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Abstract

An important aspect of property rights is specificity, the ability of a third party to enforce rights. The empirical literature rarely isolates the effect of specificity because exogenous changes, due to land reforms, either simultaneously change both control and specificity or exclusively change control. We investigate the effect of specificity in the context of the 1992 Salinas land reforms in Mexico, which constitutionally changed individual control rights for all communal landholders but reserved changes to specificity for a subsequent voluntary land certification program. We are able to address selection into the program by taking advantage of the peculiarities in the certification process. Using agricultural production data from before and after the reform, we demonstrate that land certification significantly increases agricultural investments but only for investments directly affected by the changes in control. We explain the results using a simple model that shows how specificity can better coordinate landholders' beliefs about the implementation of changes in control.

JEL Classification: K49, O10, O12
Keywords: Property Rights, Specificity, Land Reform, Mexico, Ejido

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1 Introduction

Among the two dimensions of property rights, control and specificity, economists bestow a more prominent role to control because of its effect on incentives (North 1981). Consequently, specificity receives less attention despite its modest Humean contributions of improving coordination and lowering transaction costs. To fix ideas, by control, we mean who can do what with property and, by specificity, we mean the ability of a third party to enforce control rights. In this paper, we demonstrate the merit of specificity on economists' own preferred terms, showing dramatic effects of specificity on changes in control in the context of the 1992 Salinas land reforms in Mexico.

The Salinas land reforms consisted of a constitutional change that granted greater individual usage rights for all communal landholders (ejidatarios) and a subsequent voluntary land certification program (PROCEDE); together, these components of the reform opened the door to privatize the commune (ejido). The federal government had expected the reform to jumpstart the rural economy but, by most accounts, the reform had little impact. We reconcile the hope for the reform with its outcomes by drawing attention to the effect of certification on the de facto adoption of the reform.

We argue that specificity influences beliefs about control rights. Consider a simple example: if a farmer has an individual right to a plot of land but the location of the plot is not demarcated then this right is less specified than if the plot had been demarcated. Even if a farmer and his neighbors ex-ante all agree on the boundaries of each other's plots, a lack of specificity can open the door to ex-post bargaining that frustrates the initial beliefs about tenure security. Consequently, increasing

the specificity of control rights improves ex-ante expectations about control (Fon and Parisi 2007). Returning to the context of this land reform, small changes in specificity can dramatically influence effective control. We argue that specificity may make or break the de facto adoption of the reform. Thus, modeling specificity as merely the probability of enforcement, as the literature has commonly done, does not capture this phenomenon. However, in contrast to the simple example, specificity has a stark albeit complementary effect because the outcomes depend upon the the reform's underlying changes in control.

Our model borrows from the global games literature in which landholders must form beliefs about which property rights regime is in place, the old regime or the new one given by the reform. The difference between the two regimes is the degree of individual control, so that certain agricultural investments are profitable in the new regime but unprofitable in the old one. We allow communities to vary in a fundamental that tracks the degree of entrenchment of the pre-reform regime. Greater entrenchment requires landholders to better coordinate their beliefs about the adoption of the reform in order to actually transition to the new regime.[1] An increase in specificity improves the range of this fundamental that achieves the transition to the new regime. This model contributes to the understanding of land reforms by 1) demonstrating that beliefs about regime change function as an additional constraint on the implementation of land reforms and 2) showing how improvements in specificity can alleviate this constraint.

[1]Of course, increasing specificity may not always translate into better predictability as Shipton (1988) and Sjaastad and Bromley (2000) demonstrate. The reform we study may not suffer from these negative effects because it was a participatory and democratic reform of already strong pre-existing individual and communal rights and de facto land markets.

To test this theory, we exploit the variation in PROCEDE, which lasted over a 15 year period from 1992 to 2007 and provided a means for the ejido to increase the specificity of property rights while essentially holding control rights fixed. Hence, one can treat the program as a quasi-natural experiment to assess the change in outcomes due to improvements in specificity without conflating the effect of an increase in control. Using the last two rounds of the Mexican Agricultural Census, one right before the constitutional reform in 1991 and one right after the conclusion of the titling component of the reform in 2007, we find that land certification does matter for a number of land related investments, even though control rights do not depend on land certification.[2] In particular, we find that longer periods under land certification enable individual farmers to increase land under fallow, improve irrigation systems and increase internal land market activity, while we find no effects on access to credit or other agricultural investments such as using fertilizer or improved seed.

Voluntary participation in the certification program gives rise to concerns about selection bias. We account for the possibility of selection into the program by taking advantage of the program's peculiar implementation strategy. We use the timing of the first informational meeting about the certification program in instrumental variables estimation and find evidence of positive selection. In addition, because the program roll-out varied by state, we can address spatially correlated unobservables by employing a matching strategy that pairs municipalities in different states that share a common border but have different time periods under certification.

This evidence fills an important gap in the extensive literature on property rights

[2]A planned census in 2001 was called off due to a lack of budget.

and the formalization of individual rights to land since numerous studies do not necessarily isolate the effect of changes in specificity. Indeed, while formalization may increase specificity, it often alters control rights, whether purposefully or inadvertently. The empirical results are important for two additional reasons. First, our study evaluates one of the largest land titling programs of recent years in one of the world's biggest emerging economies. Case studies apart (Bouquet 2009), the only systematic research on the Salinas reform's impact on agricultural production comes from a series of surveys conducted by the Mexican Ministry of Agriculture with support from the University of California, Berkeley in 1990, 1994 and 1997. These studies point out that few ejidos privatized and, for those that went through the certification program, the general conclusion that the reform had a very limited impact on farmers' behavior. While it is true that relatively few ejidos privatized, from an economic point of view, our model points out that privatization is not the only feature of the reform that matters for agricultural investment and production decisions. Second, our results also contribute to the understanding of Mexico's recent agrarian reform by taking a longer term view than previous studies. All too often, controversial policies are analyzed briefly after their implementation, and consequently evaluated only based on their short-term effects. As we believe that the effects of a fundamental change in property rights requires time to materialize, taking a second look at PROCEDE 15 years after it was initiated is a fruitful exercise.

2 Previous Literature

Many countries have not experienced the expected outcomes after implementing large-scale land reforms. With the exception of a few bright spots (Deininger and Binswanger 1999, Do and Iyer 2008), relatively few studies present evidence that land titling or privatization programs have had robust positive effects (Braselle, Gaspart, and Platteau 2002, Field and Torero 2008, Galiani and Schargrodsky 2010, Bandiera 2007, Migot-Adholla, Hazell, and Place 1991, Carter and Wiebe 1994). The problem of reform implementation is argued well in Hoff and Stiglitz (2004) in the context of transition economies. However, many of the incentive problems that the authors discuss are not present for the ejido because individual control rights did exist before the reform under a system of corporate ownership; nothing as dramatic as the privatization reforms in transition countries would occur. Feder, Onchan, Chalamwong, and Hongladaron (1988) and Banerjee, Gertler, and Ghatak (2002) also find that less radical improvements in control, as opposed to a complete privatization overhaul, can be successful and face less resistance from the landless and the poor. Both Andre and Platteau (1998) and Deininger and Castagnini (2006) show that communities can differ in how they might conflict with greater individual control. Others argue that de facto rights remain unchanged or been made worse amidst de jure reforms.[3] We account for differences in conflict with individual control in our model and explain why de jure changes will be adopted as de facto changes.

[3]Land titling programs have been criticized for overstepping the complicated land tenure relationships that have evolved, resulting in both confusion and less access to land for those with less power; Carter and Olinto (2003) argue that land titling programs can translate into better outcomes for large landowners than small landowners.

With notable exceptions (Ostrom 2005, Alston, Harris, and Mueller 2010, de Soto 2000), development economists haven't given much emphasis to the role of specificity. The common argument states that increasing an individual's tenure security increases incentives to invest, access to credit and efficiency in land markets (Besley 1995). In general, the empirical literature has estimated large negative effects of tenure insecurity (Macours, de Janvry, and Sadoulet 2010, Goldstein and Udry 2008). However, Jacoby and Minten (2007) argue that the increase in tenure security due to land titles is likely small when informal individual rights function well and not enough to induce large differences in investment incentives, ignoring the coordinating role of land titles. Libecap and Lueck (2011) is the first paper to show empirically the causal effect of greater specificity. Using a natural experiment, the authors show how land demarcation systems have both short and long-term impacts on land values. In the relatively level terrain of central Ohio, the authors argue that the causal channel runs through lower transaction costs associated with the rectangular land demarcation (centralized) system outweighing any flexibility gained by the metes and bounds (decentralized) system. We argue that this direct effect of specificity matters less in our context where land markets are less active.

Our contribution, instead, concerns a causal effect of specificity that is indirect and affects beliefs about control rights. DiTella, Galiani, and Schargrodsky (2007) show how land titles can affect landholders' beliefs about market-oriented values but do not investigate beliefs about individual control rights nor do they infer this indirectly by looking at economic outcomes. Following Barzel (1997) and Alston, Harris, and Mueller (2010), we argue that who can enforce individual usage rights

is an important aspect of land tenure, and consequently will have an impact on land-specific investment decisions. We show that land titles can coordinate beliefs about changes in property rights. We use a similar identification strategy to Do and Iyer (2008) who show that handing out formal titles for usage rights has an impact on engaging in non-farm activities but they can not separately identify the effect of greater specificity since the formal title changes control rights. Our finding is consistent with the empirical results of Deininger and Jin (2009) who, in the context of institutional change in China, show that land certification decreases the incidence of illegal expropriation of land by local authorities. One could interpret their findings as consistent both with the fact that certification makes expropriation costlier, but also in the spirit of our model: that certification helps the community to coordinate their beliefs about the de facto enforcement of the legal regime.

Turning to the literature on the ejido reform itself, de Janvry, Gordillo, and Sadoulet (1997) provide a very detailed description of the evolution of the ejido sector, but without a clear focus on the effect of the reform. To construct our controls, we use the findings of Munoz-Pina, de Janvry, and Sadoulet (2003) who focus on the determinants of reform participation. Using the same data as Munoz-Pina, de Janvry, and Sadoulet (2003), plus an additional survey conducted in the year 2000 by the *Procuraduria Agraria*, Deininger and Bresciani (2001) and Deininger and Olinto (February 2002) find that the reform did improve land access for the formerly landless as well as rental markets, and reduced land related conflicts, but did not have any effect on land sales or credit access.

3 Reform Background

3.1 The Ejido: Structural Characteristics

Article 27 of Mexico's post-revolutionary Constitution of 1917 granted the government far reaching rights to expropriate private land holdings in order to redistribute them to the landless.[4] The constitution required this redistributed land to be organized into groups of farmers with communal land holdings known as ejidos.[5] Ejidos gave its members, the *ejidatarios*, individual usage rights over plots of land, contingent on cultivation. These usage rights were hereditary but were not guaranteed by any form of official title, nor were the rights divisible. Land conflicts between different ejidatarios were mostly resolved internally by the ejido's governing body (the *comisariado ejidal*), which also had far reaching authority to reallocate usage rights if an ejidatario fell foul of the rules governing land use. In addition to having to keep land under cultivation, these rules outlawed land sales, rental contracts, and lending land to individuals outside the ejido (such as relatives living somewhere else) and limited the hiring of additional labor.

The ejido varied widely even within municipalities. de Janvry, Gordillo, and Sadoulet (1997) discuss the heterogeneity of the ejido across three important dimensions: member/non-member composition clearly affects the local political economy of the ejido; individual/commons land distribution affects the demand for and supply of

[4]Mexico's Agricultural Law (*Ley Agraria*) limits the maximum amount of private land holdings to 100 hectares of irrigated land or its equivalent in rainfed land. See Sanderson (1984) for an excellent description of the evolution of the ejido.

[5]A second form of communal land holdings is constituted by the Agrarian Communities *Comunidades Agrarias*, much smaller in number and located in mostly indigenous communities. These were also certified during PROCEDE, but not subject to the other reforms of article 27

certification or privatization; and the scope and effectiveness of internal governance could affect the implementation of the reform.

3.2 The Reform Details

Over the years, the government has subjected ejidos to policies aimed at increasing their notoriously low productivity or simply at political capture. Mexico's 1992 agrarian reform ended Mexico's land redistribution and automatically eased restrictions on the ejido. The reform dropped the cultivation requirement and legalized rental agreements, internal land sales (to ejido members) and external labor hires. In addition, the reform opened the door to outright privatization.

In conjunction with the reform, the government established a land registration program (*PROCEDE*) that determined an ejido's external boundaries and made usage rights enforceable by a third-party. Before the reform, each ejido's external boundaries were only loosely known, while the internal land division between individual ejido members (*ejidatarios*) was only known to the ejido itself.[6] This program concluded in November 2006 with more than 90% of communal land covered. Once certified, the ejido could democratically decide to convert part or all of their land into private property.[7] Only then could land be sold to third parties. Table 1 summarizes the changes in de jure control that the reform caused for all ejidos.

The Mexican National Statistical Institute (INEGI), together with the *Procuraduria Agraria* (PA) and the National Agrarian Registry (RAN), implemented

[6]Boundaries often depended on whether or not a more than half a century old document was in the possession of the ejido and, in many instances, a source of conflict.

[7]The decision to privatize any plot of land must be approved by the ejido assembly with a two-thirds majority and cannot simply be taken by any individual producer.

PROCEDE in several stages, each of which was initiated by a meeting with the ejidal authorities or a general ejido assembly.[8] First, the PA contacted the Comisariado Ejidal in order to set up an initial general assembly of ejido members. The principal aim of this first assembly, the *Asamblea de Informacion y Anuencia* (AIA), was to inform ejido members about PROCEDE. It had a quorum requirement of 50% plus one and took a vote on whether or not to create an auxiliary commission (*Comision Auxiliar*), which consisted of a small number of ejidatarios who should elaborate a rough draft of the ejido's external and internal boundaries, including parceled land (over which ejidatarios had individual usage rights), common lands, and housing areas. These drafts were then presented at a second assembly which took a vote, again with a 50% plus one quorum, on whether or not to accept them. If accepted, INEGI and ejido members would jointly start to carry out detailed land measurements and to generate the corresponding maps for the land registry. The final result was publicly presented for two weeks, during which complaints could be filed.[9] A third and final assembly, with a 75% plus one quorum, had to accept the results, which were then sent to RAN. Individual land titles were given out on all housing plots and sent to the municipal authorities for inclusion in the local land registry.

The voluntary nature of this process means that ejidos to a certain extent selected into treatment. But the timing of certification was also determined by a number of

[8]We had the chance to interview one person who actively participated in PROCEDE over the course of many years in a position of responsibility and who is currently still working at INEGI. We also had conversations with officials in the PA.

[9]Owners of land adjacent to the ejido also had to be contacted in order to give their written consent to the established external boundaries.

factors beyond the ejidos' control. Firstly, PROCEDE was effectively carried out at the state level. INEGI and the PA were staffing their existing offices in state capitals with the personnel in charge of carrying out the program. Any factor affecting INEGI's decision about which ejido to measure out first is relative to other ejidos within the state. In addition to the state capital offices, PROCEDE also established local offices responsible for a sub-state area consisting of a varying number of municipalities (*jefaturas de zona*).[10] Second, the program also faced severe budgetary constraints.[11] The budgetary pressure made it necessary to show tangible results by certifying the largest number of ejidos as quickly as possible.[12] According to our interviewee, the principal factors that determined an early treatment were i) absence of internal or external land conflicts, ii) a small land area, iii) a level geography (i.e. mostly non-mountainous terrain), and iv) large proportion of external boundaries with other ejidos in the process of certification.

While the date of certification may have been influenced by ejido selection as well as strategic considerations by INEGI, the AIA meeting carried out by the PA followed no strategical considerations. Its officials simply moved from one ejido to the next (as long as the ejido agreed to the meeting, which was almost always the

[10]Most of the rank and file employees were locally recruited and worked exclusively for one of the jefaturas. At its peak more than 15,000 people, divided into more than 800 groups (*brigadas*), worked for PROCEDE nationwide. This set-up provided PROCEDE with a fairly flexible workforce as jefaturas could be dismantled if the workload in its area dropped too low. The precise location and lifespan of jefaturas could give us important additional information with respect to time of treatment, but, unfortunately, to our knowledge that information is not available.

[11]During the last two years of the Salinas presidential administration (1988-1994) the program was well funded, but progress was much slower than expected. The change in presidential administrations together with the currency crisis in late 1994 resulted in a much smaller budget over the coming years.

[12]Or, in the words of our interviewee *"se echaba toda la carne al asador"*

case). Since officials were based in each state capital, ejidos farther away from it or generally located in less accessible areas were given the meeting at a later point in time. In our own conversations with PA officials, we were assured that the agency followed no strategic plan in contacting ejidos to set up the first assembly. Given the openness of INEGI about its strategic motives, we have no reason to doubt the PA.

4 Specificity and Land Reform

Specificity can have a direct or indirect effect on economic outcomes. The direct effects of specificity are more familiar and include increases in the collateral value of property and cheaper enforcement. This section highlights an indirect effect – how specificity can coordinate beliefs about regime change in property rights. Specificity can affect the beliefs about the reform implementation in two ways, through cheaper enforcement costs and through better public information. The model in subsection 4.1 presents the former mechanism and we discuss the latter mechanism in the appendix. In the model, specificity only plays a complementary role; both of these indirect effects depend upon the nature of the underlying changes in control. However, the model shows how specificity, even in this complementary role, can have a dramatic impact on outcomes and serves the purpose of attracting more attention to other and perhaps more important coordinating roles of specificity.

4.1 A Simple Model

We consider a two period model of an ejido community with a continuum of members. Each member is endowed with the same standard indirect utility function increasing and concave in wealth. Ejido membership means access to an agricultural production function, $F(K; L_0)$, where L_0 is a fixed stock of land and K is land investment. Wealth is given by $Y_{t+1} = pF(K_t; L_0)$, where p is the price of the agricultural good. Wealth is produced by using land to produce current period output or by taking land out of current production and investing it to produce future output. We normalize the price of the agricultural good to one so we reinterpret the argument in the utility function as the consumption of land.

We characterize the ejido rules using a standard risk of eviction that depends on a minimum consumption requirement for land. These rules give the control rights for the community. In this context, the minimum consumption requirement should be interpreted rather broadly since we are using indirect utility. For example, cultivating land instead of fallowing is a form of consumption in this model. Moreover, investments that only improve contemporary period production, such as using fertilizer, also fall into the consumption category. For simplicity, assume that the probability of eviction equals one if $C < C_{min}$ and zero if $C \geq C_{min}$. We will introduce enforcement costs in a future subsection.

Before the agrarian reform, officially $C_{min} = L_0$ for all ejidos but ejidos may have differed in their enforcement of the minimum consumption constraint, resulting in lower values for C_{min}. Let $\theta = \frac{C_{min}}{L_0}$. The ejido member's problem consists of the decision of how much to invest given the risk of eviction. The ejido member evaluates

the value of investing in the agricultural sector, the future stream of agricultural output, against the risk of losing ejido membership. In the second period, it is optimal for all land to be consumed, regardless of the consumption constraint. Thus, we can write down the ejido producers' problem as only a choice about initial investment, which we denote by K. We assume that $K_0 = 0$.

$$\begin{aligned} \underset{K}{\text{maximize}} \quad & U(C_1) + \beta U(C_2) \\ \text{subject to} \quad & C_1 + K = Y_1, \\ & F(K; L_0) = [L_0(\frac{K}{L_0})^{\gamma} + L_0]\mathbf{1}[C_1 \geq C_{min}] \\ & C_2 \leq Y_2, \\ & C_1 \geq 0. \end{aligned} \tag{1}$$

From this program, we immediately see that the ejido producer will never choose to violate the minimum consumption constraint since he can guarantee a payoff of $U(C_{min})+\beta U(L_0(1+(1-\theta)^{\gamma})$ which is greater than $U(C_1)+\beta U(C_2)\ \forall\ C_1 < C_{min}$. In particular, if $C_{min} = L_0$, then $K = 0$. We can also show that the ejido producer will never consume more than C_{min} for all $C_{min} > L_0 - K^{**}$, where K^{**} is the solution to the problem when $C_{min} = 0$, or there is no minimum consumption constraint other than nonnegative consumption in the first period. That is K^{**} solves the following first order condition:

$$\frac{U'(L_0 - K^{**})}{U'(L_0(1 + (1 - \frac{K^{**}}{L_0})^{\gamma})} = \gamma\beta(\frac{K^{**}}{L_0})^{\gamma-1}.$$

Finally, we can verify that the optimal level of investment, K^*, is decreasing in C_{min}

and strictly decreasing over $L_0 \geq C_{min} \geq L_0 - K^{**}$.

4.1.1 Regime Uncertainty

According to the de jure reform, there is no longer a risk of eviction tied to C_{min} but ejido members may not trust the enforcement of these new rules. We model this situation as a global game in which there are two possible property regimes, the status quo of having a consumption constraint and the alternative, post-reform, regime. We track this outcome with $R = 0$ if the status quo survives and $R = 1$ if there is de facto adoption of the new regime. We assume that which property regime is in place is not common knowledge. The status of R depends upon the actions of the ejido members since it represents de facto adoption of the new regime. All agents move simultaneously. After the agents take their actions, the status-quo regime survives or not. The status quo is abandoned if enough agents attack it. We assume that the new regime is an absorbing state.

To simplify the regime change game, we make an additional assumption that $K = K^{**}$ means attacking the status quo, denoted by $a_i = 1$. We assume that not attacking means $K = 0$, denoted by $a_i = 0$. We can then define $b = U(L_0 - K^{**}) + U(L_0(1 + (1 - \frac{K^{**}}{L_0})^{\gamma}) - U(L_0)(1 + \beta)$ as the payoff from attacking if the status quo does not survive and $-d = U(L_0 - K^{**}) + \beta U(0) - U(L_0)(1 + \beta)$ as the payoff if it does survive. Then the ejido producers utility can now be written in a condensed form: $u_i = a_i((b + d)R - d)$.

On top of the basic land allocation decision, the individual must form a belief about R. We choose to model this process as an ejido member making use of knowl-

edge about an ejido-level fundamental, θ. We interpret θ as tracking the historical solution to owning communal lands that may or may not be in conflict with the new property regime. In this sense, θ reflects how entrenched the status quo is. Thus, one could think of θ as C_{min}/L_0.

Those with low θ require only a few ejido users to attack the status quo to instigate regime change. A more coordinated attack is required in ejidos with higher conflict possibilities. That is, $R = 1$ if and only if $\alpha \geq \theta$ where $\alpha = \int a_i di$ now also denotes the mass of agents attacking.

We assume that ejido members have a common uniformly distributed prior on θ. Ejido members receive a normally distributed private signal, μ_v, with mean θ and variance σ_v in the spirit of Morris and Shin (1998). The private signal comes from the agent's interactions with neighbors and personal assessment of the ejido's situation. The information about the fundamental is dispersed even though the true value can be determined using everyone's information. A well-known result when restricting attention to monotone strategies (requiring the decision to attack to be decreasing in μ_v) is that this additional uncertainty dramatically shrinks the range that allow players to coordinate. Given this dispersion, the belief about others' actions given others' signals reduces the multiplicity of equilibria.

If θ is common knowledge, then there exist $\underline{\theta}$ and $\bar{\theta}$ for which θ such that $\underline{\theta} < \theta < \bar{\theta}$, both the$R = 0$ and $R = 1$ are possible. In contrast, when the private signal has moderate to high precision, multiple equilibria can not survive and the equilibrium outcome depends only upon whether θ is above or below a threshold $\theta^* \in (\underline{\theta}, \bar{\theta})$. In the limit, $\theta^* = 1 - d/b$ where θ's below this value succeed in regime change and θ's

above this value can not escape the status quo. This threshold creates the benchmark and demonstrates the underlying issue of selection for the empirical analysis.[13]

4.1.2 Specificity and Enforcement Costs

We now are ready to introduce specificity into the analysis. Recall that specificity refers to the ability of a third-party to enforce the control rights. After the de jure reform, a land title certification program makes it possible for third-party enforcement and potentially alters the enforcement costs (assumed to be zero up to this point) faced by the ejido members. The ejido member must pay a cost to ensure that other members respect the ejido member's claim, regardless of the property regime and action taken. Before certification, we assume that only (first or) second-party enforcement is possible. For first or second party enforcement, the cost of enforcement depends upon other ejido members' actions. We denote first and second party enforcement costs as $E(\alpha)$, decreasing in α, where α tracks the proportion of the ejido that have violated the consumption constraint. Notice that these enforcement costs do not depend on the agent's action or the property regime, the payoffs above, defined in relative terms, remain unchanged. Thus, without certification, enforcement costs do not affect the threshold level for θ. In contrast, third party enforcement is

[13]We are interested in how certification can improve the possibilities of those who could not successfully adopt the new regime. Since the new regime is an absorbing state, we do not need to consider the possibility that certification might cause an ejido to switch from the new regime to the status quo.Given the simple framework, this is a technical possibility if we drop the absorbing state assumption. However, there are good reasons to think this simplification is warranted. For example, one could easily extend the above model so that the payoffs affected by the member's attack decisions are realized with delay. The value of access to the ejido in the future is contingent on the actions of the present. Such models exhibit history dependence as shown in Proposition 1 of Adsera and Ray (1998).

independent of other members' actions but does depend on the property regime in place. We assume that an ejido member facing enforcement costs will prefer second party enforcement in the case of the status quo (because contingencies are difficult to observe by a third party) and third party enforcement (if the ejido has certified) for the new regime. Specifically, we assume that when $R = 1$, third party enforcement is always cheaper, i.e. $E(1) > e > 0$, where $\frac{e}{R}$ is the cost of third-party enforcement. In other words, if $R = 0$, the ejido producer would never choose third party enforcement.

The movement from second-party enforcement to third-party enforcement introduces congestion externalities. Under second party enforcement, these costs are decreasing in the number of members who attack the status quo because the probability that someone has a legitimate claim to the asset in question goes down. Recall that under third-party enforcement, enforcement costs are independent of such actions. Thus, at the moment of attacking if usage rights have been certified, the ejido member can pull out second party enforcement costs and apply them towards third-party enforcement. Hence, with the option for third party enforcement, the utility function is: $u_i = a_i(b + d + E(\alpha) - e)R - d)$. We see that with certification the payoff for attacking is higher if $R = 1$ while the payoff for attacking if $R = 0$ and the payoff for not attacking remain unchanged. This immediately raises the cut-off θ. Moreover, this holds for every α due to the strategic choice of enforcement type. Thus, regardless of what other ejido producers do, there is greater pressure to err on the side of attacking.

Above we assumed that certification is costless so every ejido gets certified imme-

diately. However, more realistic would be to assume that at some cost ejidos could certify sooner even though the government would eventually provide costless certification. We see that if $R = 1$, and as soon as certification appears, the ejido chooses to certify since enforcement costs are lower in this case (as long as e is low enough). If $R = 0$, when the certification program is initially offered, third party enforcement is not desirable. However, when certification is offered for free, congestion externalities occur and the ejido could transition to the new regime. Some ejidos will be stuck in $R = 0$ and third party enforcement is never cheaper even certification is offered for free. The ejidos with lower θs are more likely to switch to the new regime following the reform. Hence, they may certify earlier if there is some cost to certify. These ejidos are also likely to experience a smaller effect from switching to the new regime because the constraint on control rights is less tight. Since these ejidos are better-off to begin with, we refer to this as positive selection.

4.2 Discussion of Hypotheses

The model demonstrates that changes in specificity (land certification) will influence the adoption of the reform and hence have an impact on outcomes affected by the reform-altered control rights. Important for our argument is that we difference out any increase in control due to mere changes in de jure control when we compare ejidos that certified with those that did not. That said, two alternative hypothesis also explain why land certification would have an impact on agricultural investments: 1) land certification improves tenure security (through lower enforcement costs) and 2) land certification improves access to credit. In this section, we argue that we can

empirically distinguish our hypothesis from these more standard ones.

First, the two alternative explanations should result in unconditionally greater land-specific investments and/or increased access to credit while our hypothesis limits the effect to the outcomes affected by the de jure change in control. Using the terminology developed in the model section, we first turn to the main agricultural investment that withdraws land out of contemporary consumption, fallowing. Focusing on fallowing is also useful since it does not require credit while other investments such as fertilizer use, improved seed, irrigation, etc., might. The model predicts that certification will increase investments in fallowing since the higher likelihood of the adoption of the new regime leads to a decrease in contemporary land consumption. In addition, certification should have no effect on incentives to use fertilizer since this investment is not in conflict with contemporary land consumption. In contrast, the alternative hypothesis predicts that both fallowing and fertilizer use should increase after certification. Here we take advantage of the fact that conditional individual usage rights were well-developed and enforceable before the reform.

Second, we can use the model to understand the nature of selection and derive a second hypothesis: there is positive selection. "Better-off" ejidos (higher θs) will tend to certify earlier because the benefits of certification are more likely to be apparent given the regime change. Thus, since we would see less change in production decisions over time in these ejidos, OLS estimates would be biased downwards. Under the alternative hypothesis, one would expect ejidos that see the biggest gains from certification to certify first. These ejidos are the ones with relatively costly second party enforcement and hence are "worse-off", leading to negative selection

and upward biased OLS results.

Finally, we discuss implications for rental and sales markets. Rental markets offer another opportunity to test our prediction that specificity helps coordination of beliefs about changes in de jure control since the reform legalized rental agreements. In terms of the model, one can interpret renting as an increase in K and decrease in C, indicating that certification would increase renting. We contrast this hypothesis with one in the literature given by (Giné 2005) who has suggested that land certification would produce a negative effect on renting. Certification may have an indirect effect on internal land sales because other member's approval is easier to coordinate and the collective action problem is less severe than when the old regime is in place. The direct of certification may increase the demand for ejido lands by non-ejido members (although, technically, only privatized plots could be sold to third-parties).

5 Data

Our data come from a variety of sources. The Mexican National Agrarian Registry (RAN) reports the exact date at which each ejido became certified from which we construct our treatment of interest. Out of nearly 2500 municipalities, 1962 had at least one of Mexico's 29,259 ejidos.[14] We construct an index that measures the average monthly proportion of ejidos that have been certified between 1993 and 2007 for each municipality. This measure would be equal to one if all the ejidos in a municipality were certified in January 1993 and equal to zero if none had been certified in December 2006.

[14]This excludes municipalities that had agrarian communities but no ejido.

The 1991 and 2007 Mexican Agricultural Censuses provide the outcomes of interest and some control variables. These data are aggregated over all producers (not ejidos) in a given municipality that declare to be producing on ejido land (but we are unaware whether or not the producer actually is an ejidatario). The nature of this data source will, unfortunately, introduce some measurement error. Ejidos constitute a parallel political and administrative system, directly underneath the federal level and are therefore not necessarily contained within a single municipality's boundaries. The National Agrarian Registry nonetheless assigns each ejido to a single municipality[15], while the Agricultural Census assigns individual producers to ejidos based on the location of their plots, but irrespective of the ejido the land belongs to.

The 1991 census only reports data on ejido producers for 1839 municipalities, somewhat restricting the scope of our analysis. The more than 100 municipality difference is likely due to newly formed municipalities between 1991 and 2007. But we cannot rule out that the 1991 census may have produced some under-counting, or mismatches, especially in very small municipalities.[16] In addition, in a small number of these municipalities, information on some variables is missing. While our data are,

[15]One single ejido was assigned to two different municipalities and excluded from the data.

[16]We find more evidence of under-reporting in 1991. The total number of ejido producers between the two censuses increased from 2,165,560 to 2,609,244. This is roughly in line with the increase in Mexico's total population, but since ejido rights are non-divisible when bequeathed and no ejido land was granted during this period, it is likely to reflect undercounting in 1992. Both numbers also fall short of the total of around 3.5m ejidatarios, indicating that there is still some serious undercounting present in the 2007 data. At the same time, the total area belonging to these producers increased from 15,070,990 hectares to 15,774,470. As the total area should have stayed constant, this again points to some under-counting in 1991. More importantly, there is huge variation in the percentage change of producers between the two years, indicating that the assignment of individual producers to municipalities is somewhat imprecise. Given the general large improvements in data quality in Mexico over the course of the last two decades, it seems fair to assume that most mismatches stem from the 1991 data. The percentage change in the number of producers has a highly skewed distribution with a mean of 1.01 (i.e. 101%), a median of 0.11 and a standard deviation of 7.28.

in theory, complete, we have to be aware of some gaps and potential measurement error.

The 2001 and 2007 Ejido Census and the 2005 Mini Census (*Conteo de Poblacion y Vivienda*) supply the additional control variables. The data from the latter simply gives characteristics of the entire municipality. The Ejido Census, on the other hand, consists of one interview per ejido, usually with a person belonging to the Comisariado Ejidal, asking for ejido-specific characteristics. Here we face the same problem as with the Agricultural Census in that ejidos cannot be unambiguously assigned to one single municipality. So there are some inevitable misalignments between these controls and the treatment variable during aggregation at the municipal level. We believe them to be minor though since the simple correlation between number of ejidos in a municipality between the RAN data and the Ejido Census is larger than 0.97.

Table (7) provides summary statistics for the outcomes examined. All our summary statistics represent the average municipality not as national averages. Table (7) provides the definitions of each variable and how we constructed each. Abandoned land, fallowed land, land under perennial crops, and irrigated land are in proportions of total ejido land area and producers who rent land, use chemical fertilizer, and take out credit are in proportions of the number of ejido producers. The number of ejidos that reported land sales in the 2007 Ejido Census is the only outcome not taken from the Agricultural Censuses and for which we do not observe the baseline in 1991, but legally it should have been zero. Abandoned land changed slightly from 13% in 1991 to 15% in 2007, land left fallowing decreased from 10% to around 4%. The area under

irrigation has remained fairly constant. The area covered with perennials increased drastically from 15% to 23%. Rented land increased slightly, but remains very low at 3% and fertilizer use fell from 54% to 34%. The proportion of producers using credit plummeted from 21% to 4%. In the average municipality, a full 66% of ejidos report sales taking place.

Table (7) also shows summary statistics of the treatment and control variables. PROCEDE denotes the average time under certification and the instrument is the average time passed since the first informational meeting. As would be expected the latter is substantially larger than the former. In the average municipality, ejidos have been certified for around half the time since the start of the certification program. The parsimonious set of control variables we use are the total population, the share who speak an indigenous language in 2005, the share who are illiterate, the proportion of the labor force working in the primary sector (*Empl. Agro*), the distance to the state capital, and an index for the ruggedness of the municipality's territory. For additional control variables, we use the proportion of ejidos that report internal or external conflicts or invasion of its lands, the proportion of the ejido land that is held as commons, the number of ejidos that report that some land has been privatized (i.e.passed to *dominio pleno*), and the proportion of the ejido land area privatized, all taken from the 2007 Ejido Census. Lastly, we control for the total area of ejido land reported in the Agricultural Censuses (*Plot Area*), the share of number of producers in the total population (*Share Producers*), the proportion of producers that report having signed a contract with some agroindustrial business (*Contract*). We also control for the total number of producers and will use that information to run a

weighted regression as explained below. The minimum value of zero is due to the fact that we have data from the Ejido Census on three municipalities (with one ejido each) for which no producers are reported in the Agricultural Censuses.

5.1 Estimation Strategy

The advantages of the data are that we have information on close to every producer in the country and the data structure allows for a clear identification of the reform's long term effects. We measure 2007 outcomes against baseline data collected in 1991, one year before the (unexpected) constitutional change was legislated and two years before the first ejido received certification. By 2007, more than 90% of ejidos had been certified, virtually only leaving those with severe land conflicts and/or distrust of the government. Instead of using whether the ejido is certified as a treatment variable, we construct the average time a municipality's ejidos have spent under certification up to the year 2007. The structure of the treatment variable, by its very nature, will not pick up any short-term effects of the certification program as these will be unaffected by the length of time since certification. It will, on the other hand, capture long-term effects of the reform since these are more salient in ejidos that have been under certification for a longer period of time.

Our basic model to be estimated would therefore be:

$$y_{is07} = \alpha + \gamma PROCEDE_{is} + \phi y_{is91} + X_{is}\beta + X_{is}^{ejido}\psi + u_s + \epsilon_{is} \qquad (2)$$

where u_s is the state level fixed effect. The vectors X_{is} and X_{is}^{ejido} are a number of additional cross-sectional variables to control for the extent of the change in the

outcome variable over the course of 16 years may have also been affected by a number of ejido sector and general municipal characteristics. We prefer this specification instead of the equivalent differenced specification because we want to control for the initial level of the outcome in 1991.[17]

Our data from the Agricultural Censuses represents aggregates at the municipal level that are based on a widely differing number of respondents, ranging from one respondent (in three cases) to more than 14,000. As can be seen from the last line in table (7), the standard deviation for this variable is actually larger than the mean. This is a standard case of heteroskedasticity of known form and all results presented will be based on the appropriate weighted regression, where the number of producers (*Num. Producers*) will be used as analytical weights. For the case of land sales, a similar problem exists and the number of ejidos according to the 2007 Ejido Census will be used as weights.

Since the ejido voluntarily underwent certification, unobservable characteristics may determine early treatment as well as the outcome of interest. Concerns about selection are tempered given that certification was seriously slowed down by externally imposed budget constraints and the laborious nature of the entire process and, to a large extent, the year in which an ejido would finally be measured lay outside its control, ultimately determined by INEGI. Nevertheless, our identification strategy consists of using the average time passed since the initial AIA meeting was held, constructed as the average time over all ejidos that agreed to a meeting with the

[17]As mentioned, we are only able to observe land sales in 2007. For this particular outcome, we are therefore constrained to estimating a corresponding model in levels where state level fixed effects and ejido and municipality characteristics have to be interpreted as affecting the level of the outcome variable.

government, as an instrument for the treatment (average time since certification).[18] This instrument is certainly relevant since the AIA meeting is required to set the certification process in motion and the absence of strategic delivery or acceptance of the AIA meeting makes the instrument valid. We control for state level fixed effects as different states may have moved at different speeds due to unobservable state level characteristics.

Table (4) shows the results for a (unweighted) regression of the instrument on each '91 outcome variable, entered separately in each column, along with all the control variables that we use. As can be seen, almost all statistically significant correlations occur with variables one would expect. Either they are close proxies for remoteness (such as proportion of the agricultural employment) or they are related to conflict. Given that certification is a prerequisite for land privatization, its significance is probably the result of reverse causation. A similar reasoning may hold for the proportion of common land. This leaves only credit and insurance as unexplained significant correlates. Once we control for credit, insurance is not significant. Both credit and insurance could merely reflect remoteness.

The first stage results show that the instrument is highly significant throughout and explains around 70% of time under certification. The parameters on the instrument are of very similar magnitude and very highly significant. All the significant control variables are either explained by the roll-out of the initial meetings or by the strategy followed deliberately by INEGI during the certification process. There are some notable differences in the significance levels of the 1991 baseline outcomes on

[18]Ejidos that had no meeting during the entire 14 years of the program may not be willing to deal with the government at all, which raises a potential endogeneity concern.

the average time under certification. The sign, and in some cases the magnitude, are the same, however. For convenience we include first-stage results in the bottom of the corresponding column of second stage estimates in all tables.

5.2 Additional Specifications

In order to provide additional support for the empirical strategy, we employ several robustness exercises. The first exercise relates to the validity of our instrument. If ejidos had the opportunity to select into an earlier first informational meeting based on their expected benefits from certification, the instrument would clearly violate the exclusion restriction. A second, and somewhat more subtle, possibility is that these meetings did not just convey information about the certification process, but about the 1992 reform itself. In this case, the instrument would have a direct effect on the outcome and hence be invalid. Even though we do not believe this to be the case, since the reform was very controversial and has been very been widely publicized at the time, this possibility needs to be addressed.

To address the validity of our instrument, we include a second instrumental variable that operates via a separate channel than the AIA instrument. We know that municipalities with more ejido members (i.e. with more ejidos and/or larger ones) were treated later. We also know that the certification process proceeded at the state level. A municipality should, therefore, be certified earlier if it contains fewer ejido members relative to the rest of the state. The second instrument is constructed as follows: i) order all municipalities within each state in ascending order according to the total number of ejido members in 1991, ii) compute for each municipality the

proportion of ejido members in the state in municipalities with a lower rank in that ordering than the municipality itself. This instrument is principally based on characteristics of all the other municipalities in the state, rather than characteristics of the municipality itself. Since we also control for the total number of ejido members in 2007 in our regressions, instrument exogeneity is assured by construction. This instrument enters the first stage statistically significantly and with the expected sign. Moreover, it does not explain the timing of the informational meetings. An over-identification restrictions test will then be employed to test for the validity of the first instrument employed.

A second concern relates to the possibility that our results are driven by spatial correlation. While the timing of the first informational meeting with the ejido was not determined by ejido specific characteristics, the process was highly spatially dependent in the sense that municipalities in close proximity to one another had their meetings around similar dates. The same is probably true for the certification process itself, albeit to a lesser extent. If omitted variables are also spatially correlated, they would introduce a correlation between the instrument (and the treatment) and the error term, even in the absence of any causal effects between them. However, given that informational meetings and certification roll-out happened at the state level, their spatial correlation should break down across state lines.[19] We therefore formed all pairs of municipalities that share a common boundary across a state line and estimate the model in (2) on the differences between each pair. Differences in

[19]We have state-level data on some of our outcome variables for 1981. Using these data, we construct state-level pretrends and regress each pretrend on time under certification and the AIA instrument (separately). The results show no statistical relationship between the state-level pretrend and time under certification or AIA.

the time since the informational meeting or under certification between two paired municipalities should then spatially uncorrelated and only depend on the characteristics of the other municipalities in each state. For this reason, we have to control for fixed effects at the level of each pair of neighboring states (which results in the loss of a handful of observations). As before, due to the differing number of producers in each municipality, we need to estimate a weighted regression. Under the assumption that the municipality specific error terms entering the differences are uncorrelated and share the same variance, the weights can be derived as follows:

$VAR(u_i - u_j) = \left(\frac{\sigma^2}{n_i} + \frac{\sigma^2}{n_j}\right) = \sigma^2 \left(\frac{n_i+n_j}{n_i n_j}\right)$, for neighboring municipalities i and j, and $\left(\frac{n_i n_j}{n_i+n_j}\right)$ are used as analytical weights.

Lastly, we also want to make sure that our results are representative for all municipalities and not merely driven by the idiosyncratic characteristics of the very first and very last to certify. We present results excluding municipalities in the highest and lowest quintile of the treatment variable. If our results are generalizable across all municipalities we would not expect to find very different point estimates. We will, however, lose some statistical significance since standard errors will increase due to less variation in the treatment variable and the reduction in the sample size.

6 Results

Tables 5-7 present the main regression results. We provide point estimates for the included variables and standard errors in parentheses. On the bottom of each table, we report the number of observations, the relevant first-stage results, the Cragg-

Donald statistic (e(widstat)), which for strong instruments should be higher than 16, and the F-statistic.

For abandoned land and fallowing, each pair of columns shows first a parsimonious specification, controlling only for the baseline and the likely determinants of an early meeting, followed by an estimation including the full set of controls. The first pair of columns show OLS results, followed by their IV counterpart. Table (5) shows that certification increases fallowing in a statistically significant manner. The proportion of land left fallowing is estimated to have increased by around 1.3 percentage points in response to a one standard deviation increase in time under certification. This effect is very large given that the mean of the proportion of fallowed land in 2007 is 4%. In the context of the model, certification allows ejido producers to protect the fallowing investments in land quality without worrying about violating the land use requirement. For abandoned land, the negative coefficient on land certification is consistent with greater control of land, allowing ejido members to fallow or rent land instead of abandoning it. However, abandoned land does not allow us to distinguish between the direct effect of certification and the indirect effect of increased adoption of the reform.

Table (6) shows results concerning other investments in land such as fertilizer use, perennials and irrigation. The first two of these investments are not prima facie affected by changes in control outlined in the reform but should be affected by the direct effect of land certification. [20] There is no significant effect of land certification on the use of chemical fertilizers or perennials. The same result holds for improved

[20]Since the cultivation requirement is more ambiguous for perennials than annuals, investments in perennials may also be affected by the changes in control due to the reform.

seeds (results not presented). In contrast to fertilizer use, irrigation may require taking land out of production and hence violating the land use constraint. We see an effect of certification on the proportion of land under irrigation. Point estimates are positive throughout and signifiant at the 5% level for the IV specification, where a one standard deviation increase in time under certification is estimated to increase the proportion of land under irrigation by around 0.8 percentage points. Table (6) also shows the result on the use of credit by producers. We do not find statistically significant results and the direct effect does not appear to affect outcomes.

Comparing the IV estimates with the OLS ones in tables (5) and (6), we actually observe less selection than we had anticipated suggesting that fewer ejidos were able to abandon the minimum consumption constraint before the reform than we had thought. Nevertheless, we do observe positive selection in fallowing, irrigation, and rented area and in none of the results do we observe negative selection.

We now turn to the results on land markets. First, we discuss rental markets since this outcome variable helps us further distinguish our model from the literature. In column 2 of table (7), the IV specification gives a statistically significant (at the 5% level) positive effect on the proportion of land rented by around 0.5 percentage points for a one standard deviation increase in treatment. The positive effect is not consistent with the findings of (Giné 2005) suggesting that the indirect effect may dominate the direct effect.

We have three different types of land sales, sales between ejido members, sales between an ejido member and a posesionario or avecinado, and sales between an ejido member and an outsider, all shown in table (7). Unlike the previous regressions, these

do not constitute a differenced estimation since we do not observe land sales in 1991. Certification has a positive and statistically significant in all specifications, except for OLS estimates in column 5. All these results control for actual privatization (measured ex-post, not varying across time), therefore, technically, since sales to non-members could only occur if privatized, we should already be accounting for the direct effect. Moreover, if only the direct effect is at play, it is not clear why sales between ejido members would be affected.

6.1 Robustness Checks

In this subsection, we discuss our robustness checks. First, in table 8, we present the main results rerun using the second instrumental variable. The results actually get stronger. For all outcome variables, the instruments pass the over-identification test, although for renting the p-value is above but close to the 10% level. Moreover, the AIA instrument does not explain the second instrument.

Second, in table 9, we check to see if the main results hold up for the spatially matched sample. In general, the IV results for the spatially matched sample give fairly similar point estimates, but with higher standard errors that make results statistically insignificant. For fallowing, the estimated marginal effect is similar in size although the p-value is close but above the 10% level. Low statistical significance may well be the result of the smaller sample size and the additional differencing.

Estimates from both the second IV and the spatially-matched sample alleviate concerns about the validity of the AIA instrument. In particular, the results reject selection on the timing of the AIA meeting, inconsistent with the hypothesis that the

effects are driven by the AIA meeting increasing the awareness of the reform, and are robust to allowing unobservable spatial correlation. The one exception to the main results comes from the spatially matched pairs. The effect of certification on perennials turns large and very significant, where a one standard deviation increase in certification increases land planted with perennials by around 3 percentage points. At this point, we can only speculate to the difference in the estimated effects between the two samples, but we believe that state boundaries tend to run along high lying mountain ranges with a more temperate climate that is better suited to perennials. While this is evidence for a direct effect of land certification since this is an investment tied to cultivation, one could argue that ejido members might view a switch to perennials as violating the land use requirement.

Finally, in table 10, we present the main results on a subsample that includes only the three middle quintiles of the treatment variable. The coefficients have similar signs and higher magnitudes although standard errors, not surprisingly, increase.

7 Conclusion

Mexico's second agrarian reform and accompanying land certification program are widely viewed as having little impact on agricultural production. Armed with a rigorous estimation strategy and a theoretical model, we find that only in conjunction with each other did they have a positive impact on agricultural investment and rural land markets. Indeed, the success of the reform turns on the length of time under certification.

We find that the length of time under certification increases fallowing, investments in irrigation and land rentals. However, we do not observe an impact of certification on a number of other land-related investments nor on credit access. To explain these puzzling results, we employ a simple model. Our model demonstrates the problem of treating specificity as a mere component of effective control. According to the model, certification enables ejido members to coordinate their beliefs about the de facto enforcement of the constitutional change. Since all ejidos benefited from the constitutional reform but were not treated equally under the certification program, we can distinguish between the direct effect and the coordination effect. An improvement in effective control should uniformly affect investments, whereas the coordination effect should only have an impact on investment decisions that were directly targeted by the de jure changes in control.

Although economists have been finding mixed evidence concerning the effects of land reforms, a clearer picture of the role of property institutions is beginning to emerge. In more recent analyses of property institutions, economists appeal to informal institutions that influence de facto rights. This innovation has proved fruitful: we have learned that formalization of property claims is not always appropriate. Formalization may not improve control for those with pre-existing claims and may rather cause confusion. Our results suggest that formalization can improve specificity in the context of Mexico's ejidos where strong individual control rights existed before the reform and that improving specificity can have important economic effects. Thus, our findings point to another important innovation: separating the notions of specificity and control.

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Table 1: Post-Salinas Change in De Jure Control

	Individual Plots	Common Lands
Pre-Reform	TSC, RC	TSC
Post-Reform:	TS, R, SC	TSC
Certification	TS, R, SC	TS, R, SC
Privatization	TS, R, S	TS, R, S

TS=Secure tenure; TSC=Conditional tenure security; R = Unconditional right to rent; RC=Conditional right to rent; S = Unconditional right to sell; SC=Conditional right to sell

Table 2: Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Abandoned 91	0.14	0.14	0	0.98	1823
Abandoned 07	0.17	0.15	0	0.88	1834
Fallow 91	0.12	0.13	0	1	1823
Fallow 07	0.04	0.07	0	0.88	1834
Rented 91	0.01	0.04	0	0.83	1823
Rented 07	0.03	0.05	0	0.62	1849
Sales Eji 07	0.55	0.3	0	1	1853
Sales Pos&Avec 07	0.38	0.29	0	1	1853
Sales Others 07	0.35	0.29	0	1	1853
Irrigation 91	0.17	0.24	0	1	1836
Irrigation 07	0.16	0.24	0	1	1848
Tractors 91	0.05	0.08	0	0.85	1832
Tractors 07	0.04	0.07	0	0.61	1850
Perennials 91	0.15	0.25	0	1.3	1836
Perennials 07	0.23	0.27	0	1.46	1848
Fertilizer 91	0.54	0.31	0	1	1832
Fertilizer 07	0.34	0.31	0	0.98	1850
Credit 91	0.21	0.2	0	2.67	1823
Credit 07	0.04	0.07	0	0.79	1850
PROCEDE	0.5	0.16	0	0.8	1853
Instrument	0.73	0.16	0.04	0.96	1850
Population 05	47602.19	127653.16	242	1688258	1853
Indigenous 05	0.13	0.25	0	1	1853
Empl. Agro 90	0.52	0.23	0	0.98	1853
Illiterate 90	0.19	0.12	0.02	0.77	1853
Distance Capital	107.18	76.71	0	548.15	1853
Ruggedness	57.04	46.69	0.03	263.04	1853
Int. Conflicts 07	0.16	0.19	0	1	1853
Ext. Conflicts 07	0.23	0.24	0	1	1853
Invasions 07	0.19	0.21	0	1	1853
Common Area 07	0.45	0.32	0	1	1853
Plot Area 07	8511.42	14488.03	0	238385.77	1853
Share Producers 07	0.07	0.06	0	0.45	1853
Num. Privatized 07	0.23	0.27	0	1	1838
Area Privatized 07	0.06	0.18	0	3.63	1838
Contract 07	0.02	0.06	0	0.78	1845
Num. Ejidos 07	16.29	20.84	1	185	1853
Num. Ejidatarios 07	1963.36	2462.8	2	28085	1853

Table 3: Description of variables

Variable	Description	Source	Years
Dependent Variables:			
Abandoned	Proportion of total agricultural ejido land that is neither under cultivation nor left fallowing.	Agricultural Census	1991, 2007
Fallow	Proportion of total agricultural ejido land left fallowing.	Agricultural Census	1991, 2007
Rented	Proportion of total ejido land area that is rented.	Agricultural Census	1991, 2007
Sales Eji	Proportion of ejidos reporting land sales to ejidatarios.	Ejido Census	2007
Sales Pos&Avec	Proportion of ejidos reporting land sales to posesionarios or avecinados.	Ejido Census	2007
Sales Others	Proportion of ejidos reporting land sales to persons outside the ejido.	Ejido Census	2007
Irrigation	Proportion of agricultural ejido land with an irrigation system.	Agricultural Census	1991, 2007
Perennials	Proportion of agricultural ejido land planted with perennials.	Agricultural Census	1991, 2007
Fertilizer	Proportion of agricultural ejido land treated with chemical fertilizer.	Agricultural Census	1991, 2007
Credit	Proportion of producers on ejido land who are using credit.	Agricultural Census	1991, 2007
Independent Variables:			
PROCEDE	Monthly average over the 1993-2007 period of ejidos that have been certified.	National Agrarian Registry	1993-2007
Instrument	Monthly average over the 1993-2007 period of ejidos that have had the first informational meeting (AIA).	Procurarduría Agraria	1993-2007
Population 05	Total population of municipality	Conteo	2005
Indigenous 05	Proportion of municipality's population five years of age or older that speaks an indigenous language.	Conteo	2005
Empl. Agro 90	Proportion of amunicipality's labor force working in the primary sector.	Census	1990
Illiterate 90	Proportion of municipality's population 12 years of age or older is illiterate.	Census	1990
Distance Capital	Euclidian distance from seat of municipality to state capital measured. in kilometers	INEGI	constant
Ruggedness	Index of the ruggedness of a municipality's territory following Nunn and Puga (2012) The index represents the average change in altitude (in meters) moving between two points one km apart in four directions.	US Geological Survey	constant
Int. Conflicts 07	Proportion of ejidos reporting internal conflicts over land.	Ejido Census	2007
Ext. Conflicts 07	Proportion of ejidos reporting external conflicts over land.	Ejido Census	2007
Invasions 07	Proportion of ejidos reporting land invasions.	Ejido Census	2007
Common Area 07	Proportion of ejido area that is communally held.	Ejido Census	2007
Plot Area 07	Proportion of ejido area that is divided into individual plots.	Ejido Census	2007
Share Producers 07	Share of producers on ejido land in municipality's total population	Agricultural Census	2007
Num. Privatized 07	Proportion of ejidos reporting privatization of some land.	Ejido Census	2007
Area Privatized 07	Proportion of ejido land that has been privatized.	Ejido Census	2007
Contract 07	Proportion of producers on ejido land who produce under a contract with agroindustrial companies.	Agricultural Census	2007
Num. Ejidos 07	Total number of ejidos in municipality.	Ejido Census	2007
Num. Ejidatarios 07	Total number of ejidatarios in municipality.	Ejido Census	2007

Notes: All variables are either averages or totals aggregated at the municipal level.

Table 4: Instrument Validity for Dependent Variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome 91	.046	.012	.172	-.002	.003	-.003	.048*
	(.029)	(.040)	(.113)	(.016)	(.022)	(.026)	(.026)
Population 05	-2.87e-08	-3.05e-08	-2.73e-08	-2.91e-08	-2.86e-08	-2.86e-08	-2.67e-08
	(2.87e-08)	(2.83e-08)	(2.85e-08)	(2.86e-08)	(2.86e-08)	(2.87e-08)	(2.89e-08)
Indigenous 05	-.016	-.017	-.015	-.016	-.016	-.016	-.017
	(.030)	(.030)	(.030)	(.030)	(.030)	(.030)	(.030)
Distance Capital	.0000205	.0000137	.000024	.0000203	.0000201	.0000196	.0000182
	(.0000491)	(.000049)	(.0000491)	(.0000493)	(.0000492)	(.0000495)	(.0000492)
Ruggedness	-.0001	-.0001	-.0001	-.0001	-.0001	-.0001	-.0002
	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)
Int. Conflicts 07	-.020	-.017	-.019	-.019	-.019	-.019	-.017
	(.035)	(.035)	(.036)	(.036)	(.036)	(.036)	(.036)
Ext. Conflicts 07	-.105***	-.106***	-.106***	-.106***	-.105***	-.105***	-.106***
	(.030)	(.030)	(.030)	(.030)	(.030)	(.030)	(.030)
Invasions 07	-.091**	-.094**	-.091**	-.091**	-.091**	-.091**	-.091**
	(.036)	(.036)	(.036)	(.036)	(.037)	(.037)	(.036)
Common Area 07	-.035*	-.034*	-.036*	-.034*	-.034	-.034*	-.035*
	(.020)	(.020)	(.020)	(.020)	(.021)	(.020)	(.020)
Plot Area 07	6.89e-08	5.42e-08	8.90e-08	7.13e-08	7.16e-08	7.14e-08	7.65e-08
	(2.59e-07)	(2.60e-07)	(2.61e-07)	(2.63e-07)	(2.63e-07)	(2.63e-07)	(2.61e-07)
Share Producers 07	-.038	-.031	-.037	-.039	-.038	-.039	-.054
	(.093)	(.093)	(.092)	(.093)	(.094)	(.093)	(.094)
Num. Privatized 07	.044***	.044***	.044***	.044***	.044***	.044***	.043***
	(.017)	(.017)	(.017)	(.017)	(.017)	(.017)	(.017)
Area Privatized 07	-.023	-.024	-.022	-.023	-.023	-.023	-.022
	(.020)	(.020)	(.020)	(.020)	(.020)	(.020)	(.020)
Contract 07	-.067	-.066	-.064	-.067	-.068	-.066	-.095*
	(.048)	(.048)	(.048)	(.048)	(.051)	(.048)	(.051)
Num. Ejidos 07	-.0003	-.0003	-.0003	-.0003	-.0003	-.0003	-.0003
	(.0003)	(.0003)	(.0003)	(.0003)	(.0003)	(.0003)	(.0003)
Technology 91	.039*	.038*	.037	.039*	.039*	.043	.036
	(.024)	(.023)	(.023)	(.023)	(.024)	(.037)	(.023)
Tractors 91	.019	.020	.011	.019	.020	.019	.014
	(.051)	(.051)	(.052)	(.051)	(.052)	(.051)	(.051)
National Markets 91	-.012	-.010	-.015	-.013	-.013	-.013	-.019
	(.021)	(.022)	(.021)	(.021)	(.021)	(.021)	(.021)
Exports 91	-.344	-.365	-.353	-.340	-.341	-.339	-.352
	(.398)	(.395)	(.398)	(.398)	(.397)	(.398)	(.398)
Credit 91	.090**	.088**	.086**	.092**	.091**	.091**	.061
	(.037)	(.037)	(.037)	(.038)	(.036)	(.037)	(.040)
Other Activity 91	.105	.092	.105	.104	.104	.104	.117
	(.095)	(.092)	(.095)	(.095)	(.095)	(.095)	(.100)
Econ Active 90	.035	.043	.043	.036	.036	.036	.025
	(.143)	(.144)	(.143)	(.143)	(.143)	(.143)	(.144)
Illiterate 90	-.061	-.064	-.063	-.062	-.062	-.062	-.064
	(.085)	(.085)	(.085)	(.085)	(.085)	(.085)	(.085)
Unemployed 90	.029	.019	.025	.029	.030	.028	.024
	(.211)	(.211)	(.210)	(.212)	(.210)	(.212)	(.213)
Sec. Education 90	-.091	-.102	-.092	-.090	-.090	-.091	-.082
	(.112)	(.111)	(.111)	(.112)	(.112)	(.111)	(.111)
Higher Education 90	-.075	-.058	-.084	-.075	-.076	-.074	-.094
	(.197)	(.197)	(.196)	(.197)	(.198)	(.197)	(.197)
HH Income 90	-.006	-.007	-.006	-.006	-.007	-.007	-.006
	(.011)	(.011)	(.011)	(.011)	(.011)	(.011)	(.011)
Empl. Agro 90	-.106***	-.111***	-.107***	-.105***	-.106***	-.105***	-.105***
	(.035)	(.035)	(.035)	(.035)	(.035)	(.035)	(.035)
Obs.	1802	1802	1802	1802	1802	1802	1802
F statistic	4.891	5.19	4.873	4.866	4.929	4.864	4.942
Outcome 91 Variable	Fallow	Abandon	Rent	Irrig	Perenne	Fertil	Credit

Notes: ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors in parentheses. The first three columns are run using OLS with different sets of controls. The second triplet repeat this exercise using spatial matching. We report the F-statistic.

Table 5: Land abandoned and land left fallowing

	Aband OLS (1)	Aband OLS (2)	Aband IV (3)	Aband IV (4)	Fallow OLS (5)	Fallow OLS (6)	Fallow IV (7)	Fallow IV (8)
PROCEDE	-.073***	-.081***	-.108***	-.125***	.027	.019	.078**	.079**
	(.024)	(.025)	(.035)	(.040)	(.020)	(.019)	(.031)	(.031)
Outcome 91	.244***	.240***	.245***	.240***	.238***	.239***	.240***	.240***
	(.031)	(.030)	(.031)	(.030)	(.040)	(.042)	(.040)	(.042)
Population 05	1.12e-08	9.71e-09	5.40e-09	1.80e-09	1.64e-08	9.99e-11	2.49e-08	1.13e-08
	(2.25e-08)	(2.32e-08)	(2.25e-08)	(2.36e-08)	(1.78e-08)	(1.57e-08)	(1.82e-08)	(1.62e-08)
Indigenous 05	-.054***	-.050***	-.052***	-.048***	.088***	.087***	.087***	.088***
	(.019)	(.019)	(.019)	(.019)	(.021)	(.020)	(.021)	(.020)
Empl. Agro 90	-.003	-.003	-.005	-.005	.022	.032*	.027*	.037*
	(.017)	(.022)	(.017)	(.022)	(.015)	(.020)	(.015)	(.020)
Illiterate 90	.147***	.154***	.139***	.145***	-.076	-.072	-.075	-.071
	(.050)	(.053)	(.050)	(.053)	(.055)	(.059)	(.055)	(.059)
Distance Capital	.000079	.0000954**	.0000749	.0000914*	-.0000589	-.0000669*	-.000054	-.0000624
	(.0000488)	(.0000485)	(.0000486)	(.0000482)	(.0000371)	(.0000382)	(.0000369)	(.0000382)
Ruggedness	.0004***	.0004***	.0004***	.0003***	-.0000919	-.0000569	-.0000452	-.0000179
	(.0000895)	(.0000887)	(.0000929)	(.0000907)	(.000074)	(.0000697)	(.0000745)	(.0000703)
Int. Conflicts 07		.004		.003		.002		.003
		(.022)		(.022)		(.016)		(.016)
Ext. Conflicts 07		-.026		-.033		.008		.015
		(.020)		(.020)		(.016)		(.016)
Invasions 07		-.009		-.014		-.019		-.013
		(.022)		(.022)		(.018)		(.018)
Common Area 07		.053***		.053***		-.008		-.007
		(.014)		(.014)		(.013)		(.013)
Plot Area 07		3.58e-07		3.30e-07		-1.39e-09		3.65e-08
		(2.29e-07)		(2.34e-07)		(1.88e-07)		(1.94e-07)
Share Producers 07		.003		-.001		-.126*		-.119*
		(.071)		(.072)		(.068)		(.070)
Num. Privatized 07		.024		.026		-.012		-.015
		(.017)		(.018)		(.014)		(.014)
Area Privatized 07		.025		.024		.010		.012
		(.018)		(.019)		(.017)		(.017)
Contract 07		-.003		-.006		-.058***		-.055**
		(.027)		(.028)		(.021)		(.022)
Num. Ejidos 07		.0000295		.0000314		.0001		.0000978
		(.0001)		(.0001)		(.0001)		(.0002)
Num. Ejidatarios 07		-1.40e-06		-1.19e-06		-4.98e-07		2.21e-07
		(1.01e-06)		(1.04e-06)		(1.18e-06)		(1.20e-06)
First Stage:								
Instrument			.719***	.686***			.717***	.685***
			(.031)	(.031)			(.032)	(.031)
Outcome 91			-.021	-.014			-.041	-.029
			(.029)	(.027)			(.028)	(.029)
Obs.	1812	1799	1809	1796	1812	1799	1809	1796
Cragg-Donald			526.112	476.156			514.098	469.122
F statistic	21.824	11.837	22.443	11.815	9.342	7.216	9.71	7.166

Notes: ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors in parentheses. We report the log-likelihood (e(ll)), the root mean squared error (e(rmse)), the Cragg-Donald statistic (e(widstat)), and the F-statistic. The first two columns show OLS results for the full sample, followed by their IV counterpart. This is repeated for the spatially matched differences in the last four columns. Each pair of columns shows first a parsimonious specification, controlling only for the baseline and the likely determinants of an early meeting, followed by an estimation including the full set of controls. Regressions are weighted using the number of producers and include state fixed effects (state-pair fixed effects in the spatial matching regressions).

Table 6: Investment related outcomes

	Irrig OLS (1)	Irrig IV (2)	Perenne OLS (3)	Perenne IV (4)	Fertil OLS (5)	Fertil IV (6)	Credit OLS (7)	Credit IV (8)
PROCEDE	-.017	.052**	.065	-.004	-.060	.014	.006	.017
	(.017)	(.024)	(.041)	(.057)	(.048)	(.079)	(.013)	(.018)
Outcome 91	.874***	.875***	.719***	.717***	.600***	.599***	.103***	.103***
	(.016)	(.015)	(.026)	(.027)	(.024)	(.024)	(.015)	(.015)
Population 05	-1.56e-08	-9.16e-09	6.80e-09	-5.92e-09	-9.36e-08***	-7.98e-08**	3.27e-09	5.46e-09
	(1.74e-08)	(1.77e-08)	(2.51e-08)	(2.68e-08)	(3.44e-08)	(3.65e-08)	(1.43e-08)	(1.45e-08)
Indigenous 05	-.015*	-.014*	-.008	-.011	-.030	-.030	-.002	-.001
	(.008)	(.008)	(.028)	(.028)	(.032)	(.032)	(.006)	(.007)
Empl. Agro 90	.008	.010	.109***	.104***	-.080*	-.074*	.049***	.050***
	(.016)	(.016)	(.028)	(.028)	(.044)	(.044)	(.012)	(.012)
Illiterate 90	-.057**	-.052*	-.274***	-.269***	-.042	-.039	-.049**	-.050**
	(.027)	(.027)	(.066)	(.066)	(.100)	(.101)	(.020)	(.020)
Distance Capital	.0000319	.0000355	.0004***	.0004***	-.0000338	-.0000279	-.0000857***	-.0000848***
	(.0000251)	(.0000252)	(.0000754)	(.000076)	(.0000748)	(.0000746)	(.0000271)	(.0000271)
Ruggedness	-.0000795	-.0000621	.0004**	.0004**	-.001***	-.001***	-.0002***	-.0002***
	(.0000489)	(.0000493)	(.0002)	(.0002)	(.0002)	(.0002)	(.0000462)	(.0000455)
Int. Conflicts 07	.003	.004	-.032	-.034	-.059	-.056	-.004	-.004
	(.015)	(.015)	(.027)	(.028)	(.047)	(.047)	(.010)	(.010)
Ext. Conflicts 07	.025	.030*	.005	-.003	.077**	.086**	.023**	.024**
	(.016)	(.017)	(.032)	(.031)	(.039)	(.040)	(.010)	(.010)
Invasions 07	-.020	-.016	.038	.032	-.109***	-.102**	-.021*	-.020*
	(.016)	(.016)	(.035)	(.037)	(.042)	(.042)	(.011)	(.011)
Common Area 07	-.021**	-.021**	-.106***	-.107***	.046*	.047*	-.040***	-.039***
	(.008)	(.008)	(.026)	(.027)	(.027)	(.027)	(.009)	(.009)
Plot Area 07	-1.38e-07	-1.14e-07	9.88e-08	5.75e-08	-8.49e-07***	-8.02e-07***	-2.33e-07**	-2.26e-07**
	(9.43e-08)	(9.46e-08)	(2.99e-07)	(3.09e-07)	(3.06e-07)	(3.03e-07)	(1.10e-07)	(1.08e-07)
Share Producers 07	-.061	-.057	-.298***	-.308***	.229	.236*	-.162***	-.160***
	(.048)	(.048)	(.095)	(.096)	(.139)	(.140)	(.043)	(.043)
Num. Privatized 07	.002	.0004	-.007	-.004	.002	-.001	-.007	-.007
	(.011)	(.011)	(.020)	(.020)	(.029)	(.029)	(.008)	(.008)
Area Privatized 07	-.012	-.011	.032	.031	.013	.015	.002	.002
	(.010)	(.011)	(.028)	(.028)	(.031)	(.031)	(.010)	(.010)
Contract 07	.124***	.126***	.386***	.384***	.042	.047	.310***	.311***
	(.033)	(.034)	(.067)	(.067)	(.067)	(.068)	(.054)	(.054)
Num. Ejidos 07	-.0000332	-.0000358	-.0007***	-.0007***	.001***	.001***	.0003**	.0003**
	(.0000931)	(.0000923)	(.0002)	(.0002)	(.0003)	(.0003)	(.0001)	(.0001)
Num. Ejidatarios 07	9.91e-07	8.21e-07	3.89e-06*	4.19e-06*	8.91e-07	5.56e-07	2.02e-06**	1.97e-06**
	(6.74e-07)	(6.63e-07)	(2.09e-06)	(2.19e-06)	(2.72e-06)	(2.82e-06)	(8.02e-07)	(7.91e-07)
First Stage:								
Instrument		.685***		.683***		.686***		.683***
		(.031)		(.031)		(.031)		(.031)
Outcome 91		-.035***		-.042***		-.009		.023
		(.013)		(.014)		(.012)		(.019)
Obs.	1817	1814	1817	1814	1814	1811	1808	1805
Cragg-Donald		486.889		489.946		487.609		465.021
F statistic	211.474	217.103	106.841	107.096	46.486	45.631	10.205	9.593

Notes: ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors in parentheses. We report the log-likelihood (e(ll)), the root mean squared error (e(rmse)), the Cragg-Donald statistic (e(widstat)), and the F-statistic. The first two columns show OLS results for the full sample, followed by their IV counterpart. This is repeated for the spatially matched differences in the last four columns. Each pair of columns shows first a parsimonious specification, controlling only for the baseline and the likely determinants of an early meeting, followed by an estimation including the full set of controls. Regressions are weighted using the number of producers and include state fixed effects (state-pair fixed effects in the spatial matching regressions).

Table 7: Land markets

	Rent OLS (1)	Rent IV (2)	Sales Ejidatarios OLS (3)	Sales Ejidatarios IV (4)	Sales Pos/Avec OLS (5)	Sales Pos/Avec IV (6)	Sales External OLS (7)	Sales External IV (8)
PROCEDE	.017*	.030**	.175***	.195**	.017	.172*	.112**	.244***
	(.009)	(.012)	(.063)	(.091)	(.061)	(.095)	(.052)	(.068)
Rented 91	.644***	.641***						
	(.151)	(.150)						
Population 05	-6.22e-09	-3.77e-09	-7.02e-08	-6.64e-08	-2.00e-08	8.73e-09	1.82e-08	4.26e-08
	(9.74e-09)	(9.60e-09)	(5.95e-08)	(6.04e-08)	(5.73e-08)	(5.40e-08)	(4.85e-08)	(4.79e-08)
Indigenous 05	-.008*	-.008*	-.262***	-.264***	-.235***	-.237***	-.143***	-.142***
	(.004)	(.005)	(.043)	(.043)	(.043)	(.043)	(.031)	(.032)
Empl. Agro 90	.029***	.030***	.135***	.135***	.038	.047	-.049	-.040
	(.008)	(.009)	(.044)	(.044)	(.045)	(.045)	(.043)	(.043)
Illiterate 90	-.022	-.021	.018	.027	-.082	-.063	-.057	-.049
	(.013)	(.014)	(.120)	(.121)	(.113)	(.115)	(.093)	(.097)
Distance Capital	-.0000222	-.0000212	-.0002***	-.0002***	-.0001	-.0001	-.0001	-.0000965
	(.0000172)	(.0000172)	(.0000898)	(.0000906)	(.0000926)	(.0000948)	(.0000829)	(.0000842)
Ruggedness	-.0001***	-.0001***	-.0003	-.0003	-.0003*	-.0003	-.0007***	-.0006***
	(.0000277)	(.0000273)	(.0002)	(.0002)	(.0002)	(.0002)	(.0002)	(.0002)
Int. Conflicts 07	.010	.011	-.056	-.055	-.0001	.006	.043	.048
	(.008)	(.008)	(.058)	(.058)	(.061)	(.062)	(.049)	(.050)
Ext. Conflicts 07	-.0002	.001	.189***	.192***	.120**	.141**	.066	.083
	(.009)	(.009)	(.055)	(.056)	(.058)	(.059)	(.049)	(.051)
Invasions 07	-.002	-.0007	-.058	-.056	.051	.065	.032	.043
	(.008)	(.008)	(.064)	(.064)	(.066)	(.066)	(.052)	(.051)
Common Area 07	-.020***	-.020***	-.397***	-.397***	-.329***	-.328***	-.238***	-.237***
	(.006)	(.006)	(.032)	(.032)	(.033)	(.033)	(.025)	(.025)
Plot Area 07	-1.09e-07**	-1.01e-07**	2.18e-06***	2.20e-06***	2.28e-06***	2.38e-06***	1.36e-06***	1.45e-06***
	(4.51e-08)	(4.43e-08)	(4.22e-07)	(4.22e-07)	(5.40e-07)	(5.57e-07)	(4.59e-07)	(4.82e-07)
Share Producers 07	-.164***	-.163***	-.025	-.025	-.163	-.148	-.726***	-.712***
	(.031)	(.031)	(.149)	(.149)	(.160)	(.161)	(.124)	(.127)
Num. Privatized 07	-.009	-.009	-.031	-.032	.110***	.103***	.140***	.134***
	(.006)	(.006)	(.032)	(.033)	(.036)	(.037)	(.034)	(.034)
Area Privatized 07	-.002	-.002	-.006	-.005	-.083	-.079	-.092**	-.088**
	(.007)	(.007)	(.034)	(.035)	(.051)	(.051)	(.044)	(.044)
Contract 07	.007	.008	.090	.091	.120	.128*	.143*	.151**
	(.013)	(.013)	(.074)	(.073)	(.078)	(.078)	(.075)	(.076)
Num. Ejidos 07	.0000264	.0000257	-.001***	-.001***	-.002***	-.002***	-.001***	-.001***
	(.0000524)	(.000052)	(.0004)	(.0004)	(.0003)	(.0004)	(.0003)	(.0003)
Num. Ejidatarios 07	1.06e-06**	9.97e-07**	-1.54e-06	-1.64e-06	2.55e-06	1.82e-06	-4.47e-07	-1.06e-06
	(4.16e-07)	(4.02e-07)	(3.19e-06)	(3.20e-06)	(2.71e-06)	(2.77e-06)	(2.22e-06)	(2.20e-06)
First Stage:								
Instrument		.684***		.685***		.685***		.685***
		(.031)		(.031)		(.031)		(.031)
Rented 91		.122						
Obs.	1808	1805	1830	1827	1830	1827	1830	1827
Cragg-Donald		468.678		483.972		483.972		483.972
F statistic	9.502	9.071	24.197	22.61	17.609	16.727	30.782	30.427

Notes: ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors in parentheses. We report the log-likelihood (e(ll)), the root mean squared error (e(rmse)), the Cragg-Donald statistic (e(widstat)), and the F-statistic. The first two columns show OLS results for the full sample, followed by their IV counterpart. This is repeated for the spatially matched differences in the last four columns. Each pair of columns shows first a parsimonious specification, controlling only for the baseline and the likely determinants of an early meeting, followed by an estimation including the full set of controls. Regressions are weighted using the number of producers and include state fixed effects (state-pair fixed effects in the spatial matching regressions).

Table 8: Results for two instruments

	Fallow	Aband	Irrig	Perenne	Rent
	(1)	(2)	(3)	(4)	(5)
PROCEDE	.079**	-.124***	.052**	-.001	.029**
	(.031)	(.040)	(.024)	(.057)	(.012)
Outcome 91	.240***	.240***	.875***	.717***	.642***
	(.042)	(.030)	(.015)	(.027)	(.150)
First Stage:					
AIA Instrument	.686***	.686***	.686***	.684***	.684***
	(.031)	(.031)	(.031)	(.031)	(.031)
Additional Instrument	-.032*	-.034*	-.037*	-.028	-.034*
	(.019)	(.019)	(.020)	(.020)	(.019)
Outcome 91	-.028	-.014	-.036***	-.039***	.127*
	(.029)	(.026)	(.013)	(.014)	(.076)
Effect on AIA Instrument					
Additional Instrument	.026	.026	.026	.028	.025
	(.022)	(.022)	(.022)	(.023)	(.022)
Obs.	1796	1796	1814	1814	1805
Cragg-Donald	236.429	239.48	245.927	246.228	235.955
OIR test	.818	.766	.796	.275	.141
F statistic	7.183	11.847	217.701	107.25	9.041

Notes: ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors in parentheses. We report the log-likelihood (e(ll)), the root mean squared error (e(rmse)), the Cragg-Donald statistic (e(widstat)), and the F-statistic. The first two columns show OLS results for the full sample, followed by their IV counterpart. This is repeated for the spatially matched differences in the last four columns. Each pair of columns shows first a parsimonious specification, controlling only for the baseline and the likely determinants of an early meeting, followed by an estimation including the full set of controls. Regressions are weighted using the number of producers and include state fixed effects (state-pair fixed effects in the spatial matching regressions).

Table 9: Results for spatial match

	Aband (1)	Aband (2)	Fallow (3)	Fallow (4)	Irrig (5)	Irrig (6)	Perenne (7)	Perenne (8)	Rent (9)	Rent (10)
PROCEDE	-.076**	-.091*	-.020	.034	.036	.098*	.124**	.146**	-.004	.018
	(.033)	(.054)	(.020)	(.030)	(.032)	(.057)	(.049)	(.071)	(.012)	(.020)
Outcome 91	.109***	.109***	.115***	.116***	.754***	.754***	.517***	.518***	.167***	.161***
	(.042)	(.042)	(.034)	(.034)	(.026)	(.026)	(.055)	(.055)	(.049)	(.049)
Population 05	-7.66e-08	-7.72e-08*	5.80e-08***	5.99e-08***	4.20e-09	6.44e-09	-5.56e-08	-5.47e-08	1.22e-08	1.30e-08
	(4.69e-08)	(4.69e-08)	(2.17e-08)	(2.19e-08)	(2.99e-08)	(3.02e-08)	(5.60e-08)	(5.64e-08)	(1.98e-08)	(1.99e-08)
Indigenous 05	.016	.015	.072**	.076***	.005	.010	-.086	-.085	.021	.023*
	(.033)	(.034)	(.028)	(.029)	(.020)	(.021)	(.058)	(.058)	(.013)	(.013)
Empl. Agro 90	-.037	-.036	.044**	.041*	-.007	-.011	.046	.044	-.0009	-.002
	(.033)	(.033)	(.022)	(.022)	(.022)	(.022)	(.038)	(.038)	(.013)	(.013)
Illiterate 90	.128	.127	-.144**	-.144**	.046	.047	-.124	-.123	-.005	-.005
	(.088)	(.088)	(.070)	(.071)	(.049)	(.050)	(.121)	(.121)	(.029)	(.029)
Distance Capital	.0004***	.0004***	.0000508	.0000057	.0000186	.000026	.000029	.0000315	-.0000546	-.000052
	(.0000954)	(.0000957)	(.0000596)	(.0000599)	(.0000508)	(.000051)	(.0001)	(.0001)	(.0000366)	(.0000365)
Ruggedness	.0005***	.0005***	-.0000962	-.0000595	-.0000439	-3.93e-06	.0008***	.0008***	-.0003***	-.0002***
	(.0002)	(.0002)	(.0001)	(.0001)	(.0001)	(.0001)	(.0002)	(.0002)	(.0000759)	(.0000777)
Int. Conflicts 07	-.021	-.021	.027*	.027*	-.069**	-.069**	-.034	-.034	.005	.005
	(.024)	(.024)	(.014)	(.015)	(.035)	(.035)	(.034)	(.033)	(.009)	(.009)
Ext. Conflicts 07	-.023	-.025	.0004	.004	.062	.067*	.016	.018	.006	.007
	(.027)	(.028)	(.016)	(.017)	(.039)	(.040)	(.034)	(.034)	(.012)	(.012)
Invasions 07	-.037	-.039	-.019	-.015	.003	.008	.017	.019	-.012	-.010
	(.026)	(.026)	(.018)	(.018)	(.029)	(.028)	(.034)	(.034)	(.010)	(.010)
Common Area 07	-.026	-.027	.007	.008	-.058***	-.056***	-.018	-.018	-.021**	-.021**
	(.026)	(.026)	(.021)	(.020)	(.017)	(.017)	(.034)	(.034)	(.009)	(.009)
Plot Area 07	7.13e-07***	7.08e-07***	2.58e-07*	2.73e-07**	-3.31e-07**	-3.12e-07**	-3.33e-07	-3.26e-07	-4.67e-08	-4.02e-08
	(2.53e-07)	(2.50e-07)	(1.32e-07)	(1.34e-07)	(1.46e-07)	(1.51e-07)	(2.38e-07)	(2.36e-07)	(6.90e-08)	(6.71e-08)
Share Producers 07	-.206**	-.214**	.101	.073	.050	.082	-.042	-.030	-.046	-.034
	(.095)	(.096)	(.076)	(.077)	(.069)	(.077)	(.111)	(.112)	(.041)	(.042)
Num. Privatized 07	.050*	.050*	-.002	-.004	-.009	-.011	-.002	-.002	.015	.015
	(.029)	(.029)	(.016)	(.016)	(.015)	(.015)	(.026)	(.026)	(.009)	(.010)
Area Privatized 07	-.013	-.013	.054*	.053*	.028	.029	.027	.027	.032*	.031*
	(.035)	(.035)	(.033)	(.031)	(.034)	(.035)	(.055)	(.055)	(.012)	(.012)
Contract 07	-.096	-.092	.015	.003	.130**	.115**	.363***	.358***	.017	.012
	(.059)	(.060)	(.039)	(.040)	(.057)	(.058)	(.112)	(.111)	(.028)	(.029)
Num. Ejidos 07	-.0003*	-.0003*	-.0000753	-.0000649	.0002	.0002*	.0000146	.0000191	-.0001*	-.0001
	(.0002)	(.0002)	(.0001)	(.0001)	(.0001)	(.0001)	(.0003)	(.0003)	(.0000768)	(.0000773)
Num. Ejidatarios 07	-2.18e-06	-2.07e-06	-5.27e-07	-9.05e-07	-1.89e-07	-6.35e-07	5.22e-06**	5.05e-06**	1.71e-06***	1.55e-06***
	(1.50e-06)	(1.50e-06)	(1.48e-06)	(1.50e-06)	(1.25e-06)	(1.32e-06)	(2.39e-06)	(2.39e-06)	(6.01e-07)	(5.98e-07)
First Stage:										
Instrument		.624***		.624***		.622***		.620***		.622***
		(.040)		(.041)		(.040)		(.041)		(.040)
Outcome 91		-.020		.004		-.026		-.053*		.239**
		(.033)		(.033)		(.023)		(.031)		(.114)
Obs.	743	743	743	743	745	745	745	745	744	744
Cragg-Donald		231.566		228.592		230.094		227.114		230.88
F statistic	3.491	3.44	3.874	3.639	58.763	58.889	11.65	10.805	2.809	2.791

Notes: ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors in parentheses. We report the log-likelihood (e(ll)), the root mean squared error (e(rmse)), the Cragg-Donald statistic (e(widstat)), and the F-statistic. The first two columns show OLS results for the full sample, followed by their IV counterpart. This is repeated for the spatially matched differences in the last four columns. Each pair of columns shows first a parsimonious specification, controlling only for the baseline and the likely determinants of an early meeting, followed by an estimation including the full set of controls. Regressions are weighted using the number of producers and include state fixed effects (state-pair fixed effects in the spatial matching regressions).

Table 10: Results for three central quintiles of treatment variable

	Fallow	Aband	Irrig	Perenne	Rent
	(1)	(2)	(3)	(4)	(5)
PROCEDE	.121*	-.101	.085	-.183	.066**
	(.071)	(.095)	(.077)	(.137)	(.032)
Outcome 91	.221***	.242***	.880***	.699***	.719***
	(.049)	(.038)	(.020)	(.038)	(.216)
First Stage:					
Instrument	.547***	.550***	.540***	.531***	.549***
	(.035)	(.035)	(.036)	(.035)	(.035)
Outcome 91	-.048*	-.003	.008	-.038***	.034
	(.025)	(.020)	(.012)	(.013)	(.066)
Obs.	1094	1094	1101	1101	1098
Cragg-Donald	238.615	242.423	224.331	224.976	241.124
F statistic	3.342	8.998	168.904	52.486	4.849

Notes: ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors in parentheses. We report the log-likelihood (e(ll)), the root mean squared error (e(rmse)), the Cragg-Donald statistic (e(widstat)), and the F-statistic. The first two columns show OLS results for the full sample, followed by their IV counterpart. This is repeated for the spatially matched differences in the last four columns. Each pair of columns shows first a parsimonious specification, controlling only for the baseline and the likely determinants of an early meeting, followed by an estimation including the full set of controls. Regressions are weighted using the number of producers and include state fixed effects (state-pair fixed effects in the spatial matching regressions).

A Public information about θ

In this subsection, we consider the ability of certification to improve public information about the fundamental. To analyze this possibility, we need to introduce a public signal, μ_z, normally distributed about θ with variance σ_z and independent of the private signal. The public signal emerges from ejido assemblies as well as informational assemblies about the certification program. For a given μ_z, taking private noise to infinite precision, the limit value for the threshold cut-off discussed previously remains unchanged.

In this context, certification will improve the precision of the public signal because it reveals whether conflicts raised in the meetings were more apparent than real or vice versa. For simplicity, assume that the precision of the private signal is unaffected by certification, allowing us to isolate the third-party effect of specificity. Improving the public signal permits the coordination of beliefs that gives rise to multiplicity.

Even if there is no multiplicity before certification, i.e. $\frac{\sigma_v}{\sigma_z^2} < \sqrt{2\pi}$, the greater precision of the public signal makes multiplicity possible. If there is history dependence, then the multiplicity that certification opens up may be only a mirage. However, given our assumptions so far, the number of ejidos in the new regime can not go down with the introduction of certification and has the possibility of increasing. Due to the focus on monotone strategies, just as in the common knowledge case, we will have $\bar{\theta}_{public}$ and $\underline{\theta}_{public}$ that will bound the possible ejidos facing multiple equilibria. Of the ejidos that fall in this interval, only those who were above the threshold before cut-off have the possibility of changing their equilibrium outcome. Since these ejidos were in the status quo, if they switch, it will be to the new regime. Thus,

certification improves the adoption of the new regime if the change in precision of the public signal enables multiplicity.

However, the increase in precision may not result in multiplicity. In this case, one might wonder whether the cut-off θ is above or below the value relative to the level without certification. One can show, under reasonable parameter restrictions, that the cut-off value increases. Thus, again, certification results in more ejidos switching to adopt the new regime.

> Proposition (Morris and Shin (1998)): A reduction in public noise, holding the private noise fixed, results in an increase in the range of θ where multiple equilibria exist such that the status quo survives and the corresponding optimal labor allocation for $R = 0$ is one equilibrium and another is the alternative regime is adopted and the corresponding optimal labor allocation for $R = 1$.