel this model fits better the situation at hand where property rights over land are more or less secure provided the economic agent follows certain rules.

Finally, in the recent literature on the political economy of development, insecure property rights are often an equilibrium outcome of a game played by political agents (Acemoglu & Robinson 2012, Baland & Robinson 2008, Conning & Robinson 2007). In these models, insecure property rights have a social cost but confer a private benefit to those in power. Our model suggests an important revision to these models since insecure property rights can carry a social benefit (as well as a social cost).

3 The Mexican Context

As mentioned in the introduction, the national yearly homicide rate in Mexico fell dramatically from roughly 20 to 8 deaths per 100,000 people between 1992 and 2007. 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. We will show that certification indeed played an important role, and in a manner that further accentuated the steeper drop in smaller towns.

Mexico's agricultural sector is divided into two different property regimes. In addition to private property, which tends to consist of larger and more productive units, the country also has a big sector of communally held land, consisting of around 30,000 *ejidos* [6]. Each ejido is an agricultural community centered on a rural locality to which land (in proximity of up to a 7km radius) has been granted by the government.[7] This sector comprises more than 50% of Mexico's national territory (albeit mostly unproductive, arid land), and is administered by a separate ministry at the federal level.

The ejido sector is rooted in the 1910 Mexican Revolution. One of the major groups fighting in the revolution was seeking redress for the encroachment of big haciendas on land that historically had belonged to rural communities. During the constitutional convention in 1917, this faction was able to enshrine and judicially protect the existence of agricultural communities in Article 27 of the Mexican Constitution, creating the legal entity of the ejido. Following the revolution, the first ejidos were created from dissolved haciendas, mostly as a way to reestablish historical land rights. Two decades later, in the 1930s, the left leaning president Lázaro Cárdenas, used the provisions of Article 27 to further his own policy agenda, creating a large number of additional ejidos. Given Mexico's persistent problem of landless rural poor, in the 1960s and 70s, several presidents continued to create ejidos, but due to a lack of suitable land (and an unwillingness to expropriate productive private land) were restricted to handing out unproductive, arid land, located mostly in the north of the country.

In any given ejido, 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 for agricultural production. This allotted land was to be communally held with individual ejido members assigned to individual plots for their own farming. These plots, however, could be taken away if an ejido member violated the rules. The rules

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

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

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.

In the early 1990s the Mexican government, under president Carlos Salinas, decided to radically reform the ejido sector by a constitutional amendment.[8] 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. However, in order to make these new regulations workable the government also realized that it had to put in place a property registry that documents an ejidos' external boundaries, as well as, individual plots.

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. The existing archives were also subject to corruption by parties involved in land disputes. The upshot is that seeking redress for tenure rights' violations from the federal government was mostly unfeasible or prohibitively costly. In consequence, the local ejido authorities, the *Comisariado Ejidal*, held considerable sway over allocation of individual plots and access to communal lands. The best documented evidence that ejido authorities used their power to influence behavior for purposes other than what was written in the constitution 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)). Of particular importance for the present exercise, is

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

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"[9], 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"[10].

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 (called *ejidatarios*) 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 had 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

[9]"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)

[10]"[...] 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)

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 the 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.

This drawn out process is at the heart of our identification strategy. For one, 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. This idea, which will be further tested in various different specifications, is at the heart of our empirical strategy described in section 5.

4 Model

Given that we can cleanly identify an effect of changes in tenure security, we develop a model to better understand how we would expect conflict to respond to an improvement in property rights in the ejido sector. Specifically, we augment a standard, two-period conflict model for which output is contestable by incorporating tenure insecurity over plots of land. This model allows us to generate an empirical prediction and suggests a corresponding econometric specification that is not obvious from the current literature. In this section we develop the model and discuss its implications. A more detailed derivation of the solution is delegated to an appendix.

In the first period, each agent is endowed with a unit of labor time and a unit of land. The agent can allocate the labor endowment towards predatory, productive, protective and outside activities whereas land can only be allocated to productive activities (either directly or indirectly through rental income). In the second period, each agent still receives a unit of labor but the endowment of land is conditional on the agent's actions in the first period.

We model this conditional tenure security in two ways. The first is familiar and already implicit in the standard conflict model. As in any conflict model, property rights are not perfectly secure and depend upon the other agents' predation activities, the agent's first party enforcement efforts and the enforcement technology, described by the parameter $\pi \in [\pi_l, \pi_u]$ with $M > \pi_u > \pi_l > 1$, where M is finite but arbitrarily large. Essential to this enforcement technology is the extent of imperfect information concerning property rights. Land titles directly affect π by improving information and increasing tenure security. Thus, the tenure security effect operates through an increase in π.

The second way is driven by the discretionary control of local power holders. In many cases with communally held land, there is a local governing body, who is given discretionary control over assets in order to enforce the community's rules (Ostrom 2005). While this governing body is self-interested, it is limited in power. Specifically, the community members permit the expropriation of land of those who misbehave, i.e. those who allocate labor time to unproductive or outside activities. However, it is much more difficult to expropriate the land of a community member who follows the rules.[11] In our model, discretionary control is determined exogenously and governed by the parameter, $\theta \in [0, 1]$. Here, the entitlement effect operates through a decrease in θ. The expected amount that will be expropriated is increasing in θ as well as the agent's level of unproductive activities. To represent the limited powers of expropriation, we assume that no expropriation of an agent occurs if the agent engages only in productive activities. If the local governing body expropriates land, the agent will not be able to use the land for productive purposes in the following period.

The production of output within the community depends on both the amount of labor allocated to production, l, and the amount of land over which the agent has control, h, described by the function $f(l, h) = Alh$. The output that an agent produces is not secure and the competition for this output is conflictive both at the individual and societal level. Following (Gonzalez 2012, 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 his output and successfully appropriates the fraction, $1 - p(\bar{e}, v_i)$ of the community's average output, where

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

[11] In our context, not only did social norms regulate ejido authorities, the state itself was also involved in internal governance of the ejido. In order for an ejido assembly meeting to be an official one, a representative of the Ministry of Agrarian Reform (Secretarìa de la Reforma Agraria, SRA) had to be present. The SRA was also involved in arbitration matters (de Janvry et al. 1997). In addition, if ejido authorities decided to exploit their position, they were essentially stationary bandits.

, with $\bar{a}$ representing the average for $a = l, e, v$ and $p(0,0) \equiv p_0$.

The agent's problem is to allocate labor towards productive (l_i) and unproductive purposes. Unproductive labor time is allocated to predatory claims on other's output, denoted by v_i, or allocated to first-party enforcement of one's own output (protection), denoted by e_i. Outside activities, denoted by z_i, also involve both productive and unproductive activities. One can think of z_i as the endogenously determined labor endowment in a conflict model of production outside the community. In this case, in equilibrium some amount of z_i will go to unproductive activities. Since the community observes productive and unproductive activities less well for outside activities, we make a simplifying assumption that z_i is punished in the same way as e_i and v_i. From the perspective of production within the community, each of these activities has the same opportunity cost in terms of labor units.[12]

The struggle over resources occurs both within the community and between community members and outsiders. In order to describe the likelihood of competing with a community member and an outsider, we enlist the function $q(N)$, which depends on the population size of the outsiders. With probability $q(N)$, where $q(\cdot)$ is decreasing at a increasing rate in N, the community member faces within community competition over output and, with probability (1-q(N)), the community member faces competition from outsiders.The payoff for competition with outsiders is described by the function $g(z, h) = \alpha ln(1 + z) * \gamma h$, which is increasing at a decreasing rate in z but increases linearly in h. One can think of γ as the rental price of land, which the community member receives for his/her allotment from an outsider. This rental income can be then used to buy shares in outsider's projects, which give an expected return of $ln(1 + z)$, where z reflects the higher returns gained from the community member's labor investment, either as a return to search, wage income or entrepreneurial rent.

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

[12]In our particular context, there is an additional way to interpret the discouragement of z_i. From its inception, the ejido sector requited a minimum amount of labor to be performed on ejido production and this minimum amount was not trivial. Therefore, punishing outside activities would have been put into effect for some labor amounts.

$$U_i(l_{1i}, e_{1i}, v_i^1, z_i^1, l_i^2, e_i^2, v_i^2, z_i^2) =$$

$$q(N)[p(e_i^1, \bar{v}^1)Al_i^1 + (1 - p(\bar{e}^1, v_i^1))A\bar{l}^1$$

$$+\delta[p(e_i^2, \bar{v}^2)A(1 - \theta(1 - l_i^1))l_i^2 + (1 - p(\bar{e}^2, v_i^2))A(1 - \theta(1 - (\bar{l}^1)\bar{l}^2]]$$

$$+(1 - q(N))(\gamma\alpha ln(1 + z_i^1) + \delta\gamma(1 - \theta(1 - l_i^1))\alpha ln(1 + z_i^2))$$

.

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

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

.

There is a community-wide adding-up condition but we will focus on a symmetric equilibrium which will automatically satisfy the adding-up condition.

For an interior solution, the marginal returns to each labor time activity must be equalized. We solve backwards, starting with the second period and taking the first period actions as given (see the appendix for the details). Equalizing the marginal return to protection and the marginal return to predation at an interior optimum, together with symmetry, 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 $e^{2*} = v^{2*}$. This equality insures that $p(\cdot, \cdot)$ simplifies to $\frac{\pi}{\pi+1}$ at the optimum.

Equalizing the marginal returns to production and protection at the interior optimum, together with the symmetry condition, yields the following choice for $l^{2*} = e^{2*}(1 + \pi)$. Next, equating the marginal product of outside activities and protection gives $z^{2*} = \eta\frac{\pi+1}{\pi} - 1$ where $\eta \equiv \frac{(1-q(N))\gamma\alpha}{q(N)A}$. The parameter η can be interpreted as the relative attractiveness of outside activities and is increasing in N. Note that for an interior solution, $\eta \in [\frac{\pi_u}{\pi_u+1}, \frac{2\pi_l}{\pi_l+1}]$. Finally, since the resource constraint binds at an interior solution, we can solve directly for $l^{2*} = (2 - \eta\frac{\pi+1}{\pi})\frac{\pi+1}{\pi+3}$. This implies that the second period level of production activity is at most $\frac{\pi+1}{\pi+3}$, the equilibrium level when $\theta = 0$ and there is no outside production, as it is in the standard model.

In order to solve for the first period values, we plug in the second period values and again we start by considering the equalization of the marginal return to protection and the marginal return to predation at the interior optimum and, in a similar manner, derive the following relationship, $e^{1*} = v^{1*}$, implying that $p(\cdot, \cdot)$ simplifies to $\frac{\pi}{\pi+1}$ at the optimum. Equalizing the marginal returns to protection and production at the interior optimum, yields $l^{1*} = e^{1*}(\pi+1)(1+\delta\theta\frac{\pi+1}{\pi+3}(2-\eta\frac{\pi+1}{\pi})+\delta\eta\frac{\pi+1}{\pi}\theta ln(\eta\frac{\pi+1}{\pi}))$. The difference between the ratio of productive and protective activities in the first and second periods reflects the fact that the enforcement mechanism θ functions as a markup on the opportunity cost of unproductive activities. This markup is zero if $\theta = 0$. For positive values of θ, the markup also depends on the discount factor, the conflict technology, π, and the relative return to outside activities.

Next we equate the marginal product of production and outside activities to find:

$$z^{1*} = \frac{\eta\frac{\pi+1}{\pi} - (1 + \delta\theta\frac{\pi+1}{\pi+3}(2 - \eta\frac{\pi+1}{\pi}) + \delta\eta\frac{\pi+1}{\pi}\theta ln(\eta\frac{\pi+1}{\pi})}{(1 + \delta\theta\frac{\pi+1}{\pi+3}(2 - \eta\frac{\pi+1}{\pi}) + \delta\eta\frac{\pi+1}{\pi}\theta ln(\eta\frac{\pi+1}{\pi})}$$

.

Under a binding resource constraint, $l^{1*} + z^{1*} + 2e^{1*} = 1$, and plugging in the other optimal values, we solve directly for l^{1*}:

$$l^{1*} = \frac{(\pi + 1)(2(1 + \delta\theta\frac{\pi+1}{\pi+3}(2 - \eta\frac{\pi+1}{\pi}) + \delta\eta\frac{\pi+1}{\pi}\theta ln(\eta\frac{\pi+1}{\pi})) - \eta\frac{\pi+1}{\pi})}{(\pi + 3) + \delta\theta(2 - \eta\frac{\pi+1}{\pi})\frac{(\pi+1)^2}{\pi+3} + \delta\eta\frac{(\pi+1)^2}{\pi}\theta ln(\eta\frac{\pi+1}{\pi})}.$$

One can now see how π and θ influence the equilibrium level of productive activities. If $\theta = 0$, the level of productive activities is the same in both periods and increasing in π. Holding π fixed, an increase in θ shifts the allocation of labor towards productive activities in the first period but has no effect on productive activities in the second period.

The introduction of land titles alters the equilibrium level of productive activities in both periods. First, land titles remove the possibility of second party enforcement using land expropriation (θ goes to zero), causing an entitlement effect. Second, land titles can improve first (and second and third) party enforcement by limiting the effectiveness of predatory activities because claims to land become less ambiguous (an increase in π), causing a tenure security effect.

Sending θ to zero and π to π_u:

$$l^1_{limit} = l^2_{limit} = (2 - \eta\frac{\pi_u + 1}{\pi_u})\frac{\pi_u + 1}{\pi_u + 3}$$

and

$$z^1_{limit} = z^2_{limit} = \eta\frac{\pi_u + 1}{\pi_u} - 1$$

.

To find the effect of the reform for a municipality of with an outsider population of size N and the (pre-reform) ejido sector described by (π, θ), we take the average per period difference between the post-reform level of productive activities and the pre-reform ones, where we assume an interior solution. Simplifying and rearranging in terms of the tenure security effect and the entitlement effect, we find that the difference is equal to:

$$\frac{(l^1_{limit} - l^{1*}) + (l^2_{limit} - l^{2*})}{2} = [2[\frac{\pi_u + 1}{\pi_u + 3} - \frac{\pi + 1}{\pi + 3}] - \eta[\frac{(\pi_u + 1)^2}{\pi_u(\pi_u + 3)} - \frac{(\pi + 1)^2}{\pi(\pi + 3)}]]$$

$$+\frac{1}{2}[2(\frac{\pi + 1}{\pi + 3}) - \eta\frac{\pi + 1}{\pi}[\frac{\pi + 1}{\pi + 3} - \frac{1}{(\pi + 1)[1 + \delta\theta(2 - \eta\frac{\pi+1}{\pi})\frac{\pi+1}{(\pi+3)} + \delta\theta\eta\frac{\pi+1}{\pi}ln(\eta\frac{\pi+1}{\pi})] + 2}]]$$

$$-\frac{(\pi + 1)[1 + \delta\theta(2 - \eta\frac{\pi+1}{\pi})\frac{\pi+1}{(\pi+3)} + \delta\theta\eta\frac{\pi+1}{\pi}ln(\eta\frac{\pi+1}{\pi})]}{(\pi + 1)[1 + \delta\theta(2 - \eta\frac{\pi+1}{\pi})\frac{\pi+1}{(\pi+3)} + \delta\theta\eta\frac{\pi+1}{\pi}ln(\eta\frac{\pi+1}{\pi})] + 2}$$

The first term represents the sum of the tenure security effects for period 1 and 2. This term is thus always positive and it is decreasing in N (η is increasing in N) for $\pi > \sqrt{\frac{3}{2}}$. Notice that this term does not depend on θ. The second and third term represent the entitlement effect for period 1 and has both positive and negative components. While the pure entitlement effect is negative, if enforcement actually had taken place in equilibrium before the reform, then resources (land) would have exited production in the second period as a result of the punishment, yielding a possible positive effect when this enforcement mechanism shuts down. The second term is always positive but decreasing in N. Finally, the third term is always negative and decreasing in N. Moreover, when θ is higher, the entitlement effect terms decrease in N more steeply. This is due to the fact that there is less (pre-reform) punishment in equilibrium when θ is higher.

We note that the primary driver behind the negative effect is an increase in outside activities. Thus, one should not interpret the entitlement effect as reducing aggregate production in the locality. Production outside the ejido sector is simply more susceptible to the violent competition for resources.

5 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 accidental, 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 year following occurrence. 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 data at which 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 the size ejidos (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. In terms of our theoretical model, a municipalities total population should be a good proxy for the number of outsiders, given that more urban municipalities have a smaller proportion of its population working in the agricultural sector. 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 that could potentially be 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, and the population in 1990 is also normalized accordingly. 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.

We take advantage of the rich panel data structure in our data by estimating our model in first differences. Though we will present a large number of different specifications, the principal model for municipality i in year t is:

$$\Delta H_{i,t} = \beta_0 + \beta_1 \Delta Proc_{i,t} + \beta_2 \Delta Proc_{i,t} * Pop_{i,1990} + \beta_3 \Delta Proc_{i,t} * 1stQuintile_i + \beta_4 \Delta H_{i,t-1} + \epsilon_t + \varepsilon_i + e_{i,t}, \quad (1)$$

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.

$Pop_{i,1990}$ denotes the population in 1990 in units of 100,000 inhabitants. The year and municipality specific error term $e_{i,t}$ will be clustered at the municipal level. Given that expression (1) is already in first differences, it only enters in the interaction term with $\Delta Proc_{i,t}$.

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 over 20% of municipalities have less than 5,000 inhabitants, a single murder would increase the dependent variable from zero to 20. In a municipality with 2,000 inhabitants, the increase would be 50. The bottom line is that smaller municipalities have a very high variance in the outcome variable, substantially increasing the standard error in all estimations. In order not to reduce our sample further by excluding the smallest municipalities, we opted to tackle this problem simply by including a dummy variable indicating a municipality in the lowest quintile with respect to its 1990 population in the first differenced equation, $1stQuintile_i$, and interact it with our treatment variable. The textbook procedure to address this kind of heteroskedasticity would be to inversely weight each observation by its standard deviation, which is in turn inversely related to the population size. While this is straightforward in cross-sectional models, it requires strong additional assumptions for panel data first differences and fixed effects [13]

One concern with panel data of this kind is potential non-contemporaneous reverse causation. Either because a high level of violent deaths may interfere with the certification process, or because it significantly affects the population size (e.g. through migration to other municipalities). In order to rule this out, we will control for different lags of the dependent variable, $\Delta H_{i,t-s}$. If results do not change significantly due to their inclusion, it can safely be assumed that no such reverse causation exists. Lagged dependent variables in a panel are by construction endogenous. The

[13] To see this more formally, note that $VAR(\Delta y_t) = VAR(y_t - y_{t-1}) = VAR(y_t) + VAR(y_{t-1}) - 2COV(y_t, y_{t-1}) = 2(VAR(y_t) - COV(y_t, y_{t-1}))$, assuming that the variance of y is constant over time. The term depends on the autocorrelation in y and can be either larger or smaller than $VAR(y_t)$. If fixed effects are added to the first differences, the expression becomes $VAR(\Delta y_t - \overline{\Delta y_t}) = VAR(\Delta y_t) + VAR(\overline{\Delta y_t}) - 2COV(\Delta y_t, \overline{\Delta y_t})$, where $VAR(\overline{\Delta y_t})$ and the covariance term depend on the autocorrelation of y across all time periods.

point estimates on their corresponding parameters must therefore be taken with a huge grain of salt, but are of no interest to us here.[14]

Lastly, we allow for a particular structure of the first differenced error term, consisting of ϵ_t, ε_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, ε_i, can be controlled for with municipality specific fixed effects, which are to be interpreted as municipality specific linear time trends given that the expression 1 is already in first differences. As discussed in section 3, and shown in figure 1, against the background of a sharply declining violent deaths per capita over the time period under study, there has also been a convergence across municipalities. Places with an initially higher rate, in particular smaller towns, experienced a sharper decline. If we want to rule out that the interaction term $\Delta Proc_{i,t} * Pop_{i,1990}$ simply captures these differential trends, using additional municipality specific fixed effects seems to be a sensible approach. 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 that assumes the exogeneity of Procede.[15]

[14] Technically, switching to the dynamic panel with individual fixed effects makes the coefficients vulnerable to Nickell bias (Nickell 1981). In our case, the coefficient on the lagged dependent variable potentially suffers from a bias of the order $1/T = 0.036$, which is fairly small but non negligible. However, the coefficients on Procede and the other control variables do not suffer from any bias if they are uncorrelated with the lagged dependent variable, which we argue is indeed the case. Yet, for the skeptic, we run a regression of the lagged dependent variable on Procede, which produces a point estimate of .011 with a standard error of .010 and is not statistically different from zero. For thoroughness, to get an approximation of the Nickell bias for the coefficient on Procede, we would then multiply .011 by roughly $|0.036|$ and obtain 0.0004 or less than 2% of the estimated coefficient and would not have an impact on the statistical significance of the coefficient. Note that we are not making a statement about the sign of the bias.

[15] One concern is that the year of an ejido's certification is partially determined by the the size of the municipality it is located in. Coupled with year-specific shocks differing by municipality size, our estimates could pick up spurious correlation. The sign of this bias could go either way. To asses the importance of this threat, we regressed the binary variable of certification at the ejido level on municipality total population in 1990 on a yearly basis, controlling for state level fixed effects. Throughout the years, there is no clear pattern on the sign. For only four years do we find that municipality size is statistically significant at the 10% level or lower: In 1993 with a positive sign (possibly explained by ejidos in or close to the state capitals), and in 1996, 2004, and 2005 with a negative sign. The magnitude of the effect is very small in all these cases, the probability

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.

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, *Violent deaths per capita* and *Procede p.c.*, capture the number of violent deaths and certified ejidos per 100,000 inhabitants, respectively. Given that demographic data is available every five years, the variables were constructed under the assumption of a constant population growth rate in the intervening years. The average violent deaths per capita by municipality over the 1993-2007 period was around 13, which is in line with the data presented in figure 1.

The other variables in the table will be used to test for alternative causal mechanisms. For some of the electoral variables, we are not able to observe results for all municipalities in all years, given that some elections have been annulled or due to data coding errors. *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 winner takes all). *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 year that changes), and 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

of certification changes by 0.2%-0.5% for each 100,000 inhabitants and any potential bias would be very small.

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.

6 Results

We present our principal results in tables 2 and 3. In subsection 6.1, we strengthen these main results using a number of additional specifications, testing for the robustness of our specification and potential alternative causal channels.[16] All tables, in addition to the variables listed in each table, include a full set of year dummies. In

[16]In addition to the robustness checks presented in subsection 6.1, we also investigated a number of additional estimations. First, we checked whether the results change if we drop the bottom population quintile. The results hold up and, more importantly, if we alternatively run our main specifications for the bottom quintile only, the sign on Procede is negative. Second, we excluded municipalities that still had uncertified ejidos at the end of 2007. Municipalities with ejidos that were not able to certify by 2007 would have a consistently lower value of our treatment variable (in levels and in differences). Using this sample, we arrive at very similar results for our treatment variable, despite the fact that we lose more than 36% of municipalities. Since most of the excluded municipalities are on the larger side, the interaction terms are qualitatively similar but statistically insignificant. Lastly, as figures 1 and 2 show most of the variance in the treatment and the outcome occurs during the 1990s. We rerun the main specification for the 1993-1999 period and our results are further confirmed.

the first two tables, we present results with municipality specific time trends in the last five columns and without them in the first five. In tables 4 and 5, we restrict the analysis to the specifications with such trends. Table 2 presents the core results for the whole set of municipalities. The net effect of land title certification on violent deaths is negative and statistically significant, strongly supporting the existence of a tenure security effect. The point estimates of the effect of land certification range between -0.023 and -0.039. Referring to the summary statistics in table 1, this implies that a one-standard deviation increase in Procede results in a between 1.6 and 2.6 lower violent deaths per capita. Comparing the first five to the last five columns, the second set of point estimates is consistently slightly larger, but not by a big margin. Also, the level of significance is slightly higher. This suggests that municipality specific time trends, and hence the characteristics that determine it, are not significantly correlated with the roll-out of Procede. Columns 1 and 6 report the simplest specification possible. In columns 2 and 7, we add the lag in the difference in the violent deaths per capita. The inclusion of the latter affects the point estimates on Procede only marginally, showing that non-contemporaneous reverse causation is not a concern. These results imply that, on average, land certification reduces violent crime in rural areas and are consistent with the model given that ejido production is the primary production activity for most residents in the average rural municipality.

The model suggests that as outside activities become more attractive, the net effect of land certification will fall and may even turn negative. In columns 3-5 and 8-10 we add the interaction term of Procede with the number of inhabitants in 1990 (our proxy for the value of outside activities) and with its squared term (given the diminishing marginal productivity of labor in outside activities). Columns 3-4 and 8-9 show that, as hypothesized, the tenure security effect is mediated by municipality size, providing evidence for the entitlement effect. Here again, we present results with and without the lagged dependent variable in order to show that our results are robust to its inclusion (and that non-contemporary endogeneity is not a concern). Furthermore, as can be seen in columns 5 and 10, this relationship follows an inverted-U relationship, which would make sense if larger municipalities had more effective institutions of public order due to economies of scale. Putting these number into context, a hypothetical one standard deviation increase in land certification would reduce the violent deaths per capita by between 1.98 and 3 in a municipality with 10,000 inhabitants, but raise it by between 2.43 and 4.18 in a municipality with 100,000.

Our estimation hence predicts a *positive* effect of land certification on violent deaths for larger municipalities. Our main hypothesis is the existence of two opposing effects, whose relative strength is mediated by a municipality's size. The sign and significance on the interaction terms of Procede with total population in 1990, however, would be equally consistent with a negative effect of tenure security on land related conflicts that weakens as towns grow larger. Finding a positive marginal effect of land certification on violent deaths in larger municipalities, on the other hand, clearly shows that both the tenure security and entitlement effects are at work. The estimates in column 10 imply that a positive marginal effect of Procede would be predicted for municipalities larger than 39,200 inhabitants. The distribution of municipality size is of course highly skewed, with a large number of small municipalities and a small number of very big ones. While the average population in 1990 is 36,870 (see table 1), the cutoff for the highest quintile is at 38,594. Thus, we predict a positive effect for less than 20% of our sample.

To make sure that the predicted positive effect is not driven by the other 80%, in table 3 we present our estimations for the subsample excluding the latter. The results clearly show that we indeed estimate a positive effect for Procede. It has to be kept in mind that we are looking at a much more homogenous group of municipalities. Therefore, the interaction term with the total population in 1990, and with its squared term, can be expected to be statistically insignificant. It is instructive to compare the last four columns, which include municipality specific time trends, to the first four, which do not. The point estimates between the two groups are almost identical, while in columns 6-10 the estimates are not statistically significant. We believe this to be the result of the much smaller sample size coupled with the additional loss of variance in our estimation due to demeaning. Using the point estimates in columns 2 and 6 of table 3, we now predict that, for this subgroup, the same one standard deviation increase in land certification used above, increases the violent deaths per capita by 2.56-3.03.

6.1 Additional specifications

The results just presented provide very strong support for our principal hypotheses. We now 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 4, we address the first concern. We only show results for the specification with municipality specific trends. The output has, therefore, to be compared to column 10 in table 2 for the first four and last two columns, and to column 7 in table 2 for the three columns in between.

In column 1, we test for the possibility that the change in Procede may be correlated to changes in the violent deaths per capita in the two previous periods. While the twice lagged dependent variable is highly significant, its inclusion only changes the point estimates of interest very marginally. As before, since lagged dependent variables are by construction endogenous, their point estimates are biased and of no interest here. The next two columns test for the existence of endogeneity due to spatial interdependence. The 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. We constructed variables of population weighted averages of Procede and violent deaths per 100,000 inhabitants for all adjacent municipalities (i.e. those sharing a boundary). Columns 2 and 3 show that their respective inclusion has no significant effect on our estimates. Violent deaths in neighboring municipalities enter significantly, but with the same caveat regarding endogeneity as the lagged dependent variables. For changes in neighbors' Procede no significant effect exists, but its inclusion slightly increases the standard errors, probably due to added collinearity.

In columns 4-6 of table 4 we test for the existence of lead and lag effects of Procede. In order to keep the number of interaction and leads/lags to a manageable size, we only show results for the specification without interactions with the population size, corresponding to column 6 in table 2. 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 less than one, in most cases by far), nor does their inclusion change our previous point estimate significantly. The one-sided hypothesis of an offsetting effect of either the lead or the lag can consistently be rejected at the 5% level.

The last two columns in table 4 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 ways 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.

In table 5 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 a higher violent deaths. While this effect works against the average effect we find, the effect on competitive elections could explain why we observe an entitlement effect for larger municipalities. In columns 1-4, we control for whether an observation corresponds to a year with a municipal election, *Election Year*, the electoral margin in that elections or, in non-election years, in the next election, *Election Margin*, and whether at the time the municipality had been continuously ruled by the formerly dominant PRI, *Only PRI*, or is at least currently ruled by it, *PRI Incumbent*. As before, the baseline specification corresponds to column 8 in table 2. The only electoral variable that turns out to be statistically significant is, in line with the results in Villarreal (2002), the electoral margin. The effect of its inclusion on our parameters of interest, however, is negligible.

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. However, in this case, the bias from omitting the drug-related variables would work against both the tenure security effect and the entitlement effect, since one would expect the illegal narcotics crop production to be more prevalent in smaller, more remote, municipalities. In columns 4 and 6 we included 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.

In the last column of the table we interact a proxy for the intensity of migration. Previous work (de Janvry et al. 2011, 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, 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.

Lastly, table 6 replicates the results of columns 9 and 10 from table 2 under the exclusion of different sets of observations [17]. In columns 1 and 2, the quintile of smallest municipalities is excluded, and 3-4 show results for municipalities in the

[17]We have results for all ten specifications presented in table 2, which are omitted in the interest of space. However, they closely mirror the findings presented here.

lowest population quintile only. For the first two columns, the results are close to identical to those of table 2; for the second two columns, the results are very far from any statistical significance, but fairly similar to those in table 2 in terms of sign. Columns 5 and 6 exclude municipalities that still had non-certified ejidos by 2007 are. Again, the results are essentially identical to those in table 2. The only difference is that the interaction term with municipality size is mostly insignificant. This is because many larger municipalities, having more ejidos and therefore with higher probability at least one that resisted certification. are dropped from the analysis. Columns 7 and 8 use the full set of municipalities, but restricts the years to the 1993-99 period, i.e. to the period that saw most the the ejido certification and also the sharpest decline in homicides. The results become stronger in terms of significance and with point estimates doubling in size in some specifications. But since the point estimates on the interaction terms also increase, the predicted partial effects does not change by nearly as much as the point estimates at first indicate.

7 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. 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. In this context, land titles more clearly specify property rights in the ejido sector and, as a consequence, lead to less conflictive behavior.

This paper also argues that the conventional view, which is based on a tenure security effect, masks an entitlement effect. When local authorities use their discretionary control of land allocation to deter conflictive behavior, insecure property rights maintain order even when other institutions of public safety fail to do so. In this case, land titles will not necessarily lead to more peaceful interactions because land titles make it more difficult to punish conflictive behavior through land expropriation. We build a standard conflict model that accounts for these two opposing effects and generate an empirical prediction concerning the net effect of this large-scale land certification program. The model predicts that the tenure security effect will dominate the entitlement effect when the ejido sector constitutes a large part of the economic activity in a given locality; however, when the ejido sector is small in size relative to the locality, the entitlement effect will dominate. The empirical prediction is confirmed in the data: larger municipalities for which the ejido sector is a relatively smaller part experience an increase in violent deaths as a result of the land certification program.

From a development perspective, the model and empirical results point to the importance of local institutions and informal enforcement mechanisms in establishing state capacity in a developing economy such as Mexico. Our model shows that the fundamental political conundrum – that only a state powerful enough to break the rule of law can enforce the rule of law, as (Weingast 1995) cogently puts it – operates on the local level. Moreover, our model and results highlight an additional dilemma that arises from a trade-off between strengthening the state's capacity to enforce property rights and maintaining the power of local authorities to ensure social order.

This dilemma is especially important in the context of Mexico's democratic transition, given the hierarchical nature of the hegemonic political power of PRI. Since PRI relied on local institutions such as the ejido to enforce and maintain order, as PRI lost power, local institutions could have been weakened if they required the support of PRI or strengthened if PRI's absence led to greater scope for local control. In this particular case, the downfall of PRI and the weakening of informal enforcement coincided with the introduction of land titles. An interesting counterfactual is whether informal enforcement mechanisms would have evolved along different lines had the constitutional reform of 1992, which relaxed many of the constraints on the factors of production within the ejido sector, taken place several decades before the initiation of the land certification program. If so, perhaps the weakening of PRI may not have been associated with a power vacuum at the local level, which some have argued has been a contributor to the upsurge in drug-related violence that we observe today (O'Neil 2009). Alternatively, without the land titling program, the continued presence of tenure insecurity may have still limited the mobility of factors of production within the ejido sector despite the constitutional reforms. Given that the threat of land expropriation is a relatively cheap enforcement mechanism, there may be little incentive for other informal enforcement mechanisms to evolve and the counterfactual would then yield very similar results as to what we find.

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Figure 1: Violent deaths per 100,000 inhabitants by size of municipality.

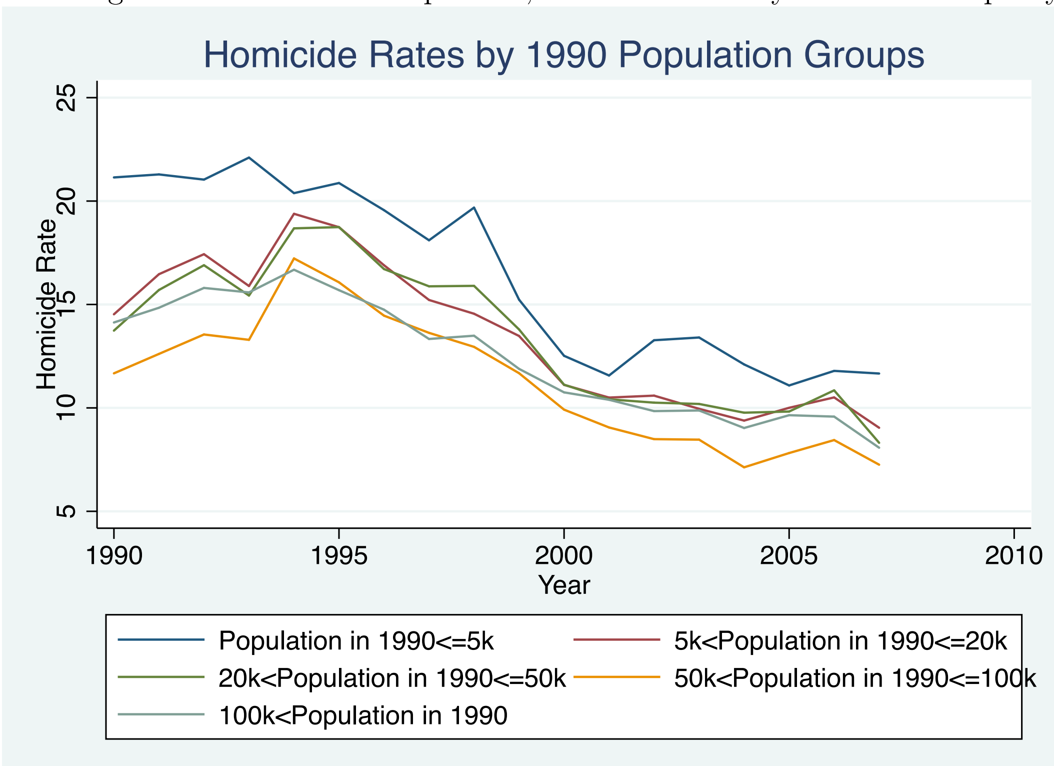

Table 1: Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Violent deaths per 100k pop.	12.5961	22.5677	0	735.7859	27765
Procede p.c.	50.9879	67.3993	0	875	27765
Population 1990	0.3687	0.9397	0.0038	16.5021	27765
1st quintile of pop. 1990	0.2004	0.4003	0	1	27765
Election Year	0.3248	0.4683	0	1	27765
Election Margin	0.2492	5.88	0	454.6364	25561
Only PRI	0.6066	0.4885	0	1	27708
PRI Incumbent	0.7462	0.4352	0	1	27708
Dummy Top100 Marijuana Producer	0.0443	0.2058	0	1	27765
Narco Crimes p.c.	31.1357	79.7165	0	1175.1266	27765
Return Migration	0.0022	0.0039	0	0.0546	27765

Figure 2: Percentage of Ejidos certified by year.

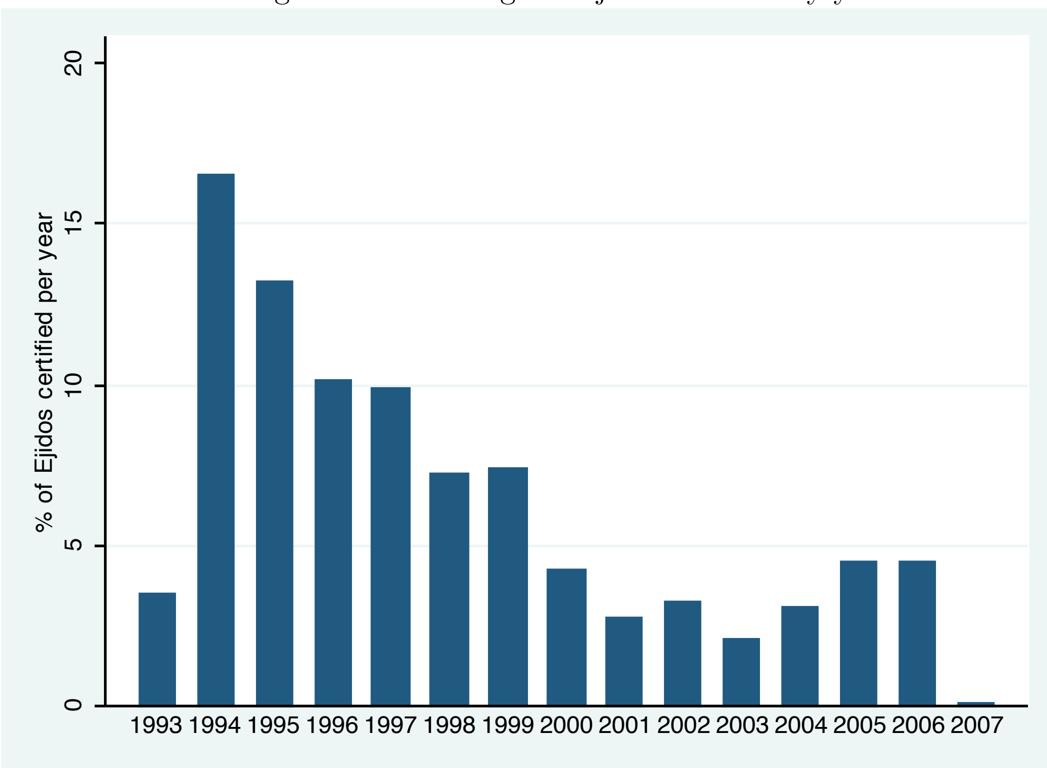

Table 2: The core result with and without municipality-specific trends

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Procede	-.029***	-.023**	-.036***	-.031**	-.040***	-.039***	-.036**	-.054***	-.050***	-.062***
	(.011)	(.010)	(.013)	(.012)	(.014)	(.015)	(.014)	(.019)	(.018)	(.021)
Procede*Pop90			.051*	.054*	.116**			.095**	.091**	.180**
			(.029)	(.030)	(.047)			(.045)	(.043)	(.070)
Procede*Pop90SQ					-.040***					-.056**
					(.014)					(.022)
1st Quintile*Procede	-.009	.017	-.002	.024	.031	-.013	.017	.0002	.030	.040
	(.025)	(.020)	(.026)	(.022)	(.022)	(.033)	(.026)	(.034)	(.028)	(.029)
Lag Homicide Rate		-.509***		-.509***	-.509***		-.516***		-.516***	-.516***
		(.016)		(.016)	(.016)		(.016)		(.016)	(.016)
Obs.	27765	27765	27765	27765	27765	27765	27765	27765	27765	27765
F statistic	11.26	88.944	11.437	86.521	81.984	11.28	87.163	11.373	84.312	79.92

Notes: ***, **, 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. The first five columns present estimates in first differences, the last five first differences with municipality fixed effects (municipality specific time trends). All have year fixed effects.

Table 3: Only municipalities for which positive results would be expected

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Procede	.039*	.045**	.055**	.067**	.084**	.037	.038	.062	.071*	.097
	(.020)	(.020)	(.028)	(.027)	(.041)	(.028)	(.026)	(.045)	(.041)	(.063)
Procede*Pop90			-.027	-.038	-.073			-.042	-.056	-.110
			(.029)	(.031)	(.067)			(.044)	(.043)	(.093)
Procede*Pop90SQ					.011					.017
					(.014)					(.019)
Lag Homicide Rate		-.378***		-.378***	-.378***		-.388***		-.388***	-.388***
		(.021)		(.021)	(.021)		(.021)		(.021)	(.021)
Obs.	5430	5430	5430	5430	5430	5430	5430	5430	5430	5430
F statistic	8.647	54	8.384	51.379	48.443	8.526	56.911	8.354	53.998	51.091

Notes: ***, **, 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. The first five columns present estimates in first differences, the last five first differences with municipality fixed effects (municipality specific time trends). All have year fixed effects.

Table 4: Additional specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Procede	-.068***	-.077***	-.065***	-.079***	-.044***	-.035**	-.044***	-.085***	-.086***
	(.020)	(.020)	(.020)	(.025)	(.014)	(.015)	(.015)	(.027)	(.025)
Lag Procede					.007		.013		
					(.015)		(.016)		
Lead Procede						.003	.001		
						(.014)	(.018)		
Procede*Pop90	.190***	.198***	.187***	.134*				.203***	.227***
	(.068)	(.067)	(.069)	(.079)				(.068)	(.085)
Procede*Pop90SQ	-.063***	-.068***	-.061***	-.054**				-.062***	-.059**
	(.022)	(.022)	(.022)	(.023)				(.022)	(.028)
Homicides Neighbors			.176***						
			(.039)						
Procede Neighbors				.158					
				(.120)					
Lag AIA								.017	.012
								(.028)	(.021)
Lag2 AIA									.020
									(.017)
1st Quintile*Procede	.043	.056*	.033	.047	.023	.014	.021	.054*	.079**
	(.029)	(.029)	(.029)	(.030)	(.027)	(.026)	(.027)	(.030)	(.037)
Lag 1st Quintile*Procede					.063		.062		
					(.042)		(.043)		
Lead 1st Quintile*Procede						.039	.063*		
						(.027)	(.034)		
Lag Homicide Rate	-.664***	-.727***	-.511***	-.515***	-.657***	-.499***	-.638***	-.657***	-.718***
	(.016)	(.017)	(.015)	(.016)	(.016)	(.013)	(.013)	(.016)	(.018)
Lag2 Homicide Rate	-.301***	-.449***			-.297***		-.290***	-.296***	-.449***
	(.017)	(.021)			(.017)		(.017)	(.017)	(.021)
Lag3 Homicide Rate		-.235***							-.239***
		(.017)							(.017)
Obs.	27765	25914	27735	27735	25914	25914	24063	25914	24063
Obs.	27765	25914	27735	27735	25914	25914	24063	25914	24063
F statistic	132.162	124.833	78.633	75.993	128.281	94.577	145.413	127.419	90.98

Notes: ***, **, 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. All columns present first differences with year and municipality fixed effects (municipality specific time trends).

Table 5: Role of political variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Procede	-.062***	-.065***	-.063***	-.063***	-.070***	-.066***	-.071***
	(.021)	(.020)	(.020)	(.020)	(.021)	(.021)	(.022)
Procede*Pop90	.179**	.201***	.186***	.189***	.186***	.184***	.189***
	(.070)	(.070)	(.070)	(.070)	(.071)	(.071)	(.071)
Procede*Pop90SQ	-.056**	-.062***	-.058***	-.058***	-.058***	-.058**	-.059***
	(.022)	(.022)	(.022)	(.022)	(.022)	(.023)	(.023)
1st Quintile*Procede	.040	.032	.042	.042	.043	.040	.040
	(.029)	(.027)	(.029)	(.029)	(.030)	(.029)	(.029)
Lag Homicide Rate	-.516***	-.502***	-.516***	-.516***	-.516***	-.516***	-.516***
	(.016)	(.015)	(.016)	(.016)	(.016)	(.016)	(.016)
Election Year	-.369						
	(.274)						
Election Margin		-.003***					
		(.0009)					
Only PRI			.510				
			(.373)				
PRI Incumbent				-.311			
				(.306)			
Top100 Marijuana Producer*Procede					.090		
					(.087)		
Narco Crimes*Procede						.0000683	
						(.0000773)	
Return Migration*Procede							2.874
							(2.216)
Obs.	27765	25561	27708	27708	27765	27765	27765
F statistic	77.878	77.594	75.223	76.897	78.369	79.517	76.289

Notes: ***, **, 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. All columns show first differences with year and municipality fixed effects (municipality specific time trends).

Table 6: Results for reduced samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Procede	-.053***	-.064***	-.021	-.007	-.061**	-.075**	-.091***	-.112***
	(.017)	(.020)	(.039)	(.036)	(.026)	(.031)	(.025)	(.029)
Procede*Pop90	.080**	.167**	.533	-1.217	.147	.291	.095	.264**
	(.041)	(.067)	(1.337)	(4.462)	(.107)	(.178)	(.066)	(.110)
Procede*Pop90SQ		-.055***		36.611		-.139		-.111***
		(.021)		(98.993)		(.087)		(.036)
1st Quintile*Procede					.024	.036	.059*	.076**
					(.033)	(.037)	(.035)	(.037)
Lag Homicide Rate	-.469***	-.469***	-.546***	-.546***	-.507***	-.507***	-.512***	-.512***
	(.015)	(.015)	(.024)	(.024)	(.014)	(.014)	(.018)	(.018)
Obs.	22200	22200	-27783.29	-27783.25	17655	17655	12957	12957
F statistic	71.734	67.779	54.466	51.669	90.616	86.168	96.382	87.686

Notes: ***, **, 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. All columns show first differences with year and municipality fixed effects (municipality specific time trends). Columns 1-2 show results excluding the quintile of smallest municipalities, 3-4 show results for only that lowest quintile. Columns 5-6 exclude municipalities that had at least one ejido that had not been certified by 2007, columns 7-8 show results for all municipalities, but only years 1993-99.

Appendix

For an interior solution, the marginal returns to each labor time activity must be equalized. We solve backwards, starting with the second period and taking the first period actions as given.

We start by considering the equalization of the marginal return to protection and the marginal return to predation at the interior optimum:

$$q(N)\delta\frac{\partial p(e_i^2,\bar{v^2})}{\partial e_i}A(l_i^2)(1-\theta(1-l_i^1)) = -q(N)\delta\frac{\partial p(\bar{e^2},v_i^2)}{\partial v_i}A(\bar{l}^2)(1-\theta(1-\bar{l}^1)).$$

By symmetry, we get $p(e_i,\bar{v}) = p(\bar{e},v_i)$ since $e_i = \bar{e}$ and $v_i = \bar{v}$. Using this equality together with the fact that $l_i = \bar{l}$, we can say that:

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

We can then determine that in a symmetric equilibrium, the optimal allocation of protection and predation is at exactly the same level and we have $e^{2*} = v^{2*}$. Notice that $p(\cdot,\cdot)$ simplifies to $\frac{\pi}{\pi+1}$ at the optimum.

Next, we account for the equalization of marginal returns to production and protection at the interior optimum, which yields the following equation:

$$q(N)\delta\frac{\partial p(e_i^2,\bar{v^2})}{\partial e_i}A(l_i^2)(1-\theta(1-l_i^1)) = q(N)\delta p(e_i^2,\bar{v^2})A(1-\theta(1-\bar{l}^1)).$$

The symmetry condition also simplifies the choice for l and $l^{2*} = e^{2*}(1+\pi)$.

Equating the marginal product of outside activities and protection gives:

$$(1-q(N))\delta\frac{\gamma(1-\theta(1-l_i^1))\alpha}{1+z^2} = q(N)\delta\frac{\partial p(e_i^2,\bar{v^2})}{\partial e_i}A(l_i^2)(1-\theta(1-l_i^1))$$

Let $\eta \equiv \frac{(1-q(N))\gamma\alpha}{q(N)A}$. The parameter η can be interpreted as the relative attractiveness of outside activities and is increasing in N. Note that for an interior solution, $\eta \in [\frac{\pi_u}{\pi_u+1}, \frac{2\pi_l}{\pi_l+1}]$.

Solving directly for $z^{2*} = \eta\frac{\pi+1}{\pi} - 1$, which gives $l^{2*} = (2-\eta\frac{\pi+1}{\pi})\frac{\pi+1}{\pi+3}$.

In order to solve for the first period values, we plug in the second period values:

$$U_i(l_i^1, e_i^1, v_i^1, z_i^1, l_i^{2*}, e_i^{2*}, v_i^{2*}, z_i^{2*}) =$$

$$q(N)[p(e_i^1, \bar{v^1})Al_i^1 + (1-p(\bar{e^1}, v_i^1))A\bar{l^1} +$$

$$\delta[\frac{\pi}{1+\pi}A\frac{\pi+1}{\pi+3}(2-\eta\frac{\pi+1}{\pi})(1-\theta(1-l_i^1))] + \frac{1}{\pi+1}A(1-\theta(1-(\frac{\pi+1}{\pi+3}(2-\eta\frac{\pi+1}{\pi})(1-\theta(1-\bar{l^1}))]]$$

$$+(1-q(N))(\gamma\alpha ln(1+z_i^1) + \delta\gamma(1-\theta(1-l_i^1))\alpha ln(\eta\frac{\pi+1}{\pi}))$$

.

We start by considering the equalization of the marginal return to protection and the marginal return to predation at the interior optimum:

$$q(N)\frac{\partial p(e_i^1, \bar{v^1})}{\partial e_i}Al_i^1 = -q(N)\frac{\partial p(\bar{e^1}, v_i^1)}{\partial v_i}A(\bar{l^1})).$$

By symmetry, we get $p(e_i, \bar{v}) = p(\bar{e}, v_i)$ since $e_i = \bar{e}$ and $v_i = \bar{v}$. Using this equality together with the fact that $l_i = \bar{l}$, we can say that:

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

We can then determine that in a symmetric equilibrium, the optimal allocation of protection and predation is at again exactly the same level and we have $e^{1*} = v^{1*}$. Again, $p(\cdot, \cdot)$ simplifies to $\frac{\pi}{\pi+1}$ at the optimum.

Next, we account for the equalization of marginal returns to protection and production at the interior optimum, which yields the following equation:

$$q(N)[\frac{\partial p(e_i^1, \bar{v^1})}{\partial e_i}A(l_i^1)] = q(N)[p(e_i^1, \bar{v^1})A + \delta\theta\frac{\pi}{\pi+1}A(2-\eta\frac{\pi+1}{\pi})\frac{\pi+1}{\pi+3}] + (1-q(N))\delta\gamma\theta\alpha ln(\eta\frac{\pi+1}{\pi}).$$

We can then rewrite $l^{1*} = e^{1*}(\pi+1)(1+\delta\theta\frac{\pi+1}{\pi+3}(2-\eta\frac{\pi+1}{\pi}) + \delta\eta\frac{\pi+1}{\pi}\theta ln(\eta\frac{\pi+1}{\pi}))$.

Equating the marginal product of production and outside activities gives:

$$q(N)[p(e_i^1, \bar{v^1})A + \delta\theta\frac{\pi}{\pi+1}A(2-\eta\frac{\pi+1}{\pi})\frac{\pi+1}{\pi+3}] + (1-q(N))\delta\gamma\theta\alpha ln(\eta\frac{\pi+1}{\pi}) = (1-q(N))\frac{\gamma\alpha}{1+z_i^1}$$

Solving directly for

$$z^{1*} = \frac{\eta\frac{\pi+1}{\pi} - (1+\delta\theta\frac{\pi+1}{\pi+3}(2-\eta\frac{\pi+1}{\pi}) + \delta\eta\frac{\pi+1}{\pi}\theta ln(\eta\frac{\pi+1}{\pi})}{(1+\delta\theta\frac{\pi+1}{\pi+3}(2-\eta\frac{\pi+1}{\pi}) + \delta\eta\frac{\pi+1}{\pi}\theta ln(\eta\frac{\pi+1}{\pi})}$$

The optimal allocation will not waste any labor resources, giving $l^{1*} + z^{1*} + 2e^{1*} = 1$.

With these equations, we can then solve for l^{1*}:

$$l^{1*} = \frac{(\pi+1)(2(1+\delta\theta\frac{\pi+1}{\pi+3}(2-\eta\frac{\pi+1}{\pi}) + \delta\eta\frac{\pi+1}{\pi}\theta ln(\eta\frac{\pi+1}{\pi})) - \eta\frac{\pi+1}{\pi})}{(\pi+3) + \delta\theta(2-\eta\frac{\pi+1}{\pi})\frac{(\pi+1)^2}{\pi+3} + \delta\eta\frac{(\pi+1)^2}{\pi}\theta ln(\eta\frac{\pi+1}{\pi})}$$