Empirical Essays on Finance and Development

by

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Abstract

The central focus of this dissertation is the role of financial instruments, in particular insurance and credit, in economic development.

Motivated by the observation that exposure to the risk of extreme weather conditions may constrain investment by subsistence farmers and lead to inefficient production choices, the first chapter evaluates whether insuring farmers against such risks alters resource allocation decisions. In particular I consider the effects of a Mexican government disaster relief program with insurance-like features. The results, based on a regression discontinuity design, indicate that insurance against losses arising from natural disasters changes how rural households invest in their farms. Insured farmers utilize more expensive capital inputs and adopt different technologies. Additionally, the insurance changes labor supply patterns. Notably, members of insured households are approximately 10% more likely to migrate internationally. Additional results, that the program matters most when the returns to migration are more unpredictable, are consistent with a model where insurance obviates the need for precautionary savings, allowing households to finance international migration.

Turning from insurance to the role of access to credit in furthering development, the second chapter considers how interest rate ceilings affected investment in agricultural capital and the tenure status of farms in the nineteenth century United States. Using within state variation in usury laws, I find that more restrictive laws lead to an economically meaningful reduction in agricultural investment. Additionally, the results pertaining to the tenure status of farms indicate that exacting usury laws reduce the share of owner-operated farms. This effect is especially pronounced for small farms, which is consistent with the notion that interest rate limits ration small-scale, risky farmers out of the credit market. To overcome the issue of omitted factors which may affect both legislation and agricultural outcomes, I employ an instrumental variables strategy. By isolating variation in usury laws associated with the historical presence of religious bodies, this study provides evidence of a causal channel from more permissive interest rate ceilings to greater agricultural investment and a more egalitarian ownership structure of agricultural land.

The third chapter departs from the more narrow focus on the provision of financial services and addresses a question of relevance to development economics in general. In particular, this chapter, which is joint work with Abhijit Banerjee, Esther Duflo and Raghabendra Chattopadhyay, evaluates how well various systems for identifying and targeting assistance to the poorest of the poor actually identify the poorest. Firstly, we consider the methods used to

identify households eligible for participation in assistance programs administered by the Indian government. Secondly, we evaluate Participatory Rural Appraisals (PRAs) as a mechanism to identify exceptionally poor households. Finally, we investigate whether additional verification of information gathered in PRAs improves targeting. For each method of targeting, we examine whether the households identified by that process are more disadvantaged according to several measures of economic well-being than households which were not identified. We conclude that PRAs and PRAs coupled with additional verification successfully identify a population which is measurably poorer in various respects, especially those which are more readily observed. The standard government procedures, however, do not appear to target the very poorest for assistance. Based on this sample, households targeted for government assistance are observationally equivalent to those that are not.

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Contents

[1] **This chapter is joint work with Abhijit Banerjee, Esther Duflo and Raghabendra Chattopadhyay.**

Chapter 1

Weather Insurance and Investment Choice

1.1 Introduction

> "If you wait until the wind and the weather are just right, you will never plant anything and never harvest anything." [Ecclesiastes 11:4]

Risk is a fundamental impediment to investment. Where it can be diversified away or insured, resources will be employed to generate the highest possible expected return. If it can not, however, risk may distort investment decisions. The risk of unfavorable weather conditions looms especially large for subsistence farmers in developing countries and their capacity to deal with weather related income shocks is limited. Furthermore, these households often lack viable alternative employment opportunities and tend to be without formal insurance. Consequently, households alter production decisions to limit their exposure to perils such as droughts and floods.

While there is considerable evidence on measures taken by households to reduce risk, comparatively little is know about how behaviors change when households face less risk. This study addresses the later question by considering whether eligibility for a Mexican government disaster relief program, which has insurance-like features, changes how households allocate resources.

Much like an insurance policy, the program provides fixed indemnity payments to rural

households whose crops or assets are damaged by natural disasters. By exploiting discontinuities in the eligibility requirements for these payments, this study provides causal evidence on the impact of greater insurance on investment decisions.[1] In particular, the outcomes considered are investment in agricultural inputs, such as fertilizer and machinery use, as well as labor allocation decisions, including off-farm labor and migration.

The empirical results, based on a regression discontinuity design, reveal that eligibility for insurance against natural disasters does influence agricultural investment. While it is not clear that insured households invest more in absolute terms, these households do substitute towards more expensive forms of capital; they use tractors more intensively and are more likely to purchase improved seeds rather than local varieties. The strongest results show that members of insured households reallocate their labor supply, changing the household's portfolio of income sources. Notably, these individuals appear to forgo local employment, choosing instead to migrate elsewhere, predominantly to the United States. The estimates indicate that members of insured households are approximately 10% more likely to migrate than comparable members of uninsured households. Additionally, the average duration of migration to the U.S. is about 1 month longer for migrants belonging to insured households. In interpreting these results as causal, it is of concern that households may endogenously select into eligibility for indemnity payments, which would invalidate the empirical design (DiNardo and Lee, 2002; McCrary, 2007). The evidence, however, does not indicate that households actively change behavior to become eligible for insurance. Section 1.8 describes several tests for endogenous selection and subjects the results to further robustness checks.

In Section 1.7, I describe a theoretical mechanism through which insurance against natural disasters affects investment. This framework suggests that, by providing resources in the event of a catastrophe, insurance frees up precautionary savings to be used for agricultural investment or staking an international migrant. By establishing an income floor when crops fail, insurance can induce farmers to substitute towards more expensive capital inputs and new technologies. Furthermore, although international migration offers an income source

[1] In this paper, the term "insurance" is used to refer to the specific form of insurance provided by these payments. It should be noted that this program only provides assistance in the event of a covarying shock, a natural disaster, where many people in the same region are affected. The specific question considered here is whether the additional insurance, above a local level of self-insurance and informal insurance, associated with eligibility for this program impacts the behavior of rural households.

uncorrelated with agricultural production, it is itself a risky endeavor (see e.g. Davis et al., 2002; Bastida, 2001). If a natural disaster strikes, and migration simultaneously fails, households may be left destitute. By easing the resource constraint in this state of the world, weather insurance may enable households to finance a potentially rewarding but uncertain migration to the U.S.

I further consider alternative mechanisms and provide empirical results consistent with the proposed channel; namely that this insurance has a larger impact on the international migration decisions of households for whom such migration is especially risky. Using the strength of the U.S.-Mexico migration network in an individuals' home state as a proxy for the risk of migration to the U.S., I find that the results pertaining to international migration are driven by migrants from states which lack a strong migrant network in the U.S. and thus face larger risks in migrating.

By providing causal evidence on the effect of insurance on investment, this study ties into a broader body of research concerned with the risk coping strategies of rural households. Firstly this paper builds on prior work (e.g. Bliss and Stern, 1982; Morduch, 1995) by demonstrating that behaviors change in predictable ways when the necessity of adopting risk mitigating measures is reduced.

When confronted by a drop in income, due to adverse weather or other risks, households in developing countries may respond by selling assets or livestock (Rosenzweg and Wolpin, 1993), increasing household labor supply (Chetty and Looney, 2006), or drawing on informal insurance arrangements (Townsend, 1994 and 1995). These mechanisms, however, do not offer adequate protection against risks which affect many households in a concentrated geographical area, or "covarying" risks, such as drought or floods.

Asset sales and increased labor supply, for example, may provide limited insurance due to general equilibrium effects; if many households are simultaneously induced to sell assets or supply labor, prices and wages will fall (Jayachandran, 2006). Informal insurance is also not particularly robust to covarying shocks. Besley (1995) highlights how informal contracts overcome problems of information asymmetry, transaction costs and enforcement (which hinder provision of formal insurance in rural areas) by making use of local information networks and community sanctions. Since the strength of peer networks tends to decline with distance, these

arrangements necessarily trade off the ability to overcome information barriers with the ability to insure against geographically covarying risks (Barnett, et al., 2006).

Given the shortcomings of these coping mechanisms, rural households undertake *ex ante* measures to limit the variability of income; vulnerable agricultural households diversify income and reduce risk by spreading their production activities over geographically separate plots and they often rely on off-farm employment for substantial portions of total income (see e.g. Skees, Varangis and Larson, 2002). Even so, a weather event which impacts a broad geographical area will likely impact all plots. Local off-farm employment opportunities, both agricultural and non-agricultural, may also be correlated with shocks that affect the household's own agricultural enterprise (Dercon, 2002). Migration to geographically distant areas, however, can provide a stream of remittance income which is not correlated with local weather conditions.

Diversification is not the only means to reduce income variability. Other studies find that farmers employ particular production techniques due to considerations of risk. Agricultural households may over supply labor (relative to a profit maximizing choice) or reduce input provision to limit the downside in the event of a poor harvest (Bliss and Stern, 1982). They may also plant particular crops which have reliable yields or delay planting until more complete information about the season's weather arrives (Morduch, 1995).

By employing these devices, households effectively insure themselves against extreme duress in the event of a covarying shock when crops fail and other coping mechanisms offer limited reprieve. This insurance, however, comes at a cost. Morduch suggests that the safer crops favored by risk prone households generate lower yields than other varieties. Similarly, delaying planting or reducing inputs can diminish expected returns.

To the extent that weather related risk does impact the decisions of susceptible households, providing insurance against these hazards may allow households to allocate resources more efficiently. Indeed, Key, et al., (2006) find that increased crop insurance subsidies are associated with changes in U.S. farmer's labor allocation decisions. In developing countries, the availability of formal risk reducing instruments is more limited, thus the potential response to formal insurance provision is much greater. The results presented here confirm this intuition by documenting that investment decisions respond to the provision of weather insurance.

Finally this study contributes to the line of argument that, even if households are able

to smooth consumption, greater insurance may be particularly welfare enhancing if it allows households to better allocate resources and avoid costly actions undertaken to limit consumption drops (see, e.g., Morduch, 1995; Chetty and Looney, 2005). If, in order to avoid extreme poverty when crops fail, rural households forgo potentially profitable investments, such as international migration, then the provision of insurance against adverse weather events not only serves to smooth consumption but may also mitigate poverty in the long run.

1.2 Program Overview

In 1996, the Mexican government established the Fund for Natural Disasters (or FONDEN for its Spanish acronym[2]) to facilitate recovery from natural disasters. To that end the agency facilitates the reconstruction of public infrastructure and provides direct aid to private households. Among other activities, such as funding reparation of damaged homes, FONDEN finances indemnity payments to rural households whose production or productive assets are damaged by natural disasters. Eligibility for agricultural producers, which are the focus of this study, is determined by criteria pertaining to land and livestock holdings. With respect to land, producers who farm less than a specified threshold of land are eligible to receive a fixed indemnity per hectare. The eligibility limit is 5, 10 or 20 hectares depending on the state where the household resides.[3] Land which is insured through other means or which is irrigated is not eligible for indemnity payments. In Section 1.8, I discuss evidence suggesting it is unlikely that FONDEN crowds out private insurance. According to Mexico's Office of the Secretary of Agriculture any land on which the output accrues to a household (owned, rented or borrowed) would count towards the limit. Furthermore, a household which owned more land than the limit would not be eligible even if a portion of the land was rented to others. A household with less than the limit of agricultural land, but a few hectares of other unproductive land, would still be eligible and a producer with both irrigated and non-irrigated land is eligible for payments on the non-irrigated area, provided the other criteria are met. In addition to meeting these criteria, the households must not own more than 25 head of cattle (or equivalent; e.g. 100 fowl = 1 cow) in order to receive indemnities.

[2] Fondo de Desastres Naturales

[3] See Appendix A for information on limits by state.

Dispersal of FONDEN aid payments firstly requires that state or federal officials petition for a declaration of disaster, which can be made only after a responsible government agency[4] verifies the occurrence. In addition to relying on an official declaration of disaster, FONDEN also uses parametrically defined triggers to determine which events merit FONDEN funds. For example drought is defined as rainfall below 50% of the historical average for 2 consecutive months and crop specific temperature definitions of frost are included in FONDEN rules (0 degrees for tomatoes, -6 degrees for wheat, etc.).

Subsequently state, local and federal authorities investigate the damages and come to an agreement regarding the funds necessary for reparation and indemnification. After an agreement is reached, states must come up with 30% of the relief funds. When this requirement is met, FONDEN resources are released to state governments and are distributed to affected producers by municipal authorities in conjunction with the Secretary of Agriculture. Working from a pre-determined list of potentially eligible recipients, local authorities visit producers in the affected areas to verify that producers do meet the eligibility requirements and were in fact affected by the disaster.[5]

The size of the indemnity depends on the crop planted. Producers of coffee are eligible for indemnities of 970 pesos per hectare up to 2 hectares, fruit growers may obtain 391 pesos per hectare up to 3 hectares and producers of other agricultural crops are eligible for indemnities of 348 pesos per hectare up to 5 hectares. Indemnities are only paid on up to 5 hectares per producer irrespective of the eligibility limit in that particular state. Indemnities are also paid for loss of animals due to natural disasters at a rate of 244 pesos per head. To give the magnitude of the indemnities some economic perspective, the indemnity for a farmer of 5 hectares of general crops represents one month of wages as an agricultural laborer.[6] That the scope, as well as the scale, of aid is meaningful is evident from FONDEN's dispersal of nearly 1 billion pesos as compensation for agricultural losses from 1997 to 1999 (Skees, Varangis and Larson, 2002). Implying that roughly 4% of rural producers received an indemnity in any of

[4] The particular agency depends on the type of disaster; drought, earthquake, etc.

[5] Information in this section comes from Martinez (2005), "Acuerdo que establece las Reglas de Operación del Fondo de Desastres Naturales (FONDEN)", Skees, Varangis and Larson (2002), and conversations with Eduardo Ramirez in the office of the Mexican Secretary of Agriculture.

[6] Based on an average agricultural wage of 56 pesos per day, calculated from the ENHRUM sample discussed in this study.

those years.[7]

1.3 Empirical Strategy

The objective of this study is to isolate the impact of weather insurance on investment decisions. Since the determinates of FONDEN eligibility (landholdings, livestock and irrigation) likely affect the outcomes of interest, a naive cross-sectional comparison of average outcomes between eligible and ineligible households, who differ in terms of land and livestock, does not identify the effect of insurance on these outcomes. That eligibility is assigned discontinuously based on these characteristics can overcome this concern; by comparing households who barely meet and barely fail to meet the eligibility criteria it is possible to isolate differences in insurance while making differences in landholdings, irrigation and livestock negligible. Estimates based on this strategy, however, are also inadequate since the attributes which determine eligibility can be altered by households. Therefore actual eligibility may be endogenous, which would undermine the causal interpretation of the estimates (DiNardo and Lee, 2002; McCrary, 2007). If, for example, insurance were more valuable to less skilled farmers, they might not irrigate their land or might sell livestock so as to become eligible. If less skilled farmers differed from others with respect to input decisions or labor supply, this difference would mistakenly be attributed to insurance.

The quantity of land owned by a household, however, is not likely to be endogenously altered to become eligible for FONDEN aid. For one thing, owing to the peculiarities of the Mexican land titling system, many households are constrained in their ability to sell land. In particular, agricultural policy formed much of Mexico's agricultural land into *ejidos,* or state owned farms where individuals have use rights to land, but are not allowed to sell their plots. Section 1.8 details the legal constraints imposed on the sale of agricultural land. As these restrictions do not apply to all land, I further investigate whether households alter their production decisions on account of the FONDEN eligibility requirements. The results, also presented in Section 1.8, suggest that sorting into the program is of limited concern.

[7] 1 BN pesos / 3 years = 333,333,333 pesos per year / 1740 (indemnity for 5 hectare farm) = 191570.9 indemnities paid per year /(24280000 rural population / avg 5 members per HH) = 3.95% of HH's receiving indemnity

I make use of variation in exogenous eligibility criteria, own land holdings, to estimate the average effect of an "offer" of insurance to households which meet the land holdings eligibility requirement. Since the outcomes of interest also depend on the quantity of land held by the household I employ a regression discontinuity design, based on the discontinuous eligibility requirements, to identify the effect of the insurance offer on investment.

Letting $\widetilde{y}_i(0)$ and $\widetilde{y}_i(1)$ denote potential outcomes for a household when not offered insurance and offered insurance, respectively, the object of interest is the impact of the insurance offer on the outcome, or $E[\widetilde{y}_i(1)] - E[\widetilde{y}_i(0)]$. As only one outcome is observed, I adopt the standard regression discontinuity assumption that as land holdings, x, become arbitrarily close to the eligibility limit, c, that $\lim_{x \to c} E[\widetilde{y}_i | x_i < c] = \lim_{x \to c} E[\widetilde{y}_i | x_i > c]$, or that any difference in outcomes is attributable to the offer of insurance rather than differences in land holdings or other variables.

Given the limited sample size, to make use of observations away from the discontinuity, I further assume that conditional on smooth functions of land holdings, differences in outcomes between those with more land than the eligibility limit and less land are due to the offer of insurance. In practice I estimate the following equation

$$y_{ikj} = \alpha_o + \boldsymbol{\alpha}_1 \mathbf{X}_{ikj} + \boldsymbol{\alpha}_2 \mathbf{X}_{ij} + \lambda E_i + \nu_j + (\varepsilon_i + \varsigma_{ikj}) \tag{1.1}$$

where y measures investment in plot inputs, labor supply or migration, i indexes households, j indexes state or region and k indexes plots controlled by the household or household members, depending on the outcome of interest. $\mathbf{X}_{ij}$ is a vector of household characteristics, including the number of males 18 years or older, the number of females 18 years or older and the number of household members under 18 years old. $\mathbf{X}_{ij}$ also includes a fourth order polynomial of total land owned by the household, which flexibly controls for the direct, continuous effect of land holdings on the outcome. In Section 1.8, I also assess the robustness of the results to alternative control functions. $\mathbf{X}_{ikj}$ contains various plot or person level control variables thought to influence y. ν_j is a state fixed effect or an eligibility limit fixed effect, which corresponds to a region fixed effect. ε_i and ς_k are idiosyncratic error terms. E_i is an indicator variable which takes value 1 if the household owns less land than the eligibility limit for FONDEN assistance. Conditional on

smooth functions of land, λ measures the discontinuous change in the outcome for households owning more or less land than the eligibility limit. Interpreting $\widehat{\lambda}$ as the causal impact of the insurance offer on the outcome of interest requires that particular "types" of households do not endogenously alter their land holdings to receive the offer of natural disaster insurance. Section 1.8 provides evidence for the validity of this assumption.

In addition to the effect of the insurance offer, I employ an instrumental variables specification to estimate the impact of insurance on households who are actually eligible for FONDEN indemnities. The regression equation is

$$y_{ikj} = \alpha_o + \boldsymbol{\alpha}_1 \mathbf{X}_{ikj} + \boldsymbol{\alpha}_2 \mathbf{X}_{ij} + \gamma T_i + \nu_j + (\varepsilon_i + \varsigma_{ikj}) \tag{1.2}$$

where T_i, an indicator that the household is eligible for FONDEN indemnities according to all criteria, is instrumented with E_i. The estimate of γ corresponds to the local average treatment effect of insurance against natural disasters on households who remain eligible on account of owning less land than the limit (see Angrist and Imbens, 1994).

A formal derivation of these estimators and a discussion of the assumptions required to identify the effect of insurance on investment is provided in Appendix C.

1.4 Data and Descriptive Statistics

The data used in this study come from The National Household Survey of Rural Mexico (ENHRUM for its Spanish acronym[8]) conducted by the University of California, Davis and El Colegio de Mexico, Mexico City. The Household Survey component of ENHRUM provides a nationally representative sample of rural Mexican households. The survey was conducted between January and mid-March 2003 among households residing in rural communities of 500 to 2,499 people. Eighty villages, distributed over 14 of the 32 Mexican states, were covered by the survey.[9]

The data provides detailed demographic information about household members and a rich set of variables describing the characteristics of agricultural plots farmed by the household.

[8] La Encuesta Nacional a Hogares Rurales de México

[9] Appendix B shows the geographical distribution of these communities.

It also records the migratory experience of members and expenditures made on agricultural inputs. The reference period for these variables is January 1 to December 31, 2002 during which the FONDEN rules discussed previously apply.

To avoid making subjective decisions as to what land would fall under FONDEN's definition of productive land, the sample was restricted to households whose land holdings were entirely agricultural (88% of plots in the sample met this criteria). I also restricted to households which either owned, rented or borrowed each of their plots (only 5% of plots did not meet this requirement, being either sharecropped or having unspecified tenure). Due to problems of interpretability, I also dropped households which farmed land communally (representing 4.6% of plots). Unless there is endogenous alteration of land type and tenure in response to the disaster relief program, the sample is restricted according to covariates rather than outcomes, which should not bias the results, although it may limit how generally they can be interpreted. One observation appeared to be an outlier with respect to land holdings (having more than 8 times as much land as the next largest land holder) and was dropped.[10]

For each household in the restricted sample, I imputed various measures of labor supply and investment. The construction of these variables is detailed in the Variable Appendix. Table 1.1 present their means and standard deviations; for the entire sample and separately for those with rights to less land than the eligibility limit and those with more land. Assessing the statistical significance of differences in covariates between the two groups, as is done in the final column of the table, provides a rudimentary check of one implication of the identifying assumption; that eligible and ineligible households have similar characteristics.

Mechanically, households which are eligible for FONDEN assistance on the basis of their own land holdings have rights to substantially less land than other households. Also, as might be expected on account of having less land, they have fewer hectares of irrigated land and own less livestock. These differences are all statistically significant above a 5% confidence level. In terms of household demographics, the number of household members and the age and gender composition of members, eligible households appear no different from other households.

The figures in Panel B suggest that members of households with less land than the limit are not different from members of other households in terms of their demographic characteristics.

[10] The main results are substantively unchanged whether this observation is included or not.

Although members of households with land less than the limit are less likely to have professional or doctoral degrees and are slightly more likely to have no schooling, there is no statistical difference with respect to the two most common levels of schooling (primary and secondary). Moreover, there are no observable differences between the two groups in terms of age, gender or conjugal status.

With respect to plot characteristics, households with less own land than the limit are predictably more likely to have smaller plots and are less likely to have own property rights to the land. These households, however, are no more likely to possess ejido land, which restricts property rights. They appear no more isolated than other households in the sense that there is no statistical difference in the distance from their plot to the community center, nor is there a discernible difference with respect to when the household acquired the plot. While they do not appear to have more productive land when measured by the number of times per year the plot can be seeded, households with less land than the limit tend to have better quality land; they are 11% more likely to have flat, as opposed to inclined, land and are 8% more likely to have "good" quality soil. These later differences, however, may in fact be attributable to eligibility for FONDEN aid. Although not statistically significant, Table 1.1 suggests eligible households spend more on fertilizer, which may impact the quality of the soil.

While the figures in Table 1.1 indicate that, on average, households which are FONDEN eligible and other households are generally similar along many dimensions which should not be impacted by FONDEN eligibility, it would be troubling if these two groups were discontinuously different around the eligibility limit in terms of such characteristics. To assess this possibility, I plot histograms of these outcomes against land holdings to inspect for sharp changes across the eligibility discontinuity. Appendix Table A1.1 presents these figures, where points indicate the average value of the variable for households with the indicated quantity of land and the lines fit a fourth order polynomial of own land holdings and a dummy indicating that the household owns less land than the eligibility limit to the observed data. With respect to plot level characteristics (whether the plot is private land, distance of the plot to the community center, year the plot was acquired) the data are quite disbursed and do not indicate a clear break across the eligibility discontinuity. The same is true for individual level characteristics; the data do not reveal a discontinuous change in age, gender, marital status or education levels

across the eligibility limits.

In the empirical investigation which follows, I control for differences along observable dimensions. While I am unable to control for unobserved characteristics which influence the outcomes under consideration (which will make the estimates less precise), the finding that households with less land than the limit are generally similar to others is at least suggestive that differences in unobserved characteristics are not correlated with FONDEN eligibility.

1.5 Empirical Results

Table 1.1 also reveals that there are differences among eligible and ineligible households with respect to outcomes which are likely affected by insurance against disasters. For example, members of households which are FONDEN eligible according to their own land holdings are 4% more likely to migrate than members of ineligible households, a difference which is significant at the 5% confidence level. The simple difference in means between the groups, however, is not indicative that insurance alters these behaviors; it is entirely plausible that eligible households migrate more owing to the fact that they necessarily own less land than other households. Evidence that insurance against natural disasters influenced these choices would be provided by a discontinuous difference in these outcomes across the eligibility limit, rather than a difference in means on either side of the limit. A visual check for discontinuous differences is shown in Figure 1.1, which displays predicted values for various investment outcomes based on a regression of the outcome on a 4^{th} order polynomial of the household's land holdings and an indicator variable for this quantity being less than the eligibility limit. The graphs also indicate the average actual investment made by households, averaged over all households within a 0.25 hectare range of landholdings in states with a given eligibility limit. The vertical lines indicate the FONDEN eligibility cut-off points.

The first image, pertaining to total expenditure on plot inputs, is suggestive of a discontinuous change in total expenditure on farm investment across the FONDEN eligibility threshold. The data points, however, are scattered and do not conform closely to the predicted values. The next image, indicating hours of tractor use on plots, is also indicative of a shift across the discontinuity. Moreover the plotted data points are also consistent with the interpretation that

eligible households use tractors more intensively.

The remaining images present analogous representations of migration decisions. The images in the middle row hint at a discontinuous change across the eligibility limit in the average time spent as a migrant to the United States, particularly in states where the cut-off is 5 hectares. The lower images do not reveal such a pattern with respect to the average duration spent by members as migrants within Mexico; there is little indication of a discontinuous change around the eligibility limits.

While the local averages presented in Figure 1.1 provide evidence on the effect of FONDEN eligibility without having to make parametric assumptions about the relationship between land holdings and investment decisions, the drawback is that the figures neglect crucial determinates of these outcomes, such as the slope and soil quality of plots or the age and education of members. To account for these factors, I investigate the relationship between investment and eligibility by imposing a parametric regression model.

1.5.1 Agricultural Investment

In light of prior studies suggesting that households may limit their use of agricultural inputs or eschew risky technologies due to considerations of risk, I consider whether insurance against natural disasters alters those margins.

Firstly I impute total expenditure on plot inputs as a weighted index of days of human labor, hours of machine or animal use, quantities of fertilizer, pesticides, water and seeds, where the weights are sample average prices.[11] I sum the weighted inputs used until harvesting, omitting expenditures during harvest so that this measure reflects investment rather than the cost of production. This measure is imperfect in that it is noisy and tends to exacerbate the problem of outliers (since large quantities tend to be associated with smaller unit prices, using average prices inflates these figures). To address this issue, I look separately at direct, unweighted measures of investment, specifically the number of hours households use tractors and traction animals (the most commonly used capital inputs). I also consider an indicator variable for whether the household purchases improved seed varieties.

[11] Average prices were used to limit measurement issues steming from inputs given as gifts or paid for in kind, discounts for bulk purchases and price variability.

When estimating the effect of insurance on these outcomes I include a vector of plot level control variables including dummies indicating plot slope and soil quality, an indicator for whether the plot is part of an ejido, plot area and plot area squared. Standard errors are clustered at the household level.

Due to the limited variation in insurance status by state, I conduct the analysis by grouping states according to the eligibility limit in that state and using eligibility limit fixed effects, which roughly correspond to region fixed effects.[12] Although I lose the ability to control for state specific influences, I gain variation in the eligibility indicator. Table 1.3 presents the results. Panel A displays the estimated effect of the insurance offer and Panel B presents the instrumental variables results. The estimates are consistent with the notion that investment in agricultural inputs increases in response to eligibility for insurance; the coefficients when considering total expenditure are positive, but not statistically significant at the 10% confidence level.

While the expenditure differences are not significant, the positive coefficient when considering hours of tractor use and negative coefficient when considering hours of traction animal use, which are generally statistically different from zero at or above the 10% confidence level, suggests substitution towards more expensive capital inputs. The positive, and statistically significant, coefficient when considering use of improved seed varieties also accords with this notion. Moreover it suggests that insured households are more likely to adopt different technologies.

Since these results provide statistically compelling evidence that insured households shift towards more expensive capital inputs[13] it is puzzling that the results with respect to total plot investment remain statistically ambivalent. One possibility is that measurement error in the expenditure variables confounds the analysis; a power calculation suggests that the change in total investment would need to be 86% to be detected with 80% power.

While omitted in the interest of readability, the coefficients on the covariates[14] suggest that

[12] The results using state fixed efects, available on request, are qualitatively similar but less precise.

[13] The average price per kilo of improved seeds is 61.5 pesos, as opposed to 5.0 pesos for a kilo of local "Criolla" seeds, which are the most commonly used variety. The average rental price for an hour of tractor use is 2.8 times that of an hour of traction animal use before the seeding season, and 3.1 times the price of traction animals from seeding until harvest.

[14] Full results are available on request.

investment is less on poorer quality plots (steeper plots and plots with low quality soil). Also, as might be expected, the coefficient on plot area suggests greater input provision on larger plots.

1.6 Labor and Migration Results

As weather insurance may also impact labor allocation decisions, I test for differences with respect to employment off the family farm between members of FONDEN eligible households and others. In particular I consider days worked locally, in agriculture or other vocations, an indicator variable that the member migrates and the number of months spent as a migrant to the United States or other parts of Mexico. When considering these outcomes I condition on indicator variables for the member's sex and education level as well as the member's age and age squared. The regressions include eligibility limit fixed effects to account for regional differences and standard errors are clustered at the household level.

Columns 1 and 2 of Table 1.4 display results for the local labor outcomes. The results indicate that FONDEN eligible households tend to work fewer days as local agricultural laborers but the coefficients are not statistically different from zero above a 10% confidence level.[15]

Columns 3-5 provide evidence that insurance against natural disasters changes migration patterns. Members of households which can count on FONDEN indemnities are approximately 10% more likely to migrate, significant at the 5% confidence level. The difference in the propensity to migrate appears driven by U.S. migrations; the predicted duration of migration to the U.S. is about a month longer for members of insured households, a difference which is significant at the 1% confidence level, but there is no noticeable difference in the duration of migrations to other parts of Mexico.

While not reported in these tables, the coefficients on the individual covariates generally enter in predictable ways. Males tend to work for a wage more days per year, both as agricultural and non-agricultural laborers, and are more likely to migrate. In all specifications, age enters positively and significantly while its square enters with the predicted negative sign. On average, members with education beyond the primary level work more days locally in non-

[15] Conducting this analysis with state fixed efects results in quite similar estimates. Results are available on request.

agricultural professions and fewer days as agricultural laborers. The presence of additional adult male household members is associated with a higher probability that members migrate, while the presence of additional children is associated with a lower probability. Although not always statistically different from zero, the coefficient on the area of the household's land enters negatively, perhaps capturing an effect of higher wealth on labor supply.

To put the estimated impact of insurance on these outcomes in context, the point estimate from the instrumental variables specification is that members of insured household are 12% more likely to migrate, which is equal to the expected difference in the propensity to migrate between males and females and about three times the expected impact of having a secondary, relative to primary, education on the probability of migration. Similarly, the estimate that insurance increases the average duration of migration to the U.S. by 1.4 months is almost twice the difference in average length of migration to the U.S. between males and females.

Since the ENHRUM data records migration histories, I am able to provide additional evidence that the reported effects of insurance on international migration are in fact attributable to the FONDEN program. FONDEN did not exist until 1996, therefore I should see no impact of meeting the FONDEN eligibility requirements on migration decisions made prior to 1996, whereas I should observe an effect for migration decisions made thereafter. In Table 1.5 I assess whether there is any indication that meeting the FONDEN requirements (in 2002) affects migration in the 6 years prior to 1996 as opposed to the 6 years following 1996. In conducting this analysis, I restrict to individuals older than 16 years in 1990 (who could have potentially migrated before the introduction of FONDEN). The results (columns 1 and 2) show that members of eligible households were no more likely to have migrated to other parts of Mexico either before or after the inauguration of FONDEN. With respect to U.S. migration (columns 3 and 4) members of households which where eligible in 2002 were no more likely to have migrated to the U.S. in any of the 6 years prior to 1996, but were 10-15% more likely to have migrated to the U.S. in any year after 1996 (statistically significant at the 1% confidence level). In columns 5-8 I consider differences in the numbers of years the individuals migrate, rather than an indicator for having migrated in at least one year. These estimates suggest that members of FONDEN eligible households migrated to the U.S. about 0.5 more years in the 6 year period following 1996, but there is no detectable difference in the 6 years prior to 1996 or with respect to the

number of years spent as a migrant to other parts of Mexico. That owning less land than the eligibility limit is associated with increased migration only after the inauguration of FONDEN supports the contention that the program affected international migration decisions.

1.7 Discussion

The theory motivating this research is that investment is constrained by uninsured risk, in particular that rural households under-invest when faced with the substantial risk imposed by variable weather conditions. Such risk leads to distortions not only in rural enterprises but also in labor supply, including migration. Stark and Levhari (1982), for example, characterize migration as a mechanism employed by rural households to diversify the risk inherent in agricultural production. This representation, however, abstracts from the fact that migration is not a dichotomous choice. Individuals may choose from many potential migration locations, each characterized by a different risk, return profile. The factors involved in the choice of whether or not to migrate internationally, which may be a high risk, high expected return investment, are likely quite different from the factors involved in the choice of whether or not to migrate domestically. While wages in the U.S. may be attractive compared to Mexico, international migration is potentially fraught with risk (migrants may fail to successfully cross the border, they may have little information about job prospects in the U.S., and so on). Although the wages of domestic migrants are lower, they remain valuable from the perspective of diversification and are likely less risky (individuals may have better information on job opportunities within Mexico and domestic migration lacks the risks associated with illegality).

To provide some intuition for how this theory relates to agricultural investment decisions and the choice between international and domestic migration, imagine a credit-constrained household which farms a parcel of land and has one unit of surplus labor, perhaps an additional household member who is not needed to farm the small plots typical of subsistence rural households. To keep things simple, I suppose that this member can migrate locally, which is costless and delivers a certain wage w. Or the household can finance international migration, which requires a fixed cost of c and returns a risky wage. If the migration is successful, which occurs with probability α, the migrant earns $W >> w$. But with probability $\alpha^{-} \equiv (1 - \alpha)$,

the migrant is not successful and earns 0.

Additionally, the household can make a divisible investment, denoted by k, in agricultural production activities. Agricultural investment is subject to the risk of a natural disaster, which occurs with probability π and destroys the crop ($f(k) = 0$) Otherwise, with probability $\pi^- \equiv (1-\pi)$ agricultural investment yields $f(k)$. Finally, the household can save any amount, s, in a safe investment technology which provides a gross interest rate of 1.

The household's decision is how to allocate its available capital, x, among the investment opportunities. If international migration is not chosen then the household solves

$$\max_s \pi^- U\left(f\left(x-s\right)+w+s\right)+\pi U\left(w+s\right) \tag{1.3}$$

whereas if international migration is optimal the household solves

$$\max_s \ \pi\alpha U(f(x-s-c)+s+W)+\pi^-\alpha U(s+W)+\pi\alpha^- U(f(x-s-c)+s)+\pi^-\alpha^- U(s) \tag{1.4}$$

whichever program yields a higher expected utility determines the household's migration decision. Assuming that destitution is extremely costly, that $U(.)$ approaches $-\infty$ as the argument goes to zero, then it will never be optimal to invest such that $x-k=0$ or $x-k-c=0$, even if international migration is an attractive investment opportunity.

Consider a household which does not find financing international migration optimal (the value of (1.3) at the optimum is greater than the value of (1.4) at the optimum), the first order condition implies

$$\pi^- U'\left(f\left(x-s\right)+w+s\right)\left(f'-1\right)=\pi U'\left(w+s\right) \tag{1.5}$$

Suppose now that the household receives an indemnity, D, in case of a natural disaster. If not financing migration remains optimal, the first order condition is then

$$\left(1-\pi\right)U'\left(f\left(x-s\right)+w+s\right)\left(f'-1\right)=\pi U'\left(w+s+D\right) \tag{1.6}$$

Since insurance decreases the marginal utility of wealth in the state where a disaster occurs, a reduction of precautionary savings (equivalently an increase in agricultural investment) relative

to the no insurance world is required to satisfy the first order condition. A similar argument suggest that households which find financing an international migrant optimal whether insured or not will also increase agricultural investment when provided with insurance against natural disasters.

It is possible, however, that some households who initially do not finance migration find migration optimal when insured. In particular, if c represents a significant portion of the resources available for investment, financing migration may leave the household with very little in the event that migration does not pay off and a natural disaster strikes. In such a case the final term of (1.4) will dominate, leading the household to forgo financing international migration. Thus even if the expected returns to international migration are quite large, risk aversion prevents the household from making this investment. When the household is insured, the need to maintain savings in case unfavorable realizations occur is reduced and so long as the expected returns to migration are large relative to agricultural investment, the household will choose to send an international migrant. For this set of households investment in agricultural inputs may decrease with insurance as the household foregoes divisible agricultural investment to finance indivisible migration expenditures.

In this framework, the effect of insurance against natural disasters is a consequence of the risk inherent in the international migration investment. If the migrant's wages were certain, then households would invest in profitable international migration whether insured or not. Therefore, if this dynamic is at work, one would expect that insurance matters for migration undertaken as a risky investment, as migration to the U.S. may be, and less so for migration motivated by the need to diversify income, which is what the empirical results indicate. Moreover, it would be expected that the effect of insurance would vary depending on the degree of risk associated with the household's migration decision.

This model is only one of many which would generate these results; for example since the insurance is costless to eligible households income effects might also result in increased investment. In the following section I discuss the empirical implications of this model versus alternative models, and present empirical evidence consistent with the notion that FONDEN eligibility affects households' migration decisions through changes in the degree of risk faced by the household rather than through other mechanisms.

1.7.1 Empirical Evidence on the Role of Risk

Various studies note that the risk associated with international migration is a function of the potential migrants' social network in the receiving country (see e.g. Lauby and Stark, 1988). It is argued that prior migrants can facilitate new migrant's search for employment, navigate hurdles associated with illegal migration and otherwise reduce the costs and uncertainty associated with migration. Bastida (2001) provides evidence indicating that this dynamic is at work with respect to Mexico-U.S. migration.

To the extent that migrant networks proxy for the risk of the migration decision, I can test the implications of the model by assessing whether the estimated impact of FONDEN eligibility is larger in areas lacking extensive migrant networks (high migration risk areas) relative to high migration network areas (low risk areas).

Using data on the percentage of households in a given Mexican state with at least one international migrant (from Woodruff and Zenteno, 2001) I split the sample into "High Network States" (with above average percentages of households with international migrants) and "Low Network States" and conduct the analysis on the two sub-samples.

The results in Table 1.6 demonstrate that the estimated impact of eligibility for natural disaster insurance on migration decisions is driven by households residing in states with low migrant networks, who face a greater degree of risk in migration. Members of eligible households in these states are 10-15% more likely to migrate, statistically significant at a 5% confidence level. They also appear to opt for migration to the U.S. instead of less risky domestic migration. On average they spend 1-2 months longer as migrants to the U.S. (significant at a 1% confidence level) while the point estimates, which are not statistically different from zero, imply they work less as domestic migrants. The estimates do not reveal any statistically significant effect of eligibility for insurance on the migration decisions of individuals in high network (low risk) states.

1.7.2 Alternative Models

Although the results presented above are indicative that risk is a central determinate of how households respond to insurance against natural disasters, an alternative explanation, which would imply that the results have nothing to do with insurance *per se*, is that the observed

responses merely capture income effects. Since households do not pay a premium for the insurance, the increase in expected wealth generated by insurance could induce households to invest more in agricultural inputs or migration. Additionally, insurance might directly relax borrowing constraints, allowing insured households to borrow to finance migration or investment.

Using independent estimates of income effects on migration (from Angelucci, 2004), I can assess whether the income effect attributable to the implied value of the indemnities can plausibly explain the magnitude of the effects I estimate.

Angelucci assesses the impact of PROGRESSA, a direct income transfer program, on the propensity of households to send migrants. The results indicate that members of recipient households, which receive 4,176 pesos per year on average, are 0.42% more likely to migrate to the U.S. (or 66% of the sample average propensity to migrate).

The study goes on to consider the relationship between potential PROGRESSA grant size and migration. The analysis suggests that the increased propensity to migrate is driven by those households which receive a grant above the mean level. The estimated impact of PROGRESSA on migration outcomes for households with a potential grant less than the mean is zero.

The FONDEN program offers indemnities which represent a smaller income effect than PROGRESSA. The indemnity to a farmer of general crops is 1,740 pesos compared to the 4,176 pesos for PROGRESSA. Moreover, the indemnities must be discounted by the probability that the indemnity is actually received. A back of the envelope calculation suggests that about 4% of rural households received payments in a given year. Thus the expected indemnity is about 70 pesos per year.

Although Angelucci's results suggest that PROGRESSA does increase international migration, and may do so by relaxing credit constraints, the results also suggest that the magnitude of the income effect must be substantial to generate any response. Therefore, given the small expected value of FONDEN indemnities, it is unlikely that the relatively large estimated response to insurance is attributable to an income effect.

Another crucial difference between the FONDEN insurance program and PROGRESSA is that FONDEN is a state contingent transfer program; households are not given funds *ex ante*. It is possible that some households would use received indemnity payments to finance

migration *ex post*, but if eligibility for insurance induces migration through an income effect it must be that households borrow against their expected indemnities to finance migration. Appendix Table A1.1 shows that FONDEN eligible households are no more likely to report having financed migration by borrowing. Since money is fungible, in Table A1.2 I check whether eligible households are any more likely than ineligible households to have sought or received a loan from a bank, business or other individual. I find no indication that FONDEN eligible households borrow more than comparable ineligible households, which is inconsistent with the contention that the results are attributable to an income effect rather than an insurance effect.

1.8 Robustness Checks

Since the causal interpretation of the results is not valid if farmers actively sort into the FONDEN program, this section provides extensive evidence on the likelihood of such selection. I discuss institutional constraints on land sales, test for discontinuities in the land distribution around the eligibility limit and investigate whether FONDEN eligibility requirements, as opposed to eligibility itself, appear to change household behavior. I also consider other potential sources of bias and assess the robustness of the results to these concerns.

1.8.1 Endogenous Sorting into Eligibility

Mexican Land Reform

Although some farmers may find it optimal to sell a small amount of land to become qualified for FONDEN indemnities, there are exogenous factors operating in this context which make it difficult to do so. In particular, the institutional details of the Mexican agricultural land market hinder the sale of agricultural land. Mexico began a program of agricultural reform in 1917 which included taking large areas of land from wealthy land owners and forming *ejidos*, or communal farms. A portion of ejido land becomes collective property while the remainder is divided into individual parcels; some of which are farmed communally, but the vast majority of which are farmed by individual households. Households are granted private use rights to these plots, but the ownership of the land remains with the state and households are not empowered

to sell, lease, subdivide or mortgage the property. They are, however, allowed to pass use rights along to heirs.[16] The law further states that if ejido members attempt to sell or rent their land, their use rights to the plot may be revoked. In spite of these prohibitions, some illicit leasing of ejido land appears to have occurred, a land sale, however, is likely more difficult to arrange unlawfully.

In 1992, another agrarian reform act established a process by which ejido members could obtain formal title to their land. This process firstly requires that a majority of ejido members wish to initiate the formalization of land title. If this condition is met, the government begins a survey of each parcel and an inspection of ejido records. Additionally, ejido members must come together to decide which household is entitled to receive which plot and to resolve any boundary or ownership disputes between households. When an allocation decision has been reached the government issues certificates to households, but these certificates do not necessarily entail ownership. Often the household must go through another certification process in order to convert their plot into private property.[17]

In light of the extensiveness of the earlier agricultural reform (by the 1990s two-thirds of Mexican cropland had undergone reform[18]) it is not surprising that only 30% of plots in my sample are private tracts of land. Of the non-private plots, about half had undergone the privatization process. Although a substantial portion of the land in my sample can be sold, the complexities of land reform and institutional constraints on sales may have dampened the land market. Ideally I would want to know if households in my sample had sold any land after 1996, when FONDEN came into existence. Unfortunately I lack this data. I do, however, have information on the other side of the market. In the sample, only 16% of plots were acquired through purchase, inheritance being a much more common avenue of obtaining land. Fifteen percent of plots were acquired after 1996 and 3% were purchased after 1996. Without data on the quantity of land held before 1996, it is difficult to incontrovertibly establish that households did not sort into eligibility by altering land holdings, but these figures suggest that rural land markets were not especially active since 1996.

[16] http://www.mexicolaw.com/LawInfo02.htm; http://www.country-studies.com/mexico/rural-society.html
[17] http://www.mexicolaw.com.mx/ejido.html
[18] Heath, 1992.

Discontinuities in Land Holdings Distribution

By applying similar analysis to that used in Figure 1.1 it is possible to check whether households appear to alter their land holdings in order to become eligible for FONDEN aid. Absent this behavior one would expect the distribution of land holdings to be smooth across the eligibility limit, whereas if households sorted into eligibility one would expect a discontinuity at the cut-off point.

Following McCrary (2008) Figure 1.2 plots histograms of own land holdings and then uses a kernel regression to smooth the histogram. The points in the images represent the frequency with which households have own land holdings falling in a given range, which is selected using the automatic procedure in McCrary (2008). Using these frequency counts, non-parametric regressions fit curves to the data, separately on either side of the eligibility limit. The height at which the fitted curve reaches the eligibility discontinuity from the left represents the predicted density of land based on landholdings less than the limit while the corresponding height of the curve coming from the right estimates the density based on landholdings greater that the limit. Absent any endogenous selection, the predicted density should be equal when estimated from the left or right of the eligibility threshold. Based on visual inspection, the predicted densities at the eligibility limit do not appear exceptionally far apart for states where the limit is 5 or 10 hectares. In the 20 hectare limit states, there does appear to be a discontinuity in land holdings at the limit.

McCrary also suggests a test for whether the gap between the estimated densities is statistically different from zero. The top panel of Table 1.7 provides the estimated difference in densities and the standard deviation of the estimate. As suggested by the images, the predicted densities are not statistically different from one another in the 5 and 10 hectare limit states, but are in the 20 hectare limit states.

Further inspection of the histograms, and the data, reveals that there is a propensity for households to own integer quantities of land. Since the eligibility limits are also integers, this may contribute to the perception that there is a discontinuity at the limits. To assess this I conduct a counterfactual exercise, where I redefine the eligibility limit as 3 or 4 hectares for 5 hectare limit states, 5 or 8 hectares for 10 hectare limit states and 10 or 15 hectares for 20 limit states and carry out the analysis described above. Images pertaining to counterfactual tests of

discontinuities in the land distribution are shown in Figure 1.3 and the statistical significance of the estimated discontinuities is given in the lower panels of Table 1.7. The images and estimates suggest that when using counterfactual integer eligibility limits, the statistical significance of the estimated gap in the density is similar to when the actual eligibility limits are used. That the distribution of land appears no more discontinuous at other integers as it does at the actual eligibility limits mitigates concerns about endogenous selection into eligibility.

Another way to check whether the mass of households at the limit is purposeful or due to an "integer effect" is to make comparisons across states where there is an incentive to target a given quantity of land and states where there is not. In Table 1.8 I regress an indicator for the household owning exactly 5 hectares of land on an indicator for the household residing in a state where the eligibility limit is 5 hectares. The coefficient is quite small and not statistically different from zero, indicating that households which have an incentive to sell land until their holding are 5 hectares are not more likely to own 5 hectares than households which have no such incentive.

Other Eligibility Criteria

In addition to selling land, households can alter FONDEN eligibility status by not irrigating land, not acquiring livestock above the limit and not renting in more land than the limit. Since these margins are potentially easier to manipulate than the quantity of land owned by the household, eligibility according to the quantity of land the household owns forms the basis of my identification strategy. I am, however, also able to look for evidence that households alter their rental, irrigation and livestock decisions in order to meet FONDEN's requirements.

Since FONDEN only makes indemnity payments on non-irrigated land, the program provides an incentive for farmers who would otherwise be eligible not to irrigate their land. To see whether households respond to this incentive I regress the percent of irrigated land farmed by the household on a 4^{th} order polynomial in of the quantity of land owned by the household and an indicator variable which takes value 1 if the household owns less land than the limit in that state. Columns 1 and 2 of Table 1.9 displays these results, with and without eligibility limit fixed effects. The point estimates, which are not statistically significant, indicate that households which meet the own land holdings eligibility requirement have a higher percentage of

irrigated land, the opposite of what would be expected if households altered irrigation decisions on account of the FONDEN program.

Columns 3 and 4 of Table 1.9 show results from a similar check of whether households target livestock holdings below the limit. These estimates come from a regression of an indicator for the household having more than 25 animal units on the land holdings variables. The estimates do not suggest that households which own less land than the eligibility limit are discontinuously less likely to have more than 25 animal units.

Finally I look at households' land rental decisions. If households avoid renting in land beyond the eligibility limit then estimates of the impact of FONDEN eligibility on investment decisions could be biased. For example, if certain farmers wished to expand their operations by renting in more land but did not do so in order to remain eligible for FONDEN, they might farm their existing quantity of land more intensively instead. This effect would not be due to the risk reduction attributable to the program but to the distortion of land acquisition decisions induced by the program requirements. If farmers did wish to rent in more land, but did not do so to keep the quantity of land they farmed under the limit, I would expect to see them rent in land exactly up to the limit. As an initial check of whether this dynamic is at work, the top panel of Figure 1.4 plots the quantity of land households rent or borrow in as a function of how much land they could rent or borrow in without becoming ineligible for FONDEN aid. If farmers targeted the eligibility limit, one would expect to see this plot conform to the 45 degree line. In fact, most households rent in much less land than they could while remaining eligible for FONDEN. The lower panel plots the average quantity of land rented or borrowed in by households against the gap between the eligibility limit they face and the quantity of land they own. There is a trend towards renting or borrowing in more land as the gap increases, which may be due to these households mechanically owning less land, but the average is far short of the maximum amount that could be rented in without becoming ineligible.

As a further test, Table 1.10 shows a regression of the quantity of land rented or borrowed on how much the household could rent or borrow without becoming ineligible for FONDEN, as well as smooth functions of land owned by the household which are also thought to influence rental decisions. The variation exploited in these regressions is between two households with the same quantity of land owned but facing different eligibility requirements. If farmers did rent

in land until they were just under the limit, I would expect a coefficient of 1 on the quantity of land they could rent in without becoming ineligible. The coefficient suggests that households that can rent in more land without becoming ineligible for FONDEN aid do so (about 0.07 hectares for each additional 1 hectare they could rent in without passing the limit). An F-test that this coefficient is equal to 1, however, decisively rejects the null hypothesis.

1.8.2 Specification Checks

Given the limitations of the data analyzed in this study, identification of the effect of insurance on investment decisions rests upon the assumptions enumerated in Section 1.3. The evidence above, that households do not manipulate own land holdings on account of the incentives generated by the FONDEN program, suggests that the exogeneity assumption holds. Another assumption which must be examined, however, is whether inclusion of smooth functions of land holdings adequately controls for the effect of land on outcomes.

Adopting alternative control functions shows that the results are not artefacts of the specific control function employed. When using a 2^{nd} order polynomial of land or including sin and cosine terms[19] similar results are obtained. The point estimates are different and significance levels change slightly in some instances but the differences are not extreme (see Appendix Tables A1.3-A1.6).

Another check on the adequacy of the control function is to restrict the sample to households near the eligibility discontinuity. Presuming that own land holdings will have similar impacts on outcomes for households with land holdings slightly greater and slightly less that the eligibility limit obviates the need to control parametrically for the direct effect of own land holdings. In practice, due to limited observations near the eligibility discontinuities, I restrict to a sample within either 1 or 2 hectares of the limit and continue to condition on smooth functions of own land holdings.

The estimated impact of natural disaster insurance on farm investment are not generally robust to these sample restrictions (see Table 1.11). In some cases the estimated coefficients have the opposite sign as in the full sample specification. The estimates are very imprecise, however, owing to the exceptionally small number of observations in the restricted samples (91

[19] The specifc control funtion or own land, x_i, is $f(x_i) = x_i + x_i^2 + \sin(x_i) + \cos(x_i) + \sin(2x_i) + \cos(2x_i)$

when restricting to within 1 hectare, 173 when restricting to within 2 hectares). Since the function describing the relationship between land and outcomes should be approximated with a less flexible functional form when considering a localized range near the eligibility discontinuity, I also perform these robustness checks using a 2^{nd} order polynomial of own land as the control function. In this case the estimates more closely resemble the full specification results, but remain exceedingly imprecise.

The results pertaining to migration (Table 1.12), however, remain generally stable as the sample is narrowed in on the eligibility discontinuity. When restricting to households owning a quantity of land within one hectare of the eligibility limit, the signs and magnitudes of the coefficients when taking the migration indicator or the number of months worked as a migrant to the U.S. as the dependent variable are very similar to the full sample results (although the estimates are not statistically distinguishable from zero in this smaller sample of 283 observations). When restricting to the within 2 hectare sample, these coefficients are not only similar in sign and magnitude but also enter the regression at or above the 10% confidence level.

1.8.3 Other Insurance and Assistance Programs

It must be noted that the FONDEN program does not exist in isolation, there are various other sources of insurance for Mexican farmers as well as other development projects which target similar populations as the one eligible for FONDEN indemnities. If access to other sources of insurance or eligibility for other programs changed discontinuously at the FONDEN cutoff, any observed behavioral shift could not properly be attributed to the program.

In 2002, outside of government programs, the main sources of insurance available to agricultural producers were private insurance and mutual insurance organizations, know as *Fondos*. According to Zorrilla (2006 and personal interview) access to private insurance contracts is limited to very large companies and producers, thus it is unlikely that farmers in my sample contract private insurance. Zorrilla also suggests that farmers without a regularly profitable enterprise cannot afford Fondos' premiums. Based on his fieldwork, he notes that "Fondos is an unused instrument among middle- and low income farmers." It is therefore unlikely that farmers below the eligibility limit would be discontinuously more likely to belong to Fondos. Indeed the creation of the FONDEN program itself suggests that there is a lack of insurance

available to this population.

In addition to FONDEN, the Mexican government operates various programs to assist the rural sector. PROCAMPO, a prominent such program, provides cash transfers to farmers but the eligibility requirements for this program are not directly parallel to those for FONDEN. There is no threshold level of landholdings which makes a farmer ineligible nor is aid dependent on the irrigation status of the land.[20] Due to the difference in eligibility requirements, the existence of programs such as PROCAMPO does not compromise the causal interpretation of the results. *Alianza Para El Campo* (Alliance for the Country or APC), however, does use total land holdings as an eligibility requirement. This program provides subsidies (in the form of matching funds) to agricultural producers wishing to make productive investments as well as offering technical support services to farmers. While there is some overlap, the eligibility limits are not exactly those defining access to FONDEN aid. Inspection of APC regulations suggests that many programs are available to all producers with less than 30 hectares, while others are aimed at agricultural producers with less than 10 hectares in irrigated plots or less than 20 hectares of non-irrigated land. To address the concern that changes in household behavior are attributable to APC assistance rather than FONDEN eligibility, the 5^{th} and 6^{th} panels of Table 1.11 replicate the analysis on a restricted sample comprising only those households which are eligible for APC, exploiting variation in FONDEN eligibility within the subsample.

In this sample, the estimated impact of FONDEN eligibility on total farm input expenditure is positive and statistically significant at the 10% confidence level. As in the full sample results, eligible households appear to use tractors more intensively, an estimate which is also significant at the 10% confidence level. Although the standard errors are larger in this smaller sample, the signs and magnitudes of the coefficients when considering the other outcomes, hours of traction animal use and the indicator for purchase of improved seed varieties, are quite similar to the full sample specification.

Table 1.12 applies the same robustness check to the labor and migration outcomes. The estimates pertaining to labor outcomes are somewhat changed, and not statistically different from zero, whereas the estimates when considering migration outcomes are congruous with the full sample results.

[20] http://www.aserca.gob.mx/artman/publish/article_183.asp

1.8.4 Confounding Program Details

It is possible that the way the program was implemented or the context in which it operated would affect the analysis and interpretation discussed above. For example, while certain producers may be statutorily ineligible for FONDEN, it is possible that the officials granting the payments do not enforce the cutoff. The flip side of this concern is that eligible producers may not have know about the program. Or, even if farmers were aware of the program, they may have discounted the benefits given that indemnities often arrived with considerable delay.[21]

In terms of the implications of these issues for this study, the effect would be to make it more difficult to detect any impact of insurance on labor and investment decisions. For example, if the limits were not enforced then producers on both sides of the eligibility limit would behave as though insured against natural disasters. Or, if farmers were unaware of the program, then this insurance would not be expected to impact choices. Similarly, if some farmers discounted the insurance due to delays, any behavioral change by other farmers would have to be much larger in order to find statistically significant differences between insured and uninsured households.

Another concern is that the procedure by which indemnities are distributed opens up the possibility that local politics may affect aid dispersal. This could complicate the interpretation of the results, although it does not invalidate them. If politically "connected" farmers are the only ones able to obtain indemnities, the estimated impact of FONDEN eligibility can not be interpreted as a general response but as the average response among politically connected farmers. On the other hand, if only politically connected farmers get indemnities, then there should be no response to insurance eligibility by unconnected farmers, making it less likely that I would detect an average effect.

Issues of moral hazard might also complicate the interpretation of the results, but in this context the opportunities for individual moral hazard are limited. To receive FONDEN indemnities, it must be apparent that the household's production was impacted by the disaster, but since the magnitude of the indemnity does not depend on the extent of the damage, it is still in the farmers interest to end up with as high an output as possible in all states of the

[21] An external evaluation done by the Universidad Autónomia del Estado de México (http://www.sagarpa.gob.mx/fapracc/files/informe_final.pdf) documents delays at various stages of the aid dispersal process which resulted in beneficiaries obtaining relief payments much later than the date of the disaster. In extreme cases, indemnities did not reach producers until over a year after the event.

world. A related concern is that there may be a form of moral hazard related to crop choice if farmers shift to planting certain crops on account of the crop-specific disaster definitions. But since municipal authorities have the flexibility to grant indemnities to producers of any crop they deem damaged by the disaster (conditional on an official declaration of disaster and the producer meeting the eligibility requirements) the effect of the program on crop choice is likely limited.

Finally, there is the issue of the general moral hazard effects of insurance. It is possible that insurance allows farmers to engage in risk shifting; perhaps switching to socially inefficient crops or techniques which offer exceptionally high returns in the good state since they know they will be insured in the bad state. In such a case, it would not be correct to say that the insurance is efficiency enhancing but the results would still show a causal impact of insurance on investment decisions.

1.8.5 Additional Robustness Checks

An additional robustness check is to see whether counterfactual regression discontinuity designs suggest statistically significant "effects." To that end, I re-estimate the main results (in Appendix Tables A1.7 and A1.8) including an indicator for the household owning own land less than a counterfactual eligibility limit rather than the actual eligibility limit. I do this for 4 counterfactual limits (the actual limit minus 1, 2, 3 or 4). If the results are not artifactual, I would not expect to see any "effects" from satisfying the counterfactual eligibility requirements. For the most part that is the case.

When hours of traction animal use is the dependent variable I observe statistically significant and negative "effects" for some counterfactual eligibility limits. But the estimated "effect" is not statistically different from zero for any of the other agricultural investment outcomes when using any of the counterfactual eligibility limits.

With respect to the labor supply results, there are a few instances of estimated "effects" on the estimated days that household members work as non-agricultural and agricultural laborers. When the migration indicator or months of migration to Mexico is the dependent variable the coefficient on the falsified eligibility indicator is estimated to be statistically different from zero at the 10% confidence level for a few counterfactual limits, but never above a 5% confidence

level. In none of the instance is the estimated coefficient statistically different from zero when considering the number of months spent by household members as migrants to the U.S.

Since insurance is only valuable in the presence of risk, if the results do in fact reflect a response to weather insurance, the estimated impact should be larger for households which face especially large weather risks. Using historical data from the EM DAT emergency events database[22] I estimate the frequency with which natural disasters occur in any given Mexican state. I then split the sample into states with above average disaster risk and below average disaster risk and conduct the analysis separately for each subsample. These results are presented in Appendix Tables A1.9 and A1.10.

When considering the farm investment outcomes, the coefficients are not estimated precisely in the smaller samples; none of the estimates are distinguishable from zero. The estimated impact of FONDEN insurance on international migration, however, is driven by decisions of households residing in high disaster risk states, as would be expected if these results were attributable to insurance. The coefficients estimated from the high risk state subsample are nearly double the magnitude of those estimated from the low risk states.

1.9 Conclusion

That households engaged in subsistence agriculture make resource allocation decisions, some of which may be inefficient, to limit their exposure to uninsurable weather risk is a phenomenon which has been documented by prior research. This study offers causal evidence that insuring households against the risk to their livelihoods posed by natural disasters affects investment choices. In particular, I assess how eligibility for a Mexican government program, which provides indemnities to farmers in case of a natural disaster, affects investment in agricultural production and the employment decisions of household members.

While the empirical results do not conclusively demonstrate that insurance causes households to make greater expenditures on their agricultural activities, there is evidence that insurance induces a shift to more expensive forms of capital. Eligible households use tractors more intensively, relying less on traction animals, and are more likely to have invested in improved

[22] http://www.emdat.be/

seed varieties rather than local varieties.

The strongest results concern the effect of insurance on member's migration decisions. Members of insured households are around 10% more likely to migrate than comparable individuals belonging to uninsured households. Notably, the difference appears driven by migration to the United States, as is indicated by the significant difference in the average duration of migration to the U.S. between members of insured and uninsured households. These results are consistent with a model where insurance allows households to take on the risk of staking an international migrant. Additional empirical evidence, suggesting that insurance has a greater effect when international migration is riskier, corroborates this intuition.

In addition to confirming that consideration of risk does affect investment decisions, this study has broader implications for development policy. The results provide suggestive evidence that insured households are more likely adopt different technologies, such as improved seed varieties. It is also shown that insuring the household facilitates international migration, perhaps leading to increased remittances as members travel further for higher wages. Therefore, providing insurance to rural households in developing countries, either through government programs such as this one or through other channels, not only has the direct value of increased consumption smoothing, but may also serve development objectives as insured households are better able to allocate resources and improve their economic situation.

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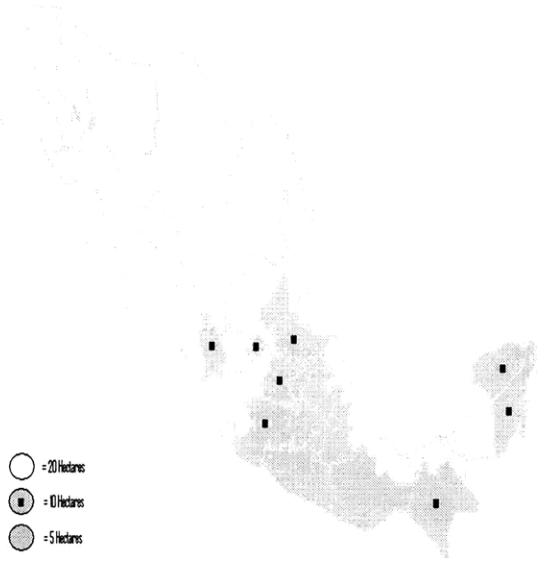
Appendix A: Distribution of FONDEN Eligibility Limits

- 20 Hectare limit states: Baja California, Baja California Sur, Campeche, Coahuila, Colima, Chihuahua, Durango, Jalisco, Nuevo León, Sinaloa, Sonora, Tabasco, Tamaulipas, Veracruz, Zacatecas
- 10 Hectare limit states: Aguascalientes, Chiapas, Guanajuato, Michoacán, Nayarit, Quintana Roo, San Luis Potosí, Yucatán
- 5 Hectare limit states: Distrito Federal, Guerrero, Hidalgo, México, Morelos, Oaxaca, Puebla, Querétaro, Tlaxcala



Appendix B: Geography of ENHRUM Sample

Source: http://precesam.colmex.mx/ENHRUM/PAG%20PRIN_ENHRUM_.htm

Appendix C: Empirical Specification

Let x_i denote the quantity of land owned by a household and c_i the state-specific eligibility requirement faced by the household.

Further let E_i be an indicator which takes value 1 if $x_i \leq c_i$. E_i can be thought of as an indicator that the household was offered disaster insurance (conditional on them complying with the additional eligibility criteria).

Following Hahn, Todd and Van der Klaauw (2001), let $\widetilde{y}_i(0)$ and $\widetilde{y}_i(1)$ denote potential outcomes for a household when not offered insurance and offered insurance, respectively. The object of interest is

$$\widetilde{\delta}_i = \widetilde{y}_i(1) - \widetilde{y}_i(0) \tag{1.7}$$

Assumption 1

Assume that $E[\widetilde{y}_i(1)|x_i = x]$ *and* $E[\widetilde{y}_i(0)|x_i = x]$ *are independent of* E_i.

This assumption implies that conditional on the household's land holdings, knowing if the household was "offered" insurance is uninformative about their potential outcomes when offered insurance or not.

As it will generally be the case that $\widetilde{y}_i(1)$ and $\widetilde{y}_i(0)$ depend directly on x_i assume

Assumption 2

Assume that $E[\widetilde{y}_i(1)|x_i = x]$ *and* $E[\widetilde{y}_i(0)|x_i = x]$ *are continuous in* x.

As shown in Hahn, Todd and Van der Klaauw (2001), under Assumptions 1 and 2 the average effect of the insurance offer on y_i can be consistently estimated as

$$\widetilde{\delta} = E\left[\widetilde{\delta}_i | x_i = c_i\right] = \lim_{x \uparrow c_i} E\left[\widetilde{y}_i | x_i = c_i\right] - \lim_{x \downarrow c_i} E\left[\widetilde{y}_i | x_i = c_i\right] \tag{1.8}$$

provided the limits exist.

In practice, due to limited observations near the eligibility discontinuities, the limits in (1.8) would be imprecisely estimated and more structure is required to estimate the effect of the insurance offer.

Assume that the observed outcome y_i is determined according to the following additive model

$$y_i = \beta_i g(Z_i, x_i) + \lambda_i E_i + \varepsilon_i \tag{1.9}$$

where Z_i is a vector of exogenous covariates and ε_i is a mean zero idiosyncratic error term. In addition to Assumption 1 and 2, further assume

Assumption 3

The effects of Z_i and x_i on $\widetilde{y}_i$ are constant across households, $\beta_i = \beta$.

Note that by Assumption 2 $g(Z_i, x_i)$ is a continuous function of x_i.

The average effect of the insurance offer is the difference in expected outcomes for households offered insurance less the expected outcome when not offered insurance

$$E[\widetilde{y}_i(1)|x_i = x, Z_i = Z] - E[\widetilde{y}_i(0)|x_i = x, Z_i = Z] \tag{1.10}$$

By (1.9) and Assumption 3 this is equal to

$$E[\beta g(Z_i, x_i) + \lambda_i E_i|x_i = x, Z_i = Z, E_i = 1] - E[\beta g(Z_i, x_i) + \lambda_i E_i|x_i = x, Z_i = Z, E_i = 0]$$

Assumption 1 implies that

$$\beta E[g(Z_i, x_i)|x_i = x, Z_i = Z, E_i = 1] = \beta E[g(Z_i, x_i)|x_i = x, Z_i = Z, E_i = 0]$$

or that the expected direct effect of land holdings on the outcome is the same if the eligibility limit is above or below the household's level of landholdings.

Thus (1.10) is equal to $E[\lambda_i|x_i = x, Z_i = Z]$.

Consider the regression equation

$$y_i = \beta g(Z_i, x_i) + \lambda E_i + \varepsilon_i \tag{1.11}$$

By Assumption 3, β can be consistently estimated as $\widehat{\beta}$ with cross sectional data. Assumption 2 (continuity) ensures that E_i is not colinear with functions of x_i in $g(Z_i, x_i)$ and, therefore, β and λ are empirically distinguishable.[23]

The estimate of λ, $\widehat{\lambda}$, is given by

[23] A weaker assumption of continuity near c_i is sufficient. Since there is not any particular reason to think the outcomes will be discontinuous in x_i I make the broader assumption and in practice control for smooth functions of x_i, omitting discontinuous functions of x_i away from c_i.

$$\widehat{\lambda} = \left[\frac{\sum_i (y_i - \widehat{\beta} g(Z_i, x_i)) E_i}{\sum_i E_i}\right] = \left[\frac{\sum_i (y_i - \widehat{\beta} g(Z_i, x_i)) E_i}{N}\right] \left[\frac{\sum_i E_i}{N}\right]^{-1} \tag{1.12}$$

which is the sample analogue to

$$\frac{E\left[y_i - \widehat{\beta} g(Z_i, x_i)\right]}{\Pr(E_i = 1)} \tag{1.13}$$

It is straightforward to show that the above expression is equal to $E[\lambda_i]$.

Therefore the estimate obtained from (1.11) estimates the average effect of the "offer" of insurance on the outcome.[24] In the empirical results, I adopt the particular form for $g(Z_i, x_i)$ as specified in (1.1).

To estimate the effect of insurance on insured households, rather than the average effect of the "offer" of insurance, I employ a methodology which parallels Angrist and Lavy (1999). In particular, I instrument for insurance eligibility with the "offer" of insurance, made on the basis of land holdings.

Let T_i denote treatment status (eligibility for FONDEN aid) with $T_i = 1$ if the household is insured and $T_i = 0$ if the household is not insured.

Again assume an additive model

$$y_i = \beta_i g(, Z_i, x_i) + \gamma_i T_i + \varepsilon_i \tag{1.14}$$

Although I present evidence that own land, x_i, can be taken as exogenous, actual FONDEN eligibility depends not only on x_i but also on irrigation status and livestock holdings. It is possible that households will alter irrigation and livestock decisions on account of the incentives generated by the FONDEN program. Moreover, it may be that the unobservable factors, u_i, which determine whether a household self-selects into eligibility also impact the investment outcomes of interest.

Suppose

[24] Note this is the estimate of the average effect of the insurance offer across households estimated at the eligibility cut-off that they face.

$$T_i = \pi_o + \pi_1 E_i + \pi_2 g(Z_i, x_i) + u_i \tag{1.15}$$

Since y_i likely depends on u_i estimating (1.14) by OLS fails since u_i, and by extension T_i, is correlated with ε_i.

Under certain conditions, however, the parameter of interest can be recovered. Assume that E_i is independent of u_i (that there is no manipulation of own land holdings). Also assume Assumptions 1 and 3 (applied to actual insurance status rather than the offer of insurance). Finally assume that owning less land than the eligibility limit makes all households more likely to satisfy all criteria for FONDEN eligibility (monotonicity).

Under these assumptions any direct effect of land holdings on outcomes is controlled for by the inclusion of smooth functions of land holdings in $g(Z_i, x_i)$. Thus we can think of partialing these effects out of (1.14). The first stage equation (1.15) partials out any direct effects of land holdings on treatment status. Therefore the residual variation in treatment status is due to the discontinuous land eligibility requirement. Under the assumption that, conditional on smooth functions of land holdings, owning less land than the eligibility limit affects outcomes only through its effect on insurance eligibility, then any correlation in the residual outcome and the residual treatment status is attributable to the effect of insurance on outcomes.

Estimating (1.14) via two stage least squares, where E_i is used for an instrument for T_i, yields the local average treatment effect ($\widehat{\gamma}$) of insurance against natural disasters on y for households who remain eligible on account of owning less land than the limit (see Angrist and Imbens, 1994).

Variable Appendix

(Alphanumeric codes indicate question numbers from ENHRUM dataset)

Household Level Variables

FONDEN eligible (0-1 Indicator)

Indicator variable taking value 1 if household meets all criteria for FONDEN eligibility: owns (classified according to question p3_1) fewer hectares of land than state specific eligibility limit, not all land is irrigated and *Animal units at start of 2002* is less than or equal to 25.

FONDEN eligible according only to land criteria (0-1 Indicator)

Indicator variable taking value 1 if household owns (classified according to question p3_1) fewer hectares of land than state specific eligibility limit.

Total HH members

Total number of household members enumerated in ENHRUM dataset (2 Sociodemografia file).

Number of males 18 or older

Number of male household members 18 or more years old enumerated in ENHRUM dataset (2 Sociodemografia file).

Number of females 18 or older

Number of female household members 18 or more years old enumerated in ENHRUM dataset (2 Sociodemografia file).

Number of HH members younger than 18

Number of household members younger than 18 years old enumerated in ENHRUM dataset (2 Sociodemografia file).

Total land with own rights (hectares)

Total hectares of land to which the household has own property rights (classified according to question p3_1).

Total irrigated land controlled by household (hectares)

Total hectares of land to which the household has own property rights which is irrigated (classified according to question p3_1 and p3_4_1).

Animal units at start of 2002

Number of specific types of animals owned at beginning of 2002 (g5_4) scaled into "animal units" as defined by FONDEN rules, then summed over all animal types to form total animal units at beginning of 2002.

Individual Level Variables

Sex (dummy for male)

Indicator variable taking value 1 if household member is male (s2_5).

Age

Age of household member in years (s2_5_1).

Married (0-1 indicator)

Indicator variable taking value 1 if household member is married (s2_5_2).

Education: Primary

Indicator variable taking value 1 if household member is has partial or complete primary education (hf2_02b).

Education: Secondary

Indicator variable taking value 1 if household member is has partial or complete secondary education (hf2_02b).

Education: Preparatory

Indicator variable taking value 1 if household member is has partial or complete preparatory education (hf2_02b).

Education: Technical

Indicator variable taking value 1 if household member is has partial or complete technical education (hf2_02b).

Education: Professional

Indicator variable taking value 1 if household member is has partial or complete professional education (hf2_02b).

Education: Preschool

Indicator variable taking value 1 if household member is has partial or complete preschool education (hf2_02b).

Education: Doctorate

Indicator variable taking value 1 if household member is has partially or complete doctoral education (hf2_02b).

Education: No school

Indicator variable taking value 1 if household member is has no schooling (hf2_02b).

Member migrates to US or Mexico (0-1 Indicator)

Indicator variable taking value 1 if *Months worked as migrant in US* or *Months worked as migrant in Mexico* is greater than 0.

Months worked as migrant in US

Months worked by member as a migrant to the U.S. (sum of indicators te_"month" that member worked in the U.S. during "month").

Months worked as migrant in Mexico

Months worked by member as a migrant to other parts of Mexico (sum of indicators tm_"month" that member worked in Mexico during "month").

Estimated days agricultural labor (total)

For each household member, the number of days they worked as causal agricultural laborers was estimated as 30*Number of complete months worked (tc2_31b1) + 22*Number of months worked more than 15 days (tc2_31b2) + 7*Number of months worked less than 15 days (tc2_31b3). Estimated days worked as regular agricultural laborers was estimated as reported months worked (sum of indicators tcj_"month") times number of days worked per week (tc2_31a1)*4.3(weeks/month). Total days is the sum of estimated days as casual and regular agricultural laborers.

Estimated days non-agricultural labor (total)

For each household member, the number of days they worked as causal non-agricultural laborers was estimated as 30*Number of complete months worked (tf2_35b1)+ 22*Number of months worked more than 15 days (tf2_35b2) + 7*Number of months worked less than 15 days(tf2_35b3). Estimated days worked as regular non-agricultural laborers was estimated as reported months worked (sum of indicators tfp_"month") times number of days worked per week (tf2_35a1)*4.3(weeks/month). Total days is the sum of estimated days as casual and regular non-agricultural laborers.

Plot Level Variables

Area of plot (hectares)

Area of plot in hectares (p3_2_5).

Household has rights to plot (0-1 Indicator)

Indicator variable taking value 1 if household has own property rights over the plot (p3_1=1).

Non-ejidal or communal land (0-1 Indicator)

Indicator variable taking value 1 if the type of land tenure of the plot is private land (i.e. plot is not part of an ejido or communal farm; p3_2_1=1).

Number of times plot can be seeded

Number of times per year plot can be seeded (p3_4_2).

Time to walk from community center to plot (minutes)

Time, in minutes, to walk from plot to center of the community (p3_2_8).

Year plot was acquired

Year plot was acquired (p3_2_4).

Inclined plot

Indicator variable taking value 1 if plot is inclined (p3_2_6=2).

Very inclined plot

Indicator variable taking value 1 if plot is very inclined (p3_2_6=3).

Flat plot

Indicator variable taking value 1 if plot is flat (p3_2_6=1).

Plot quality: poor ('malo')

Indicator variable taking value 1 if soil quality is poor (p3_2_7=M).

Plot quality: regular ('regular')

Indicator variable taking value 1 if soil quality is regular (p3_2_7=R).

Plot quality: good ('bueno')

Indicator variable taking value 1 if soil quality is good (p3_2_7=B).

Irrigated plot (0-1 Indicator)

Indicator variable taking value 1 if plot is irrigated (p3_4_1=1).

Imputed expenditure on plot inputs (until harvest)

Sum of *Imputed expenditure on plot capital inputs (until harvest)*, *Imputed expenditure on plot labor inputs (until harvest)*, *Expense on pesticides (this plot)*, *Expense on fertilizer (this plot)*, expenditure on seeds (sum of quantity of each type of seed used times sample average price for that type) and expenditure on water (sum of p3R_1 and p3R_2).

Imputed expenditure on plot capital inputs (until harvest)

For each type of machine listed, an hourly rental price was computed as the average of (price paid by household which rented the item/hours used). An imputed expenditure on that item (for owners and renters) was calculated as the estimated hourly rental rate multiplied by the hours the machine was used. Total expenditure for each season was calculated separately as the sum of imputed expenditure on each category of machine used in the specified period. *Imputed expenditure on plot capital inputs (until harvest)* is the sum of expenditure on machinery (excluding the harvest season), *Expense on pesticides (this plot)*, *Expense on fertilizer (this plot)*, expenditure on seeds (sum of quantity of each type of seed used times sample average price for that type) and expenditure on water (sum of p3R_1 and p3R_2).

Imputed expenditure on plot labor inputs (until harvest)

For each season, the daily agricultural wage in that season was estimated as the average of (payments to hired workers/days worked by hired workers). For each season, expenditure on labor was estimated as the number of days worked by household members plus number of days worked by hired workers, multiplied by the estimated daily agricultural wage. *Imputed expenditure on plot labor inputs (until harvest)* is the sum of estimated labor expenditure excluding the harvest season.

Hours of tractor use (until harvest)

Hours of tractor use for each season is reported by households. If multiple machines were grouped together, hours of use was estimated as total hours divided by the number of items listed. *Hours of tractor use (until harvest)* is the sum of tractor hours used in each season, excluding the harvest season.

Hours of traction animal use (until harvest)

Hours of traction animal use for each season is reported by households. If multiple machines were grouped together, hours of use was estimated as total hours divided by the number of items listed. *Hours of traction animal use (until harvest)* is the sum of traction animal hours used in each season, excluding the harvest season.

Uses improved seeds (0-1 Indicator)

Indicator variable taking value 1 if the household purchased improved varieties of seeds (p3_11=8).

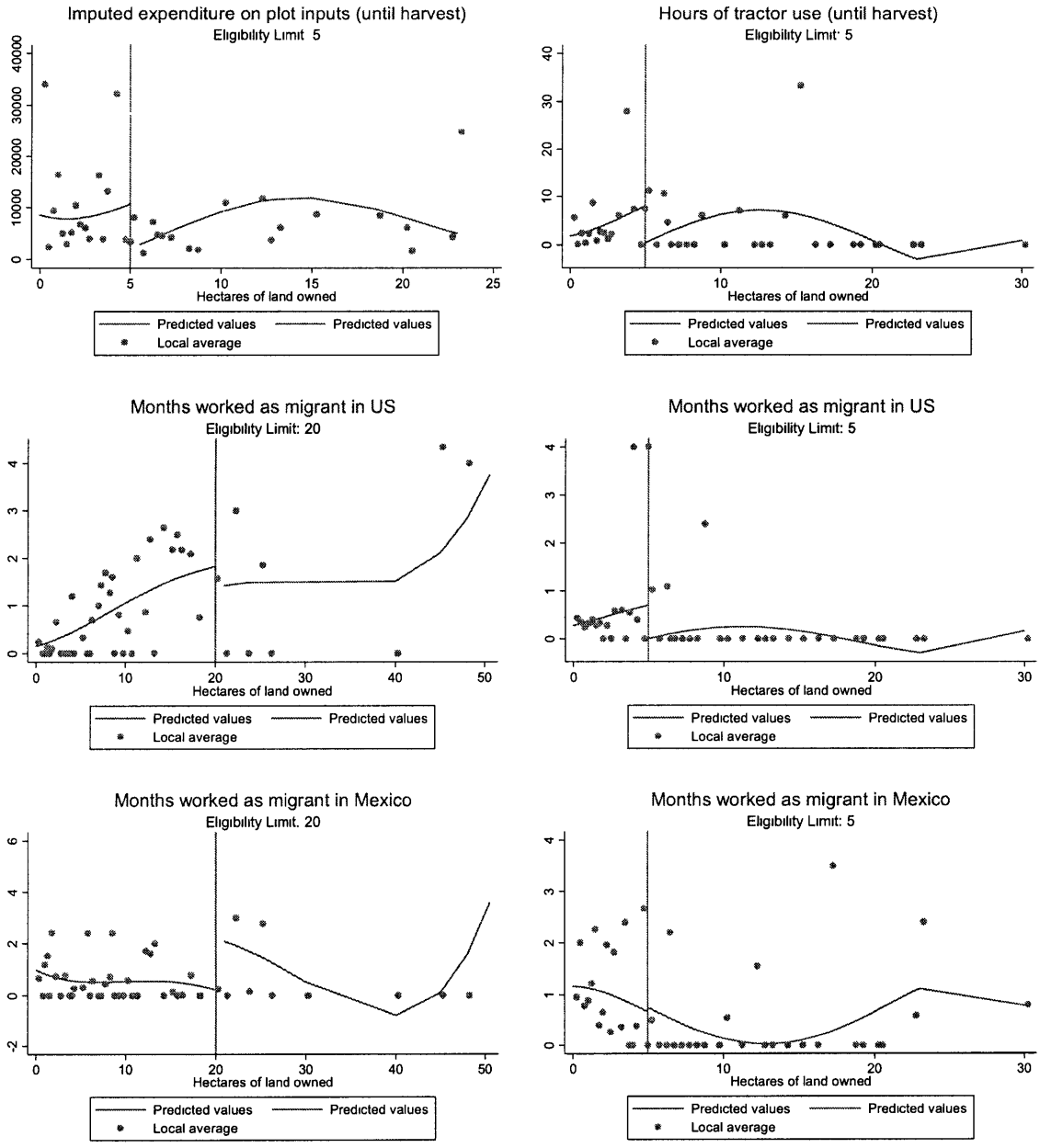
Expense on pesticides (this plot)

A sample average price for each type of pesticide was calculated, *Expense on pesticides (this plot)* is the sum of quantity of each kind of pesticide used times the sample average price for that type of pesticide.

Expense on fertilizer (this plot)

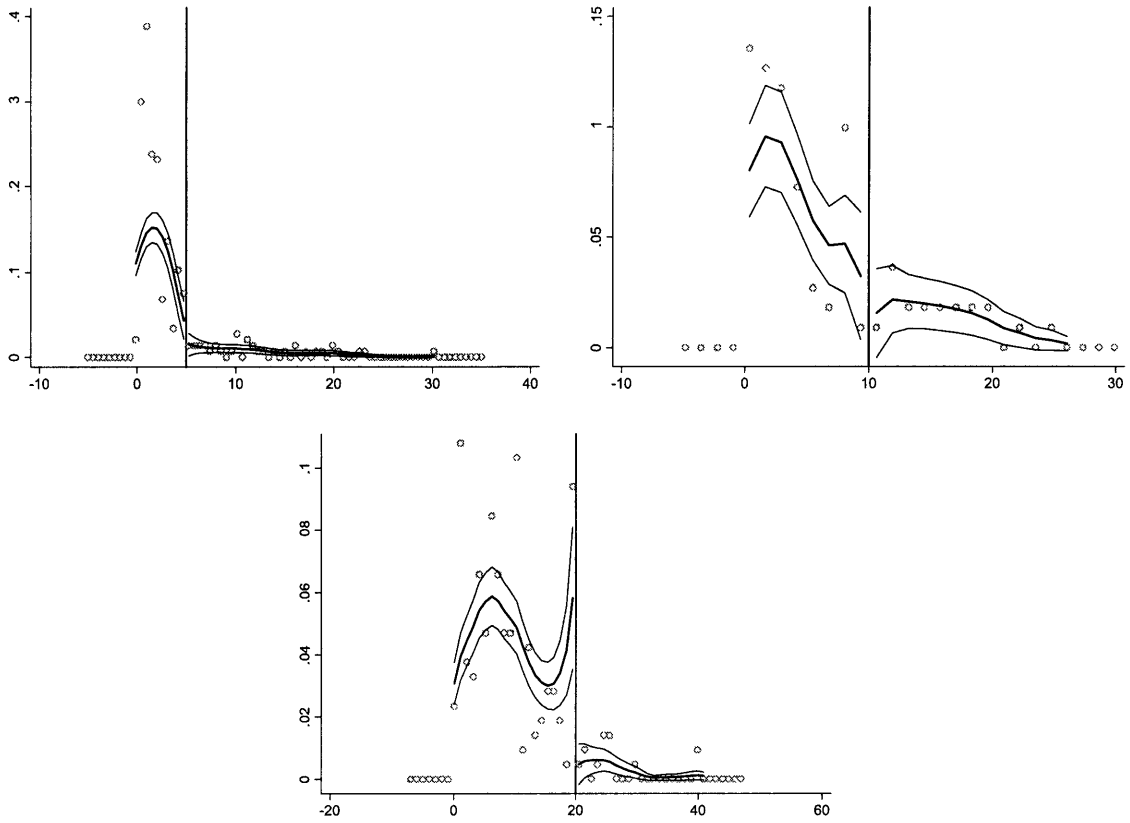
A sample average price for each type of fertilizer was calculated, *Expense on fertilizer (this plot)* is the sum of quantity of each kind of fertilizer used times the sample average price for that type of fertilizer.

Figure 1.1: Outcomes by Landholdings



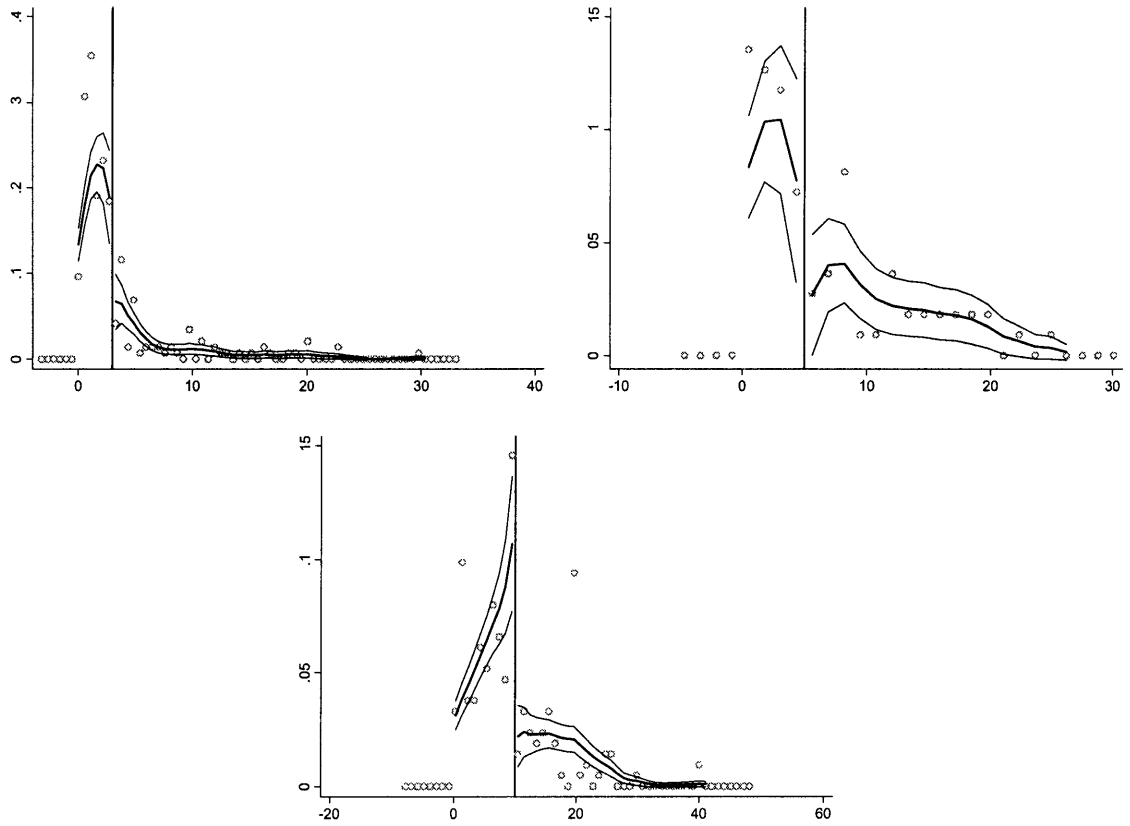
Notes: Lines show the predicted values from a regression of the outcome on a 4th order polynomial of own land and an indicator for own land less than the FONDEN eligibility limit. Regressions are done separately for subsamples corresponding to each eligibility limit. Points indicate the average outcome among households residing in states with the indicated eligibility limit and having landholdings within a 0.25 hectare range of the quantity indicated on the x-axis. To give the graphs meaningful scale, the top 2% of observations, ranked by outcome, were omitted from the analysis when considering agricultural investment variables.

Figure 1.2: Distribution of Landholdings around FONDEN Eligibility Limits



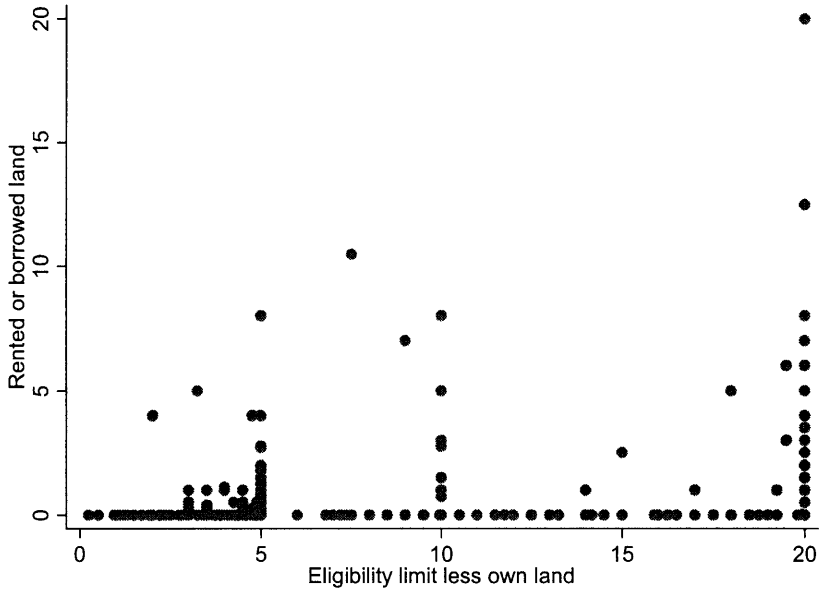
Notes: Points indicate histograms of landholdings. The fitted curves (center lines) smooth the histograms using non-parametric kernel regressions, separately on each side of the FONDEN eligibility limit, which is indicated by the vertical line. The outer curves are confidence bounds. See text for further details.

Figure 1.3: Distribution of Landholdings around Counterfactual Eligibility Limits

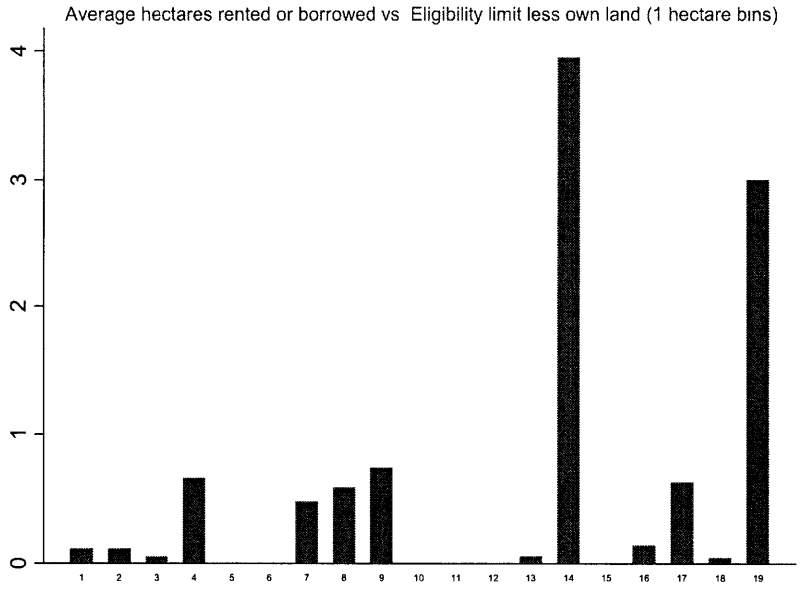


Notes: Points indicate histograms of landholdings. The fitted curves (center lines) smooth the histograms using non-parametric kernel regressions, separately on each side of the counterfactual FONDEN eligibility limit, which is indicated by the vertical line. The outer curves are confidence bounds. See text for further details.

Figure 1.4: Land Rental vs. Distance from Eligibility Limit



Note: Each point represents the quantity of land rented or borrowed in (y-axis) as a function of the how much land the household could rent or borrow in without becoming ineligible for FONDEN aid (x-axis).



Notes: The height of each bar represents the average number of hectares rented or borrowed in among household which could rent or borrow in the quantity of land indicated on the x-axis without becoming ineligible for FONDEN aid. Hectares rented or borrowed in are averaged across households within 1 hectare ranges of the figures indicated on the x-axis.

Table 1.1: Descriptive Statistics

Variable	Full Sample			Land<Limit		Land>Limit		Difference	
	Obs. (1)	Mean (2)	s d. (3)	Mean (4)	s.d (5)	Mean (6)	s.d (7)	Diff. (8)	s e Diff (9)
Panel A: Household Variables									
Total HH members	654	5 06	2.13	5 06	2 12	5.07	2 19	-0.01	(0.27)
Number of males 18 or older	654	1 85	1.10	1 87	1 11	1 76	0 99	0 11	(0 14)
Number of females 18 or older	654	1.87	1.10	1 88	1 09	1 79	1 17	0.09	(0.14)
Number of HH members younger than 18	654	1 34	1 46	1 31	1 44	1 52	1 65	-0.21	(0 18)
Total land with own rights (hectares)	654	5 43	7 04	3 97	4 92	17.40	9 88	-13 44	(0 71)***
Total irrigated land controlled by household (hectares)	654	1 58	4.62	1.38	3.80	3 23	8 74	-1.85	(0.58)***
Animal units at start of 2002	654	5 33	16.76	4.85	16.06	9.29	21 43	-4.44	(2.10)**
FONDEN eligible (0-1 Indicator)	654	0 63	0 48	0 71	0 45	0.00	0 00	0 71	(0 05)***
FONDEN eligible according only to land criteria (0-1 Indicator)	654	0.89	0.31						
Panel B: Individual Variables									
Sex (dummy for male)	3311	0.50	0.50	0 50	0 50	0 51	0.50	-0.01	(0 03)
Age	3294	32.76	19.35	32.69	19.15	33.28	20 92	-0.59	(1.08)
Married (0-1 indicator)	3311	0 46	0 50	0 46	0.50	0.44	0.50	0.01	(0 03)
Education: Primary	3311	0 58	0 49	0 58	0.49	0 60	0.49	-0 02	(0 03)
Education: Secondary	3311	0.21	0.41	0 21	0 41	0 18	0.39	0.03	(0 02)
Education: Preparatory	3311	0 06	0 23	0.05	0 23	0 06	0.24	-0.01	(0 01)
Education: Technical	3311	0 02	0 12	0.02	0 12	0.02	0.13	0 00	(0 01)
Education: Professional	3311	0.02	0.15	0 02	0 14	0 04	0.19	-0.02	(0.008)**
Education. Preschool	3311	0 01	0 07	0 00	0 07	0 01	0.10	-0.01	(0 00)
Education· Doctorate	3311	0.00	0.02	0.00	0.02	0.00	0 05	-0 002	(0 001)*
Education: No school	3311	0.11	0.31	0 11	0 31	0 07	0 25	0.04	(0.017)**
Member migrates to US or Mexico (0-1 Indicator)	3311	0 14	0 35	0.14	0.35	0.10	0 30	0 04	(0 019)**
Months worked as migrant in US	3311	0.63	2 56	0 64	2 58	0 52	2 37	0.12	(0.14)
Months worked as migrant in Mexico	3311	0 76	2 80	0.79	2.86	0.49	2.27	0.30	(0.16)*
Estimated days agricultural labor (total)	3311	17.58	58.96	17.35	57.95	19.45	66 71	-2.10	(3 29)
Estimated days non-agricultural labor (total)	3311	17 84	66 76	18.72	68.18	10.61	53 33	8 11	(3.73)**
FONDEN eligible (0-1 Indicator)	3311	0 63	0 48	0.71	0 46	0.00	0.00	0.71	(0 02)***
FONDEN eligible according only to land criteria (0-1 Indicator)	3311	0 89	0 31						
Panel C: Plot Variables									
Area of plot (hectares)	1039	3.76	4.94	3.00	4.07	7.91	6.91	-4 91	(0 40)***
Household has rights to plot (0-1 Indicator)	1043	0 85	0.36	0.83	0.38	0 97	0 17	-0 14	(0 03)***
Non-ejidal or communal land (0-1 Indicator)	1043	0 30	0.46	0 31	0 46	0.26	0.44	0.05	(0 04)
Number of times plot can be seeded	228	1 53	0 63	1 52	0 64	1.62	0 50	-0.10	(0 13)
Time to walk from community center to plot (minutes)	1030	38 74	47 13	37 79	49 16	43 96	33 61	-6 16	(4.06)
Year plot was acquired	816	1979	17	1979	17	1980	17.63	-1.07	(1 51)
Inclined plot	1043	0.41	0 49	0.39	0 49	0 47	0 50	-0 08	(0 04)*
Very inclined plot	1043	0 05	0 23	0 05	0 22	0 07	0 26	-0.02	(0 02)
Flat plot	1043	0.54	0.50	0.56	0.50	0 45	0 50	0.11	(0 04)**
Plot quality· poor ('malo')	1043	0 08	0 26	0 08	0 27	0 07	0 25	0 01	(0 02)
Plot quality regular ('regular')	1043	0 60	0 49	0.59	0 49	0 68	0 47	-0 09	(0 04)**
Plot quality: good ('bueno')	1043	0 32	0.47	0.34	0.47	0.26	0.44	0.08	(0.04)**
Imputed expenditure on plot inputs (until harvest)	1043	19949	99883	18542	98327	27714	108066	-9172	(8581)
Hours of tractor use (until harvest)	1043	10 05	26 03	9.74	26.41	11.76	23.82	-2 02	(2.24)
Hours of traction animal use (until harvest)	1043	12.60	35.78	11 26	26.17	19 99	67.27	-8 73	(3.06)***
Uses improved seeds (0-1 Indicator)	834	0 22	0 41	0 22	0.42	0 19	0 40	0 03	(0 04)
Expense on pesticides (this plot)	1043	865	8804	902	9439	660	3701	242	(757)
Expense on fertilizer (this plot)	1043	1847	11698	1947	12603	1298	3915	649	(1005)
Irrigated plot (0-1 Indicator)	1043	0 22	0 41	0 23	0 42	0.17	0 38	0 05	(0 04)
FONDEN eligible (0-1 Indicator)	1043	0 64	0.48	0 75	0.43	0.00	0 00	0 75	(0 03)***
FONDEN eligible according only to land criteria (0-1 Indicator)	1043	0 85	0.36						

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes Table shows selected summary statistics for the entire sample (columns 2 and 3) and separately for households which own less land than the FONDEN eligibility limit (columns 4 and 5) and those which own more land than the limit (columns 6 and 7). Column 8 shows the mean difference between those with less land than the FONDEN eligibility limit and those with more while column 9 gives the standard error of the difference. In Panel A the unit of observation is the household, in Panel B it is individual household members and in Panel C it is agricultural plots farmed by the household

Table 1.2: First Stage Relationship between Own Land Criteria and FONDEN Eligibility

	FONDEN eligible (0-1 Indicator)	
	Plot Level Covariates	*Person Level Covariates*
	(1)	(2)
FONDEN eligible according only to land criteria (0-1 Indicator)	0.83	0.719
	(0.057)***	(0.063)***
Observations	1039	3294
R-Squared	0.49	0.38
Mean of dependent variable	0.64	0.63

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: Table presents results from regressing an indicator that the household meets all criteria for FONDEN aid (own less land than eligibility limit, some land is not irrigated, own less than 25 animal units) on an indicator that the household meets the own land criteria. The regression in column 1 includes eligibility limit fixed effects, household level covariates (the number of males 18 years or older, the number of females 18 years or older, the number of household members under 18 years old and a 4th order polynomial of total land owned by the household). Agricultural plot level covariates include: dummies indicating plot slope and soil quality, an indicator for whether the plot is part of an ejido, plot area and plot area squared.

The regression in column 2 includes eligibility limit fixed effects, household level covariates (the number of males 18 years or older, the number of females 18 years or older, the number of household members under 18 years old and a 4th order polynomial of total land owned by the household) and individual level covariates (dummy for male, indicator variables for education level, age and age squared).

Standard errors are clustered at the household level.

Table 1.3: Effect of FONDEN Eligibility on Agricultural Investment

	Imputed expenditure on plot inputs (until harvest) (1)	Hours of tractor use (until harvest) (2)	Hours of traction animal use (until harvest) (3)	Uses improved seeds (0-1 Indicator) (4)
	Panel A: Eligibility According to Own Land			
FONDEN eligible according only to land criteria (0-1 Indicator)	2869	7.12	-17.53	0.13
	(15702)	(4.252)*	(8.590)**	(0.075)*
Observations	1039	1039	1039	831
R-Squared	0.05	0.14	0.11	0.11
	Panel B: Two Stage Least Squares Results			
FONDEN eligible (0-1 Indicator)	4046	10.04	-24.72	0.20
	(22142)	(6.23)	(12.483)**	(0.115)*
Observations	1039	1039	1039	831
Mean of dependent variable	20014	10.09	12.6	0.22

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: Panel A presents results from regressing the variable indicated in the column heading on an indicator that the household owns less land than the FONDEN eligibility limit. In Panel B, an indicator that the household is eligible for FONDEN aid according to all criteria (own less land than eligibility limit, some land is not irrigated, own less than 25 animal units) is instrumented by the indicator for owning less land than the limit. The regressions include eligibility limit fixed effects, household level covariates (the number of males 18 years or older, the number of females 18 years or older, the number of household members under 18 years old and a 4th order polynomial of total land owned by the household) and agricultural plot level covariates (dummies indicating plot slope and soil quality, an indicator for whether the plot is part of an ejido, plot area and plot area squared).

Standard errors are clustered at the household level.

Table 1.4: Effect of FONDEN Eligibility on Labor and Migration Outcomes

	Estimated days agricultural labor (total) (1)	Estimated days non-agricultural labor (total) (2)	Member migrates to US or Mexico (0-1 Indicator) (3)	Months worked as migrant in US (4)	Months worked as migrant in Mexico (5)
	Panel A: Eligibility According to Own Land				
FONDEN eligible according only to land criteria (0-1 Indicator)	-8.32	9.05	0.07	0.80	-0.16
	(5.23)	(5.419)*	(0.029)**	(0.234)***	(0.22)
Observations	3294	3294	3294	3294	3294
R-Squared	0.09	0.07	0.11	0.09	0.06
	Panel B: Two Stage Least Squares Results				
FONDEN eligible (0-1 Indicator)	-14.77	16.06	0.12	1.42	-0.28
	(9.41)	(10.01)	(0.051)**	(0.433)***	(0.39)
Observations	3294	3294	3294	3294	3294
Mean of dependent variable	17.44	17.84	0.14	0.63	0.76

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: Panel A presents results from regressing the variable indicated in the column heading on an indicator that the household owns less land than the FONDEN eligibility limit. In Panel B, an indicator that the household is eligible for FONDEN aid according to all criteria (own less land than eligibility limit, some land is not irrigated, own less than 25 animal units) is instrumented by the indicator for owning less land than the limit. The regressions include eligibility limit fixed effects, household level covariates (the number of males 18 years or older, the number of females 18 years or older, the number of household members under 18 years old and a 4th order polynomial of total land owned by the household) and individual level covariates (dummy for male, indicator variables for education level, age and age squared).

Standard errors are clustered at the household level.

Table 1.5: Migration Pre and Post FONDEN

	Member migrated to Mexico pre-1996 (0-1 Indicator) (1)	Member migrated to Mexico post-1996 (0-1 Indicator) (2)	Member migrated to US pre-1996 (0-1 Indicator) (3)	Member migrated to US post-1996 (0-1 Indicator) (4)	Number of years migrated to Mexico Pre-1996 (5)	Number of years migrated to Mexico Post-1996 (6)	Number of years migrated to US Pre-1996 (7)	Number of years migrated to US Post-1996 (8)
				Panel A: Eligibility According to Own Land				
FONDEN eligible according only to land criteria (0-1 Indicator)	-0.01	-0.02	0.04	0.09	0.04	0.03	0.22	0.36
	(0.03)	(0.04)	(0.03)	(0.031)***	(0.15)	(0.16)	(0.15)	(0.153)**
Observations	1645	1645	1645	1645	1645	1645	1645	1645
R-Squared	0.11	0.11	0.12	0.14	0.1	0.11	0.12	0.13
				Panel B: Two Stage Least Squares Results				
FONDEN eligible (0-1 Indicator)	-0.02	-0.04	0.07	0.16	0.07	0.06	0.39	0.63
	(0.06)	(0.07)	(0.06)	(0.053)***	(0.27)	(0.29)	(0.26)	(0.265)**
Observations	1645	1645	1645	1645	1645	1645	1645	1645
Mean of dependent variable	0.1	0.1	0.07	0.09	0.44	0.42	0.3	0.36

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: Panel A presents results from regressing the variable indicated in the column heading on an indicator that the household owns less land than the FONDEN eligibility limit. In Panel B, an indicator that the household is eligible for FONDEN aid according to all criteria (own less land than eligibility limit, some land is not irrigated, own less than 25 animal units) is instrumented by the indicator for owning less land than the limit. The regressions include eligibility limit fixed effects, household level covariates (the number of males 18 years or older, the number of females 18 years or older, the number of household members under 18 years old and a 4th order polynomial of total land owned by the household) and individual level covariates (dummy for male, indicator variables for education level, age and age squared).

Standard errors clustered at the household level.

Sample restricted to individuals older than 16 in 1990 (6 years prior to FONDEN).

Table 1.6: Results by Migration Network Strength

	Member migrates to US or Mexico (0-1 Indicator) (1)	Months worked as migrant in US (2)	Months worked as migrant in Mexico (3)
	High Migration Networks States		
	Panel A: Eligibility According to Own Land		
FONDEN eligible according only to land criteria (0-1 Indicator)	0.05	-0.30	0.42
	(0.05)	(0.51)	(0.54)
Observations	662	662	662
R-Squared	0.17	0.13	0.09
	Panel B: Two Stage Least Squares Results		
FONDEN eligible (0-1 Indicator)	0.06	-0.33	0.44
	(0.05)	(0.55)	(0.58)
Observations	662	662	662
Mean of dependent variable	0.13	0.89	0.44
	Low Migration Networks States		
	Panel A: Eligibility According to Own Land		
FONDEN eligible according only to land criteria (0-1 Indicator)	0.08	0.93	-0.18
	(0.031)**	(0.246)***	(0.24)
Observations	2632	2632	2632
R-Squared	0.1	0.09	0.06
	Panel B: Two Stage Least Squares Results		
FONDEN eligible (0-1 Indicator)	0.15	1.81	-0.35
	(0.062)**	(0.528)***	(0.46)
Observations	2632	2632	2632
Mean of dependent variable	0.14	0.56	0.84

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: Panel A presents results from regressing the variable indicated in the column heading on an indicator that the household owns less land than the FONDEN eligibility limit. In Panel B, an indicator that the household is eligible for FONDEN aid according to all criteria (own less land than eligibility limit, some land is not irrigated, own less than 25 animal units) is instrumented by the indicator for owning less land than the limit. The regressions include eligibility limit fixed effects, household level covariates (the number of males 18 years or older, the number of females 18 years or older, the number of household members under 18 years old and a 4th order polynomial of total land owned by the household) and individual level covariates (dummy for male, indicator variables for education level, age and age squared).

Standard errors clustered at the household level.

Table 1.7: Testing Discontinuities in Distribution of Land

	Actual Land Limit		
Land Limit	5	10	20
Gap: estimate	-0.77	-0.56	-2.76
s.e. (Gap)	(0.61)	(1.44)	(0.91)
	Counterfactual Land Limit		
Land Limit	4	8	15
Gap: estimate	-0.97	-2.33	0.01
s.e. (Gap)	(0.46)	(1.12)	(0.39)
	Counterfactual Land Limit		
Land Limit	3	5	10
Gap: estimate	-0.87	-1.55	-1.70
s.e. (Gap)	(0.36)	(1.14)	(0.41)

Notes: Table shows check that distribution of land is no more discontinuous at integer quantities of land other than those determining eligibility for FONDEN. See text for further discussion.

Table 1.8: Test of Targeting Landholdings Eligibility Criteria

	Owns five hectares of land (1)
Eligiblity limit: 5 hectares	0.00
	(0.02)
Eligibility Limit Fixed Effects	No
Observations	654
R-Squared	0.00

* Significant at the 10% confidence level
** Significant at the 5% confidence level
*** Significant at the 1% confidence level

Notes: Table displays results from regressing an indicator that the household owns exactly 5 hectares of land on an indicator that the household resides in a state where the FONDEN eligibility limit is 5 hectares.
Standard errors clustered at the state level.

Table 1.9: Tests for Targeting of FONDEN Eligibility Criteria

	Percent of land farmed which is irrigated (1)	Percent of land farmed which is irrigated (2)	Household has more than 25 animal units (3)	Household has more than 25 animal units (4)
FONDEN eligible according only to land criteria (0-1 Indicator)	0.15	0.16	0.10	0.07
	(0.14)	(0.13)	(0.08)	(0.07)
Eligibility Limit Fixed Effects	No	Yes	No	Yes
Observations	652	652	654	654
R-Squared	0.01	0.01	0.13	0.13
Mean of dependent variable	0.25	0.25	0.05	0.05

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: Table displays results from regression of variable indicated in column heading on an indicatior that the household owns less land than the FONDEN eligibility limit and smooth functions of total hectares of land owned.

Standard errors clustered at the state level.

Table 1.10: Rental Decisions

	Rented or borrowed land
Eligibility limit less own land	0.07
	(0.037)*
F-test: Beta=1	631
P-value	0.00
Observations	555
R-Squared	0.08
Mean of dependent variable	0.53

* Significant at the 10% confidence level
** Significant at the 5% confidence level
*** Significant at the 1% confidence level
Notes: Table displays results from regression of hectares of land rented in or borrowed by the household on the FONDEN eligibility limit in their state of residence minus the quantity of land the household owns. The regression also includes smooth functions of total hectares of land owned by the household.
Standard errors clustered at the state level.
Sample restricted to households with less own land than limit.

Table 1.11: Robustness Tests of the Effect of FONDEN Eligibility on Agricultural Investment

	Imputed expenditure on plot inputs (until harvest) (1)	Hours of tractor use (until harvest) (2)	Hours of traction animal use (until harvest) (3)	Uses improved seeds (0-1 Indicator) (4)
Restricted to Households Within 1 hectare of eligibility limit (based on own land)				
FONDEN eligible according only to land criteria (0-1 Indicator)	-35844	-22.16	-11.39	0.42
	(26372)	(18.69)	(23.77)	(0.42)
Observations	91	91	91	66
R-Squared	0.31	0.25	0.30	0.18
Two Stage Least Squares Results: Restricted to Households Within 1 hectare of eligibility limit (based on own land)				
FONDEN eligible (0-1 Indicator)	-37052	-22.93	-11.75	0.46
	(26062)	(18.99)	(24.32)	(0.51)
Observations	91	91	91	66
Mean of dependent variable	17556	17.60	14.27	0.24
Restricted to Households Within 2 hectares of eligibility limit (based on own land)				
FONDEN eligible according only to land criteria (0-1 Indicator)	-464	-4.59	8.44	0.28
	(29083)	(9.72)	(18.77)	(0.26)
Observations	173	173	173	137
R-Squared	0.14	0.22	0.20	0.16
Two Stage Least Squares Results: Restricted to Households Within 2 hectares of eligibility limit (based on own land)				
FONDEN eligible (0-1 Indicator)	-504	-4.98	9.15	0.26
	(31544)	(10.37)	(20.38)	(0.27)
Observations	173	173	173	137
Mean of dependent variable	26411	13.91	16.40	0.20
Restricted to Households Eligible for Alliance for the Country (0-1 Indicator)				
FONDEN eligible according only to land criteria (0-1 Indicator)	22378	7.20	-15.25	0.10
	(11681.271)*	(4.001)*	(9.45)	(0.08)
Observations	947	947	947	763
R-Squared	0.05	0.14	0.06	0.09
Two Stage Least Squares Results: Restricted to Households Eligible for Alliance for the Country (0-1 Indicator)				
FONDEN eligible (0-1 Indicator)	27845	8.96	-18.98	0.14
	(14719.064)*	(5.056)*	(12.00)	(0.11)
Observations	947	947	947	763
Mean of dependent variable	17649	9.06	12.47	0.20

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: OLS reults obtain from regressing the variable indicated in the column heading on an indicator that the household owns less land than the FONDEN eligibility limit. In Two Stage Least Squares Results, an indicator that the household is eligible for FONDEN aid according to all criteria (own less land than eligibility limit, some land is not irrigated, own less than 25 animal units) is instrumented by the indicator for owning less land than the limit. The regressions include eligibility limit fixed effects, household level covariates (the number of males 18 years or older, the number of females 18 years or older, the number of household members under 18 years old and a 4th order polynomial of total land owned by the household) and agricultural plot level covariates (dummies indicating plot slope and soil quality, an indicator for whether the plot is part of an ejido, plot area and plot area squared).

Table 1.12: Robustness Tests of the Effect of FONDEN Eligibility on Labor Outcomes

	Estimated days agricultural labor (total) (1)	Estimated days non-agricultural labor (total) (2)	Member migrates to US or Mexico (0-1 Indicator) (3)	Months worked as migrant in US (4)	Months worked as migrant in Mexico (5)
Restricted to Households Within 1 hectare of eligibility limit (based on own land)					
FONDEN eligible according only to land criteria (0-1 Indicator)	20.13	-23.32	0.11	0.63	0.69
	(13.11)	(13.544)*	(0.07)	(0.62)	(0.53)
Observations	283	283	283	283	283
R-Squared	0.14	0.23	0.12	0.11	0.10
Two Stage Least Squares Results: Restricted to Households Within 1 hectare of eligibility limit (based on own land)					
FONDEN eligible (0-1 Indicator)	20.54	-23.79	0.11	0.65	0.70
	(13.52)	(14.39)	(0.07)	(0.64)	(0.55)
Observations	283		283	283	283
Mean of dependent variable	9.20	21.63	0.13	0.99	0.41
Restricted to Households Within 2 hectares of eligibility limit (based on own land)					
FONDEN eligible according only to land criteria (0-1 Indicator)	29.29	-34.16	0.17	1.10	1.14
	(12.122)**	(11.739)***	(0.071)**	(0.605)*	(0.500)**
Observations	538.00	538.00	538.00	538.00	538.00
R-Squared	0.12	0.15	0.13	0.11	0.09
Two Stage Least Squares Results: Restricted to Households Within 2 hectares of eligibility limit (based on own land)					
FONDEN eligible (0-1 Indicator)	30.48	-35.54	0.18	1.15	1.19
	(14.567)**	(14.583)**	(0.079)**	(0.663)*	(0.538)**
Observations	538	538	538	538	538
Mean of dependent variable	7.88	17.94	0.12	0.84	0.42
Restricted to Households Eligible for Alliance for the Country (0-1 Indicator)					
FONDEN eligible according only to land criteria (0-1 Indicator)	-5.89	2.97	0.11	1.12	0.07
	(6.30)	(5.50)	(0.028)***	(0.251)***	(0.25)
Observations	3061	3061	3061	3061	3061
R-Squared	0.09	0.05	0.11	0.09	0.06
Two Stage Least Squares Results: Restricted to Households Eligible for Alliance for the Country (0-1 Indicator)					
FONDEN eligible (0-1 Indicator)	-8.37	4.21	0.16	1.58	0.10
	(8.86)	(7.82)	(0.041)***	(0.391)***	(0.35)
Observations	3061	3061	3061	3061	3061
Mean of dependent variable	17.69	17.02	0.14	0.59	0.77

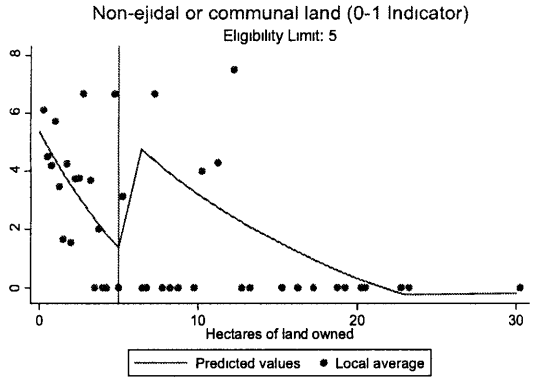
* Significant at the 10% confidence level

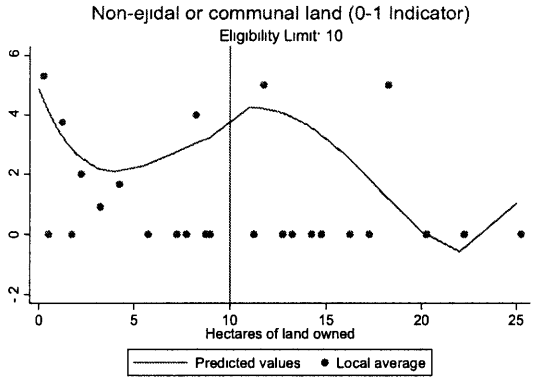
** Significant at the 5% confidence level

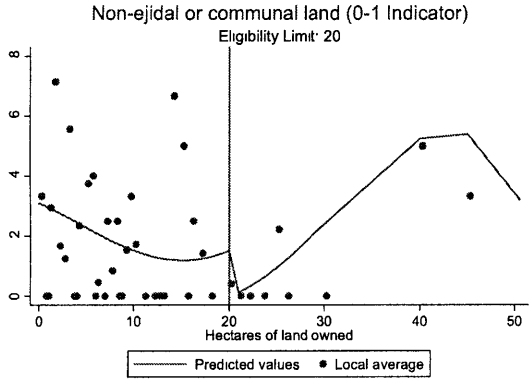
*** Significant at the 1% confidence level

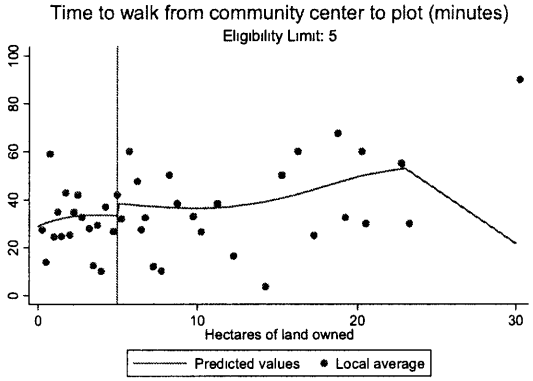
Notes: OLS results obtain from regressing the variable indicated in the column heading on an indicator that the household owns less land thar the FONDEN eligibility limit. In Two Stage Least Squares Results, an indicator that the household is eligible for FONDEN aid according to all criteria (own less land than eligibility limit, some land is not irrigated, own less than 25 animal units) is instrumented by the indicator for owning less land than the limit. The regressions include eligibility limit fixed effects, household level covariates (the number of males 18 years or older, Standard errors are clustered at the household level.

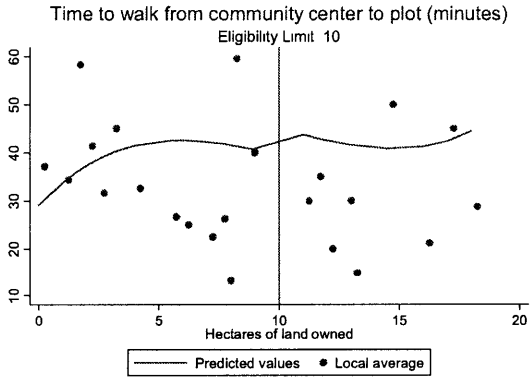
Appendix Figures A1.1: Checking Discontinuity Breaks in Control Variables

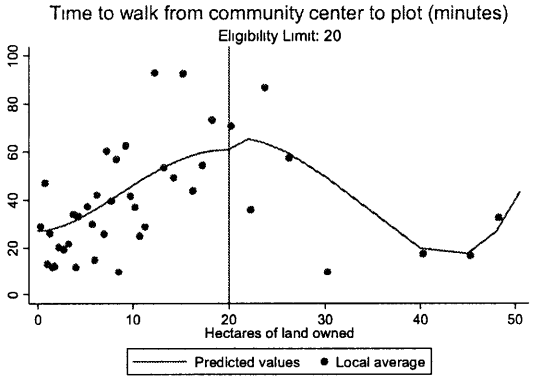




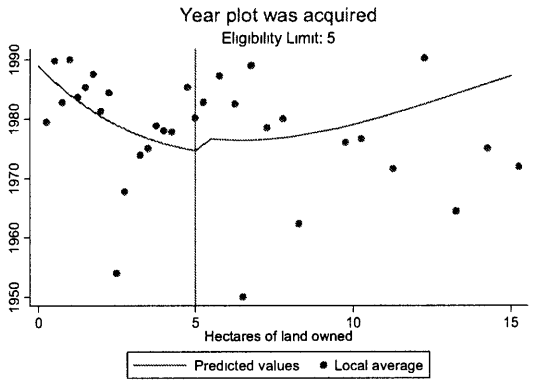


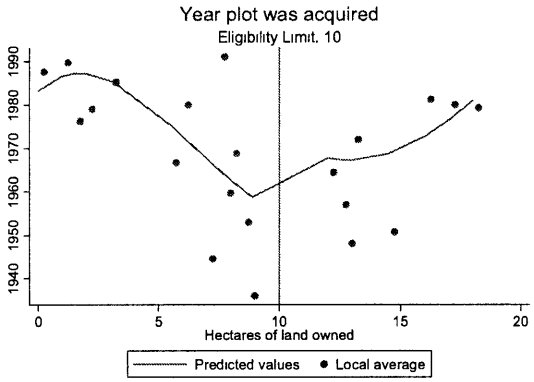


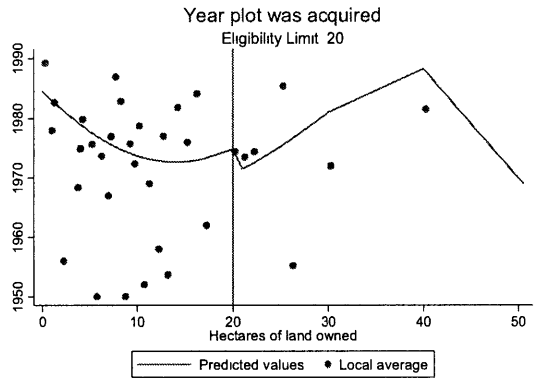


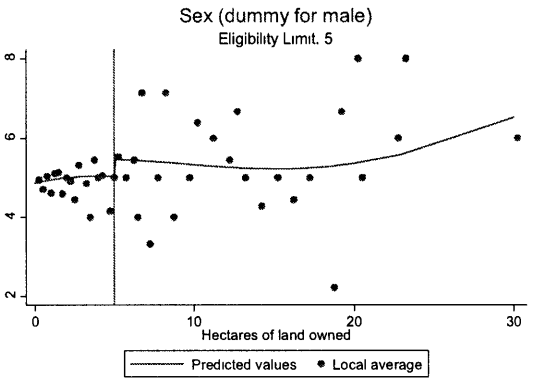


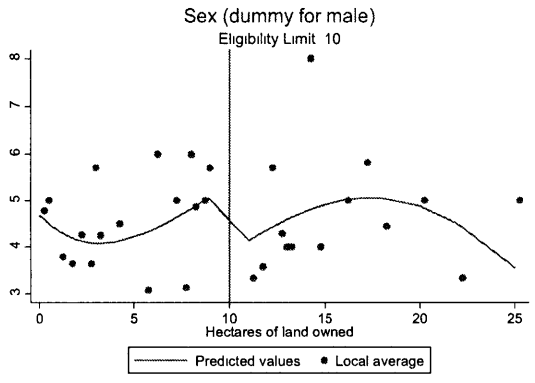
Appendix Figures A1.1: Checking Discontinuity Breaks in Control Variables (Continued)

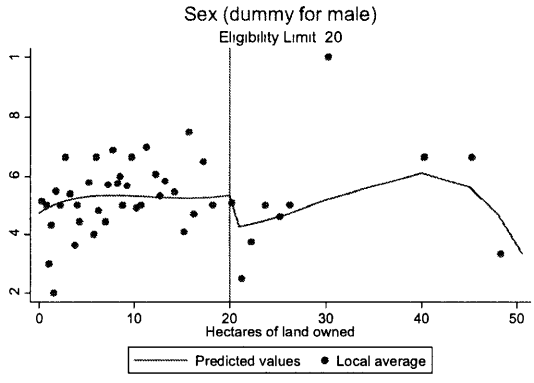




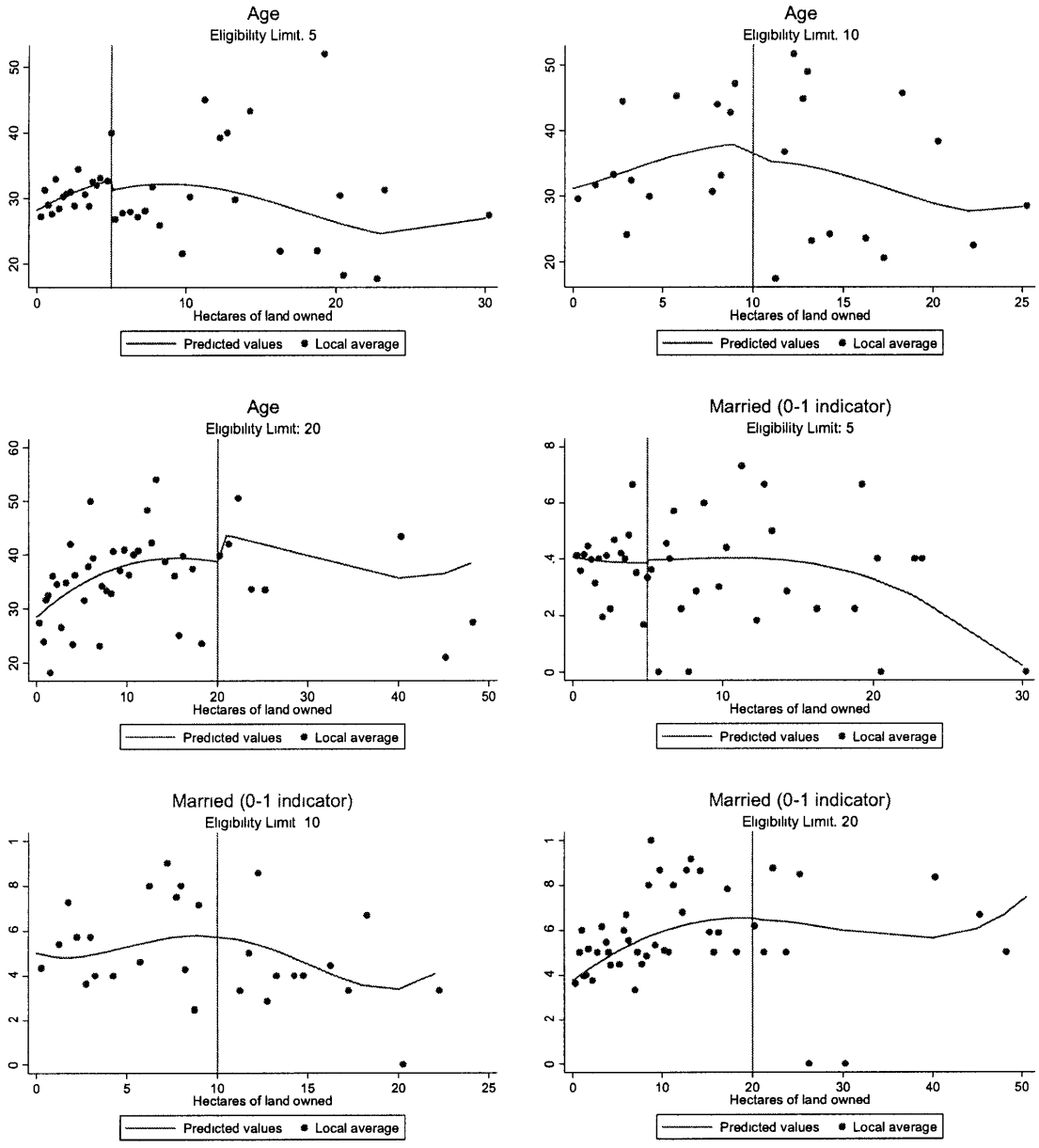




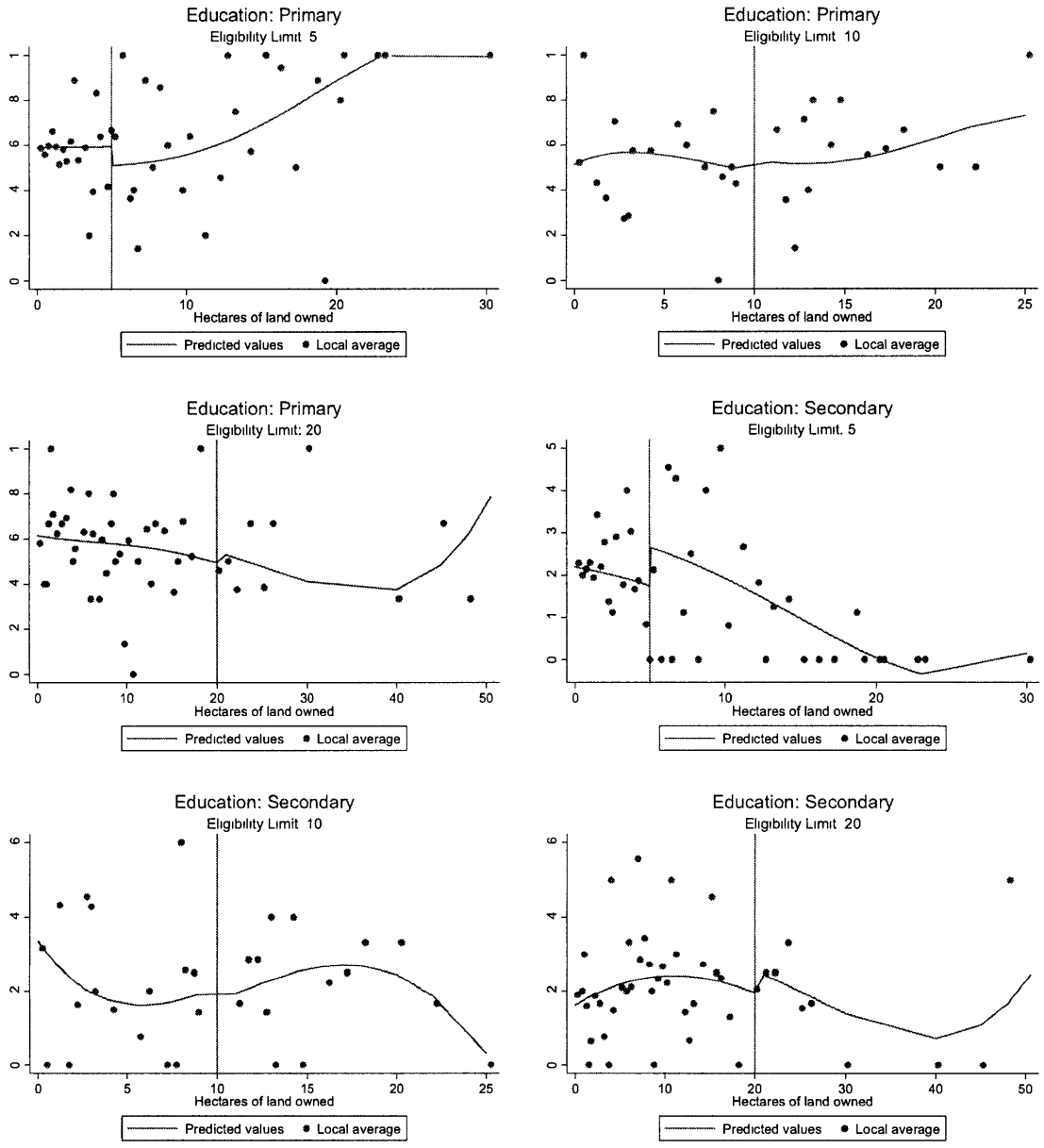




Appendix Figures A1.1: Checking Discontinuity Breaks in Control Variables (Continued)



Appendix Figures A1.1: Checking Discontinuity Breaks in Control Variables (Continued)



Notes: Lines show the predicted values from a regression of the outcome on a 4th order polynomial of own land and an indicator for own land less than the FONDEN eligibility limit. Regressions are done separately for subsamples corresponding to each eligibility limit. Points indicate the average outcome among households residing in states with the indicated eligibility limit and having landholdings within a 0.25 hectare range of the quantity indicated on the x-axis.

Table A1.1: Insurance and Migration Finance

	Borrowed to finance migration (1)	Borrowed to finance migration, conditional on migration (2)
	Panel A: Eligibility According to Own Land	
FONDEN eligible according only to land criteria (0-1 Indicator)	0.01	0.01
	(0.01)	(0.07)
Observations	3294	457
R-Squared	0.02	0.08
	Panel B: Two Stage Least Squares Results	
FONDEN eligible (0-1 Indicator)	0.02	0.02
	(0.02)	(0.12)
Observations	3294	457
Mean of dependent variable	0.02	0.11

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: Panel A presents results from regressing the variable indicated in the column heading on an indicator that the household owns less land than the FONDEN eligibility limit. In Panel B, an indicator that the household is eligible for FONDEN aid according to all criteria (own less land than eligibility limit, some land is not irrigated, own less than 25 animal units) is instrumented by the indicator for owning less land than the limit. The regressions include eligibility limit fixed effects, household level covariates (the number of males 18 years or older, the number of females 18 years or older, the number of household members under 18 years old and a 4th order polynomial of total land owned by the household) and individual level covariates (dummy for male, indicator variables for education level, age and age squared).

Standard errors clustered at the household level.

Table A1.2: Insurance and Borrowing

	Has debt with bank (1)	Has debt with business (2)	Has debt with other person (3)	Sought debt from bank (4)	Sought debt from business (5)	Sought debt from other person (6)
			Panel A: Eligibility According to Own Land			
FONDEN eligible according only to land criteria (0-1 Indicator)	0.01	0.02	-0.13	-0.03	0.04	0.10
	(0.04)	(0.05)	(0.076)*	(0.03)	(0.04)	(0.06)
Observations	538	538	538	538	538	538
R-Squared	0.03	0.05	0.02	0.11	0.03	0.03
			Panel B: Two Stage Least Squares Results			
FONDEN eligible (0-1 Indicator)	0.01	0.04	-0.22	-0.05	0.07	0.17
	(0.07)	(0.08)	(0.14)	(0.06)	(0.07)	(0.11)
Observations	538	538	538	538	538	538
Mean of dependent variable	0.03	0.04	0.14	0.01	0.04	0.15

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: Panel A presents results from regressing the variable indicated in the column heading on an indicator that the household owns less land than the FONDEN eligibility limit. In Panel B, an indicator that the household is eligible for FONDEN aid according to all criteria (own less land than eligibility limit, some land is not irrigated, own less than 25 animal units) is instrumented by the indicator for owning less land than the limit. The regressions include eligibility limit fixed effects, household level covariates (the number of males 18 years or older, the number of females 18 years or older, the number of household members under 18 years old and a 4th order polynomial of total land owned by the household).

Table A1.3: The Effect of FONDEN Eligibility on Agricultural Investment (2nd order polynomial control function)

	Imputed expenditure on plot inputs (until harvest) (1)	Hours of tractor use (until harvest) (2)	Hours of traction animal use (until harvest) (3)	Uses improved seeds (0-1 Indicator) (4)
	Panel A: Eligibility According to Own Land			
FONDEN eligible according only to land criteria (0-1 Indicator)	1829	6.61	-15.27	0.13
	(15840)	(4.19)	(8.527)*	(0.076)*
Observations	1039	1039	1039	831
R-Squared	0.04	0.13	0.08	0.1
	Panel B: Two Stage Least Squares Results			
FONDEN eligible (0-1 Indicator)	2539	9.17	-21.20	0.19
	(21986)	(5.99)	(12.099)*	(0.111)*
Observations	1039	1039	1039	831
Mean of dependent variable	20014	10.09	12.6	0.22

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: Panel A presents results from regressing the variable indicated in the column heading on an indicator that the household owns less land than the FONDEN eligibility limit. In Panel B, an indicator that the household is eligible for FONDEN aid according to all criteria (own less land than eligibility limit, some land is not irrigated, own less than 25 animal units) is instrumented by the indicator for owning less land than the limit. The regressions include eligibility limit fixed effects, household level covariates (the number of males 18 years or older, the number of females 18 years or older, the number of household members under 18 years old, total land owned by the household and its square) and agricultural plot level covariates (dummies indicating plot slope and soil quality, an indicator for whether the plot is part of an ejido, plot area and plot area squared).

Standard errors are clustered at the household level.

Table A1.4: The Effect of FONDEN Eligibility on Labor Outcomes (2nd order polynomial control function)

	Estimated days agricultural labor (total) (1)	Estimated days non-agricultural labor (total) (2)	Member migrates to US or Mexico (0-1 Indicator) (3)	Months worked as migrant in US (4)	Months worked as migrant in Mexico (5)
	Panel A: Eligibility According to Own Land				
FONDEN eligible according only to land criteria (0-1 Indicator)	-8.76	8.92	0.07	0.80	-0.18
	(5.248)*	(5.325)*	(0.029)**	(0.231)***	(0.22)
Observations	3294	3294	3294	3294	3294
R-Squared	0.09	0.07	0.11	0.09	0.06
	Panel B: Two Stage Least Squares Results				
FONDEN eligible (0-1 Indicator)	-15.32	15.59	0.12	1.40	-0.31
	(9.284)*	(9.65)	(0.050)**	(0.419)***	(0.39)
Observations	3294	3294	3294	3294	3294
Mean of dependent variable	17.44	17.84	0.14	0.63	0.76

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: Panel A presents results from regressing the variable indicated in the column heading on an indicator that the household owns less land than the FONDEN eligibility limit. In Panel B, an indicator that the household is eligible for FONDEN aid according to all criteria (own less land than eligibility limit, some land is not irrigated, own less than 25 animal units) is instrumented by the indicator for owning less land than the limit. The regressions include eligibility limit fixed effects, household level covariates (the number of males 18 years or older, the number of females 18 years or older, the number of household members under 18 years old, total land owned by the household and its square) and individual level covariates (dummy for male, indicator variables for education level, age and age squared).

Standard errors are clustered at the household level.

Table A1.5: The Effect of FONDEN Eligibility on Agricultural Investment (flexible control function)

	Imputed expenditure on plot inputs (until harvest) (1)	Hours of tractor use (until harvest) (2)	Hours of traction animal use (until harvest) (3)	Uses improved seeds (0-1 Indicator) (4)
	Panel A: Eligibility According to Own Land			
FONDEN eligible according only to land criteria (0-1 Indicator)	1079	6.78	-15.46	0.12
	(17139)	(4.46)	(8.796)*	(0.08)
Observations	1039	1039	1039	831
R-Squared	0.05	0.13	0.08	0.11
	Panel B: Two Stage Least Squares Results			
FONDEN eligible (0-1 Indicator)	1542	9.70	-22.09	0.17
	(24494)	(6.56)	(12.871)*	(0.12)
Observations	1039	1039	1039	831
Mean of dependent variable	20014	10.09	12.6	0.22

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: Panel A presents results from regressing the variable indicated in the column heading on an indicator that the household owns less land than the FONDEN eligibility limit. In Panel B, an indicator that the household is eligible for FONDEN aid according to all criteria (own less land than eligibility limit, some land is not irrigated, own less than 25 animal units) is instrumented by the indicator for owning less land than the limit. The regressions include eligibility limit fixed effects, household level covariates (the number of males 18 years or older, the number of females 18 years or older, the number of household members under 18 years old, total land owned by the household, its square, sin and cos of total land owned and sin and cos of two times own total land owned) and agricultural plot level covariates (dummies indicating plot slope and soil quality, an indicator for whether the plot is part of an ejido, plot area and plot area squared).

Standard errors are clustered at the household level.

Table A1.6: The Effect of FONDEN Eligibility on Labor Outcomes (flexible control function)

	Estimated days agricultural labor (total) (1)	Estimated days non-agricultural labor (total) (2)	Member migrates to US or Mexico (0-1 Indicator) (3)	Months worked as migrant in US (4)	Months worked as migrant in Mexico (5)
	Panel A: Eligibility According to Own Land				
FONDEN eligible according only to land criteria (0-1 Indicator)	-6.81	9.80	0.06	0.84	-0.22
	(5.56)	(5.518)*	(0.031)**	(0.247)***	(0.25)
Observations	3294	3294	3294	3294	3294
R-Squared	0.09	0.07	0.11	0.09	0.06
	Panel B: Two Stage Least Squares Results				
FONDEN eligible (0-1 Indicator)	-12.43	17.89	0.12	1.53	-0.40
	(10.21)	(10.561)*	(0.055)**	(0.476)***	(0.46)
Observations	3294	3294	3294	3294	3294
Mean of dependent variable	17.44	17.84	0.14	0.63	0.76

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: Panel A presents results from regressing the variable indicated in the column heading on an indicator that the household owns less land than the FONDEN eligibility limit. In Panel B, an indicator that the household is eligible for FONDEN aid according to all criteria (own less land than eligibility limit, some land is not irrigated, own less than 25 animal units) is instrumented by the indicator for owning less land than the limit. The regressions include eligibility limit fixed effects, household level covariates (the number of males 18 years or older, the number of females 18 years or older, the number of household members under 18 years old, total land owned by the household, its square, sin and cos of total land owned and sin and cos of two times own total land owned) and individual level covariates (dummy for male, indicator variables for education level, age and age squared).

Standard errors are clustered at the household level.

Table A1.7: Counterfactual RD Design, Agricultural Investment

	Imputed expenditure on plot inputs (until harvest) (1)	Hours of tractor use (until harvest) (2)	Hours of traction animal use (until harvest) (3)	Uses improved seeds (0-1 Indicator) (4)
Eligibility According to Own Land				
Conterfactual insurance (eligibility limit - 1)	8871	-0.95	-11.37	0.07
	(17280)	(2.28)	(5.956)*	(0.08)
Observations	1039	1039	1039	831
R-Squared	0.05	0.14	0.11	0.11
Two Stage Least Squares Results				
Conterfactual insurance (eligibility limit - 1)	11786	-1.27	-15.10	0.09
	(22722)	(3.01)	(8.203)*	(0.11)
Observations	1039	1039	1039	831
Eligibility According to Own Land				
Conterfactual insurance (eligibility limit - 2)	-1289	-1.51	-12.31	0.04
	(16674)	(1.87)	(4.231)**	(0.06)
Observations	1039	1039	1039	831
R-Squared	0.05	0.14	0.11	0.11
Two Stage Least Squares Results				
Conterfactual insurance (eligibility limit - 2)	-1663	-1.95	-15.90	0.05
	(21527)	(2.38)	(5.138)***	(0.08)
Observations	1039	1039	1039	831
Eligibility According to Own Land				
Conterfactual insurance (eligibility limit - 3)	90	-0.93	-7.46	0.06
	(7145)	(1.50)	(2.317)***	(0.05)
Observations	1039	1039	1039	831
R-Squared	0.05	0.14	0.1	0.11
Two Stage Least Squares Results				
Conterfactual insurance (eligibility limit - 3)	129	-1.33	-10.71	0.08
	(10255)	(2.12)	(3.068)***	(0.08)
Observations	1039	1039	1039	831
Eligibility According to Own Land				
Conterfactual insurance (eligibility limit - 4)	2462	0.12	-4.68	-0.01
	(5565)	(1.11)	(2.177)*	(0.06)
Observations	1039	1039	1039	831
R-Squared	0.05	0.14	0.1	0.11
Two Stage Least Squares Results				
Conterfactual insurance (eligibility limit - 4)	3757	0.18	-7.14	-0.01
	(8421)	(1.69)	(3.152)**	(0.10)
Observations	1039	1039	1039	831
Mean of dependent variable	20014	10.09	12.6	0.22

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: Tables show regressions as in Table 3, except that the "eligibility limit" for FONDEN aid is counterfactually supposed to be the actual eligibility limit less the number of hectares indicated in the row labels.
Standard errors clustered at the household level.

Table A1.8: Counterfactual RD Design, Labor and Migration

	Estimated days agricultural labor (total) (1)	Estimated days non-agricultural labor (total) (2)	Member migrates to US or Mexico (0-1 Indicator) (3)	Months worked as migrant in US (4)	Months worked as migrant in Mexico (5)
Eligibility According to Own Land					
Conterfactual insurance (eligibility limit - 1)	-3.49	5.00	0.05	0.32	0.08
	(4.65)	(4.50)	(0.027)*	(0.26)	(0.21)
Observations	3294	3294	3294	3294	3294
R-Squared	0.09	0.07	0.11	0.08	0.06
Two Stage Least Squares Results					
Conterfactual insurance (eligibility limit - 1)	-5.42	7.76	0.07	0.50	0.12
	(7.19)	(7.03)	(0.042)*	(0.40)	(0.32)
Observations	3294	3294	3294	3294	3294
Eligibility According to Own Land					
Conterfactual insurance (eligibility limit - 2)	1.27	8.98	0.05	0.08	0.36
	(3.67)	(4.455)**	(0.025)*	(0.22)	(0.189)*
Observations	3294	3294	3294	3294	3294
R-Squared	0.09	0.07	0.11	0.08	0.06
Two Stage Least Squares Results					
Conterfactual insurance (eligibility limit - 2)	1.87	13.26	0.07	0.12	0.54
	(5.44)	(6.638)**	(0.037)*	(0.32)	(0.280)*
Observations	3294	3294	3294	3294	3294
Eligibility According to Own Land					
Conterfactual insurance (eligibility limit - 3)	5.63	9.52	0.03	0.03	0.23
	(3.53)	(4.872)*	(0.02)	(0.19)	(0.20)
Observations	3294	3294	3294	3294	3294
R-Squared	0.09	0.07	0.11	0.08	0.06
Two Stage Least Squares Results					
Conterfactual insurance (eligibility limit - 3)	8.95	15.13	0.05	0.05	0.37
	(5.73)	(7.788)*	(0.04)	(0.29)	(0.32)
Observations	3294	3294	3294	3294	3294
Eligibility According to Own Land					
Conterfactual insurance (eligibility limit - 4)	8.95	4.82	-0.01	0.13	-0.32
	(3.847)**	(4.70)	(0.02)	(0.16)	(0.192)*
Observations	3294	3294	3294	3294	3294
R-Squared	0.09	0.07	0.11	0.08	0.06
Two Stage Least Squares Results					
Conterfactual insurance (eligibility limit - 4)	15.03	8.09	-0.02	0.22	-0.54
	(6.717)**	(7.95)	(0.04)	(0.27)	(0.328)*
Observations	3294	3294	3294	3294	3294
Mean of dependent variable	17.44	17.84	0.14	0.63	0.76

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: Tables show regressions as in Table 4, except that the "eligibility limit" for FONDEN aid is counterfactually supposed to be the actual eligibility limit less the number of hectares indicated in the row labels.

Standard errors clustered at the household level

Table A1.9: Agricultural Investment Results by Disaster Risk

	Imputed expenditure on plot inputs (until harvest) (1)	Hours of tractor use (until harvest) (2)	Hours of traction animal use (until harvest) (3)	Uses improved seeds (0-1 Indicator) (4)
	High Disaster Risk States			
	Panel A: Eligibility According to Own Land			
FONDEN eligible according only to land criteria (0-1 Indicator)	-3249.85	7.62	-8.72	0.10
	(5816.17)	(4.92)	(5.49)	(0.09)
Observations	598	598	598	460
R-Squared	0.05	0.22	0.15	0.2
	Panel B: Two Stage Least Squares Results			
FONDEN eligible (0-1 Indicator)	-4650.00	10.90	-12.48	0.16
	(8197.69)	(7.34)	(8.05)	(0.14)
Observations	598	598	598	460
Mean of dependent variable	14618	9.22	10.93	0.2
	Low Disaster Risk States			
	Panel A: Eligibility According to Own Land			
FONDEN eligible according only to land criteria (0-1 Indicator)	-5328.42	-0.66	-25.46	0.15
	(54330.66)	(7.57)	(25.41)	(0.18)
Observations	441	441	441	371
R-Squared	0.07	0.09	0.18	0.12
	Panel B: Two Stage Least Squares Results			
FONDEN eligible (0-1 Indicator)	-7027.11	-0.87	-33.58	0.19
	(71820.73)	(9.98)	(34.24)	(0.24)
Observations	441	441	441	371
Mean of dependent variable	27330	11.27	14.87	0.23

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: Panel A presents results from regressing the variable indicated in the column heading on an indicator that the household owns less land than the FONDEN eligibility limit. In Panel B, an indicator that the household is eligible for FONDEN aid according to all criteria (own less land than eligibility limit, some land is not irrigated, own less than 25 animal units) is instrumented by the indicator for owning less land than the limit. The regressions include eligibility limit fixed effects, household level covariates (the number of males 18 years or older, the number of females 18 years or older, the number of household members under 18 years old and a 4th order polynomial of total land owned by the household) and agricultural plot level covariates (dummies indicating plot slope and soil quality, an indicator for whether the plot is part of an ejido, plot area and plot area squared).

Standard errors are clustered at the household level.

Table A1.10: Migration Results by Disaster Risk

	Member migrates to US or Mexico (0-1 Indicator) (1)	Months worked as migrant in US (2)	Months worked as migrant in Mexico (3)
	High Disaster Risk States		
	Panel A: Eligibility According to Own Land		
FONDEN eligible according only to land criteria (0-1 Indicator)	0.07	0.96	-0.26
	(0.036)**	(0.282)***	(0.26)
Observations	1820	1820	1820
R-Squared	0.12	0.11	0.07
	Panel B: Two Stage Least Squares Results		
FONDEN eligible (0-1 Indicator)	0.14	1.81	-0.49
	(0.066)**	(0.566)***	(0.51)
Observations	1820	1820	1820
Mean of dependent variable	0.15	0.65	0.8
	Low Disaster Risk States		
	Panel A: Eligibility According to Own Land		
FONDEN eligible according only to land criteria (0-1 Indicator)	0.10	0.58	0.32
	(0.043)**	(0.351)*	(0.39)
Observations	1474	1474	1474
R-Squared	0.11	0.08	0.07
	Panel B: Two Stage Least Squares Results		
FONDEN eligible (0-1 Indicator)	0.13	0.76	0.42
	(0.057)**	(0.459)*	(0.51)
Observations	1474	1474	1474
Mean of dependent variable	0.13	0.6	0.71

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: Panel A presents results from regressing the variable indicated in the column heading on an indicator that the household owns less land than the FONDEN eligibility limit. In Panel B, an indicator that the household is eligible for FONDEN aid according to all criteria (own less land than eligibility limit, some land is not irrigated, own less than 25 animal units) is instrumented by the indicator for owning less land than the limit. The regressions include eligibility limit fixed effects, household level covariates (the number of males 18 years or older, the number of females 18 years or older, the number of household members under 18 years old and a 4th order polynomial of total land owned by the household) and individual level covariates (dummy for male, indicator variables for education level, age and age squared).

Standard errors are clustered at the household level.

Chapter 2

The Fruits of Usury: Interest Rate Regulation and Agricultural Sector Development in United States History

2.1 Introduction

In both developed and developing countries debate continues regarding the role of legislation in establishing the rate at which financial intermediaries can extend loans. Previous research on this topic generally supports the notion that less stringent financial regulation is associated with greater economic growth (see e.g. Temin and Voth, 2005; Benmelech and Moskowitz, 2007). This study builds on prior work by empirically considering the relationship between interest rate ceilings and characteristics of the agricultural sector in the nineteenth century United States. The value in focusing specifically on the agricultural sector derives from the current policy relevance of interest rate ceilings and financial regulation for developing countries, which remain largely agricultural. The relatively high rates of interest charged by some microcredit lenders, for example, has prompted calls to cap interest rates, and a number of governments,

including Indian state governments and that of Bangladesh, have responded to these pressures.[1] Although the culture and context clearly differ, isolating the impact of usury laws on nineteenth century U.S. agricultural sector development provides a valuable piece of evidence in the ongoing debate over such regulation.

Interest rate ceilings in the nineteenth century were comparatively low, and prior economic research indicates that these laws were in fact binding (Eichengreen, 1984; Snowden, 1988; Benmelech and Moskowitz, 2007; Rockoff 2003). Economic theory further suggests that even where the letter of the law was circumvented, usury laws will impact the terms of credit faced by borrowers (Blitz and Long, 1965). As Adam Smith eloquently put it "the debtor being obliged to pay, not only for the use of the money, but for the risk which his creditor runs by accepting a compensation for that use. He is obliged, if one may say so, to insure his creditor from the penalties of usury." Thus these laws may have has substantial economic effects.

Theoretical predictions of the effect of usury laws on agricultural development, however, are ambiguous. On the one hand, if the laws are passed with the public interest in mind, to limit the profits of a noncompetitive financial sector, farmers may be able to take advantage of cheaper capital and the agricultural sector would expand. Alternatively, if the maximum allowable interest rate is set below the rate required to make financing risky agricultural enterprises feasible, agriculture will tend to contract as interest rate ceilings are lowered (see e.g. Blitz and Long, 1965; Bodenhorn, 2005; Benmelech and Moskowitz, 2007).

In light of the theoretical ambiguity, I approach the question empirically. Using a panel data set, which controls for state and time specific influences, I document that less restrictive usury laws are correlated with higher growth rates of agricultural investment and with more widespread ownership of farms. By making use of within country variation in interest rate ceilings, I am able to control for national political and regulatory conditions, as well as other national institutions, which complicate cross-country analysis of the role of financial regulation in development. Moreover, the outcomes which I consider, investment in agricultural production and the ownership structure of farmland, are closely linked to development, allowing me to establish a direct link between interest rate regulation and agricultural sector development.

[1] The Indian state of Tamil Nadu enacted interest rate caps (Radcliffe and Tripathi, 2006). The microcredit regulatory authority of Bangladesh also limited the interest rates that microfinance institutions may charge (The Financial Express, Dhaka, April 29, 2009).

Since the determination of usury laws is potentially endogenous with respect to the development of the agricultural sector, I employ an instrumental variables strategy to capture the causal relationship between usury laws, investment and farm ownership. Motivated by the fact that justification for usury laws, which have existed for centuries, derives largely from religious or philosophical objections to charging interest on loans,[2] I instrument for states' maximum legal interest rates with the historical presence of churches in that state. In Section 2.2 I discuss the relationship between religion and politics in the nineteenth century United States and in Section 2.5 I assess the statistical association between the historical presence of churches and interest rate ceilings. The historical evidence indicates that the nature of this relationship varied over the course of the century and the statistical evidence confirms this suggestion; the results indicate that greater prevalence of religious bodies resulted in more stringent usury laws but the extent to which the historical presence of churches affected legislation varied over time. My empirical strategy exploits the combination of cross-sectional variation in the historical presence of churches and the time-varying influence of churches on usury laws.

In Section 2.6 I present the instrumental variables estimates. The results suggest a causal channel, consistent with the observed correlations, from more lenient usury laws to faster expansion of farmland, higher growth rates of investment in agricultural capital goods and a larger share of owner-operated farms, especially among small farms. Furthermore, the estimated magnitudes are economically significant, indicating that this form of financial regulation had a non-trivial impact on cross-state variation in the development of the agricultural sector and would be expected to hamper progress if enacted at a national level. A 1 percentage point decrease in the maximum allowable rate of interest, for example, is expected to reduce the proportion of owner-operated small farms by 4%.

This study compliments other work which focuses primarily on the political economy aspects of usury laws, notably Benmelech and Moskowitz (2007) and Rajan and Ramcharan (2008). These results are the flip-side of those discussed in Rajan and Ramcharan (2008), who contend that concentration in agricultural land holdings hampers financial development, leading to fewer banks per capita and more restrictive financial legislation. While the results presented

[2] Aristotle, for instance, wrote that "(of all bad forms of finance) there is none which so well deserves abhorrence as petty usury or interest." The Old Testiment disparages usury in several passages.

here are concerned with the reverse channel, from financial regulation to the characteristics of the agricultural sector, they are not inconsistent with those of Rajan and Ramcharan. It is entirely possible that there is simultaneous feedback from land concentration to financial regulation and from financial regulation to the ownership structure of farmland. The use of instrumental variables here is intended to quantify the importance of the later channel.

This study is also closely related to Benmelech and Moskowitz (2007), indeed it is a natural extension of their work. What distinguishes this study is the particular focus on agricultural development. Benmelech and Moskowitz (2007) are largely concerned with the political economy of interest rate regulation in the United States. While they offer evidence that less restrictive interest rate regulation is associated with greater economic growth, including agricultural output and the number of farms, this study considers investment in agriculture and the relationship between the terms of credit and the ownership structure of farmland, in particular the tenure status of farms. Furthermore this study incorporates an instrumental variables strategy in order to provide causal evidence on these relationships.

2.2 Empirical Strategy

The intent of this study is to understand how the imposition of statutory maximum interest rates affected investment in agriculture. Let I_{it} be the investment occurring in state i during period t, which corresponds to the years between Census observations (which occur each decade). The specific agricultural outcomes considered are investment in farm implements and/or machinery and investment in improving agricultural land. Letting r_{itj} be the maximum legal rate of interest in state i in year j of period t (for example, the interest rate in 1896 would be r_{i96}) and normalizing investment by the stock at the beginning of the period, Y_{it-1}, I posit that

$$\frac{I_{it}}{Y_{it-1}} = \mu + \boldsymbol{\beta} g(r_{it0,\ldots,}r_{it9}) + \boldsymbol{\delta}\mathbf{X}_{it} + \alpha_i + \alpha_t + \underbrace{h(\eta_{it0},\ldots,\eta_{it9})}_{\varepsilon_{it}} \tag{2.1}$$

Investment between census years is modeled as a function of the path of maximum legal interest rates in the years comprising the prior decade, time-varying characteristics of the state, $\mathbf{X}_{it}$, time-invariant state characteristics, α_i, a census year (decade) specific shock, α_t, and the

realization of state-year shocks over the years comprising the prior decade. In practice, I replace $g(r_{it0}, r_{it2}, ..., r_{it9})$ with $\overline{r}_{it} = \frac{1}{10}\sum_{j=0}^{9} r_{ij}$ or the average maximum legal rate of interest in that state over the 10 years prior to the indicated census year.

To construct $\frac{I_{it}}{Y_{it-1}}$ from the stock variables reported in the Census data, note that

$$\frac{I_{it}}{Y_{it-1}} \approx \ln\left(1 + \frac{I_{it}}{Y_{it-1}}\right) = \ln\left(1 + \frac{Y_{it} - Y_{it-1}}{Y_{it-1}}\right) = \ln\left(\frac{Y_{it}}{Y_{it-1}}\right) \tag{2.2}$$

Thus, in the empirical results which follow I estimate the following state-level equation

$$y_{it} - y_{it-1} = \mu + \beta\overline{r}_{it} + \boldsymbol{\delta}\mathbf{X}_{it} + \alpha_i + \alpha_t + \varepsilon_{it} \tag{2.3}$$

where y is the natural logarithm of Y. The inclusion of state-specific intercepts absorbs the effect of time-invariant state characteristics, such as climate and geography, on agricultural outcomes. Similarly, time fixed effects control for national trends, technological progress for example. Time-varying state level covariates, $\mathbf{X}_{it}$, are included to increase precision. In the main results $\mathbf{X}_{it}$ contains population,[3] which should be related to growth, and the percent of the population which is white, which may be particularly relevant given the pervasive racial discrimination of the era. Additional controls are introduced in Section 2.7.

This specification exploits time variation within states in the maximum average prior legal rate of interest. The estimate $\widehat{\beta}$ indicates the relationship between investment growth, relative to the state average growth rate, and the maximum prior average interest rate, relative to the average maximum rate over all years (average of averages). If $\widehat{\beta} > 0$ the implication is that decades where the maximum legal interest rate had been relatively high tended to coincide with decades of above average investment growth.

In addition to affecting the intensive margin of agricultural investment, usury laws may impact the extensive margin. In particular, it is possible that favorable financing terms makes staking a claim to one's own farm more attractive. Alternatively, interest rate ceilings may ration risky nascent farmers out of the credit market, or expensive debt may induce farmers to prefer equity-like contracts (such as sharecropping). These forces would tend to either help or

[3] While this specification suggests levels of population, the results are quite similar using the logarithm of population instead.

hinder the formation of new farms and may also alter the mix of farm tenure as existing farms change hands.

To investigate these impacts I estimate analogues of (2.3) where I replace $y_{it} - y_{it-1}$ with $f_{it} - f_{it-1}$, the growth rate of the number of farms, or with $\frac{F_{itj}}{F_{it}}$, the ratio of farms with tenure status j (owner-operated, sharecropped or cash tenant) to the total number of farms in the state in the indicated period.

State fixed effects control for any time-invariant factors which simultaneously affect the political forces determining financial legislation and the fortune of the agricultural sector. It is possible, however, that unobserved time varying factors which determine agricultural progress are correlated with usury laws, causing $\overline{r}_{it}$ to be correlated with ε_{it}. The strength of various political parties within a state, for example, may have implications for financial legislation and agriculture. Or if, for instance, farmers experience a negative shock to their livelihood, which impacts investment in machinery, expansion of improved acreage or the tenure mix of farms, it is plausible that a political reaction to tough times will lead to an endogenous change in the legal rate of interest. Such concerns are of particular relevance since the Populist Party was gathering strength over the sample period.[4] Moreover, Eichengreen (1984) notes that farmers advocated directly for interest rate limits in some instances and Rockoff (2003) suggests that Michigan altered its interest rate ceiling legislation to accommodate an agricultural land boom and an increased demand for mortgages.

As political economic forces, such as those discussed in Rajan and Ramcharan (2008) are likely to influence the regulation and strength of the financial sector (and the evidence suggests they do) as well as to impact outcomes such as the ownership structure of agricultural land, simultaneous determination of usury laws and agricultural growth or tenure is of concern. To provide causal evidence on the relationship between interest rate ceilings and agricultural sector development, I employ an instrumental variables approach. In particular, I instrument for $\overline{r}_{it}$ with the historical strength of religious bodies in state i. The choice of this instrument is

[4]Born in 1889 from the union of the Knights of Labor and the Farmers' Alliance (which formed in 1876), the Populists exerted considerable political influence in the later part of the nineteenth century: there were numerous Populist congressmen as well as 10 Populist governors elected between 1887 and 1901. Given that one of the Farmers' Alliance stated goals was to "unite the farmers of America for their protection against class legislation and the encroachments of concentrated capital," it is not surprising that their agenda included the establishment of low-interest financing sources to aid debt-burdened farmers.

motivated by the religious foundations of usury laws. Deuteronomy 023:019, for example, reads "Thou shalt not lend upon usury to thy brother; usury of money, usury of victuals, usury of any thing that is lent upon usury." To the extent that religious doctrines influenced voters, politicians and legislation, the prevalence of churches should be correlated with usury laws.

The exclusion restriction upon which the results rest is that the historical prevalence of religious bodies does not directly determine agricultural outcomes and is not correlated with unobserved factors which do. If religion was in fact correlated with entrepreneurial ability or an individual's propensity to work in the agricultural sector and if religious individuals tended to reside in states where churches historically had a strong presence, then religious variables would be correlated with agricultural outcomes through channels other than financial regulation. As there is *a priori* reason to believe that religious factors determine usury laws, but no particular reason I am aware of to believe that churches are expected to directly be related to agricultural outcomes, the results presented here are at least suggestive that there is a causal channel from usury laws to agricultural development. In Section 2.7, however, I consider the robustness of the results to this particular instrument.

Since the Catholic church tended to adopt a more stringent attitude towards usury (see e.g. Taeusch, 1942),[5] I allow the impact of the historical presence of Catholic and non-Catholic churches to have a differential effect on usury laws. Moreover, in light of historical evidence that the influence of religion on state legislation varied over the course of the nineteenth century, I allow the historical presence of each denomination to have a distinct effect on usury laws in each decade. Since the historical prevalence of churches in each state is collinear with state fixed effects, the use of this time variation in the impact of churches on usury legislation makes identification possible while controlling for time invariant state characteristics.

At a national level, there were various historical developments in the nineteenth century which tended to reduce religious sentiment and may have attenuated the relationship between the presence of churches and legislative outcomes. Following the Civil War, for example, the United States became increasingly urbanized; going from 6 to 40% urban population over the

[5] Taeusch discusses the evolution of Christian doctrine concerning the practice of interest taking. He documents early Christian opposition to lending on interest based on the writings of Aristotle and the Old Testament and notes the more permissive attitude towards usury taken by Protestants, an attitude which is succinctly summed up by John Calvin's comment that "Usury must be judged not by a particular passage of Scripture, but simply by the rules of equity."

nineteenth century. Additionally there was the publication of Darwin's theory of evolution in 1859.[6] There were also decade-specific events which would have affected the role of religion in political life. One such event was the passage of the Fourteenth Amendment to the U.S. constitution in 1868. The amendment made the First Amendment, related to the separation of church and state, binding for all states. This prevented states' constitutions from endorsing particular religions as some had done earlier in the century.[7] Some, however, felt that the Amendment did not go far enough in explicitly separating church and state and a campaign for an stronger amendment ensued.[8] Central to the agenda of those advocating for a separation amendment was to check the influence of religious morality on laws, and usury laws in particular. The National Liberal League, a group advocating for the amendment, wrote "Usury laws, in especial, which sometimes work great detriment to the business interests of whole communities, are in fact based upon the Bible conception that it is a crime to take interest for money loaned; although the common sense of mankind rejects the notion in fact."[9] This movement for a constitutional amendment to explicitly separate church and state appeared to reach its apogee around 1880, at which point momentum for the amendment receded.[10]

Given that the stance adopted towards separation of church and state by the Catholic church differed from that of other denominations, these developments may have had different implications for the effect of Catholic churches on politics. Moreover there were distinct phenomenon, particular to the Catholic church, which counterbalanced the attenuating effects of other events on the relationship between religion and legislation. Separation of church and state, for instance, was explicitly condemned by the Pope in 1864.[11] Furthermore, the

[6] Boyer, Paul. The Oxford Companion to United States History, 2001. Viewed at: http://www.encyclopedia.com/doc/1O119-Secularization.html on June 14, 2009.

[7] Tussman; xiv.

[8] In his 1875 message to Congress, President Grant advocated an amendment which would declare "Church and State forever separate and distinct." The message further stressed limiting the political influence of the Catholic Church by advocating that states provide public education and that tax revenues not go to fund schools with religions affiliation (Hamburger; 322-323).

[9] Report of the Centennial Congress of Liberals; July 4, 1876.

[10] In 1880 the National Liberal Party fielded a presidential candidate running on the platform of total separation of church and state. Although the issue had national appeal, the party had limited success and, after the election and a 1879 Supreme Court ruling which considered interpretation of the First Amendment and explicitly referenced separation for the first time (Tussman; 20), amendment advocates shifted their energy towards interpreting the existing text of the constitution as being consistent with their cause (Hamberger; 326-328, 334).

[11] Pope Pius IX issued the *Syllabus of Errors* in 1864 which condemned the notion of separation. *The Syllabus*, Pope Pius IX. Viewed at http://www.ewtn.com/library/PAPALDOC/P9SYLL.HTM June 14, 2009.

changing profile of immigrants in the late nineteenth century plausibly altered the influence of the Catholic church on legislative outcomes in those decades. In particular, the composition of immigrants to the United States changed noticeable around 1880 as increased numbers of individuals from southern and eastern Europe migrated to the U.S., resulting in a growing number of Catholic immigrants; around 3 million between 1870 and 1900.[12] To the extent that religion influenced political opinions, the flow of Catholic immigrants may have affected legislative outcomes, especially in light of evidence that religion and nationality played a role in machine politics in the nineteenth century.[13] Indeed, the wave of Catholic immigration prompted the rise of nationalist groups in the late 1880s and 1890s which sought to limit the influence of Catholicism on politics.[14] It is therefore reasonable to suppose that the historical presence of the Catholic church had an impact on future financial regulation which differed from that of non-Catholic churches.

In light of the historical developments which affected the relationship between religion and legislation, I allow the historical strength of Catholic and non-Catholic churches to have a differential effect on state's usury laws in future decades. In particular the first stage specification employed in the result presented below is

$$\overline{r}_{it} = \mu + \sum_{t=\tau+1}^{T} \pi_{1t} D_t C_{i\tau-1} + \sum_{t=\tau+1}^{T} \gamma_{1t} D_t NC_{i\tau-1} + \delta \mathbf{X}_{it} + \alpha_i + \alpha_t + \varepsilon_{it} \tag{2.4}$$

where $C_{i\tau-1}$ and $NC_{i\tau-1}$ are the number of Catholic and non-Catholic churches per capita in state i in the Census period prior to the base period τ. These variables are interacted with time dummies which allows for the initial presence of Catholic and non-Catholic churches to have a differential effect on financial legislation in each future decade. Since the full set of interaction terms are collinear, I omit the base year, τ, interaction term which gives $\boldsymbol{\pi}$ and $\boldsymbol{\gamma}$ the interpretation of the change in the slope of the estimated effect of historical church presence on current usury laws relative to the effect in the base year. To make full use of the available sample I take the base year to be the first decade for which I observe the agricultural outcome

[12] Gutfeld; 108. Noll; 287.
[13] Noll; 309.
[14] Hamberger; 366.

of interest.[15] This specification exploits cross-sectional variation in the initial pervasiveness of Catholic and non-Catholic churches combined with temporal variation in the influence of religious organizations on legislative outcomes to isolate exogenous variation in $\overline{r}_{it}$.

Although legislation is more directly determined by the number and preferences of constituents, as opposed to the number of religious bodies, an instrument such as the number of Catholics in a given state would be problematic since it may be driven by factors which also determine agricultural development. A pre-determined measure of the local presence of churches does not suffer from this concern and is also predictive of the future strength and influence of particular denominations to the extent that immigrants locate in culturally similar enclaves (Card, 2001).

2.3 Data

The nineteenth century United States represents a fruitful setting to examine the relationship between financial institutions, such as usury laws, and the growth and characteristics of the agricultural sector. For one thing, this period was characterized by substantial expansion of the agricultural sector; from 1860 to 1910 the number of farms tripled and acreage in farms increased sizably. Additionally the era was marked by the invention of numerous pieces of agricultural equipment, including the reaper and thresher as well as mechanical planters, huskers and manure spreaders.[16] Thus the potential for expansion and investment by farmers was large and financing was required to realize this potential. Table 2.1 quantitatively describes this growth; improved acres of farmland[17] grew at a continually compounded average annual rate of 4% while the corresponding rate for the value of equipment and machinery was 5%. The table also indicates the average breakdown of farm tenure status. The majority (76%) of farms in this period were owner-operated, but this was less true for small farms, of which 68% were

[15] As indicated in the discussion of the data below, τ is 1860 for the investment outcomes and the growth of farms and 1880 for tenure outcomes.

[16] U.S. Department of State: Publication from the Bureau of International Information Programs. An Outline of American History: Agrarian Distress and the Rise of Populism. http://www.humanitiesweb.org/human.php?s=s&p=h&ID=798

[17] The 1880 Census (Statistics of Agriculture) describes improved acres as "Tilled, including fallow and grass in rotation (whether pasture or meadow). Permanent meadows, permanent pastures, orchards, and vineyards." Conversely, unimproved land is defined as "Woodland and forest. Other unimproved, including 'old fields' not growing wood."

owner-operated. Sharecropping, as opposed to cash tenancy, was the more prevalent tenure status for remaining farms.

The table also shows the mean, 9%, and standard deviation, 0.03, of maximum legal interest rates for the sample period. One thing to note about these figures is that they are quite low compared to the rates which currently spark debate over interest rate regulation. Microfinance institutions, for example, often charge as much as 50% interest (Morduch, 1999) and current proposals discuss capping rates at double digit levels. Although inflation was generally low throughout the sample period, with the exception of a large inflationary spike during the Civil War years (Hanes and James, 2003), these laws mandated relatively low real interest rates and thus may have had substantial economic impact.

While they indicate the general level of interest rate ceilings, the full sample mean and standard deviation mask the considerable variation in states' interest rate ceilings. Not only did states differ from one another in their usury laws, there was variation of states' usury laws over time. To better illustrate the evolution of interest rate ceilings, Figure 2.1 graphically shows the cross-sectional and inter-temporal variation in usury laws. The data used to generate these images, and in the analysis, originates from Holmes (1892) and was compiled by Benmelech and Moskowitz (2007), who graciously shared this data.

I matched the maximum legal interest rates with Census data from the Inter-university Consortium for Political and Social Research.[18] I omit from this data states for which I have 2 or fewer observations (Alaska, Hawaii and Oklahoma). These data derive from the Censuses of Population, Agriculture and Religious Bodies for the years 1850 through 1900. The Census of Agriculture recorded the agricultural investment outcomes of interest, acres of improved farmland and the value of farm equipment and machinery for the entire sample period but only documented farm tenure status for a limited number of years. In particular tenure status of farms is observed in 1880, 1890 and 1900. Tenure is broken out by farm size, however, only in 1880 and 1890. For apparently unknown reasons the Census of Religious Bodies of 1880 was not published and is presumed lost (Finke and Stark, 2005; Engelman, 1935), thus the data does not include figures related to the existence of churches in that decade.

[18] Historical, Demographic, Economic, and Social Data: The United States, 1790-2000, authored by Michael R. Haines.

2.4 OLS Estimates of the Relationship between Usury Laws and Agricultural Outcomes

As an initial look at the relationship between usury laws and the development of the agricultural sector, I employ a simple OLS fixed effects specification. Panel A of Table 2.2, which estimates equation (2.3), takes a naive look at the relationship between usury laws and the growth rate of agricultural investment and farmland. The coefficient on the average maximum legal interest rate over the prior decade indicates that a 1% interest rate increase is associated with a 1.5% increase in the growth rate of improved acres of farmland and a similar increase in the growth rate of the value of farm equipment and machinery. This association is statistically significant at the 1% confidence level.

With respect to farm outcomes, Table 2.3 shows that more permissive usury laws are associated with a higher rate of growth in the number of farms and suggests that such laws also tend to be associated with greater ownership of farms. The point estimates on the interest rate variable are positive for the percent of owner-operated farms and negative for the percentage of sharecropped farms, but the estimates are only statistically distinguishable from zero in the case of the percentage of sharecropped farms. I separately consider the tenure status of small farms. Since data on the tenure and size of farms was collected only in a few Census years, these regressions are performed on a limited sample. The fixed effects specification fails to detect any relationship between interest rate ceilings and the percentage of sharecropped or cash tenant farms but the results indicate a statistically significant and positive correlation between the maximum legal rate of interest and the percentage of owner-operated farms.

As noted above, these estimates do not necessarily represent the causal impact of interest rate regulation on the agricultural sector if there are time-variant omitted factors which simultaneously determine usury laws and agricultural outcomes. To provide evidence of a causal channel, I isolate the variation in usury laws associated with the historical presence of Catholic and non-Catholic churches and use this variation to establish the relationship between interest rate ceilings and agricultural sector characteristics.

2.5 Usury Laws and the Historical Presence of Religious Bodies

Table 2.4 presents an initial look at the relationship between the presence of churches and usury laws. The table shows the results of a regression of the average maximum legal interest rate over a given decade on the number of Catholic and non-Catholic churches per capita in that state at the beginning of the decade. Column 1 displays the results including both church variables, while columns 2 and 3 include each separately. As would be expected the point estimates are negative, indicating that states which had more churches per capita at the beginning of the decade tended to have lower (more restrictive) interest rate caps over the following decade. Moreover, the results are suggestive of a differential relationship for Catholic and non-Catholic churches. The coefficient on Catholic churches is larger in magnitude, as predicted based on the more stringent attitude towards usury adopted by the Catholic church. To put these figures in perspective, a one standard deviation increase in non-Catholic churches per capita would be associated with a 0.3% decrease in the maximum legal rate of interest. The corresponding figure for Catholic churches is 4%.

To understand how the effect of the historical presence of churches on usury laws varied over time, Figure 2.2 shows the coefficients from a regression of the maximum legal rate of interest on interactions between Census year dummies and Catholic and non-Catholic churches per capita in 1850, the first year in which these variables are observed. The top panel displays the raw point estimates and the lower panel displays coefficients from a normalized regression where the church variables are centered at zero and scaled to have a standard deviation of 1. Considering the estimates from 1870, for example, the results indicate that the estimated relationship between non-Catholic churches and usury laws is approximately the same in 1870 as in 1850 but that the relationship between Catholic church presence and interest rate caps is steeper in 1870 than in 1850; a 1 standard deviation increase in Catholic churches per capita in 1850 would be associated with a 1% lower interest rate ceiling in 1870 that in 1850.

Interpreting the signs and magnitudes of the coefficients through the lens of the historical evidence discussed above reveals that the statistical information is broadly consistent with the qualitative discussion. The coefficient on the 1880 interaction term, for both Catholic and non-Catholic churches, is positive, which would suggest that the negative slope describing the relationship between the presence of churches and maximum legal interest rates is less steep in

1880 than it was in 1850, as might be expected given the passage of the 14th amendment and push for greater separation of church and state from 1870 to 1880. For non-Catholic churches the coefficients remain positive in 1890 and 1900, again indicating an attenuated relationship between religion and usury legislation and consistent with growing secularization. For Catholic churches, however, the coefficients on the 1880 and 1890 interaction terms are slightly negative but fairly small, which can be interpreted as implying that the estimated relationship between the presence of Catholic churches per capita and interest rate caps is approximately the same in the late nineteenth century as it was in 1850. To the extent that migration from Catholic countries counterbalanced other historical events in terms of the influence of the Church on political outcomes, such an implication accords with the historical record.

The first stage results, presented in Table 2.5, demonstrate that the church variables have predictive power for future interest rate regulation in each of the time periods considered in this study. Each column of the table is a separate regression of equation (2.4) for a sub-sample corresponding to the period for which a particular outcome is observed. The first column corresponds to the sample for which agricultural investment outcomes are observed and includes all relevant church-by-time interaction terms as regressors, the second and third columns present similar regressions for the periods in which farm tenure and farm size and tenure are available. The coefficients in these regression should be interpreted as the effect of the presence of historical churches per capita on interest rate ceilings relative to the effect in the initial decade. In each regression an F-test that the instruments have zero effect is rejected above a 5% confidence level.

2.6 IV Estimates of the Impact of Usury Laws on Agricultural Outcomes

The instrumental variables estimates for agricultural investment are show in Panel B of Table 2.2. The point estimates are larger that in the OLS specification, implying that a 1% increase in the ceiling is associated with 2 and 4% changes in the growth of improved acres of farmland and the value of farm equipment and machinery, respectively. The estimates are again significant at the 1% confidence level. These estimates also represent a nontrivial economic effect; the point

estimates imply that a 1% increase in the maximum legal interest rate would be associated with a 0.25 to 0.5 standard deviation increase in the growth rate. Put another way, a 1% decrease in the maximum legal interest rate would be expected to lower the value of farm equipment and machinery in the median state by about $200,000 or about $5 per farm, representing 4% of the average such investment per farm.[19]

That the instrumental variables estimates are larger is intuitive; if stringent usury laws were a (perhaps misguided) policy designed to aid struggling farmers, one might expect that states which had low limits would pursue other pro-agriculture policies, augmenting agricultural growth and lessening the correlation between low interest rate ceilings and low growth. Additionally, the fixed effects specification may suffer from attenuation bias due to the difficulty in measuring these variables and the challenges of data collection in the nineteenth century. As the instrumental variables specification mitigates attenuation bias, the coefficients will be larger.

Since access to credit and the terms of loans are likely to affect farmers' ability to acquire mortgages as well as expansion and investment decisions, I present parallel results looking at the growth of farms and tenure status in Panel B of Table 2.3. The first column, which considers the growth rate of the number of farms, indicates that more lax usury laws contributed to agricultural sector growth along the extensive margin. The results, significant at a 1% confidence level, suggest that a 1% increase in interest rate caps leads to a 2% increase in the growth rate of farms. With respect to the tenure status of farms, columns 2-4, the results are generally not distinguishable from zero, with the exception of the percentage of sharecropped farms, in which case the point estimate indicates that a 1% increase in the maximum interest rate results in a 1% decline in the percentage of sharecropped farms.

Based on the hypothesis that small farms in particular will have difficulty obtaining credit and will be the first to be rationed if rationing occurs, I also consider the tenure of small farms separately in columns 5-7. The instrumental variables estimates indicate that higher interest rate ceilings lead to a higher percentage of owner-operated farms and fewer cash tenants or sharecroppers; approximately 4% more owner-operated farms, significant above a 5% confidence

[19]The median value of farm equipment and machinery is $ 5,169,037 per state, while the median number of farms per state is 41,964, yielding $123 per farm. The estimated impact of a 1% change in the interest rate limit is $5,169,037*0.0384=198,491, or $4.73 per farm.

level, and 2% fewer sharecropped farms, significant above a 10% confidence level. Again, the estimated effects are of considerable economic magnitude; representing 7 and 11 percent of the sample mean for these outcomes. To put this effect in context, a 4% increase is equivalent to an additional 284 small owner-operated farms in the median state, which is the same magnitude as the median number of sharecropped small farms in a state.

These results confirm the evidence in Benmelech and Moskowitz (2007) that less restrictive usury laws encouraged growth in the number of small farms in particular. The analysis presented here goes a step further by demonstrating that this growth did in fact represent an expansion of farm ownership as opposed to subdivision and cash tenancy or shifts towards equty-like contracts such as sharecropping. Moreover, the instrumental variables approach employed here provides evidence that there was a causal channel running from interest rate regulation to the ownership structure of agricultural land.

2.7 Robustness

The main results presented above suggest that higher maximum legal rates of interest are associated with higher growth rates of agricultural investment and made ownership possible for small farmers, leading to a higher percentage of owner-operated farms. In the following section I examine the robustness of the results to potentially confounding factors.

2.7.1 Robustness of the Instrument

While the historical and statistical evidence indicates that the number of religious accommodations influenced future usury laws there were undoubtedly other important factors. Benmelech and Moskowitz (2007) emphasize political economy considerations[20] and, in particular, posit that states' were constrained in the maximum legal interest rate they could impose by the laws

[20]In their discussion of the political economic determinates of usury laws, Benmelech and Moskowitz (2007) contend that religious intensity was not a main factor. Using a cross section from 1850, they report a negative effect of the number of churches and, when Catholic churches are included simultaneously, a positive effect of the number of Catholic churches in particular. This estimate, however, comes from a small cross-sectional sample of states in one Census year. My first stage results, using a panel of data, imply a different and statistically significant relationship between religious factors and usury laws. This is not to say that the political economy considerations discussed in Benmelech and Moskowitz (2007) are unimportant, indeed they may be the main determinates of usury laws, but to the extent that religious variables are partially correlated with maximum legal rates of interest, and arguably do not directly affect agricultural outcomes, they should be valid instruments.

of neighboring states, presumably owing to the need to compete for capital flows. Thus, an alternative variable which should predict the highest legal interest rate in a given state is the average allowable rate in adjacent states. The top panel of Table 2.6 demonstrates that this is the case. The table shows results from a regression of a state's average maximum legal interest rate over a given decade on the average legal rate in bordering states over the same decade and, to allow for delays in legislative adjustment, the average legal rate in adjacent states over the previous decade. The first column corresponds to a regression using the sample period for which the agricultural investment outcomes are available, the second to the sample for which tenure outcomes are observed and the third to the sample for which tenure by size of farm was recorded. As would be predicted by the hypothesis of competition for capital flows, the coefficients on the allowable rates in adjacent states are positive, indicating that states which bordered other states with permissive usury laws tended to adopt such laws themselves.

Although these variables strongly predict usury laws, they are not necessarily valid instruments for an instrumental variables specification. In particular, the exclusion restriction, that financial regulation in neighboring states does not impact economic outcomes such as agricultural development in the bordering state except through the influence on the state's own legislation is tenuous. It is plausible that time-varying, region specific factors, weather for example, might affect agricultural development and generate political legislative responses. Due to these confounding factors I use the predetermined historical presence of churches as an instrument, but as a robustness check I replicate the main results using adjacent states' usury laws as instruments. This specification relates variation in agricultural outcomes to the component of usury laws associated with the variation in neighboring states' laws. Panel B of Table 2.7 shows the results for investment outcomes. Comparison with Panel A, which replicates the main results, reveals that the estimates are quite similar regardless of the choice of instruments.

Using adjacent states' rates as the set of instruments changes the point estimates more substantially when considering farm tenure outcomes, as is done in Table 2.8. For example the coefficient on the interest rate ceiling variable when taking the percentage of small owner-operated farms as the dependent variable drops from 4% to 3%. But the estimates are qualitatively very similar, implying that states with higher interest rate caps had more

owner-operated and fewer sharecropped or cash tenant farms, especially for small farms.

While the fundamental rationale for using historical church variables as instruments is discussed above and the choice to interact these variables with time is motivated by further historical evidence that the effect of religious intensity on political outcomes varied over time, I check the robustness of the results to an alternative functional form. In particular, in Panel C of Tables 2.7 and 2.8, I instrument state's average maximum legal interest rate over a given decade with the value of non-Catholic and Catholic churches per capita at the beginning of that decade. There are several limitation to this specification, leading me to prefer the specification used in the main results. For one thing, using lagged, rather than predetermined, variables with state fixed effects requires the additional assumption that church variables are uncorrelated with the error term in all decades. In other words, this specification assumes that usury laws did not factor into Catholics' and members of other denominations decision of where to reside. Moreover, due to the missing Census of Religious Bodies in 1880, the sample is reduced in this specification and, since this missing data overlaps with the years in which farm tenure by size was collected, it is impossible to consider the tenure status of small farms using this set of instruments. In spite of the reduced sample size, Panel B of Table 2.6 indicates that lagged values of churches per capita strongly predict future interest rate limits. As expected, higher levels of churches per capita are associated with lower interest rate caps. Panel C of Tables 2.7 and 2.8 provide the instrumental variables estimates for this specification, demonstrating that the results are not an artifact of the choice of instruments. The magnitude of the point estimates increases somewhat relative to the main set of estimates but the results are generally similar in terms of size and statistical significance.

2.7.2 Additional Controls

The instrumental variables strategy is designed to mitigate the confounding influence of omitted variables which are correlated with both agricultural sector development and usury laws, but there are various other determinates of agricultural development. Conditioning on such variables can increase power and assess the robustness of the results.

For one thing, as noted by Benmelech and Moskowitz (2007), states which formally joined the Untied States in later years tended to impose relatively high maximum interest rate limits.

Since such states may also be expected to exhibit higher rates of growth, or more owner-friendly agricultural land markets, the "age" of a state (current year less the year when the area became an official state) may be one such important factor. State age, however, is a linear function of state and time fixed effects, which are included in this study. Thus the results account for differential ages of states.

The importance of manufacturing in a given state represents another potentially important variable. Since industrial development and the fortunes of the agricultural sector are linked it may be important to condition on manufacturing variables. Because these variables are plausible outcomes of the estimating equation it is tenuous to include them on the right hand side, leading me to omit them in the main results. But as a robustness check, I assess the sensitivity of the results for the more important outcomes identified above (growth of improved acres, investment growth and the tenure mix of small farms) to different sets of covariates in Tables 2.9 and 2.10. In various specifications I drop all controls except for state and time fixed effects, incorporate population controls and finally control for the size of the manufacturing sector (manufacturing jobs per capita and capital in manufacturing per capita). As is evident from these tables, the results are largely insensitive to such controls.

2.8 Conclusion

Although the debate over interest rate regulation continues in many countries, particularly developing countries, the economic impacts of such laws are uncertain. This study specifically investigates the effect of usury laws on the agricultural sector, which remains important in developing countries, by considering the historical example of usury laws in the nineteenth century United States.

Building on prior work which examines usury laws in the United States (Benmelech and Moskowitz, 2007; Rajan and Ramcharan, 2008) I confirm that less restrictive legislation was associated with greater economic growth. I extend previous research by focusing specifically on the relationship between interest rate ceilings and investment growth in agriculture and the ownership structure of agricultural land. Moreover, this study augments earlier work by providing evidence that there is a causal channel running from financial regulation to agricultural

sector development; using an instrumental variables strategy to isolate exogenous variation in usury laws these results indicate that higher interest rate ceilings lead to accelerated investment in agriculture and to a greater prevalence of owner-operated small farms at the expense of cash tenant or sharecropping arrangements. The economic magnitude of the estimated effects suggest that usury laws were a non-trivial determinant of investment and the ability of small farmers to stake their own farms.

Although contexts clearly differ across time and space, these results can inform current debates over interest rate regulation by suggesting that the imposition of strict interest rate ceilings may slow investment and can have detrimental impacts on the prospects of aspiring small scale farmers.

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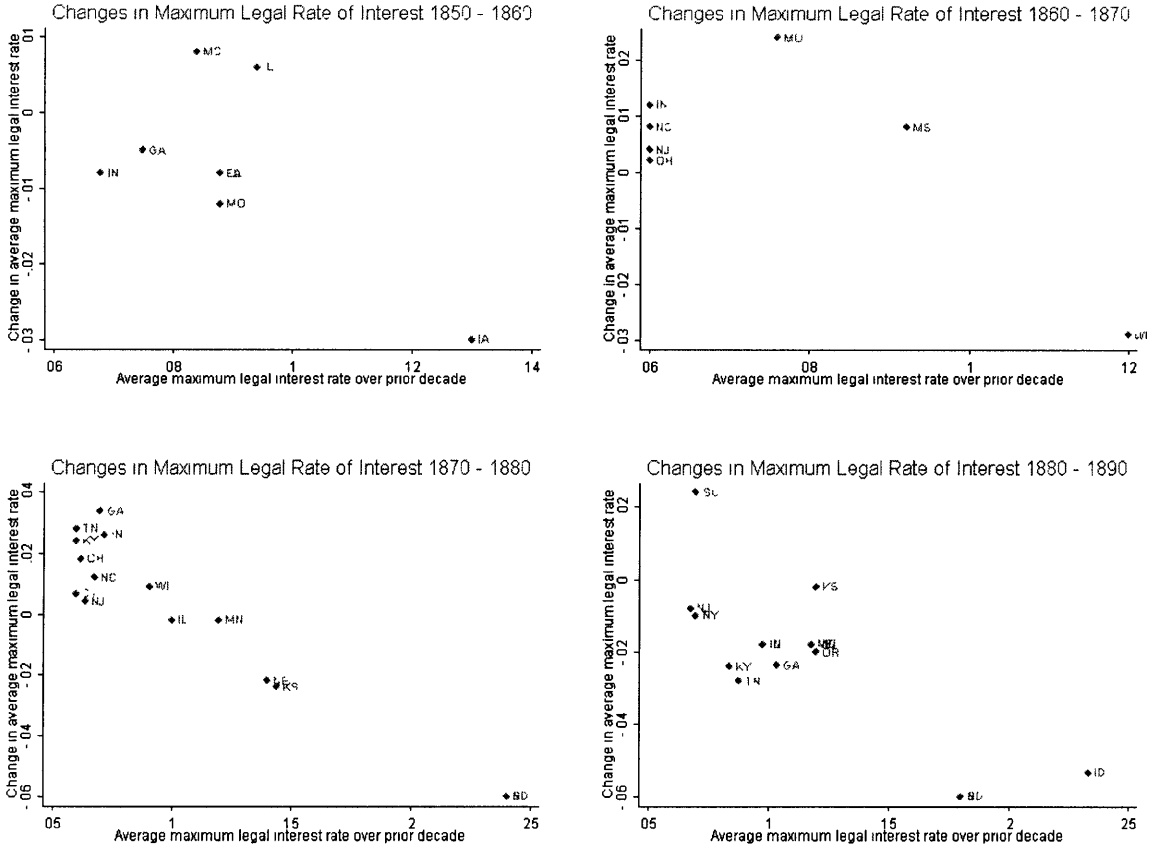
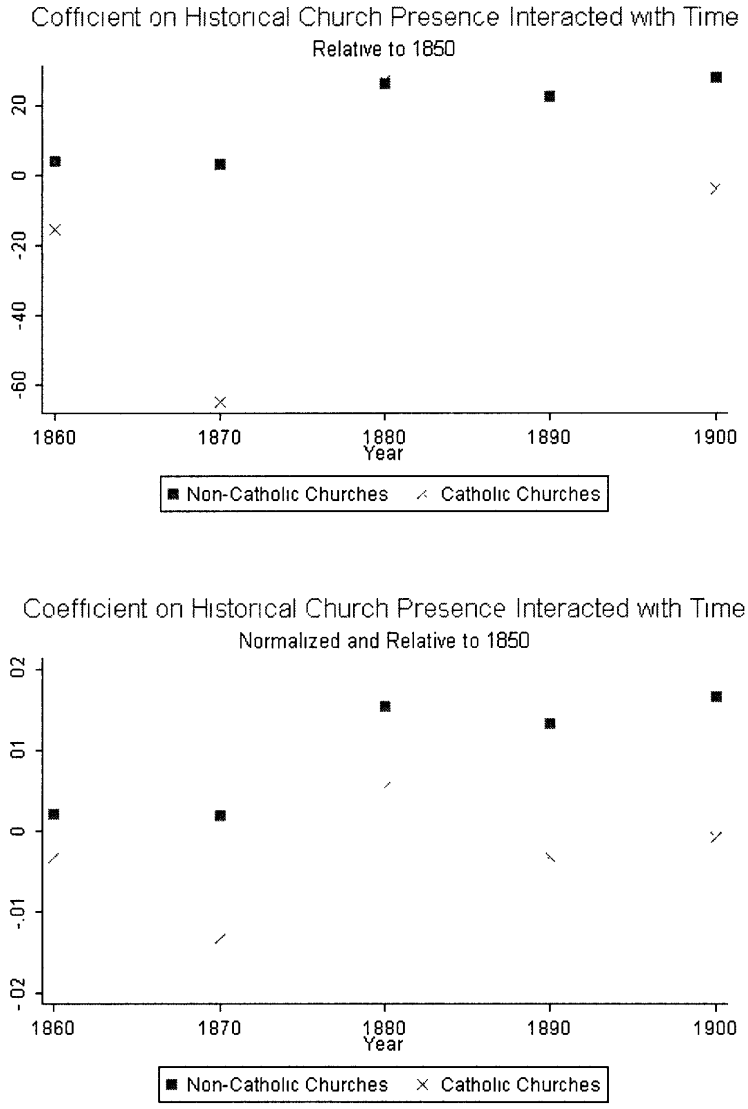
Figure 2.1: State Level Changes in Usury Laws Over the Nineteenth Century



Notes: The y-axis shows the difference between the average maximum legal rate of interest over a given decade and the average maximum legal rate over the prior decade. This difference is plotted against the average maximum legal rate in the prior decade (x-axis). States for which the average maximum legal rate of interest did not change are omitted.

Figure 2.2: Historical Presence of Churches and Usury Laws



Notes: The top image plots the coefficients from a regression of the maximum legal rate of interest in a given state on the number of Catholic and non-Catholic churches per capita in that state in 1850 interacted with Census year dummies. The x-axis indicates which Census year dummy is included in the interaction term. The bottom image plots similar coefficients where the church variables have been standardized to have zero mean and a standard deviation of 1.

Table 2.1: Descriptive Statistics

	Observations	Mean	sd
Maximum legal interest rate (avg. prior decade)	197	0.09	0.03
Annual Growth Rate of Improved Acres of Farmland	214	0.04	0.08
Annual Growth Rate of Value of Equipment and Machinery	214	0.05	0.07
Annual Growth Rate of Number of Farms	214	0.04	0.06
Percent owner operated farms	140	0.76	0.16
Percent sharecropped farms	140	0.15	0.1
Percent tenant farms	140	0.09	0.07
Percent small owner operated farms	93	0.68	0.26
Percent small sharecropped farms	93	0.18	0.19
Percent small tenant farms	93	0.14	0.09
Number of Non-Catholic churches in state (Per 1,000 people)	164	1.82	0.89
Number of Catholic churches in state (Per 1,000 people)	164	0.18	0.27
State population (millions)	262	1.07	1.16
White Population (%)	262	0.86	0.18
Manufacturing Employment (Jobs Per Capita)	260	0.05	0.05
Capital in Manufacturing ($1K Per Capita)	260	0.06	0.07

Notes: The table shows selected summary statistics. The unit of observation is a U.S. state. These statistics are generated using data spanning from 1850-1990, as described in Section 2.3.

Table 2.2: Effect of Usury Laws on Agricultural Investment

	Annual Growth Rate of Improved Acres of Farmland (1)	Annual Growth Rate of Value of Equipment and Machinery (2)
Panel A: OLS		
Maximum legal interest rate (avg. prior decade)	1.56	1.65
	(0.20)***	(0.28)***
State population (millions)	-0.01	-0.01
	(0.01)	(0.01)
White Population (%)	0.28	0.51
	(0.15)*	(0.21)**
Observations	163	163
R-Squared	0.57	0.34
Panel B: IV		
Maximum legal interest rate (avg. prior decade)	2.32	3.84
	(0.58)***	(0.99)***
State population (millions)	0.00	0.00
	(0.01)	(0.01)
White Population (%)	0.05	0.21
	(0.15)	(0.25)
Observations	144	144
Mean of dependent variable	0.02	0.03

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: Panel A shows the results from a regression of the variable indicated in the column heading on the maximum legal interest rate in that state. Panel B shows the results where the maximum legal interest rate is instrumented with the number of Catholic and non-Catholic churches per capita in 1850 interacted with Census year dummies. These specifications include observations in each Census year from 1860 to 1900.

All regressions include state and year fixed effects.

Table 2.3: Effect of Usury Laws on Farm Tenure Status

	Annual Growth Rate of Number of Farms (1)	Percent owner operated farms (2)	Percent sharecropped farms (3)	Percent tenant farms (4)	Percent small owner operated farms (5)	Percent small sharecropped farms (6)	Percent small tenant farms (7)
				Panel A: OLS			
Maximum legal interest rate (avg. prior decade)	1.35	0.58	-0.74	0.14	1.72	-0.83	-0.90
	(0.17)***	(0.40)	(0.37)**	(0.35)	(0.75)**	(0.60)	(0.65)
State population (millions)	-0.01	0.00	0.00	0.00	0.01	-0.04	0.03
	(0.01)	(0.01)	(0.01)	(0.01)	(0.05)	(0.04)	(0.04)
White Population (%)	0.18	0.84	-0.33	-0.41	1.67	-0.98	-0.69
	(0.13)	(0.30)***	(0.28)	(0.27)	(0.74)**	(0.59)	(0.64)
Observations	163	100	100	100	66	66	66
R-Squared	0.54	0.69	0.41	0.43	0.35	0.49	0.16
				Panel B: IV			
Maximum legal interest rate (avg. prior decade)	2.08	-0.29	-1.21	0.78	4.10	-2.44	-1.65
	(0.52)***	(0.80)	(0.72)*	(0.69)	(1.81)**	(1.39)*	(1.37)
State population (millions)	-0.01	0.00	0.00	0.00	0.02	-0.05	0.03
	(0.01)	(0.01)	(0.01)	(0.01)	(0.06)	(0.04)	(0.04)
White Population (%)	-0.08	0.77	-0.40	-0.38	2.56	-1.59	-0.97
	(0.13)	(0.33)**	(0.30)	(0.29)	(1.05)**	(0.80)**	(0.79)
Observations	144	98	98	98	65	65	65
Mean of dependent variable	0.03	0.72	0.18	0.09	0.63	0.23	0.15

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: Panel A shows the results from a regression of the variable indicated in the column heading on the maximum legal interest rate in that state. Panel B shows the results where the maximum legal interest rate is instrumented with the number of Catholic and non-Catholic churches per capita in 1850 (column 1) or 1870 (columns 2-7) interacted with Census year dummies. Column 1 includes observations in each Census year from 1860 to 1900, columns 2-4 include observations in each Census year from 1880 to 1900 and columns 5-7 include observations in each Census year from 1880 to 1890.
All regressions include state and year fixed effects.

Table 2.4: Relationship Between Church Presence and Usury Laws

	Maximum legal interest rate (avg. prior decade)		
	(1)	(2)	(3)
Lagged Non-Catholic Churches (Per Capita)	-1.64	-2.62	
	(1.84)	(2.24)	
Lagged Catholic Churches (Per Capita)	-150.88		-152.5
	(22.58)***		(22.48)***
Observations	130	130	130
R-Squared	0.48	0.21	0.47
Mean of dependent variable	0.08	0.08	0.08

* Significant at the 10% confidence level
** Significant at the 5% confidence level
*** Significant at the 1% confidence level
Notes: The table shows results from regressing the average maximum legal rate of interest in a given state over a given decade on the number of churches per capita in that state at the beginning of the decade.
All regressions include state and year fixed effects.

Table 2.5: First Stage Relationship Between Historical Church Presence and Usury Laws

	Maximum legal interest rate (avg. prior decade)		
	(1)	(2)	(3)
Non-Catholic Churches (per capita 1850) X 1(1870)	-0.23		
	(7.26)		
Non-Catholic Churches (per capita 1850) X 1(1880)	22.69		
	(7.61)***		
Non-Catholic Churches (per capita 1850) X 1(1890)	19.32		
	(7.53)**		
Non-Catholic Churches (per capita 1850) X 1(1900)	25.01		
	(7.62)***		
Catholic Churches (per capita 1850) X 1(1870)	-51.86		
	(53.17)		
Catholic Churches (per capita 1850) X 1(1880)	40.27		
	(54.21)		
Catholic Churches (per capita 1850) X 1(1890)	-1.65		
	(51.16)		
Catholic Churches (per capita 1850) X 1(1900)	10.42		
	(51.17)		
Non-Catholic Churches (per capita 1870) X 1(1890)		4.74	2.41
		(4.70)	(6.61)
Non-Catholic Churches (per capita 1870) X 1(1900)		8.06	
		(4.83)	
Catholic Churches (per capita 1870) X 1(1890)		-72.05	-76.72
		(26.04)***	(35.49)**
Catholic Churches (per capita 1870) X 1(1900)		-66.19	
		(26.47)**	
F-test: Instruments	5	5	4
P-value: F-Test	0	0	0.03
Observations	144	98	65
R-Squared	0.41	0.5	0.5
Mean of dependent variable	0.08	0.09	0.09

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: The table shows results from a regression of the maximum legal rate of interest in a state on the number of Catholic and non-Catholic churches per capita in that state in the indicated year interacted with Census year dummies. Column 1 includes observations in each Census year from 1860 to 1900, corresponding to the years in which growth rate outcomes are observed, column 2 includes observations in each Census year from 1880 to 1900, corresponding to the years in which farm tenure status is observed and Column 3 includes observations in each Census year from 1880 to 1890, corresponding to the years in which farm size and tenure status are observed.

All regressions include state and year fixed effects.

Table 2.6: First Stage for Alternative Specifications

	Maximum legal interest rate (avg. prior decade)		
	(1)	(2)	(3)
Panel A: Average Maximum Legal Rate in Adjacent States			
Average maximum rate, adjacent states	0.87	0.46	0.5
	(0.12)***	(0.25)*	(0.37)
Lagged average maximum rate, adjacent states	0.05	0.31	0.5
	(0.11)	(0.14)**	(0.26)*
F-test: Instruments	28	9	5
P-value: F-Test	0	0	0.01
Observations	163	99	65
R-Squared	0.43	0.47	0.52
Panel B: Lagged Catholic and Non-Catholic Churches per capita			
Lagged Non-Catholic Churches (Per Capita)	-2.29	-4.05	
	(1.83)	(2.74)	
Lagged Catholic Churches (Per Capita)	-156.16	-131.86	
	(22.38)***	(30.73)***	
F-test: Instruments	26	14	
P-value: F-Test	0	0	
Observations	130	67	
R-Squared	0.51	0.7	
Mean of dependent variable	0.08	0.09	

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: The top panel of the table shows results from a regression of the maximum legal rate of interest in a state on the average maximum legal rate of interest in adjacent states, and the average maximum legal rate in adjacent states in the prior decade. The lower panel shows results from a regression of the maximum legal rate of interest in a state on the number of Catholic and non-Catholic churches per capita in that state 10 years prior. Column 1 includes observations in each Census year from 1860 to 1900, corresponding to the years in which growth rate outcomes are observed, column 2 includes observations in each Census year from 1880 to 1900, corresponding to the years in which farm tenure status is observed and Column 3 includes observations in each Census year from 1880 to 1890, corresponding to the years in which farm size and tenure status are observed.

All regressions include state and year fixed effects.

Table 2.7: Effect of Usury Laws on Agricultural Investment, Robustness to Alternative Specifications

	Annual Growth Rate of Improved Acres of Farmland (1)	Annual Growth Rate of Value of Equipment and Machinery (2)
Panel A: Initial Church Presence Interacted with Time		
Maximum legal interest rate (avg. prior decade)	2.32	3.84
	(0.58)***	(0.99)***
State population (millions)	0.00	0.00
	(0.01)	(0.01)
White Population (%)	0.05	0.21
	(0.15)	(0.25)
Observations	144	144
Panel B: Average Maximum Legal Rate in Adjacent States		
Maximum legal interest rate (avg. prior decade)	2.25	3.26
	(0.36)***	(0.56)***
State population (millions)	0.00	-0.01
	(0.01)	(0.01)
White Population (%)	0.40	0.78
	(0.16)**	(0.25)***
Observations	161	161
Panel C: Lagged Catholic and Non-Catholic Churches per capita		
Maximum legal interest rate (avg. prior decade)	3.49	3.62
	(0.56)***	(0.73)***
State population (millions)	0.00	0.00
	(0.01)	(0.01)
White Population (%)	0.20	0.38
	(0.20)	(0.26)
Observations	130	130
Mean of dependent variable	0.03	0.04

* Significant at the 10% confidence level
** Significant at the 5% confidence level
*** Significant at the 1% confidence level

Notes: Panel A replicates the results from Table 2.2. Panel B shows the results from a regression of the variable indicated in the column heading on the average maximum legal interest rate where the maximum legal interest rate is instrumented with the average maximum legal rate of interest in adjacent states in the current and previous decades. These specifications include observations in each Census year from 1860 to 1900. Panel C shows the results from a regression of the variable indicated in the column heading on the average maximum legal interest rate where the maximum legal interest rate is instrumented with the number of Catholic and non-Catholic churches per capita at the start of the decade. This specifications includes observations in Census years 1860, 1870, 1880 and 1900.
All regressions include state and year fixed effects.

Table 2.8: Effect of Usury Laws on Farm Tenure, Robustness to Alternative Specifications

	Annual Growth Rate of Number of Farms (1)	Percent owner operated farms (2)	Percent sharecropped farms (3)	Percent tenant farms (4)	Percent small owner operated farms (5)	Percent small sharecropped farms (6)	Percent small tenant farms (7)
Panel A: Initial Church Presence Interacted with Time							
Maximum legal interest rate (avg. prior decade)	2.08	-0.29	-1.21	0.78	4.10	-2.44	-1.65
	(0.52)***	(0.80)	(0.72)*	(0.69)	(1.81)**	(1.39)*	(1.37)
State population (millions)	-0.01	0.00	0.00	0.00	0.02	-0.05	0.03
	(0.01)	(0.01)	(0.01)	(0.01)	(0.06)	(0.04)	(0.04)
White Population (%)	-0.08	0.77	-0.40	-0.38	2.56	-1.59	-0.97
	(0.13)	(0.33)**	(0.30)	(0.29)	(1.05)**	(0.80)**	(0.79)
Observations	144	98	98	98	65	65	65
Panel B: Average Maximum Legal Rate in Adjacent States							
Maximum legal interest rate (avg. prior decade)	1.91	1.14	-1.02	-0.25	3.29	-0.35	-2.94
	(0.31)***	(0.81)	(0.75)	(0.73)	(1.54)**	(1.14)	(1.43)**
State population (millions)	-0.01	0.00	0.00	0.00	0.02	-0.04	0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.05)	(0.04)	(0.05)
White Population (%)	0.27	0.85	-0.34	-0.44	2.25	-0.86	-1.39
	(0.14)**	(0.32)***	(0.29)	(0.28)	(0.94)**	(0.70)	(0.88)
Observations	161	99	99	99	65	65	65
Panel C: Lagged Catholic and Non-Catholic Churches per capita							
Maximum legal interest rate (avg. prior decade)	2.75	1.34	-1.46	0.13			
	(0.46)***	(0.87)	(0.74)**	(0.83)			
State population (millions)	0.00	0.00	0.01	0.00			
	(0.01)	(0.02)	(0.02)	(0.02)			
White Population (%)	0.11	0.82	-0.22	-0.60			
	(0.17)	(0.44)*	(0.38)	(0.43)			
Observations	130	67	67	67			
Mean of dependent variable	0.04	0.73	0.18	0.1	0.61	0.24	0.15

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: Panel A replicates the results from Table 2.3. Panel B shows the results from a regression of the variable indicated in the column heading on the average maximum legal interest rate where the maximum legal interest rate is instrumented with the average maximum legal rate of interest in adjacent states in the current and previous decades. These specifications includes observations in each Census year from 1860 to 1900 (column 1), 1880 to 1900 (columns 2-4) or 1880 to 1890 (columns 5-7). Panel C shows the results from a regression of the variable indicated in the column heading on the average maximum legal interest rate where the maximum legal interest rate is instrumented with the number of Catholic and non-Catholic churches per capita at the start of the decade. This specifications includes observations in Census years 1860, 1870, 1880 and 1900 (column 1) or 1880 and 1900 (columns 2-4).

All regressions include state and year fixed effects.

Table 2.9: Effect of Usury Laws on Agricultural Investment, Robustness to Additional Controls

	Annual Growth Rate of Improved Acres of Farmland (1)	Annual Growth Rate of Value of Equipment and Machinery (2)	Annual Growth Rate of Improved Acres of Farmland (3)	Annual Growth Rate of Value of Equipment and Machinery (4)	Annual Growth Rate of Improved Acres of Farmland (5)	Annual Growth Rate of Value of Equipment and Machinery (6)
			Panel A: OLS			
Maximum legal interest rate (avg. prior decade)	1.49	1.51	1.56	1.65	1.54	1.62
	(0.19)***	(0.28)***	(0.20)***	(0.28)***	(0.20)***	(0.28)***
State population (millions)			-0.01	-0.01	-0.01	-0.01
			(0.01)	(0.01)	(0.01)	(0.01)
White Population (%)			0.28	0.51	0.30	0.51
			(0.15)*	(0.21)**	(0.15)**	(0.21)**
Manufacturing Employment (Jobs Per Capita)					0.09	0.27
					(0.38)	(0.55)
Capital in Manufacturing ($1K Per Capita)					0.13	0.03
					(0.13)	(0.19)
Observations	163	163	163	163	163	163
R-Squared	0.55	0.3	0.57	0.34	0.58	0.35
			Panel B: IV			
Maximum legal interest rate (avg. prior decade)	2.47	3.92	2.32	3.84	2.27	3.79
	(0.56)***	(0.94)***	(0.58)***	(0.99)***	(0.58)***	(1.00)***
State population (millions)			0.00	0.00	-0.01	0.00
			(0.01)	(0.01)	(0.01)	(0.01)
White Population (%)			0.05	0.21	0.08	0.23
			(0.15)	(0.25)	(0.15)	(0.25)
Manufacturing Employment (Jobs Per Capita)					-0.14	0.03
					(0.36)	(0.62)
Capital in Manufacturing ($1K Per Capita)					0.14	0.07
					(0.12)	(0.21)
Observations	144	144	144	144	144	144
Mean of dependent variable	0.02	0.03	0.02	0.03	0.02	0.03

* Significant at the 10% confidence level
** Significant at the 5% confidence level
*** Significant at the 1% confidence level

Notes: Panel A shows the results from a regression of the variable indicated in the column heading on the averge maximum legal interest rate. Panel B shows the results where the maximum legal interest rate is instrumented with the number of Catholic and non-Catholic churches per capita in 1850 interacted with Census year dummies. These specifications include observations in each Census year from 1860 to 1900.

All regressions include state and year fixed effects.

Table 2.10: Effect of Usury Laws on Farm Tenure, Robustness to Additional Controls

	Annual Growth Rate of Number of Farms (1)	Percent small owner operated farms (2)	Percent small sharecropped farms (3)	Percent small tenant farms (4)	Annual Growth Rate of Number of Farms (5)	Percent small owner operated farms (6)	Percent small sharecropped farms (7)	Percent small tenant farms (8)	Annual Growth Rate of Number of Farms (9)	Percent small owner operated farms (10)	Percent small sharecropped farms (11)	Percent small tenant farms (12)
						Panel A: OLS						
Maximum legal interest rate (avg prior decade)	1 31	1.05	-0 47	-0 58	1 35	1.72	-0.83	-0 90	1 36	1 79	-1 02	-0 77
	(0.17)***	(0.73)	(0.56)	(0 60)	(0.17)***	(0.75)**	(0 60)	(0.65)	(0 17)***	(0 78)**	(0 54)*	(0 65)
State population (millions)					-0 01	0.01	-0 04	0.03	-0 01	0 02	-0 07	0 05
					(0 01)	(0.05)	(0 04)	(0.04)	(0 01)	(0 05)	(0 04)*	(0 04)
White Population (%)					0 18	1.67	-0 98	-0 69	0.21	1 61	-0 79	-0 83
					(0 13)	(0 74)**	(0 59)	(0 64)	(0.13)	(0 77)**	(0 53)	(0 64)
Manufacturing Employment (Jobs Per Capita)									-0 28	0 60	-1 62	1 02
									(0.34)	(1.20)	(0 83)*	(1 00)
Capital in Manufacturing ($1K Per Capita)									0.17	-0 24	0 77	-0 53
									(0.11)	(0 45)	(0 31)**	(0 38)
Observations	163	66	66	66	163	66	66	66	163	66	66	66
						Panel B: IV						
Maximum legal interest rate (avg prior decade)	2 23	3 62	-1 82	-1 80	2 08	4 10	-2 44	-1 65	2 08	3 78	-1 95	-1 83
	(0 50)***	(1 77)**	(1 24)	(1 31)	(0 52)***	(1 81)**	(1 39)*	(1 37)	(0 51)***	(1 59)**	(1 03)*	(1.24)
State population (millions)					-0 01	0 02	-0 05	0 03	-0 01	0 03	-0 07	0 04
					(0 01)	(0 06)	(0 04)	(0.04)	(0.01)	(0 06)	(0 04)*	(0 05)
White Population (%)					-0.08	2 56	-1 59	-0 97	-0 04	2 29	-1 10	-1 19
					(0 13)	(1 05)**	(0 80)**	(0.79)	(0 13)	(0 98)**	(0 64)*	(0 76)
Manufacturing Employment (Jobs Per Capita)									-0 54	0 67	-1 65	0 98
									(0 32)*	(1 34)	(0 87)*	(1.05)
Capital in Manufacturing ($1K Per Capita)									0 20	-0.41	0 85	-0 44
									(0 11)*	(0 52)	(0 34)**	(0 40)
Observations	144	65	65	65	144	65	65	65	144	65	65	65
Mean of dependent variable	0 03	0 63	0 23	0 15	0.03	0 63	0 23	0 15	0 03	0 63	0 23	0 15

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: Panel A shows the results from a regression of the variable indicated in the column heading on the average maximum legal interest rate Panel B shows the results where the maximum legal interest rate is instrumented with the number of Catholic and non-Catholic churches per capita in 1850 (columns 1, 5 and 9) or 1870 (columns 2-4, 6-8 and 10-12) interacted with Census year dummies. Columns 1, 5 and 9 includes observations in each Census year from 1860 to 1900, columns 2-4, 6-8 and 10-12 include observations in each Census year from 1880 to 1890.

All regressions include state and year fixed effects

Chapter 3

Targeting Efficiency: How well can we identify the poorest of the poor?[1]

3.1 Introduction

Nearly all poverty alleviation programs target a particular sub-population. This feature is most readily apparent in programs designed to aid those who have suffered a particular tragedy, such as grants to widows of debt-ridden Maharashtra farmers, but is also generally true of large, broad based development interventions. Conditional and unconditional cash transfer programs, for example, are also designed to reach specific households, such as the most impoverished or households with school children. At first blush, this may seem unremarkable and not to warrant particular consideration. But effective identification of the target population is crucial to the success of aid programs which operate with limited resources. If, for instance, households which are adequately nourished are identified as eligible for subsidized food, the program is unlikely to significantly reduce malnutrition. Given that several countries have begun large scale cash transfer programs, the issue of effective targeting has become especially important.

When the targeted population is not distinguished by a well-defined, observable trait, however, identifying members of that population becomes complicated. Evidence suggests that the targeting efficiency of aid programs is less than perfect. A report by the Indian National Sample

[1] **This chapter is joint work with Abhijit Banerjee, Esther Duflo and Raghabendra Chattopadhyay.**

Survey Organization, for example, found that 18% of the wealthiest 20% of the rural population (ranked by monthly per capita expenditure) held Below Poverty Line (BPL) rationing cards.[2] The imperfect track record of such expansive development projects makes effective targeting not only important but controversial.

Part of the debate about targeting revolves around which methods should be used, in particular whether these methods should rely on administrative data or on information generated through participatory processes. In this study we assess the relative performance of administrative and participatory methods in identifying the poorest of the poor, who may be particularly marginalized and difficult to single out. Importantly, we conduct this analysis on the same sample, allowing us to make a direct comparison between the two methods.

Firstly, we consider the targeting efficiency of various assistance programs operated by the government of India, which are targeted using an administrative census. We find that the methods used to identify eligible households do not particularly target the very poorest. Since our sample is drawn from the lower economic spectrum, we can not evaluate the overall targeting efficiency of these programs, but we find that within this group of households, those who actually receive government assistance do not appear worse off, according to our measures of poverty, than households which do not.

We also evaluate the targeting efficiency, in terms of identifying the very poorest segment of the population, of Participatory Rural Appraisals (PRAs) which are a popular alternative to census methodologies. PRAs are widely practiced by NGOs, both within India and internationally, when conducting development interventions. Increasingly, PRA methodologies are used to identify beneficiaries for assistance programs. Consequently, it is important that the information collected from a PRA accurately reflects the conditions within the village where it was conducted.

Other studies provide evidence suggesting that certain types of information, such as the presence of village infrastructure (e.g. water systems) or students' needs for scholarships, can accurately be obtained using PRAs (Chattopadhyay and Duflo, 2001; Duflo et al., 2008; Chambers, 1994). In this study we assess whether PRAs reliably rank village residents according to

[2]National Sample Survey Organisation (NSSO), Ministry of Statistics and Programme Implementation. Report No. 510 "Public Distribution System and Other Sources of Household Consumption, 2004-05." Summary at: http://mospi.nic.in/press_note_510-Final.htm

economic status.

Using data generated in PRAs conducted by Bandhan, a Kolkata-based microfinance institution, we evaluate how well measures of poverty collected in a detailed household survey accord with the evaluation of poverty established by the PRA. Since the information collected in the PRA is used to identify households eligible for a program to enable the poorest of the poor to access microcredit, targeting the most poor households is crucial in this context. Along some dimensions of poverty, notably consumption and expenditure, the results are imprecise; it does not appear that per capita consumption among those identified as poor in the PRA is less than among those not so classified. The analysis does reveal, however, that those ranked as most poor in the PRA are in fact poorer than others in terms of observable characteristics such as land and asset ownership. They also have less access to credit.

As Bandhan's process incorporated additional verification of the information collected in the PRA, we also assess the extent to which this verification improves targeting. Our results indicate that further verification successfully narrows in on a group which appears poorer in various respects, particularly land ownership.

A limitation of this analysis is that, although comprehensive and detailed, our household survey is not an error-free measure of economic well-being. Consumption and expenditure, for example, is not always reliably measured with household interviews (see e.g. Deaton, 1997). Moreover, poverty can be defined in various ways; the indicators collected in this survey are only one way of doing so and are not necessarily perfectly aligned with the definition of poverty established in a PRA. Finally poverty is dynamic, low consumption today is not always indicative of long term depravation. As community members have long-term relationships, it is possible that participatory targeting methods capture more of the dynamic element of poverty than our household survey. Notwithstanding these concerns, we are able to assess how classification as impoverished through various targeting techniques correlates with important indicators of poverty captured in our survey, such as land holdings and credit access. Moreover, this study is able to contrast census and participatory methods by comparing them to an equivalent external benchmark of poverty.

This study is closely related to Alatas et al. (2009) who contrast the targeting performance of census techniques and participatory community wealth rankings in Indonesia. Their findings

indicate that participatory methods do not identify a poorer population in terms of consumption and suggest that community members may perceive poverty along other dimensions. The results presented here coincide with those of Alatas et al.; we find that PRAs identify a population which does not appear worse off in terms of consumption but which is poorer according to other important poverty metrics, suggesting that PRA rankings accord with multiple dimensions of economic well-being and can serve as the basis for targeting.

3.2 Data

In order to improve their targeting process, Bandhan requested that we do a study to assess how effectively they were identifying the poorest households in each village, or the "Ultra Poor." To accomplish this we conducted a detailed survey among those not identified as Ultra Poor as well as among those identified as Ultra Poor in a sample of villages where Bandhan operates.

Initially, the surveying team conducted a census of all households in the village. Each household was classified on a 1-5 scale along several characteristics, such as land holdings, quality of house, ownership of assets, educational achievement, employment status and access to credit. This census utilized similar classification criteria as the government administered BPL census, which is intended to identify the population living below the poverty line and determine who is eligible for certain government assistance programs.

In line with our objective to understand the feasibility of identifying the very poorest of the poor, the sampling frame was restricted to the poorer population within the village. To be considered for our survey, a household must meet one of the following requirements: own less than 1 acre of irrigated land or less than 2 acres of non-irrigated land, not live in a pucca house (i.e. one made of brick, stone or concrete), own less that 4 articles of clothing, and own none or only one durable household good.[3]

Of 1,757 households enumerated in the economic census, 605 satisfied the criteria above. From this restricted list, a random sample of households was selected and administered a survey similar to that given to households identified as Ultra Poor by Bandhan. This survey

[3]The items considered were: computer, telephone, refrigerator, husking machine, color television, electric cooking appliances, costly furniture, LPG (gas) connection, light motor vehicle or commercial vehicle, tractor, two or three wheeler, motor van, power driven tiller.

was conducted among 178 households in five villages. Table 3.1 shows a breakdown of this sampling by village.

Among these surveyed households, 48 were not enumerated in the PRA conducted by Bandhan. That the PRA process fails to enumerate some households which are relatively worse off (as determined by the economic census) is indicative that it may be especially difficult to identify the poorest of the poor within a village. For the purposes of this study, however, we restrict our analysis to the households appearing in the PRA list since we are interested in making comparisons across targeting mechanisms, including the additional verification done by Bandhan, which was only done for those households appearing in the PRA. Our final dataset contains 215 households, 93 were identified as Ultra Poor by Bandhan and 122 were identified as impoverished by the economic census but not classified as Ultra Poor by Bandhan.[4]

Table 3.2 provides summary statistics for our entire sample as well as separately according to whether households were identified as Ultra Poor by Bandhan or not. As might be expected given the mandate of Bandhan's identification process and the sampling design of the additional survey, this is a relatively poor population. The mean per capita monthly average expenditure is Rs. 425 (Rs. 14 per day or $1.25 in PPP adjusted 2006 U.S. dollars). Average monthly per capita expenditure on food and fuel is Rs. 302 (Rs. 10 per day or $0.89 in PPP adjusted 2006 U.S. dollars). For both measures of consumption, approximately half the sample population spends less than one dollar a day and nearly all the population spends less than two dollars a day.

Other variables conform to what one would expect in this sample. Mean land holdings are 5.6 katthas (approximately 0.11 acres). In addition 21% of the sample is landless. While 46% of households have obtained loans, only 8% obtained credit from a formal source.[5] As well as being poor, this population lacks education; average completed years of education per household member is 1.2 years and 25% of households have school aged children (5-14 years old) out of school.

[4] Eight of the surveyed households from the economic census were under consideration by Bandhan and were subsequently verified as eligible to receive a grant. Thus the number of non Ultra Poor households in the sample is 122 (170-48) rather than 130 (178-48). These households are included in the figure of 93 identified as Ultra Poor.

[5] A formal source is defined as a commercial bank, government bank, self-help group or a cooperative. Informal sources include family members, friends, neighbors, moneylenders and shopkeepers.

This is also a vulnerable population; only 67% report that everyone in the household regularly eats two meals a day, approximately half of those surveyed report having experienced a medical shock in the last year, 22% suffered a medical shock requiring institutional care and 41% suffered an economic shock.[6] Moreover, to the extent that receipt of assistance is an indication of need, this is a needy population. Two thirds report receiving assistance from one of the government programs listed in the questionnaire (such as Below Poverty Line rationing, subsidized housing or participation in employment generating schemes). Figures for the most common assistance programs are reported separately in Table 3.2.

3.2.1 Empirical Strategy

In what follows, we are primarily concerned with the difference in some numerical measure of poverty or economic status, denoted y, between sub-groups. These groups will be households receiving some form of government assistance and those that do not, households identified as poor in the PRA and those that are not or households identified as Ultra Poor by Bandhan and those that are not so identified. Letting D_i be an indicator variable that household i receives a certain form of government aid or that the household was identified as poor in the PRA or was identified as Ultra Poor, we estimate the following equation

$$y_{iv} = \beta D_{iv} + \alpha_v + \varepsilon_{iv} \tag{3.1}$$

where the subscript v indicates villages. In some specifications we include household covariates, X_{iv}, in addition to the village fixed effects, α_v. The parameter of interest in β, which measures the mean difference in y between those who are somehow identified as poor and those that are not after removing the effect of common village level determinates of y.

In addition to assessing whether having been identified as poor coincides with differences in measured indicators of poverty, we are also interested in which particular factors are correlated with a household being classified as impoverished, either by a government program, by their

[6] A medical shock is defined as having spent more than Rs. 500 (44 PPP adjusted 2006 $U.S.) on any one household member's medical care. A medical shock requiring institutional care is defined as having spent more than Rs. 500 (44 PPP adjusted 2006 $U.S.) on institutional medicine in the last year. An economic shock is defined as any of the following occurring in the past year: house was severely damaged, livestock became ill, livestock died, conflict/dispute/legal case, or theft.

peers in the PRA or by Bandhan. To answer this question we take D_{iv} as the outcome of interest and investigate how it correlates with various household characteristics. We estimate the following linear probability model

$$D_{iv} = \delta Z_{iv} + \gamma \mathbf{X}_{iv} + \alpha_v + \eta_{iv} \tag{3.2}$$

where Z_{iv} is a variable hypothesized to determine the perception of poverty, such as the presence of able bodied earners, receiving official government aid or having suffered a serious health shock, and $\mathbf{X}_{iv}$ is a vector of household characteristics including per capita average expenditure, land holdings and the number of household members.

3.3 Targeting Efficiency of Government Aid Programs

Since our survey inquired about receipt of assistance from various government poverty alleviation programs, we are able to assess to what extent this aid reaches the poorest segment of the population. By design, all households in our sample are drawn from the bottom of India's economic spectrum. While these government programs are not explicitly designed to target the very poorest of the poor, to the extent that they are intended to benefit impoverished households we should expect that either the poorest within our sample overwhelmingly receive this aid or that all households in our sample do. As is evident from Table 3.2 the latter case does not appear true; for instance only 30% receive BPL rationing and 10% have an Antodaya card (variables which indicate participation in government food assistance programs).

Targeting for many government aid programs is based on the BPL census, conducted by the government to identify those households living below the poverty line. This census, however, has been criticized for systematic exclusion of extremely poor households. Moreover, there are concerns that the final lists of BPL households are directly manipulated to include non-poor households (Mukherjee, 2005). Jalan and Murgai (2007) find that many households who are below the poverty line according to consumption measures are incorrectly classified by the BPL census.

To assess the efficiency of this targeting process in these villages, we contrast the features of those who participate in government programs and those who do not. Specifically, we estimate

(3.1) where y_{iv} is taken to be per capita expenditure,[7] land holdings, house size, whether members eat two meals a day, access to credit, self-classification of financial condition, an index of asset holdings based on principal component analysis of durable goods and livestock holdings or an indicator for the presence of an able bodied male adult in the household.

In particular, we perform this comparison separately for four government aid programs by letting D_{iv} be an indicator that the household receives BPL rationing, receives Antodaya rationing, participates in the Indira housing program or participates in an employment generating scheme. The BPL and Antodaya programs provide a card which entitles households to purchase subsidized food and fuel at ration shops. BPL cards are intended for those living below the poverty line while Antodaya cards are intended to go to exceptionally poor households. The Indira housing program (Indira Awaas Yojana) evolved into its present form by 1996, the goal of this program is to improve housing for the disadvantaged rural population. To this end grants are distributed to build or repair homes and, in some cases, loans are facilitated for these purposes. Preference for the Indira housing program is supposed to be given to those identified as below the poverty line by the government BPL census (Jalan and Murgai, 2007). Preference may also be given to widows of servicemen.

The National Rural Employment Guarantee Act (NREGA) was launched in 2005. The mission of NREGA is to provide "at least one hundred days of guaranteed wage employment in every financial year to every household whose adult members volunteer to do unskilled manual work and for matters connected therewith or incidental thereto."[8] Participation in the program requires registration with the Gram Panchayat (local official) to obtain a job card. Holders of this card become eligible to apply for jobs allocated under the program.

According to our results, the population which receives assistance from these programs is not statistically different, with respect to our poverty indicators, from the population which does not. Table 3.3 presents the results. For recipients of BPL rationing we find that these households are slightly more likely to have an able bodied adult male member, which is the opposite of what might be expected if this program targeted particularly disadvantaged households. We

[7] Replacing the level of expenditure with the logarithm of expenditure does not substantively change the results discussed below.

[8] The National Rural Employment Guarantee Act of 2005. Retrieved from: The Gazette of India, New Delhi. Wednesday, September 7 2005 pp:1. http://rural.nic.in/rajaswa.pdf [viewed October 2007].

are unable to reject that the means between those that receive BPL rationing and those that do not are equal for any other indicator of poverty. Moreover, some of these coefficients take the opposite sign than would be expected. Comparing households which have Antodaya cards with those that do not we can not reject that the means between the groups are equal for any outcome.

There is at least the suggestion that households which have received work from an employment generating scheme are poorer than others. The coefficient on participation in this program enters with the predicted negative sign when any of the expenditure measures are taken as the left hand side variable, although no coefficient is significant at the 10% level. The results also suggest that these households own an average of 4.3 katthas (0.09 acres) less land, a difference which is significant at the 10% level. We also find that these households are more likely to include an able bodied male member. These results may be driven by the fact that there is also a component of self-selection in employment generating programs. Since benefits require work, only households who are poor enough to lack more attractive work opportunities will take up these programs. Mukherjee (2005) notes the potential of self-selecting programs to overcome barriers, whether political or logistical, to effective targeting.

In terms of consumption, only with respect to per capita non food expenditure do beneficiaries of the Indira housing program appear statistically different (at the 10% confidence level) from their peers. Also, beneficiaries are less likely to have an able bodied male in the household, indicating the targeting of widows was likely effective. No other measure is significantly different between recipients and non-recipients.

Perhaps owning to the failures of censuses to identify poor households, many organizations have turned to other methods. A particularly popular method used for ascertaining the economic status of households is the Participatory Rural Appraisal (PRA). Indeed, Mukherjee (2005) draws on information gathered in PRAs to evaluate the targeting efficiency of the BPL census. The PRA process was pioneered in the 1980's and 90's, largely by government and non-government organizations in Kenya and India. By 1997, the practice had spread globally; PRA activities had been conducted in over 30 countries, both developing and developed, by the end of 1996. In India, PRA methods have been used by numerous NGOs as well as by

several government agencies.[9] International organizations, such as USAID, Save the Children and Care International, also employ PRA methods in conducting their operations.[10] In light of the targeting process used by Bandhan, we evaluate the accuracy with which PRAs can identify especially poor households. Before proceeding, however, we provide an overview of Bandhan's assistance program and the specifics of the process used to identify beneficiaries.

3.4 Analysis of Bandhan's Identification Process

3.4.1 Overview of Bandan's "Targeting the Ultra Poor"

In light of evidence that microfinance does not reach the poorest of the poor (Morduch, 1999; Rabbani, et al., 2006) various initiatives have begun which aim to "graduate" the poorest to microfinance. The intervention operated by Bandhan is intended to ease credit constraints for exceptionally poor individuals by helping them establish a reliable income stream which can be used to service loan payments.[11] To that end, Consultative Group to Assist the Poor (CGAP) provided $30,000 as grants for the purchase of income generating assets to be distributed to households identified as "Ultra Poor." Grants of $100 were distributed to 300 beneficiaries residing in rural villages in Murshidabad, India (a district north of Kolkata) by Bandhan. The design of this program was based on the pioneering work of BRAC, a Bangladeshi development organization. For several years, BRAC has been distributing grants through its "Challenging the Frontiers of Poverty Reduction-Targeting the Ultra Poor" (CFPR-TUP) program with the aim of helping the absolute poorest graduate to microfinance.[12] Working in close consultation with BRAC, Bandhan developed the criteria to identify the Ultra Poor.

The initial phase of the intervention consists of Bandhan identifying those eligible for the grants; the poorest of the poor within each village. An average of 17 households were identified as Ultra Poor in each village. Following identification, half of the potential beneficiaries were

[9] Chambers, 1997. p.114, 248.

[10] Burde, Dana. Save the Children's Afghan Refugee Education Program in Balochistan, Pakistan, 1995- 2005 2 Report, 2005 http://www.savethechildren.org/publications/technical-resources/education/pakistan-afghan-refugees-education-project-report-9-26-05.pdf [viewed October 2007]; http://www.usaid.gov/regions/afr/success_stories/ghana.html[viewed October 2007]; http://www.care.org/careswork/projects/ETH051.asp [viewed October 2007].

[11] The impact of this intervention is the subject of an ongoing study by the authors.

[12] BRAC website http://www.brac.net/cfpr.htm [viewed October 2007].

randomly selected to receive assets. Rather than transferring cash, Bandhan procures assets, such as livestock or inventory, and distributes them to beneficiaries. The grants are also used to finance other inputs, such as fodder and sheds to house the animals. Eighteen months after receipt of the asset, the beneficiaries will be eligible for micro-finance provided by Bandhan.

3.4.2 Details of the Identification Process

To make the concept of "Ultra Poor" operational and define the targeted population, Bandhan used a set of criteria adapted from those used by BRAC in their CFPR-TUP program. Firstly, an eligible household must have an able-bodied female member. The rationale for this requirement is that the program is intended particularly to benefit women[13] and any benefit accruing from the grant requires that the beneficiary be capable of undertaking some enterprise. The second mandatory requirement is that the household not be associated with any microfinance institution (in keeping with the aim of targeting those who lack credit access) or receive sufficient support through a government aid program.[14] In addition to these two criteria, eligible households should meet three of the following five criteria: the primary source of income should be informal labor or begging, land holdings below 20 decimals (10 katthas, 0.2 acres), no ownership of productive assets other than land, no able bodied male in the household and having school-aged children working rather than attending school.

To identify those households satisfying this definition of Ultra Poor, Bandhan utilized a multi-phase process. The initial task is to identify the poorer hamlets in the region. Since Bandhan has operations in Murshidabad, this is accomplished by consulting with local branch managers who are familiar with the economic conditions in these villages.

In the second phase, Bandhan conducts Participatory Rural Appraisals (PRAs) in particular hamlets of selected villages to identify the subset of the population most likely to be Ultra Poor. To ensure that the PRA includes a sufficient number of participants, Bandhan employees enter the hamlet on the day prior to the PRA; they meet with teachers and other local figures to build rapport with the residents, announce that the PRA will occur on the following day and

[13] While the majority of beneficiaries are female, some men were identified as eligible under special circumstances such as physical disability.

[14] "Sufficient support" was determined on a case-by-case basis by Bandhan; while many of the households they identified as Ultra Poor participate in some government aid program, they determined that this assistance was not sufficient to alleviate the poverty of the household.

encourage participation. Bandhan aims for 12-15 PRA participants, but often the figure is as high as 20. Moreover, they encourage household members from various religions, castes and social groups to attend.

In this particular context, the PRA consists of social mapping and wealth ranking, following a sophisticated process to identify the poor. At the outset, the main road and any prominent hamlet landmarks (temples, mosques, rivers, etc.) are etched into the ground, usually in front of a central house in the hamlet. Subsequently the participants enumerate each household residing in the hamlet and mark the location of the households on the hamlet map. For each household, the name of the household head is recorded on an index card.

In the wealth ranking stage, the index cards are sorted into piles corresponding to socioeconomic status. To accomplish this, Bandhan's employees select one of the index cards and inquire about that household's occupation, assets, land holdings and general economic well-being. They then take another card and ask how this household compares to the prior household. A third card is selected, classified as similar in wealth to one or the other of the prior households and then whether it is better off or worse off than that household. This process is continued until all the cards have been sorted into piles, usually 5 of them, corresponding to poverty status (the fifth pile representing the poorest group). Often a large percentage of the cards end up in the fifth pile, in which case these households are sorted in a similar manner into two or more piles.

PRA participants are involved in determining what criteria constitute a disadvantaged household, relative to their neighbors, within that particular area. Additionally, the relative socioeconomic status of a given household, which determines into which pile they will be sorted, is established through the discussion of participants. Based on the belief that a lively discussion among many people will generate the most precise definition of (relative) poverty and facilitate accurate wealth ranking, Bandhan attempts to include the voices of many villagers in the discussions. Anecdotally, however, it is sometimes the case that a few prominent voices dominate the PRA process and largely determine the ranking of households. A potential concern is that these persons may misrepresent the socioeconomic status of certain households (for example friends, relatives or households favored by that individual) in the expectation that the households identified as most disadvantaged will receive some assistance. Although Bandhan does

not reveal the details of the intervention at the time of the PRA[15] there may be an implicit association between PRAs and future development programs.

Following the PRA, Bandhan selects the households assigned to the lowest few ranks, progressively taking higher categories until they have approximately 30 households. In the second phase of their identification process a Bandhan employee visits these households to conduct a short questionnaire. The questionnaire pertains to the criteria for Ultra Poor classification; inquiring about the presence of an able-bodied woman, the presence and ability to work of a male household head, land holdings, assets, NGO membership and so on. Based on the information collected in this survey, Bandhan narrows its list of potentially Ultra Poor households in that hamlet to 10-15.

In the final stage of the process, the project coordinator, who is primarily responsible for administration of this program, visits the households. He verifies the questionnaire through visual inspection and conversations with the household members. Final identification as Ultra Poor is determined by the project coordinator, according to the established criteria and his subjective evaluation of the households' economic situation.

3.4.3 Analysis of the PRA Process

Using data collected from the PRAs carried out by Bandhan, we are able to investigate the extent to which PRAs can identify the sub-population of interest. For each household in our sample, we observe the wealth rank (corresponding to the pile of index cards into which that household name was sorted) determined by the PRA. These ranks range from 1 to 6, representing categories classified as "very rich", "rich", "average", "poor", "very poor" and "exceptionally poor." A lower rank corresponds to richer households. In Panel A of Table 3.4 we investigate how those identified in the PRA as "very poor" or "exceptionally poor" (PRA rank of 5 or 6) compare to those with a PRA rank below 5. Specifically we regress the indicators of poverty obtained in the household survey on a dummy indicating PRA rank of 5 or 6 and a set of village dummies. From the perspective of targeting, it may be less of a concern if those ranked as "very" or "exceptionally" poor are not especially different from those classified as "average" or "poor" but more concerning if they were not observably poorer than those ranked

[15] The stated intent of the PRA is simply to assess the economic situation of the villages for research purposes.

as rich. Comparing only the highest ranked to the lowest ranked households, however, generates qualitatively similar, but predictably amplified, estimates to those discussed below.[16]

Those assigned a high PRA rank appear poorer than others in several important respects. For one thing, these households tend to have substantially less land than others. On average, very or exceptionally poor households own 6.3 katthas (0.13 acres) less land. The coefficient is statistically significant at the 1% confidence level and the magnitude of the point estimate is substantial; this difference represents 75% of mean land holdings among those not identified as Ultra Poor (8.4 katthas).

Figure 3.1, which plots the cumulative distribution functions (cdfs) of land holdings separately for those ranked very or exceptionally poor in the PRA and those given a lower rank, confirms these results. A statistical test (Abadie, 2002) indicates that the distribution of households ranked 5 or 6 in the PRA stochastically dominates the distribution of those given a lower rank ($p\text{-}value < 0.01$), meaning that for a given level of land holdings a higher percentage of those ranked 5 or 6 own less than that quantity of land than the corresponding percentage for those ranked 1-4. The advantage of this comparison relative to the regression analysis is that it reveals differences between the two groups that are unaffected by a few exceptionally large landowners; focusing on the population with low values of land holdings, the figure reveals that those ranked 5 or 6 tend to own even less than others.

We also find that these households are poorer in terms of asset holdings: when our index of durable goods and livestock is taken as the left hand variable the coefficient on the PRA rank dummy is negative and significant at the 1% confidence level. While these households do not appear to be any less likely to have taken loans, they are 11% less likely to have obtained loans from a formal source, a difference which is also significant at the 1% confidence level. The table also indicates that these households are 17% less likely to report regularly eating two meals a day. This coefficient is significant at a 5% confidence level. While not statistically different from zero, our point estimates suggest that this group lives in smaller homes and self-classify their financial situation as worse than their lower ranked neighbors. When we consider our various measures of expenditure, the coefficients take the unexpected, positive, sign; but none

[16] In particular, comparing those with a PRA rank of 5 or 6 only to those ranked 1, 2 or 3 or only those ranked 1 or 2 amplifies the results pertaining to land holdings, assets, self-classification of financial status and credit from a formal source. The results pertaining to other outcomes are generally unchanged.

of these coefficients are statistically distinguishable from zero.

Differences in per capita expenditure, however, are not entirely informative when the outcome of interest is not expenditure itself but the economic well-being implied by an expenditure level (Olken 2003). One issue is with equivalence scales; certain household members, such as the elderly, may require only a fraction of the expenditure required by others to achieve the same level of well-being (nutritional status for example). Furthermore, per capita variables do not account for economies of scale (it may be cheaper per capita to feed or clothe a large family) and public goods (a radio, for example, benefits all members although the per capita cost is higher in a small household). In light of these considerations, we re-run the regressions while controlling for household size, and present these results in Panel B of Table 3.4.[17] When considering food and fuel expenditures and total expenditures less institutional medical expenditures the coefficient on the PRA rank dummy now takes the expected negative sign, although the estimates are not significant at the 10% confidence level. When total expenditures or non-food expenditures are taken as the left hand side variable, the coefficients remain positive but are drastically smaller. The statistically significant and negative coefficient on the number of household members indicates that expenditure per capita falls as household size increases, which is indicative of economies of scale in household consumption. These results suggest that when averaging across households of all sizes those ranked very or exceptionally poor appear to spend more per capita. When comparing two households with the same number of members, however, the households ranked poorer appear to spend less per capita (with respect to food and fuel expenditures and total expenditures less institutional medical expenditures).

As a robustness check, we also controlled for total household members when considering other indicators of poverty which should not necessarily be impacted by household size (land holdings, credit access, etc.). When considering these other variables the estimated differences between those ranked very or extremely poor and those ranked richer do not change appreciably.

These expenditure patterns are illustrated visually in Figure 3.2 which shows the cdfs for per capita total expenditure, food expenditure, non-food expenditure and total less institutional medical expenditure for the two groups. The divergence of the cdfs for higher levels of expenditure when considering non-food expenditures suggests that higher expenditure and

[17] The results are similar using the equivalence scales reported in Meenakshi and Ray (2002).

higher PRA rank could both be driven by an omitted variable. For example, an economic shock to the household could simultaneously increase expenditures and also cause villagers to view the afflicted household as less fortunate. If that were the case, per capita expenditure would be mis-measuring true household well-being. In Table 3.5 we investigate this hypothesis.

Using the linear probability model specification from (3.2), we regress a dummy indicating PRA rank of 5 or 6 on land holdings, per capita consumption and a set of variables which may cause villagers to perceive a household as especially poor.[18] Since PRA rank is relative to other households in the same geographic area, these specifications contain a set of village dummies. Also, in light of the importance of household size, we condition on the number of household members. In all specifications the coefficient on per capita total monthly expenditure is statistically indistinguishable from zero. For land holdings the coefficient takes the predicted negative sign and is statistically significant. The table shows that having suffered a shock is not a significant determinant of high PRA status; the coefficients on having experienced a medical shock in the last year (i.e. having spent more than Rs. 500 on any member's medical care), having experienced a medical shock requiring institutional care (i.e. having spent more than Rs. 500 on institutional medical care) and on having experienced an economic shock (house was severely damaged, livestock became ill, livestock died, conflict/dispute/legal case or theft) are all indistinguishable from zero. Nor are households which have been identified by the government as in need of aid, indicated by participation in some government aid program, more likely to be seen as particularly poor by their neighbors. We do find that education is correlated with PRA status; an additional year of schooling per capita makes households 5% less likely to be ranked very or exceptionally poor and a household with a child out of school is 18% more likely to be so ranked. Both of these coefficients are significant at the 5% confidence level. Another result from this exercise is that the presence of an able bodied adult (older than 14) male makes households 36% less likely to be assigned the highest PRA ranks.[19]

[18] We also estimated an OLS specification where the outcome is PRA rank in levels (1-6) rather than a binary varible, the results are similar.

[19] This coefficient is similar in magnitude using over 18 years as the definition of adult.

3.4.4 Comparing PRA and Government Targeting

In addition to considering whether different targeting procedures successfully identify the poorest of the poor, we are also interested in making comparisons across methods. Tables 3.3 and 3.4 seem to suggest that the PRA identifies individuals who are relatively more disadvantaged according to various measures than government procedures, but we also test these apparent differences formally. In particular we statistically test for equality of the coefficients on the indicator for receiving a particular form of government aid and the indicator on having been identified as poor in the PRA. These results (which are relegated to Appendix Table A3.1) demonstrate that there are statistically significant differences between the coefficients for the outcomes which generate statistically significant results in Table 3.4.

With the exception of participants in employment generating schemes, we can reject equality of the coefficients for land holdings above a 5% confidence level, indicating that the difference in land holdings between those identified as poor in the PRA and others is larger than the difference for individuals participating in government assistance programs and those that are not. We can also reject equality of the coefficients above a 5% level for all government programs when considering having taken a loan from a formal source. With respect to the other outcomes for which we found a significant difference between those identified as poor in the PRA and those not identified (food security, asset ownership and the presence of an able bodied male) the coefficients are statistically different above a 10% confidence level only when compared to 2 or 3 of the 4 government programs.

Another important concern is how potential differences in the objectives of the PRA and government identification affects targeting. The PRA studied here was intended to identify a particularly poor population to participate in a local anti-poverty program. Government programs, on the other hand, reach millions of people and may target at a different poverty threshold. If the threshold for government assistance is set above the level captured in our sample of fairly impoverished households and targeting were perfect, we would expect to see all households in our sample receiving aid, which is not the case empirically. Even so, the threshold for identification may be different for government programs.

While different thresholds for some poverty measure does not necessarily affect the difference in means between households above and below the threshold (even though it affects levels), it

may affect how targeting is done. For example if the aim of the program is to reduce the number of households in poverty, targeting may focus specifically on households just below the threshold as it is easier to move these households above the poverty line. To investigate this possibility we plot the percentage of households participating in a given government program against quintiles of poverty measures in Figure 3.3. In some cases, the figure shows that a higher percentage of individuals in the lower quintiles are receiving aid or identified as poor in the PRA, suggesting targeting along this dimension of poverty, but the figure does not suggest an obvious targeting threshold at which the percent receiving aid drops and remains persistently low. Moreover, there does not appear to be a systematically different threshold for identification in the PRA and receiving government assistance.

A related concern is that the concept of poverty used for classification in the PRA is locally defined, thus our analysis includes village level fixed effects. Government programs, however, may be less concerned with targeting those who are relatively disadvantaged vis-à-vis their neighbors than with targeting according to state or national benchmarks. In light of this, we conduct similar analysis without fixed effects which compares targeting across rather than within villages. The estimates from this exercise (shown in Appendix Tables A3.2 and A3.3) are quite similar to those including village fixed effects. Comparing across villages, the estimated differences between recipients of BPL, Antodaya rationing or government housing support and non-recipients are striking similar to the within village comparisons; recipient are not notably worse off than non-recipients. For participants in employment generating schemes, the across village comparison with non-participants suggests that participants may be disadvantaged in some respects (they have lower monthly food consumption) but it no longer appears that they own less land.

Across villages, it remains the case that PRAs identify households which own less land, have limited credit access and are less likely to have an able bodied male member. The results with respect to food security and assets are somewhat attenuated without village fixed effects, but continue to indicate that households identified as poor in the PRA have greater food insecurity and fewer assets.

3.4.5 Analysis of Bandhan's Verification Process

In addition to conducting PRAs, Bandhan visited and interviewed households several times to identify those to be classified as Ultra Poor. In this section, we analyze how the additional verification narrowed the targeted population and how those identified as Ultra Poor differ from those not so identified.

The fourth column in Table 3.2 offers some insight into this question. It is apparent that households identified as Ultra Poor have less land. On average they have 6.5 katthas (0.13 acres) less and they are 12 percentage points more likely to be landless, differences which are both statistically different from zero at or above a 5% confidence level. In terms of assets, the Ultra Poor are in fact poorer on average; they live in smaller homes and own fewer durable goods and livestock, these differences are also significant at or above a 5% confidence level. Like those classified as poor in the PRA, the Ultra Poor are less likely to have obtained credit from a formal source, by 9 percentage points, but are no less likely to have obtained loans. They classify themselves as poorer and are less likely to report eating two meals a day, but the difference in unconditional means are not statistically different from zero. The Ultra Poor are also less educated, the average member of an Ultra Poor household has completed 0.7 less years of schooling than non Ultra Poor individuals, significant at the 1% level. Although the differences are not generally statistically different from zero, the table indicates that Ultra Poor households report higher expenditure than other households. Another noteworthy feature of Ultra Poor households is that only 69% include an able bodied adult male member whereas nearly 94% of not Ultra Poor households do, a difference which is statistically significant at the 1% confidence level.

To increase the precision of our comparison, we control for village specific characteristics. The results, shown in Panel A of Table 3.6, confirm what can be gleaned from the summary statistics. When including village fixed effects, however, it appears that Ultra Poor households spend more per capita than other households (although the difference is not statistically distinguishable from zero when conditioning on households size). We explore this result further in Section 3.4.5. Other than for expenditure, our analysis of the PRA alone and of Bandhan's identification process as a whole have similar implications. This is not particularly surprising, since Bandhan selects households with a high PRA rank to visit for subsequent verification.

Given the similarity of the results, we assess whether additional verification of the information collected in the PRA, as Bandhan does to identify the Ultra Poor, improves targeting of the poorest households beyond what is achieved by the PRA. To accomplish this we restrict our sample only to those households which were ranked as very or exceptionally poor in the PRA, leaving us with 111 observations. Of these 111 households Bandhan identified 85 as Ultra Poor and the remaining 26 as not Ultra Poor. Panel A of Table 3.7 compares the Ultra Poor households to the others. The point estimates, while not statistically significant, suggest that the Ultra Poor have higher expenditure even when compared only to others ranked very or exceptionally poor. In Panel B we control for household size which results in smaller, but still positive coefficients. In terms of assets, credit access, food security and self-classification of financial situation we can not make a clear distinction between the Ultra Poor and others. The most salient result is that Ultra Poor households own less land, 3.1 katthas less on average. The economic magnitude of this coefficient is quite large since it represents 125% of mean land holdings within this very or exceptionally poor group. The Ultra Poor also live in smaller homes on average.

We now turn to directly investigating what determines the likelihood that a household is identified as Ultra Poor via equation (3.2). When analyzing the full sample, the results reveal that the variables which appear to determine identification as Ultra Poor are generally the same as those which determine PRA rank. Therefore, we restrict to the sample of households ranked as very or exceptionally poor in the PRA for this analysis. Table 3.8 shows that for these households, the only significant determinates of identification as Ultra Poor are the presence of an able bodied adult male, which makes identification as Ultra Poor 19% less likely, and land holdings.

3.4.6 Revisiting Consumption

A noteworthy difference between the implications of Table 3.6 and the summary statistics is that the regression framework suggests that the Ultra Poor spend more than others and that these differences are statistically different from zero. In particular, our results suggest that the average Ultra Poor household spends Rs. 68 more per household member per month than not identified households and Rs. 36 more per household member per month on food and fuel.

The point estimates are considerable in magnitude since Rs. 36 represent 12% of the mean per capita monthly food and fuel expenditure.

Although consumption and expenditure are notoriously difficult to measure (see e.g. Deaton, 1997), making these particular variables imprecise, we are interested in ascertaining what drives these estimates given that per capita consumption is a widely used and important indicator of poverty. One factor which may cause us to observe Ultra Poor households spending more than non Ultra Poor households is if Ultra Poor households have experienced economic shocks (e.g. need to repair house damage or pay medical bills). This will be particularly true if having experienced such a shock makes a household more likely to be identified as Ultra Poor. Closer inspection of the expenditures enumerated by the households revealed that this phenomenon may occur; several of the most costly single expenditures were for institutional medical care. Moreover, the largest of these expenditures were reported by those identified as Ultra Poor; the maximum such expenditure reported by a not identified household is Rs. 10,000 ($\approx$ \$255) whereas identified households reported expenditures of Rs. 10,000, 12,000, 16,000, 35,000 and 60,000 ($\approx \$255 - 1,538$).

This concern is what motivated us to look separately at per capita monthly average expenditure less institutional medical expenditure in the preceding analysis. But that we continue to observe a positive point estimate for this outcome in Table 3.6 and do not find that suffering a medical or economic shock makes a household particularly likely to be identified as Ultra Poor in Table 3.8 does not provide robust evidence for this hypothesis.

Since they tend to own much less land, it may be that the Ultra Poor spend more on food because they do not produce anything for home consumption and the non Ultra Poor may underestimate the value of what they produce at home. Since we lack complete information on home production we are unable to test this conjecture directly. We do, however, investigate this concern by restricting our sample only to those households with 15 or fewer katthas (0.3 acres) of land (this causes us to drop 21 observations or 10% of our sample). We run the same regressions for the expenditure variables as in Table 3.6, the results in Table 3.9 show that the differences in total and non food expenditure between the Ultra Poor and not Ultra Poor are amplified when considering only these households. In terms of food and fuel expenditure, the estimate of the difference between the two groups is essentially the same. This suggests that

home production of food in not the primary reason for these differences.

Additionally, although our initial survey is designed to capture all consumption, rather than just expenditure, we created a supplementary survey instrument with more detailed questions pertaining to production for own consumption and returned to the households in this study. Due to migration and absences we were not able to resurvey 11% of the households in the initial dataset. Using the data collected in this secondary survey, we again compared levels of per capita consumption between those households identified as Ultra Poor and other households. Table 3.10 presents the results from this analysis. Columns 1 and 2 repeat the analysis from Table 3.6, using the initial data but restricted to the sample which was resurveyed. Columns 3 and 4 use the data from the secondary survey. Again the point estimates suggest that households identified as Ultra Poor consume more per capita than other households, both in terms of food and fuel consumption and total consumption. These differences, however, do not appear statistically significant, as was the case when considering the initial data. That the estimates using the data from this additional, more detailed, survey are similar to those obtained using the initial data suggests that failure to capture production for own consumption is not responsible for the perplexing sign of the coefficients.

Given the potential importance of household economies of scale, we condition on household size in Panel B. When using the data from the detailed consumption resurvey the coefficients on the Ultra Poor indicator take the predicted negative sign in these regressions, but the estimates are not statistically different from zero. That the point estimates, conditional on household size, suggest that Ultra Poor households spend more than others in one survey and less than others in a secondary survey of the same households limits the credibility of the initial results; our analysis can not distinguish clear differences between the two groups in terms of per capita consumption.

To further explore the hypothesis of household economies of scale, we also ran the expenditure comparison regressions using the disaggregated components of per capita monthly food and fuel expenditure. When considering each item separately the coefficient on having been identified as Ultra Poor generally remains positive, as is shown in Table 3.11. These coefficients, however, are imprecisely estimated; the only variables for which we can detect a statistically significant difference are "Other food" and "Fuel and Light." The latter finding in particular,

coupled with the observation that Ultra Poor households tend to have fewer members, suggests that there may be economies of scale driving our previous results; if a home is to be lit or a meal cooked regardless of how many people reside in that home, then per capita fuel and light expenditure will appear larger in a smaller household.

3.5 Conclusions

Targeting a sub-population can be challenging, particularly when the target group is designated by a broad, ill-defined characteristic such as "extreme poverty." Various mechanisms can be employed to learn who the poorest of the poor actually are. Censuses which record household characteristics are one such method. This approach, however, captures only a limited set of poverty metrics and suffers from the fact that many indicators of poverty are not easily observable. Another commonly used targeting method is to conduct group discussions, such as a PRA, which rely not only on the responses of a specific household but also on the input of their neighbors to ascertain which households are most disadvantaged.

In this paper, we consider the relative performance of each of these mechanisms with respect to identifying the poorest of the poor. In particular, we evaluate how well classification as impoverished according to a particular method accords with statistical measures of poverty collected in a detailed household survey.

We firstly examine various government assistance programs which utilize a census as part of their targeting process. Our results suggest that these programs do not overwhelmingly reach the very poorest, which may be due to deficiencies in the identification process. Subsequently, we evaluate whether PRAs reliably identify the poorest households within a village. We compare characteristics of households ranked as especially poor in the PRA by their neighbors to other disadvantaged households within the village. The comparison indicates that the ranking from the PRA accurately identifies a poorer sub-population in terms of land holdings, assets and credit access.

Finally, since the PRA was part of a more extensive process conducted by Bandhan, a Kolkata-based microfinance institution, to identify the poorest of the poor, we consider what further gains can be made by verifying the information from the PRA with household visits.

We find that the additional steps taken by Bandhan narrows the identified population to those who are more disadvantaged in crucial respects, particularly land holdings.

Although our results do not indicate that either the PRA or government procedures particularly target the poorest of the poor in terms of consumption, which is a crucial measure of poverty, we do find that participatory targeting methods, such as a PRA, perform better than census techniques in identifying households which are most disadvantaged according to various other important measures of poverty.

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Figure 3.1: CDF of Land Holdings

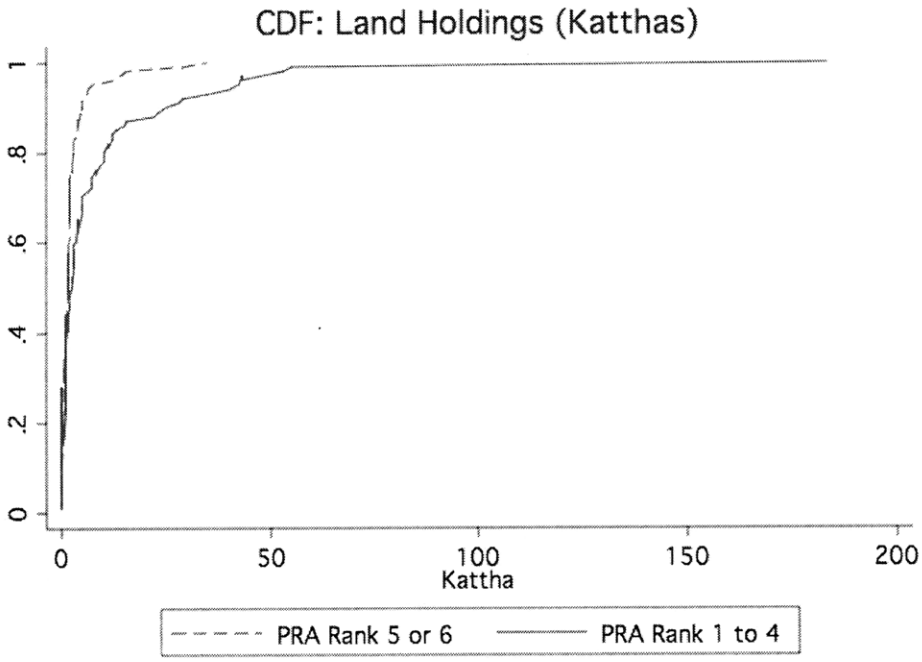
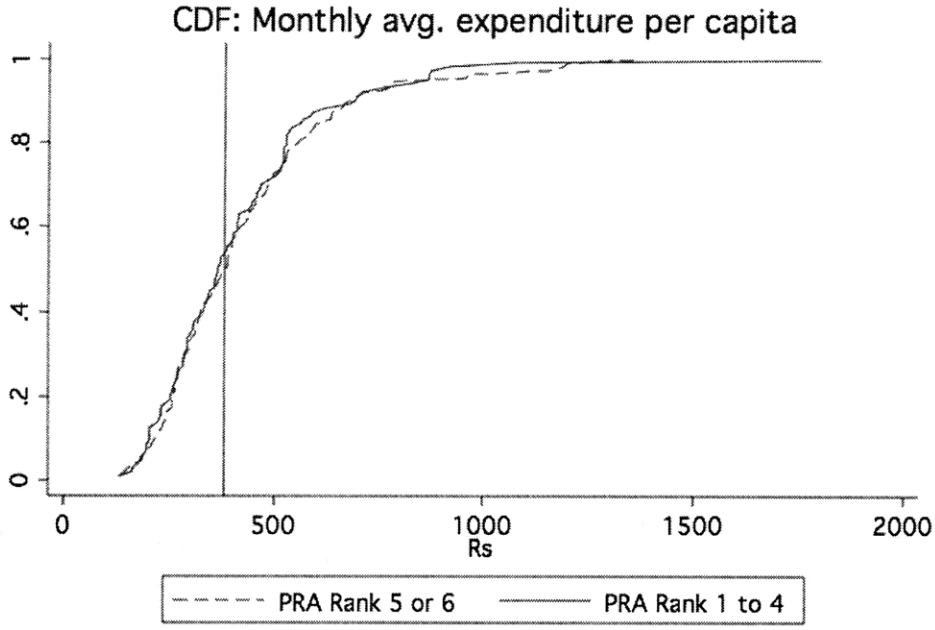


Figure 3.2: CDFs of Expenditure



Veritical line indicates 2005 povertly line for rural West Bengal

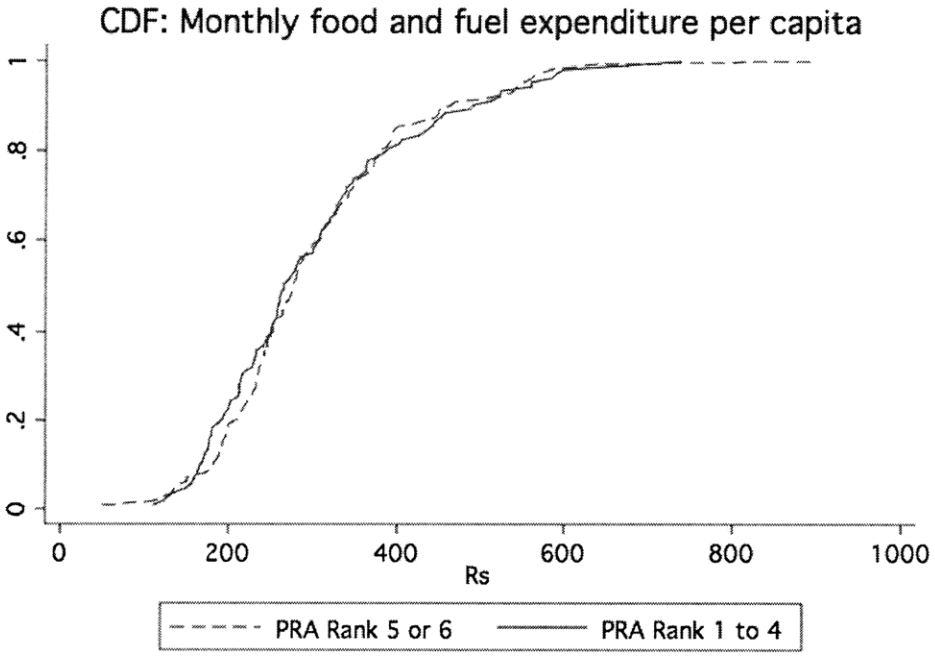
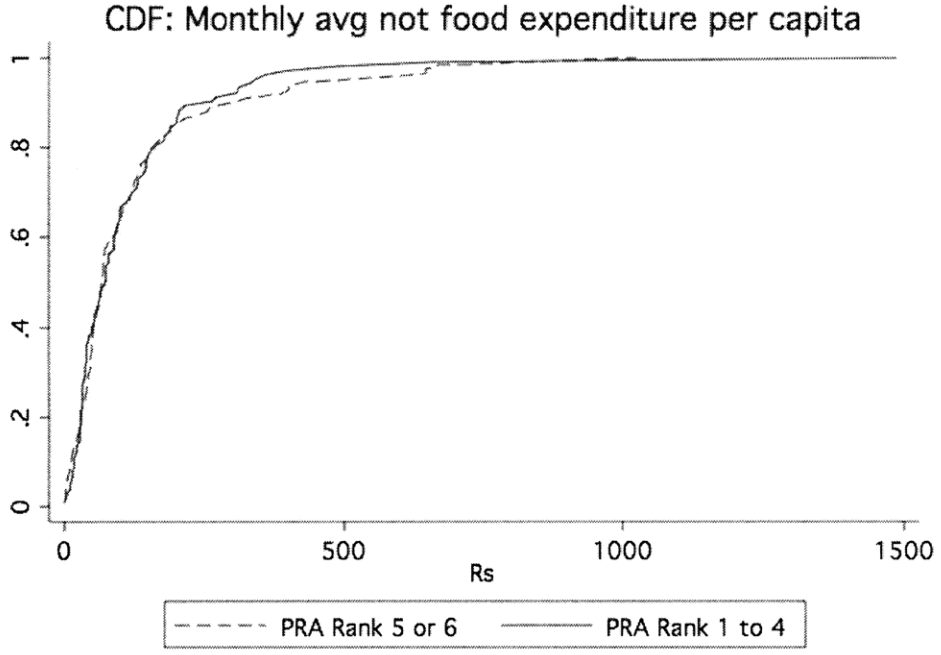


Figure 3.2: CDFs of Expenditure (continued)



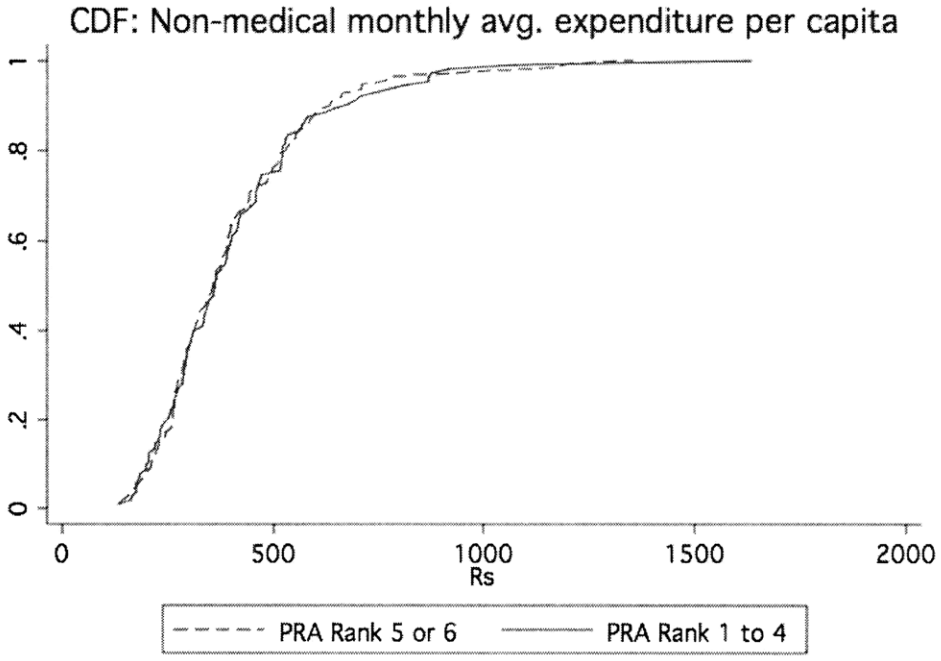
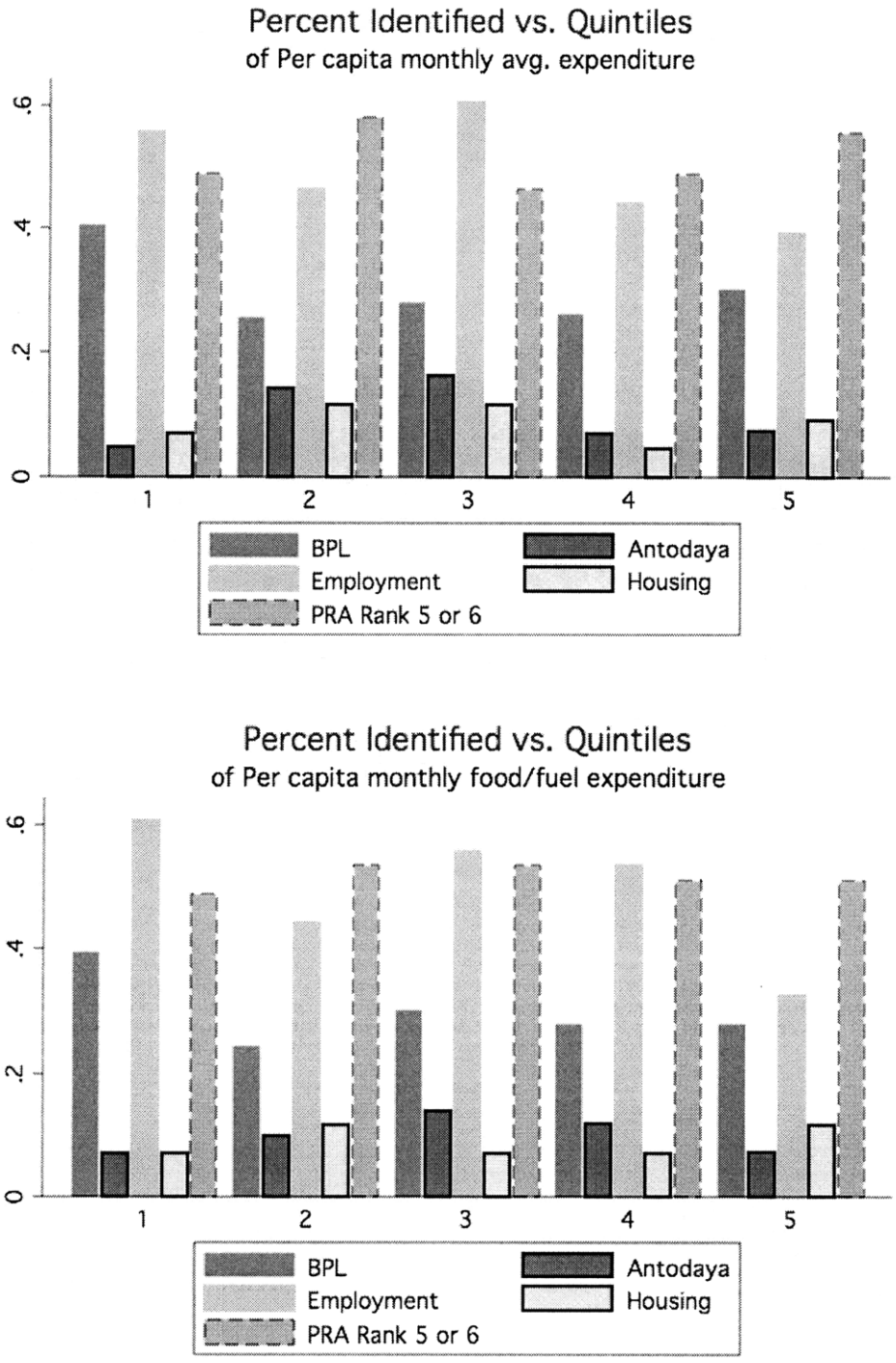
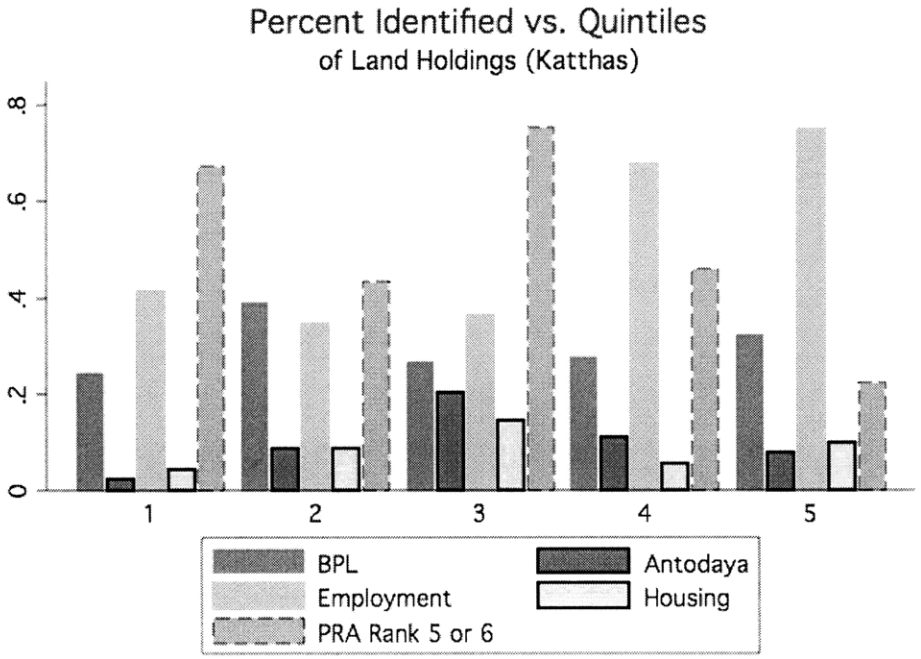


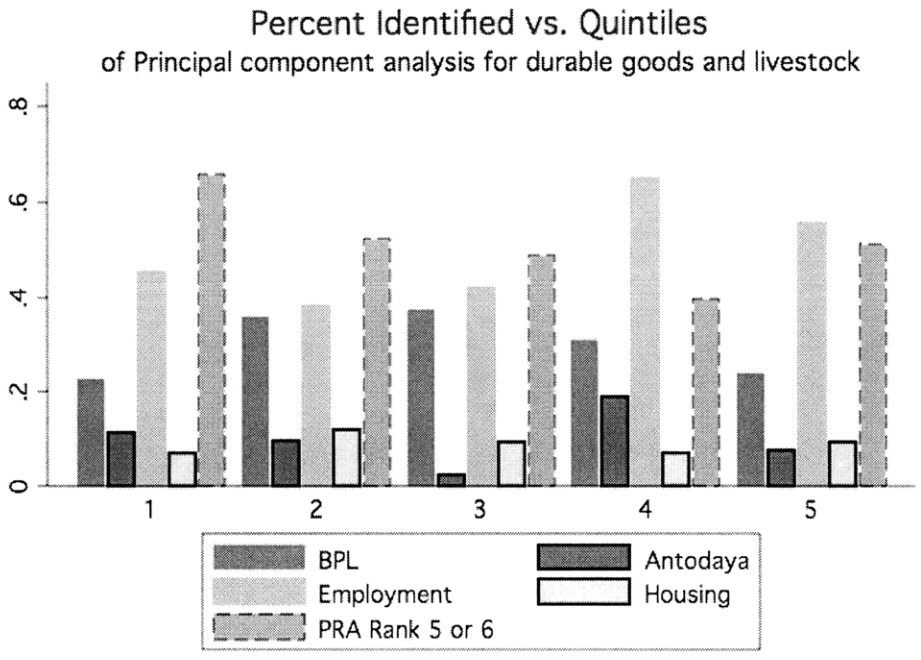
Figure 3.3: Percent Receiving Aid/PRA Rank 5 or 6 by Quintiles



Notes: The graphs show the percentage of households in the quintile of the variable indicated in the graph title who participate in the indicated government program or were identified as especially poor in the PRA.

Figure 3.3: Percent Receiving Aid/PRA Rank 5 or 6 by Quintiles (continued)





Notes: The graphs show the percentage of households in the quintile of the variable indicated in the graph title who participate in the indicated government program or were identified as especially poor in the PRA.

Table 3.1: Village-wise Sample Breakdown

Village	Number of households found in economic census	Number of households eligible for additional survey	Size of the sample for additional survey	Identified as Ultra Poor by Bandhan
Balarampur	855	254	20	38
Binkar	273	110	40	11
Chardiar	128	65	43	9
Charsungai	137	75	38	17
Khidirpur	364	101	37	18
Total	**1757**	**605**	**178**	**93**

Notes: Table shows figures pertaining to sample frame and sample selection. Column 1 shown the number of households enumerated in the village census, column 2 indicates how many met the sample selection criteria and column 3 indicates how many were surveyed. Column 4 indicates how many additional households selected by

Table 3.2: Selected Characteristics of Sample Households

	Full Sample		Non Ultra Poor		Ultra Poor (N=92)		Ultra Poor - Non Ultra Poor	
	Mean (1)	sd (2)	Mean (3)	sd (4)	Mean (5)	sd (6)	Diff. (7)	sd Diff. (8)
Ranking from PRA	4.55	1.39	3.76	1.25	5.59	0.74	1.83	(0.15)***
Number of household members	4.29	1.69	4.69	1.54	3.77	1.76	-0.91	(0.23)***
Per capita monthly avg. expenditure	425	229	404	218	453	241	49	(31.45)
Per capita monthly food/fuel expenditure	302	127	288	111	321	145	32	(17.42)*
Per capita monthly non-food expenditure	123	174	116	167	132	184	17	(24.01)
Per Capita monthly avg. expenditure minus institutional medical expenditure	406	215	393	208	423	223	30	(29.53)
Below official poverty line (for rural West Bengal 2005)	0.51	0.50	0.56	0.50	0.45	0.50	-0.11	(0.07)
Land Holdings (Katthas)	5.63	15.43	8.42	19.85	1.91	2.79	-6.51	(2.11)***
Landless	0.21	0.41	0.16	0.37	0.28	0.45	0.12	(0.06)**
Number of rooms in house	1.29	0.52	1.39	0.60	1.15	0.36	-0.24	(0.07)***
Principal component analysis for durable goods and livestock	1.61	1.12	1.75	1.22	1.41	0.94	-0.34	(0.15)**
Household has outstanding loan	0.46	0.50	0.43	0.50	0.48	0.50	0.05	(0.07)
Household has outstanding loan from formal source	0.08	0.28	0.12	0.33	0.03	0.18	-0.09	(0.04)**
Self classification of financial situation (1-10 scale)	2.38	1.53	2.51	1.54	2.22	1.51	-0.29	(0.21)
Average years of schooling per household member	1.23	1.75	1.54	1.89	0.82	1.45	-0.72	(0.24)***
There is a HH member 5-14 years old not attending school	0.25	0.43	0.23	0.42	0.28	0.45	0.05	(0.06)
Regularly eat two meals a day	0.67	0.47	0.70	0.46	0.62	0.49	-0.07	(0.07)
Household gets BPL rationing	0.30	0.46	0.33	0.47	0.27	0.45	-0.06	(0.06)
Households has Antodaya card	0.10	0.30	0.09	0.29	0.11	0.31	0.02	(0.04)
Received work from employment generating scheme	0.49	0.50	0.56	0.50	0.41	0.49	-0.15	(0.07)**
House from Indira Housing Plan	0.09	0.29	0.05	0.22	0.14	0.35	0.09	(0.04)**
Household suffered health shock	0.52	0.50	0.55	0.50	0.48	0.50	-0.07	(0.07)
Household suffered health shock requiring institutional care	0.22	0.41	0.23	0.42	0.20	0.41	-0.03	(0.06)
Household suffered economic shock	0.41	0.49	0.40	0.49	0.43	0.50	0.03	(0.07)
Able bodied male adult (15+)	0.83	0.37	0.94	0.23	0.69	0.47	-0.25	(0.05)***
Able bodied female adult (15+)	0.98	0.14	0.98	0.16	0.99	0.10	0.01	(0.02)

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: The table shows selected summary statistics for this sample. The unit of observation in the household. Statistics are shown separately for households identified as Ultra Poor by Bandhan's targeting process and household which were not.

Below the poverty line for rural West Bengal is defined as having per capita consumption under Rs. 382.82 (Based on estimates in "Poverty Line Estimates in Public Distribution System and Other Sources of Household Consumption, 2004-05." National Sample Survey Organization, Ministry of Statistics and Programme Implementation Report No. 510).

Table 3.3: Characteristics of Recipients of Government Aid

	Per capita monthly avg expenditure	Per capita monthly food/fuel expenditure	Per capita monthly non-food expenditure	Per Capita monthly avg. expenditure minus institutional medical expenditure	Land Holdings (Katthas)	Number of rooms in house	Regularly eat two meals a day	Self classification of financial situation (1-10 scale)	Household has outstanding loan	Household has outstanding loan from formal source	Below official poverty line (for rural West Bengal 2005)	Principal component analysis for durable goods and livestock	Able bodied male adult (15+)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Household gets BPL rationing	-7.23	7.93	-15.16	-1.42	-2.29	0 00	-0.03	-0 08	0 00	0.01	0.01	-0.09	0.11
	(34.82)	(18.85)	(26.70)	(32.61)	(2.31)	(0.08)	(0.07)	(0.23)	(0.08)	(0.04)	(0.07)	(0.17)	(0.06)*
Observations	213	213	213	213	208	212	213	213	213	213	213	213	213
R-Squared	0.04	0.08	0.02	0.03	0.08	0.06	0 04	0.03	0.01	0 1	0.07	0.02	0 04
Mean of dependent variable	426	303	123	407	5.66	1.29	0.66	2.38	0.46	0.08	0.51	1.59	0.83
Households has Antodaya card	-17 62	7.13	-24 75	-11.71	-1.79	-0.08	0.16	0.19	-0.15	-0.02	0 14	0.14	-0.09
	(50.35)	(28.62)	(38.50)	(48.47)	(3.57)	(0.12)	(0.11)	(0.35)	(0.12)	(0 06)	(0.11)	(0.26)	(0.09)
Observations	208	208	208	208	203	207	208	208	208	208	208	208	208
R-Squared	0.05	0.09	0.03	0 04	0.07	0.06	0.05	0.03	0.02	0.09	0.09	0.02	0.03
Mean of dependent variable	419	302	117	404	5.4	1.29	0.65	2.38	0.46	0.08	0.51	1.58	0.83
Received work from employment generating scheme	-32 54	-27.34	-5.21	-32.68	-4.33	0.04	0.00	-0.44	0.08	0.01	0.13	0.18	0.16
	(38.99)	(21.06)	(29.96)	(36.50)	(2.59)*	(0.09)	(0.08)	(0.26)*	(0.09)	(0.05)	(0.08)	(0.19)	(0.06)***
Observations	215	215	215	215	210	214	215	215	215	215	215	215	215
R-Squared	0.04	0 09	0.02	0.04	0.09	0.06	0.04	0 04	0 01	0.09	0.08	0.03	0 06
Mean of dependent variable	425	302	123	406	5.63	1 29	0.67	2.38	0.46	0.08	0.51	1.61	0.83
House from Indira Housing Plan	-41 31	31.78	-73.10	-19 96	-0.77	-0.05	0.11	0.24	0.19	0.07	-0.03	-0.16	-0.24
	(55.57)	(30.06)	(42.42)*	(52.11)	(3.77)	(0.13)	(0.11)	(0.37)	(0.12)	(0.06)	(0.12)	(0.27)	(0.09)***
Observations	214	214	214	214	209	213	214	214	214	214	214	214	214
R-Squared	0.04	0.09	0 03	0.03	0.08	0.06	0.04	0.03	0.02	0.09	0.07	0.03	0.06
Mean of dependent variable	426	303	123	407	5.65	1.29	0 67	2.39	0.45	0.08	0.51	1.61	0.83

* Significant at the 10% confidence level
** Significant at the 5% confidence level
*** Significant at the 1% confidence level

Notes: Each panel shows a separate set of regressions where the indicator variable specified in the row is taken as the independent variable and the variable indicated in the column heading is the dependent variable. All regressions include village fixed effects.

Table 3.4: Analysis of PRA Identification Process

	Per capita monthly avg expenditure (1)	Per capita monthly food/fuel expenditure (2)	Per capita monthly non-food expenditure (3)	Per Capita monthly avg. expenditure minus institutional medical expenditure (4)	Land Holdings (Katthas) (5)	Number of rooms in house (6)	Regularly eat two meals a day (7)	Self classification of financial situation (1-10 scale) (8)	Household has outstanding loan (9)	Household has outstanding loan from formal source (10)	Below official poverty line (for rural West Bengal 2005) (11)	Principal component analysis for durable goods and livestock (12)	Able bodied male adult (15+) (13)
							Panel A						
PRA Rank of 5 or 6	28 31	9 75	18.56	5.79	-6.32	-0.05	-0.17	-0.28	0.09	-0.11	-0.06	-0.43	-0.24
	(33 27)	(18.04)	(25.54)	(31.22)	(2 17)***	(0 07)	(0.07)**	(0 22)	(0 07)	(0.04)***	(0.07)	(0.16)***	(0.05)***
Observations	215	215	215	215	210	214	215	215	215	215	215	215	215
R-Squared	0 04	0.08	0.02	0.03	0.11	0.06	0.06	0.04	0.02	0 13	0.07	0.05	0 12
							Panel B: Conditional on Household Size						
PRA Rank of 5 or 6	0.74	-4 69	5.43	-23.56	-5.80	-0.01	-0.17	-0.29	0 10	-0.10	0.01	-0.26	-0 19
	(32 01)	(17 42)	(25.46)	(29 51)	(2 20)***	(0 07)	(0 07)**	(0 23)	(0 07)	(0.04)***	(0 07)	(0 15)*	(0.05)***
Number of household members	-46 01	-24 09	-21.92	-48 98	0 93	0 07	0 01	-0 02	0 02	0.02	0 10	0.28	0.09
	(9 24)***	(5 03)***	(7.35)***	(8.52)***	(0.64)	(0.02)***	(0 02)	(0 07)	(0.02)	(0.01)*	(0 02)***	(0.04)***	(0 01)***
Observations	215	215	215	215	210	214	215	215	215	215	215	215	215
R-Squared	0.14	0 17	0.06	0.17	0 12	0 11	0.07	0 04	0 02	0 14	0.19	0.21	0 25
Mean of dependent variable	425	302	123	406	5 63	1 29	0.67	2 38	0.46	0.08	0.51	1.61	0.83

* Significant at the 10% confidence level
** Significant at the 5% confidence level
*** Significant at the 1% confidence level

Notes: Each column is a regression where the dependent variable, indicated in the column heading, is regressed on an indicator variable for the household having been assigned a PRA rank equal to 5 or 6 (Panel A) or this indicator and the number of household members (Panel B). Binary outcome variables are estimated via a linear probability model.
All regressions include village fixed effects.

Table 3.5: Determinates of PRA Rank

	PRA Rank of 5 or 6						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Per capita monthly avg. expenditure	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Number of household members	-0.04	-0.04	-0.04	-0.03	-0.05	-0.04	0
	(0.02)*	(0.02)*	(0.02)*	(0.02)	(0.02)**	(0.02)*	(0.02)
Land Holdings (Katthas)	-0.01	-0.01	-0.01	0.00	-0.01	-0.01	-0.01
	(0.00)**	(0.00)**	(0.00)**	(0.00)*	(0.00)**	(0.00)**	(0.00)***
Household suffered health shock	0.06						
	(0.07)						
Household suffered health shock requiring institutional care		0.04					
		(0.08)					
Household suffered economic shock			-0.07				
			(0.07)				
Average years of schooling per household member				-0.05			
				(0.02)**			
There is a HH member 5-14 years old not attending school					0.18		
					(0.08)**		
Receives some form of government aid						0.04	
						(0.08)	
Able bodied male adult (15+)							-0.36
							(0.09)***
Observations	210	210	210	210	210	210	210
R-Squared	0.18	0.18	0.18	0.20	0.20	0.18	0.23
Mean of dependent variable	0.51	0.51	0.51	0.51	0.51	0.51	0.51

* Significant at the 10% confidence level
** Significant at the 5% confidence level
*** Significant at the 1% confidence level
Notes: The table shows a linear probability model specification where the dependent variable is an indicator variable that the household was assigned a PRA rank equal to 5 or 6.
All regressions include village fixed effects.

Table 3.6: Analysis of Bandhan's Identification Process

	Per capita monthly avg. expenditure (1)	Per capita monthly food/fuel expenditure (2)	Per capita monthly non-food expenditure (3)	Per Capita monthly avg. expenditure minus institutional medical expenditure (4)	Land Holdings (Katthas) (5)	Number of rooms in house (6)	Regularly eat two meals a day (7)	Self classification of financial situation (1-10 scale) (8)	Household has outstanding loan (9)	Household has outstanding loan from formal source (10)	Below official poverty line (for rural West Bengal 2005) (11)	Principal component analysis for durable goods and livestock (12)	Able bodied male adult (15+) (13)
							Panel A						
Identified as Ultra Poor	68.50	35.78	32.72	45.09	-6.15	-0.21	-0.13	-0.26	0 09	-0.07	-0.13	-0.46	-0.26
	(33.74)**	(18.29)*	(26.05)	(31 76)	(2.23)***	(0.08)***	(0.07)*	(0.23)	(0.08)	(0.04)*	(0.07)*	(0.16)***	(0.05)***
Observations	215	215	215	215	210	214	215	215	215	215	215	215	215
R-Squared	0.05	0.1	0 02	0.04	0.11	0.09	0.05	0.04	0 01	0.1	0.08	0.06	0.13
							Panel B: Conditional on Household Size						
Identified as Ultra Poor	36.28	19 1	17.18	10.48	-5.48	-0.16	-0.12	-0.27	0.1	-0.05	-0.05	-0.25	-0.2
	(32.85)	(17.89)	(26.18)	(30.41)	(2.28)**	(0.08)**	(0.07)*	(0.23)	(0.08)	(0.04)	(0.07)	(0.15)	(0.05)***
Number of household members	-43 93	-22.74	-21.19	-47.19	0.86	0.06	0.01	-0 02	0.02	0.02	0.10	0.28	0.08
	(9.27)***	(5.05)***	(7 39)***	(8 59)***	(0.65)	(0.02)***	(0.02)	(0.07)	(0.02)	(0.01)*	(0.02)***	(0.04)***	(0.01)***
Observations	215	215	215	215	210	214	215	215	215	215	215	215	215
R-Squared	0.15	0 18	0.06	0.16	0.11	0.13	0.05	0 04	0.02	0.12	0.19	0.21	0.25
Mean of dependent variable	425	302	123	406	5.63	1.29	0.67	2.38	0.46	0.08	0.51	1.61	0.83

* Significant at the 10% confidence level
** Significant at the 5% confidence level
*** Significant at the 1% confidence level

Notes: Each column is a regression where the dependent variable, indicated in the column heading, is regressed on an indicator variable for the household having been identified as Ultra Poor by Bandhan (Panel A) or this indicator and the number of household members (Panel B) Binary outcome variables are estimated via a linear probability model.
All regressions include village fixed effects

Table 3.7: Analysis of Bandhan's Identification Process (Sample restricted to households with PRA rank of 5 or 6)

	Per capita monthly avg. expenditure (1)	Per capita monthly food/fuel expenditure (2)	Per capita monthly non-food expenditure (3)	Per Capita monthly avg. expenditure minus institutional medical expenditure (4)	Land Holdings (Katthas) (5)	Number of rooms in house (6)	Regularly eat two meals a day (7)	Self classification of financial situation (1-10 scale) (8)	Household has outstanding loan (9)	Household has outstanding loan from formal source (10)	Below official poverty line (for rural West Bengal 2005) (11)	Principal component analysis for durable goods and livestock (12)	Able bodied male adult (15+) (13)
						Panel A							
Identified as Ultra Poor	65.61	20.56	45.04	46.08	-3.08	-0.35	-0.01	-0.07	0.04	0.03	-0.15	-0.20	-0.18
	(53.42)	(29.48)	(41.69)	(49.62)	(1.17)***	(0.11)***	(0.11)	(0.35)	(0.12)	(0.04)	(0.12)	(0.24)	(0.10)*
Observations	111	111	111	111	108	110	111	111	111	111	111	111	111
R-Squared	0.09	0.09	0.05	0.06	0.09	0.12	0.08	0.05	0.03	0.05	0.09	0.06	0.12
						Panel B: Conditional on Household Size							
Identified as Ultra Poor	46.1	10.1	36	23.09	-2.98	-0.32	-0.01	-0.12	0.06	0.04	-0.09	-0.12	-0.14
	(50.71)	(28.09)	(41.24)	(45.16)	(1.18)**	(0.11)***	(0.12)	(0 35)	(0.12)	(0.04)	(0.11)	(0.22)	(0.10)
Number of household members	-43.55	-23.35	-20.20	-51.33	0.16	0.06	0.00	-0.11	0.04	0.01	0.12	0.19	0.10
	(11.74)***	(6.50)***	(9.55)**	(10.45)***	(0.27)	(0.03)**	(0.03)	(0.08)	(0.03)	(0.01)	(0.02)***	(0.05)***	(0.02)***
Observations	111	111	111	111	108	110	111	111	111	111	111	111	111
R-Squared	0.19	0.19	0.09	0.24	0.1	0.16	0.08	0.07	0.04	0.06	0.25	0.17	0.26
Mean of dependent variable	430	303	127	402	2.49	1.25	0.61	2.24	0.49	0.03	0.5	1.46	0.72

* Significant at the 10% confidence level
** Significant at the 5% confidence level
*** Significant at the 1% confidence level

Notes: Each column is a regression where the dependent variable, indicated in the column heading, is regressed on an indicator variable for the household having been identified as Ultra Poor by Bandhan (Panel A) or this indicator and the number of household members (Panel B). Binary outcome variables are estimated via a linear probability model.

The sample is restricted to households assigned a PRA rank of 5 or 6.

All regressions include village fixed effects.

Table 3.8: Determinates of Identification as Ultra Poor (Sample restricted to households with PRA rank of 5 or 6)

	Identified as Ultra Poor						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Per capita monthly avg. expenditure	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Number of household members	-0.02	-0.02	-0.02	-0.02	0	-0.02	0
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)
Land Holdings (Katthas)	-0.02	-0.02	-0.02	-0.02	-0.02	-0.02	-0.02
	(0.01)**	(0.01)**	(0.01)**	(0.01)**	(0.01)**	(0.01)**	(0.01)**
Household suffered health shock	-0.02						
	(0.08)						
Household suffered health shock requiring institutional care		-0.03					
		(0.10)					
Household suffered economic shock			0.08				
			(0.08)				
Average years of schooling per household member				-0.02			
				(0.03)			
There is a HH member 5-14 years old not attending school					-0.15		
					(0.09)		
Receives some form of government aid						0.02	
						(0.09)	
Able bodied male adult (15+)							-0.19
							(0.10)*
Observations	108	108	108	108	108	108	108
R-Squared	0.19	0.19	0.20	0.20	0.21	0.19	0.22
Mean of dependent variable	0.77	0.77	0.77	0.77	0.77	0.77	0.77

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: The table shows a linear probability model specification where the dependent variable is a zero one indicator for having been identified as Ultra Poor by Bandhan.

The sample is restricted to households assigned a PRA rank of 5 or 6.

All regressions include village fixed effects.

Table 3.9: Analysis of Bandhan's Identification Process
(Sample restricted to households with less than 15 kattas of land)

	Per capita monthly avg. expenditure (1)	Per capita monthly food/fuel expenditure (2)	Per capita monthly non-food expenditure (3)	Per Capita monthly avg. expenditure minus institutional medical expenditure (4)
		Panel A		
Identified as Ultra Poor	80.66	37.86	42.80	57.40
	(32.60)**	(19.26)*	(23.62)*	(30.96)*
Observations	194	194	194	194
R-Squared	0.07	0.08	0.03	0.05
	Panel B: Conditional on Household Size			
Identified as Ultra Poor	50.22	20.35	29.87	24.28
	(31.42)	(18.63)	(23.69)	(29.24)
Number of household members	-43.54	-25.05	-18.49	-47.38
	(8.93)***	(5.30)***	(6.74)***	(8.31)***
Observations	194	194	194	194
R-Squared	0.17	0.18	0.06	0.19
Mean of dependent variable	421	303	118	401

* Significant at the 10% confidence level
** Significant at the 5% confidence level
*** Significant at the 1% confidence level
Notes: Each column is a regression where the dependent variable, indicated in the column heading, is regressed on an indicator variable for the household having been identified as Ultra Poor by Bandhan (Panel A) or this indicator and the number of household members The sample is restricted to households owning less than 15 kattas of land.
All regressions include village fixed effects.

Table 3.10: Results from Resurvey of Households

	Per capita monthly avg. expenditure (1)	Per capita monthly food/fuel expenditure (2)	Per capita monthly avg. expenditure (Resurvey) (3)	Per capita monthly food/fuel expenditure (Resurvey) (4)
		Panel A		
Identified as Ultra Poor	88.15	39.48	92.13	72.32
	(38.34)**	(20.47)*	(69.15)	(53.78)
Observations	190	190	190	190
R-Squared	0.06	0.1	0.03	0.04
	Panel B: Conditional on Household Size			
Identified as Ultra Poor	56.38	23.32	-3.28	-5.26
	(37.14)	(19.94)	(60.02)	(45.84)
Number of household members	-45.61	-23.21	-137.00	-111.41
	(10.11)***	(5.43)***	(16.34)***	(12.48)***
Observations	190	190	190	190
R-Squared	0.15	0.18	0.3	0.33
Mean of dependent variable	434	304	722	563

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: Each column is a regression where the dependent variable, indicated in the column heading, is regressed on an indicator variable for the household having been identified as Ultra Poor by Bandhan (Panel A) or this indicator and the number of household members Columns 1 and 2 utilize data from the initial survey, restricted to households which were resurveyed. Columns 3 and 4 utilize data obtained in the resurvey of households.
All regressions include village fixed effects.

Table 3.11: Disaggregated Components of Food and Fuel Consumption

	Cerials	Pulses	Dairy	Oil	Vegetables	Fruit and Nuts	Egg, Fish, Meat	Other food (sipce, sugar, salt, beverage)	Pan, Tobacco, etc.	Fuel and Light
Identified as Ultra Poor	10.00	0.21	-2.10	5.37	3.18	1.36	4.05	7.46	1.15	5.09
	(8.77)	(2.09)	(1.45)	(4.19)	(5.65)	(1.05)	(3.13)	(3.32)**	(3.81)	(1.75)***
Observations	215	215	215	215	215	215	215	215	215	215
R-Squared	0.05	0.04	0.02	0.07	0.06	0.03	0.01	0.08	0.02	0.09
Mean of dependent variable	136	13	3	28	53	1	15	26	15	13

* Significant at the 10% confidence level

** Significant at the 5% confidence level

*** Significant at the 1% confidence level

Notes: Each column is a regression where the dependent variable, indicated in the column heading, is regressed on an indicator variable for the household having been identified as Ultra Poor by Bandhan.

All regressions include village fixed effects.

Table A3.1: Comparing Government Targeting to PRA Targeting

	Per capita monthly avg. expenditure (1)	Per capita monthly food/fuel expenditure (2)	Per capita monthly non-food expenditure (3)	Per Capita monthly avg. expenditure minus institutional medical expenditure (4)	Land Holdings (Katthas) (5)	Number of rooms in house (6)	Regularly eat two meals a day (7)	Self classification of financial situation (1-10 scale) (8)	Household has outstanding loan (9)	Household has outstanding loan from formal source (10)	Below official poverty line (for rural West Bengal 2005) (11)	Principal component analysis for durable goods and livestock (12)	Able bodied male adult (15+) (13)
						Household gets BPL rationing							
Difference in coefficients	35.55	1.82	33.73	7.21	-4 02	-0.05	-0.15	-0 20	0.09	-0.12	-0 06	-0 34	-0.35
Chi-squared: Difference	0.64	0.00	1 28	0 03	4 50	0.26	2.23	0 35	0.66	4.89	0 40	2 84	24 09
p-value. Difference	0 42	0.94	0.26	0 86	0 03	0 61	0.14	0.55	0.42	0 03	0 53	0 09	0.00
						Households has Antodaya card							
Difference in coefficients	45.94	2.62	43 32	17 50	-4.53	0.03	-0.34	-0.46	0.24	-0.09	-0.19	-0 56	-0 15
Chi-squared· Difference	0.62	0.00	1 61	0 09	5 46	0.05	10.27	1.63	3.88	3.92	2.15	4 00	2.00
p-value. Difference	0.43	0.94	0.20	0 76	0 02	0 82	0.00	0.20	0.05	0 05	0 14	0 05	0 16
						Received work from employment generating scheme							
Difference in coefficients	60.86	37.09	23 77	38.47	-1.98	-0.10	-0 17	0 16	0.01	-0.12	-0.18	-0 60	-0 41
Chi-squared· Difference	1.06	1 29	0 35	0.44	0 35	0.62	2.45	0 27	0 01	5.36	2 32	5 98	19 56
p-value. Difference	0.30	0.26	0 56	0 51	0 55	0 43	0 12	0.61	0.93	0.02	0 13	0 01	0.00
						House from Indira Housing Plan							
Difference in coefficients	69.63	-22.03	91 66	25 75	-5 54	0.00	-0.28	-0.52	-0.10	-0.19	-0.03	-0.27	0.00
Chi-squared Difference	1.89	0.45	6 86	0.28	5 25	0.00	5 60	1.96	0.70	5.18	0.04	1 58	0 00
p-value Difference	0.17	0.50	0 01	0 59	0 02	0 99	0 02	0.16	0.40	0.02	0.84	0 21	1.00

Notes· The table shows, for each government program considered, the difference between the coefficient on an indicator for having been ranked 5 or 6 in the PRA when then outcome indicated in the column heading is taken as the dependent variable (from Table 3.4) and the corresponding coefficient on an indicator for participating in the government program indicated in the panel headings (from Table 3.3). The table also shows the test statistic and p-value from a test for equality of the two coefficients.

Table A3.2: Characteristics of Recipients of Government Aid (Across Village Comparison)

	Per capita monthly avg. expenditure (1)	Per capita monthly food/fuel expenditure (2)	Per capita monthly non-food expenditure (3)	Per Capita monthly avg. expenditure minus institutional medical expenditure (4)	Land Holdings (Katthas) (5)	Number of rooms in house (6)	Regularly eat two meals a day (7)	Self classification of financial situation (1-10 scale) (8)	Household has outstanding loan (9)	Household has outstanding loan from formal source (10)	Below official poverty line (for rural West Bengal 2005) (11)	Principal component analysis for durable goods and livestock (12)	Able bodied male adult (15+) (13)
Household gets BPL rationing	-11.85	2.39	-14 24	-4.54	-2.12	0.04	-0 05	0 03	0.02	0.01	0.03	-0.12	0.13
	(34.45)	(19.12)	(26 17)	(32 24)	(2 34)	(0 08)	(0.07)	(0.23)	(0.07)	(0.04)	(0 08)	(0.17)	(0.06)**
Observations	213	213	213	213	208	212	213	213	213	213	213	213	213
R-Squared	0	0	0	0	0	0	0	0	0	0	0	0	0.03
Mean of dependent variable	426	303	123	407	5 66	1.29	0 66	2.38	0.46	0 08	0.51	1.59	0.83
Households has Antodaya card	-9.09	13.27	-22.36	-4 52	-2.67	-0.06	0.12	0.27	-0.14	-0.04	0 12	0 06	-0.07
	(50.66)	(29.28)	(38.18)	(48 50)	(3.61)	(0.12)	(0.11)	(0.35)	(0 11)	(0.06)	(0.12)	(0 26)	(0.09)
Observations	208	208	208	208	203	207	208	208	208	208	208	208	208
R-Squared	0	0	0	0	0	0	0.01	0	0.01	0	0	0	0
Mean of dependent variable	419	302	117	404	5.4	1.29	0.65	2.38	0.46	0.08	0.51	1.58	0.83
Received work from employment generating scheme	-20.34	-38.89	18.55	-23.19	1.41	0.12	0.05	-0.36	0.07	0.10	0.09	0.24	0.13
	(31.32)	(17.19)**	(23.78)	(29.29)	(2.13)	(0 07)*	(0.06)	(0.21)*	(0.07)	(0.04)**	(0 07)	(0.15)	(0.05)**
Observations	215	215	215	215	210	214	215	215	215	215	215	215	215
R-Squared	0	0.02	0	0	0	0.01	0	0.01	0	0.03	0 01	0.01	0.03
Mean of dependent variable	425	302	123	406	5.63	1.29	0 67	2.38	0.46	0.08	0.51	1.61	0.83
House from Indira Housing Plan	-27.51	36.25	-63.77	-8.57	0.54	-0.03	0 08	0.27	0.20	0.09	-0.04	-0.21	-0.22
	(55.23)	(30.57)	(41.81)	(51.71)	(3.82)	(0 13)	(0.11)	(0.37)	(0.12)	(0.07)	(0.12)	(0.27)	(0.09)**
Observations	214	214	214	214	209	213	214	214	214	214	214	214	214
R-Squared	0	0.01	0.01	0	0	0	0	0	0.01	0.01	0	0	0 03
Mean of dependent variable	426	303	123	407	5.65	1.29	0 67	2.39	0.45	0.08	0.51	1.61	0.83

* Significant at the 10% confidence level
** Significant at the 5% confidence level
*** Significant at the 1% confidence level

Notes: Each panel shows a separate set of regressions where the indicator variable specified in the row is taken as the independent variable and the variable indicated in the column heading is the dependent variable. Regressions in this table do not include village fixed effects.

Table A3.3: Analysis of PRA Identification Process (Across Village Comparison)

	Per capita monthly avg expenditure (1)	Per capita monthly food/fuel expenditure (2)	Per capita monthly non food expenditure (3)	Per Capita monthly avg expenditure minus institutional medical expenditure (4)	Land Holdings (Katthas) (5)	Number of rooms in house (6)	Regularly eat two meals a day (7)	Self classification of financial situation (1-10 scale) (8)	Household has outstanding loan (9)	Household has outstanding loan from formal source (10)	Below official poverty line (for rural West Bengal 2005) (11)	Principal component analysis for durable goods and livestock (12)	Able bodied male adult (15+) (13)
							Panel A						
PRA Rank of 5 or 6	9.57	1 35	8 21	-8 19	-6.47	-0 07	-0.11	-0.29	0.06	-0.12	-0 03	-0 3	-0.23
	(31 35)	(17 41)	(23.82)	(29.34)	(2 09)***	(0 07)	(0 06)*	(0.21)	(0.07)	(0 04)***	(0.07)	(0 15)**	(0 05)***
Observations	215	215	215	215	210	214	215	215	215	215	215	215	215
R-Squared	0.00	0 00	0 00	0 00	0 04	0 00	0.01	0.01	0 00	0 04	0.00	0.02	0.10
							Panel B: Conditional on Household Size						
PRA Rank of 5 or 6	-22 85	-17 73	-5 12	-42.5	-5 57	-0 02	-0 1	-0 29	0.08	-0 1	0.04	-0 12	-0 17
	(30 26)	(16.67)	(24 00)	(27.81)	(2 11)***	(0.07)	(0 07)	(0.21)	(0.07)	(0.04)**	(0.07)	(0 14)	(0.05)***
Number of household members	-46.38	-27.3	-19 08	-49 1	1 37	0.08	0 01	0	0.02	0.03	0.11	0 26	0.08
	(8.94)***	(4 93)***	(7 09)***	(8.22)***	(0 62)**	(0 02)***	-0.02	-0.06	-0.02	(0.01)***	(0 02)***	(0 04)***	(0 01)***
Observations	215	215	215	215	210	214	215	215	215	215	215	215	215
R-Squared	0 11	0 13	0 03	0.14	0 07	0 07	0.01	0.01	0.01	0 08	0.14	0.17	0 24
Mean of dependent variable	425.24	302 43	122.82	405.92	5.63	1 29	0.67	2.38	0 46	0.08	0.51	1 61	0 83

* Significant at the 10% confidence level
** Significant at the 5% confidence level
*** Significant at the 1% confidence level

Notes: Each column is a regression where the dependent variable, indicated in the column heading, is regressed on an indicator variable for the household having been assigned a PRA rank equal to 5 or 6 (Panel A) or this indicator and the number of household members (Panel B). Binary outcome variables are estimated via a linear probability model.
Regressions in this table do not include village fixed effects.