

**Property Rights and Household Time Allocation in Urban Squatter Communities:
Evidence from Peru[†]**

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[†] In addition to presenting new data on household time use and neighborhood organizations, this article summarizes earlier findings from Field (2002), Entitled to Work: Urban Property Rights and Labor Supply in Peru. Research Program in Development Studies Working Paper #220, Princeton University.

1 Introduction

Urban property reform is an issue of current importance in many developing countries. Among policy-makers, property titling is increasingly considered an effective form of government intervention for targeting the poor and encouraging economic growth in urban areas (Baharoglu, 2002; Binswanger et al, 1995). While there exists a wide body of literature demonstrating the positive influence of property institutions on market outcomes, there is little micro-economic evidence documenting the cost of informality to individual households. This paper contributes to the literature by examining the time costs of informal tenure arrangements in squatter communities. A number of authors such as Carter (1994, 1996) and Galal and Razzaz (2001) have noted that, in many settings, informal institutions arise to compensate for the absence of formal property protection. In such settings, one of the principal gains of strong property institutions is to shift the burden of property protection and enforcement away from individual households and informal communities to the state. Hence, an important outcome of titling efforts that effectively increase household tenure security should be to allow households and communities to reallocate time away from this role.

To explore the relationship between institutional reform and household-level resource allocation, this paper examines whether improvements in tenure security that result from giving property titles to urban squatters in Peru influence the time allocation of household members across labor and leisure activities inside and outside the home. In particular, I explore whether receiving a legal property title reduces the likelihood that households keep individuals at home and inside the community to protect property and leads them to increase employment and leisure hours outside the home. The idea underlying these predictions is that, in the absence of formal legal property protection, households and informal communities have an incentive to provide their own human resources for residential security, and thus to consider the need to stay at home or close to home when making time allocation choices. Hence, I also examine whether strengthening property institutions encourages households to relocate entrepreneurial activity from inside to outside the home. Finally, because property formalization presumably shifts the burden of land protection from individual communities to the state, I examine whether titling is also associated with a reduction in the level of land-related neighborhood governance and an increase in the role of formal law enforcement at the neighborhood level.

I study these relationships using data from a dramatic nation-wide titling program in Peru, in which 1.2 million property titles were distributed to urban squatters on public land, the largest urban property rights reform that has occurred in the developing world. My empirical strategy takes advantage of the Peruvian titling program as a natural experiment by making use of staggered regional program timing. A general difficulty in studying the influence of property titles and a weakness inherent in the past literature is concern over endogeneity that arises in comparing titled and untitled households. In particular, the tenure status of a given household is generally a function of the household demand for legal protection, which is likely to be related to factors affecting household and community time allocation choices. The Peruvian reform, in which all households were “assigned” property titles irrespective of household demand for formalization, helps isolate the causal effect of property titling on household behavior by allowing me to compare households in early program neighborhoods to households in late program neighborhoods, all of which lie within the set of eventual program participants. Because it is impossible to completely rule out potential endogeneity of program timing, the second technique of my identification strategy is to make use of a comparison group of households in early and late neighborhoods that already possessed a property title before the program. In this manner I estimate the program effect in a difference in difference framework, in which the difference in the behavior of program beneficiaries and non-beneficiaries in early program areas is compared to the difference in late program areas.

While there is a large body of research examining the influence of property titles on market outcomes, the influence of property titles as outlined in the existing literature has focused almost entirely on three outcomes established in a seminal paper by Besley (1995): gains from trade in land, greater investment incentives, and improved credit access. Among these, a leading motivation for property reforms is the notion that strengthening ownership rights will encourage lenders to use property as collateral in loan transactions. To quote Peruvian economist and urban property reform advocate Hernando de Soto (2000), “In the midst of their own poorest neighbourhoods and shanty towns, there are trillions of dollars, all ready to be put to use ... [Yet] because the rights to these possessions are not adequately documented, these assets cannot readily be turned into capital, cannot be traded outside of narrow local circles where people know and trust each other, cannot be used as collateral for a loan, and cannot be used as a share against an investment.” In line with this reasoning, government land titling programs are widely

considered a critical instrument for increasing access to credit among the poor (Binswanger, et al., 1999).¹ Empirical estimates of the collateral value of property titles in agricultural settings corroborate the predictions of the Besley model, including studies by Alston et al. (1996), Lopez (1997) and Carter and Olinto (1997) that link land titles with improved credit access. In addition, many authors including Feder (1998), Besley (1995), Banerjee et al. (2002) and Alston et al. (1996) provide evidence that lack of property title affects agricultural investment demand.²

Meanwhile, the relationship between institutional reform and household time allocation has been absent from empirical studies of property rights. This is likely due to the fact that the past literature largely has been confined to agricultural settings, while decisions related to spending time outside the community or away from residential property are most relevant to urban households. At the same time, there is reason to believe that investment demand and credit supply will be less relevant outcomes for urban compared to rural households. Hence, the paper compliments the existing literature by considering one of the unique welfare implications of urban property titling efforts

My findings suggest that land titling is indeed associated with substantial changes in household and community time use patterns. In particular, formal property ownership is associated with a significant reduction in the amount of time household members spend inside the home, including a 48% decrease in the fraction of households that locate entrepreneurial activities inside the home and a 36% reduction in the fraction of households that report keeping individuals at home to protect property. In a parallel fashion, urban land titling is also associated with a greater number of both labor and leisure hours spent outside the home: newly titled households work an average of 17% more hours than do squatter households awaiting a title, and are also 38% more likely to participate in organized activities outside the home. Finally, although household members in titled communities spend a greater number of total leisure hours in activities outside the home, they are significantly less likely to participate in neighborhood groups responsible for public goods provision, including property allocation and protection, in informal communities. Meanwhile, the number of households that have used the formal judicial

¹ Feder and Feeny (1991) claim that “the most commonly recognized benefit from legal titling of land is the use of those secure ownership rights as collateral to solicit credit.”

² Other work, such as Migot-Adholla et al. (1998) and Kimuyu (1994) detect little impact of land titling on investment. The mixed results are commonly attributed to the difficulty of addressing the endogeneity of title status.

system is significantly higher in titled communities. All of these results suggest that households in titled communities indeed devote fewer human resources to informal property protection, both at the household and the community levels.

2 Conceptual Framework³

There are three principal mechanisms by which it is assumed that individuals in informal communities devote time to increasing tenure security. First, untitled households can provide informal policing either by guarding individual properties or participating in community enforcement efforts to protect neighborhood boundaries and individual properties from other squatters.⁴ If prospective squatters seek out abandoned land, signaling that the property is occupied may deter conflicts over land or property boundaries. As the following quote illustrates, anecdotal evidence suggests that urban squatters are commonly constrained by the need to keep a family member at or close to home to protect against residential property invasion:

“‘I go to work, and my mother looks after the house,’ says Alejandrina Matos Franco, who sells cassettes on the street in Lima and who worries that people could seize her house when she is away.” (Conger, 1999)

Second, reducing the probability of government eviction at the community level may require a critical mass of individuals squatting on neighborhood land, particularly in early stages of community formation. Finally, households may attempt to increase tenure security through formal channels by completing administrative steps to acquire land rights. In Peru, as in many developing countries, the legal process of acquiring formal property titles traditionally involved substantial monetary and time costs. According to one report, “In Peru, the process of getting a deed from the bureaucracy involved 207 steps divided among 48 government offices, took an average of 48 months to complete, and was too expensive for small property owners.” (*Economist*, 1995).

³ The next three sections borrow extensively from Field (2002).

⁴ In a related sense, it is reasonable to assume that untitled households face a greater threat from burglary given that it is more costly for them to contact local law enforcement in addition to the fact that households that do not have legal rights to a residence may have less legal claim to property inside the home.

As long as untitled households expend their own human resources in an effort to solidify informal claims to land through any of these activities related to private property protection and community enforcement, the acquisition of a property title has direct value in terms of freeing up hours of work previously devoted to maintaining tenure security through informal means. The influence of such changes in tenure security on household time allocation can be captured in a simple variation of the basic agricultural household model, in which labor hours of household members are divided between work at home (H_f) and work in the outside market (H_o), and time spent at home (Z) is divided between work at home (H_f) and leisure (L).⁵ In addition, the extended model incorporates a tenure security function, $s(\cdot)$, into the household utility function, such that home security is determined by total hours of household time at home (time spent by individuals protecting property), an exogenous parameter, θ , which reflects the household's level of formal property rights, and a summary measure, τ , which reflects the degree of informal or "de facto" rights the household has acquired. The parameter θ can be thought of either as a binary indicator of a legally registered property title, or as a more nuanced parameter which reflects the level of formal legal recognition of a household's tenure status (e.g., level of efficiency of court systems, level of police cooperation, etc.). In this framework, hours spent inside the community, which includes both leisure and home production time, enters household utility through the effect on home security in addition to their respective consumption and production values. Hence, household utility is an increasing function of per capita leisure, consumption, and home security:

$$U(\bar{x}, \bar{l}, s; \psi, E), \text{ where } s = s(Z, \theta, \tau)$$

⁵ Here, N is the number of household members, and l_i is leisure, x_i consumption, h_{fi} labor hours in home production, and h_{oi} outside labor hours of household member i , and

$$L = \sum_{i=1}^N l_i, H_f = \sum_{i=1}^N h_{fi}, H_o = \sum_{i=1}^N h_{oi}, X = \sum_{i=1}^N x_i, \bar{x} = \frac{X}{N}, \bar{l} = \frac{L}{N}, Z = H_f + L.$$

The value of labor at home is given by the production function $q(H_f)$, while the value of work outside the home is the market wage w . A formal presentation of the model including details of the comparative statics are provided in Field (2002).

The implications of the household's ability to increase tenure security through informal mechanisms on the optimal allocations of labor and leisure can be derived by maximizing the above utility function, where the choice variables for the household are: H_f, H_o, X, L and s .

The constraints to the maximization problem are:

$$s = s(H_f + L, \theta, \tau)$$

$$pX = wH_o + q(H_f)$$

$$T = L + H_o + H_f$$

$$L, H_o, H_f, X \geq 0$$

Substituting the constraints into the utility function and taking the total derivatives of the first-order conditions yields the following inequalities for values of w , θ , and τ :

$$\frac{\partial H_f}{\partial \theta} < 0 \text{ and } \frac{\partial H_o}{\partial \theta} > 0 .$$

These conditions imply that, in aggregate, strengthening formal property rights decreases work hours inside the community and increases time spent working outside. Intuitively, this reflects the fact that an exogenous increase in the level of formal property rights corresponds to a decrease in the household's need to spend time on home protection, thereby lowering the opportunity cost of outside labor force hours. In addition, two auxiliary implications follow from this model. First, the effect of a change in formal property rights on labor supply is decreasing in the household's level of informal property rights, τ :

$$\frac{\partial^2 H_f^*}{\partial \theta \partial \tau} > 0 \text{ and } \frac{\partial^2 H_o^*}{\partial \theta \partial \tau} < 0$$

Second, given average consumption level x , the effects are decreasing in the number of working-age household members, N .

$$\frac{\partial^2 H_f^*}{\partial \theta \partial N} > 0 \text{ and } \frac{\partial^2 H_o^*}{\partial \theta \partial N} < 0 .$$

The intuition behind the family size effect is that, the more family members living in a household, the more likely it is that someone chooses to stay at home independent of security considerations. Thus, large households are less distorted by the need to keep watch over the residence.⁶ These predictions will motivate me to test empirically whether the effect of acquiring a formal property title on labor supply differentially impacts households of different sizes and with different lengths of residential tenure.

The model can also be extended to incorporate differences in the household supply of adult and child labor when only adults contribute to security provision. This extension formalizes the intuitive idea that, if adults have a comparative advantage in the provision of home security, in the absence of secure property rights, children will substitute for adults in the labor market. In this case, while total household labor hours rise with an increase in formal rights, child labor hours will actually fall. Adult comparative advantage is particularly likely when security is provided through neighborhood organizations.

3 Project Background

Peru's informal urban settlements grew out of the massive urban-rural migration that occurred over the last half century as a result of the collapse of the rural economy (due in part to a failed land reform program) and the growth of terrorism. The existence of extensive barren land owned by the state on the perimeters of major cities, along with an implicit housing policy during the 1980s that allowed squatter settlements on unused government lands, led to an extended era of urban migration, often in the form of organized invasions by squatters from the same area of emigration (Olórtégui, 2001).⁷ It is estimated that in 1997, a quarter of Peru's urban population lived in marginal squatter settlements in peri-urban areas and many more untitled residents occupied inner-city neighborhoods (World Bank, 1997b).⁸

Prior to the reforms, obtaining a property title for a Peruvian household was nearly impossible due to heavy bureaucratic procedures and prohibitive fees. As described in the initial

⁶ Consistent with these implications, Lanjouw and Levy (2002) find that levels of informal tenure security in urban communities in Ecuador vary systematically with observable household characteristics such as sex of household head and length of residence.

⁷ Invasion of privately-owned property was allowed by law if the land had been unused for a period of four years. The law has since changed (in 1990) to prohibit invasions of private property under any circumstance.

⁸ See Appendix 2 for a country map of the untitled population and properties targeted for formalization.

project report: “Peru’s traditional system of titling and registration is complex, inefficient, expensive – prohibitively so for poorer people – and prone to rent-seeking. Fourteen different agencies are involved in the generation of each title, the courts have rarely been able to validate these titles as the law requires...” (World Bank, 1998a).⁹ Due to acute housing shortages and lack of legal transparency, tenants struggled not only with the government but also among themselves to secure residential properties. The common failure of the government to defend or even recognize informal tenure rights in individual disputes gave rise to rent-seeking behavior in the form of invasions of untitled land (Olórtegui, 2001).

In 1991, a Peruvian non-governmental organization embarked on an innovative property titling project in the capital city of Lima whose goal was “the rapid conversion of informal property into securely delineated land holdings by the issuing and registering of property titles” (World Bank, 1998b). Between 1992 and 1995, roughly 200,000 titles were issued at an extremely low cost, convincing the government and a growing international audience of the potential for efficiency gains from urban property formalization (World Bank, 1998a). In 1996, under the auspices of the public agency COFOPRI (Committee for the Formalization of Private Property) and *Decree 424: Law for the Formalization of Informal Properties*, the Peruvian government established a national property registry based on the early model to formalize the remaining properties in Lima and extend the program to seven other cities.^{10,11}

Just as in the pilot project, implementation of the national program involved area-wide titling by neighborhood, which was “presumed to foster, through community participation and education, a demand for formalization, reduce the unit cost of formalization, and rapidly generate a minimum critical mass of beneficiaries” (World Bank, 1997c). While the old process of acquiring a property title was prohibitively slow and expensive, the new process was free and extremely rapid. Once a local property registration system was set up, local program officials were trained, and the city’s target areas were properly identified and mapped, several project

⁹ In his groundbreaking study of the underground economy, economist Hernando de Soto documented the same phenomenon: “In ‘The Other Path’, de Soto and aids concluded that ... to get title to a house in an informal settlement whose permanence the government had already acknowledged took 728 steps from one agency alone, and ten other agencies also required approval” (Rosenberg, 2000).

¹⁰ Specific legislation related to COFOPRI is listed in Appendix 4.

¹¹ According to the World Bank Project Appraisal Document (1998), target cities were chosen according to a formula based on city size, density of informal settlements, and distance from commercial centers, measures indicating the likely ease and cost of formalization and the expected poverty impact.

teams simultaneously entered neighborhoods starting from different points in the city.¹² To be eligible for program participation, title claimants were required to verify residency predating 1995, and to live on eligible public properties.¹³ As a result of the reforms, by December 2001 nearly 1.2 million of the country's previously unregistered residents became nationally registered property owners, affecting approximately 6.3 million of the roughly 10 million untitled residents living in the range from just above to below the poverty line.¹⁴

In the realm of literature on the economic benefits of tenure security, the Peruvian experience provides a unique research opportunity for many reasons. The national formalization plan constitutes a one-of-a-kind natural experiment worldwide in terms of providing nearly cost-free improvements in ownership security on such a large scale. Unlike many large-scale government programs, the titling efforts took place at an extremely rapid pace, which facilitates program evaluation by eliminating much of the need to consider time trends that could obscure the independent effects of program participation. At the same time, in the absence of panel data on participating households, the fact that program timing was staggered proves to be an asset for evaluation purposes. A survey of 2750 urban households was conducted in March 2000 midway through program implementation. Because the sample was drawn from the universe of all target populations for eventual program intervention, the data contain a number of households in neighborhoods in which the program has not yet entered.

¹² In campaigns of two months each, project teams entered fifty to seventy neighborhoods encompassing roughly 30,000 to 35,000 plots. Within a neighborhood, teams spent five to seven weeks establishing residential claims and delineating properties before conferring state-registered property titles onto all eligible residents. The registration process for these titles took an additional period of one to six months.

¹³ Ineligible properties included archeological sites and flood plains, among other exceptions – see page 22 for a description. In the COFOPRI data, 9.42% of sampled households are ineligible according to reported length of residence, and an additional 10% remain untitled after several years of program operation.

¹⁴ Even before the end of the grant period in December 2002, 1.64 million lots had already been formalized and 1.21 million titles granted, the vast majority of which took place between 1998 and 2000. While no residents who previously possessed registered municipal titles are included in this figure, it is uncertain what fraction of this number had locally registered sales documents before the national reforms as these households were included in the government's definition of "untitled", though in reality the program simply transferred such titles to the national registry. In this paper, the term "squatter" refers only to households with no sales or judicial titles prior to the reforms, which is estimated to be 37% of the target population.

4 Empirical Strategy

My empirical analysis of household responses to changes in formal property rights relies on the COFOPRI baseline survey data. The data consist of 2750 households sampled from the universe of all residences in non-incorporated urban and peri-urban settlements identified in the 1993 census of the eight cities targeted by the titling program. The survey was stratified on city, with cluster units of ten households randomly sampled at the neighborhood level within cities. The survey instrument contains a wide variety of information on household and individual characteristics in addition to five modules designed specifically to collect information on the range of economic and social benefits associated with property formalization.

To identify the impact of receiving a property title on time use, I exploit variation in the year in which the COFOPRI program entered a neighborhood to compare households in program neighborhoods that already have been reached by the survey date to households in late program neighborhoods. Neighborhoods are classified as having been reached by the time of the survey if more than one household in the cluster reports owning a COFOPRI title.¹⁵ Clusters in which no household or only one household have a COFOPRI title are assumed to be those in which the program has not entered, although it is generally impossible to separate the neighborhoods in which the program will never enter from those which will be treated eventually. Nonetheless, such neighborhoods share the key feature of no expected program effect.¹⁶ Since the households in the treated neighborhoods may or may not actually have received a government title by the time of the survey, I employ an intent-to-treat (ITT) analysis.

The effect of the program is presumed to increase over time on account of the growing fraction of titled households within a treated neighborhood and because household behavioral adjustments or confidence in the value of a COFOPRI title may exhibit a lagged response. For this purpose, year of program entry was defined as the earliest reported COFOPRI title year

¹⁵ The survey data do not directly identify program neighborhoods, nor can this variable currently be constructed by matching geographic identifiers to COFOPRI office data.

¹⁶ Reasons that households may be excluded include: the household cannot prove residence prior to 1995; the household belongs to a cooperative association; the residence lies on an archeological site, flood plane, mining site or private property; and ambiguous or disputed ownership claims. Unfortunately, none of the above information is collected in the survey. According to anecdotal evidence from program administrators, disputed claims within families or between neighbors are the most common reason that title distribution is delayed for an untitled household in a treated neighborhood (Carlos Gandolfo, personal interview, Lima, August 9, 2000).

within the cluster.¹⁷ Dynamic response was restricted to be linear in four time periods: January 1999 to June 2000, January 1997 to December 1998, January 1995 to December 1996, and January 1992 to December 1994. This division corresponds to three major waves of program expansion: From 1992 to 1995, 200,000 titles were granted by the Institute of Liberty and Democracy as part of a pilot project prior to COFOPRI; the first wave of COFOPRI titles was initiated in 1995 in Lima and Arequipa; and beginning in 1997 the program expanded into six other cities.¹⁸ Furthermore, these intervals were consistent with the observed relationship between subjective statements on tenure security and years since program entry.

Although target areas for wide scale economic development programs are never randomly selected, these data have the advantage that all sample members live in areas that eventually will be targeted for program intervention, increasing confidence in the comparability of treated and untreated households. Furthermore, the universal nature of the treatment and the participation rules of the program generally rule out concern over individual selection bias that could arise even if program placement were random. Nonetheless, there is still potential for program *timing* bias, in which areas selected for early program participation are different from the rest. If program timing is not randomly assigned to neighborhoods conditional on observables, a comparison of pre- and post-program neighborhoods will produce a biased estimate of program effect.

The influence of non-random city timing is easily resolved by including city fixed effects in the regression estimates.¹⁹ A more complicated source of program timing bias concerns the order in which project teams entered neighborhoods *within* cities. Whereas the available information on program timing suggests that program timing was largely exogenous to the economic environment of neighborhoods, without precise knowledge of the formula for

¹⁷ Due to the fact that not all households were given property titles right away and because of measurement error in title year reporting, households in the same cluster who had received a COFOPRI title did not necessarily report the same title year. When the minimum reported title year fell below the first regional title year according to program data, the second lowest title year was assigned to the cluster.

¹⁸ This region-specific pattern of intervention makes it important to include city dummies in regression estimates of program effect.

¹⁹ The only information on the ordering of cities comes from a vague statement in World Bank Project Report (#18359), which specifies that the order was designated in advance according to “ease of entry.” As far as neighborhood program timing, there appears to have been no specific algorithm in the program guidelines. The COFOPRI office claims only that order was subject to “geographical situation, feasibility to become regularized, dwellers’ requests, existing legal and technical documents, and linkages with other institutions involved in the existing obstacles” (Yi Yang, 1999).

neighborhood timing I cannot safely assume random assignment to treatment nor accurately specify a selection on observables model. Hence, cautious quasi-experimental analysis calls for an estimation strategy that is robust to potential selection on unobservables.

To reduce the role of endogenous program timing, my identification strategy makes use of a comparison group of non-beneficiary households. In a framework analogous to difference-in-difference (DID) estimation, I compare the difference in labor supply of potential program beneficiary and non-beneficiary households in neighborhoods that the program has reached to the difference in neighborhoods that have not yet been reached. The simple idea underlying this distinction is that the tenure security effect of titling disproportionately (or perhaps solely) benefits households with weak ex ante property claims, for whom the demand for tenure security is high.²⁰ To capture this, I make use of detailed survey data on past and present property titles to construct a binary indicator of whether a household had a title at the start of the titling program. Those who do not are labeled “squatters,” whereas the term “non-squatter” refers to households with pre-program titles.²¹

While the behavior of squatters may differ systematically from that of non-squatters due to any number of unobservable factors, identification of program effect will be robust as long as it is constant across program and non-program regions. Appendix 1 provides descriptive statistics on the sample population, allowing an informal check for random assignment of program timing. As the means in the table indicate, there is variation in some demographic characteristics across program and non-program regions. Namely, sample households in program areas on average have smaller dwellings (fewer rooms), are more likely to have electricity, and have higher nativity rates (percentage of members born in the province). Although statistically significant differences exist across program and non-program areas, no statistically significant differences in differences are observed between squatters and non-squatters in program and non-program areas (column 3). This finding supports the use of non-squatters as a comparison group.

²⁰ There were several ways a household might have obtained a property title in the era before the recent titling effort. First, there was always the lengthy and costly option of following the official bureaucratic process for obtaining and registering a municipal property title. Second, there were a handful of past isolated attempts at property reform in which interim titling agencies were set up by municipal governments in an effort to incorporate some proportion of informal residents (De Soto, 1986). Finally, on a number of occasions, mayoral and presidential candidates were known to distribute property titles in an effort to win voter support prior to an election (Yi Yang, 1999).

²¹ Throughout this paper, “squatter” will refer to households lacking property titles *prior* to the program.

To further minimize the potential role of heterogeneity bias, I also control for a large set of observable household and neighborhood characteristics in an effort to capture exogenous differences in household types between program and non-program areas. I also exploit two sources of predicted variation in the impact of the treatment on different household types. As implied by the theoretical model, I expect the impact of receiving a title to be decreasing in both the number of working age members and the level of informal property rights. This allows me to additionally estimate models that test for predicted heterogeneity in response to the program according to household size and residential tenure.²² Residential tenure is used as a summary measure of a household's level of informal property rights. This stems from the assumption that households with longer community membership can rely more heavily on community enforcement, documented in studies on informal property protection such as Lanjouw and Levy (2002) and De Soto (1986).

Because both household size and residential tenure are highly correlated with poverty but in opposite directions, the dual restriction that program effect be increasing in household size and decreasing in residential tenure heavily reduces concerns over program timing bias by eliminating the possible confounding role of any unobservable trends that are correlated with household poverty.²² In order for a regional trend in some unobservable determinant of labor supply to be mistakenly attributed to the program, its influence would have to be decreasing in both residential tenure and household size, and hence no such factor could be correlated with poverty in either direction.

4.1 Regression Model

The basic DID estimate of program effect is obtained from the following ordinary least squares regression:

$$L_i = \beta_0 + \beta_1(N) + \beta_2(N)^2 + \beta_3(squatter) + \beta_4(program) + \beta_5(program*squatter) + \alpha'X_i + e_i \quad (4.1.1)$$

²² Correlations between a 3-level poverty index and household size and length of residence verify these patterns in the COFOPRI baseline survey data.

where L_i refers to some measure of household time use; N is number of household members; *squatter* refers to a household with no pre-program property title; *program* indicates whether the household lives in a neighborhood that has been reached by the program; and X_i is a vector of demographic controls. The inclusion of controls for squatter and program fixed effects corresponds to a standard DID empirical specification. The coefficients of interest are the interaction between the dummy variables for squatter household and program entry, β_5 , which can be interpreted as the marginal change in the dependent variable for the average squatter household associated with program participation.

Variation in program response by residential tenure and household size is estimated by the empirical model:

$$L_i = \beta_0 + \beta_1(N) + \beta_2(N)^2 + \beta_3(\text{squatter}) + \beta_4(\text{program}) + \beta_5(\text{program}*\text{squatter}) + \beta_8(\text{tenure}) + \beta_9(\text{tenure}*\text{squatter}) + \beta_{10}(\text{tenure}*\text{program}) + \beta_{11}(\text{tenure}*\text{program}*\text{squatter}) + \beta_{12}(N*\text{squatter}) + \beta_{13}(N*\text{program}) + \beta_{14}(N*\text{squatter})^2 + \beta_{15}(N*\text{program})^2 + \beta_{16}(N*\text{program}*\text{squatter}) + \beta_{17}(N*\text{program}*\text{squatter})^2 + \alpha'X_i + e_i, \quad (4.1.2)$$

The variable *tenure* refers to the number of years a household has lived in a residence and N is the number of working-age household members. The quadratic term in family size captures the idea that leisure hours are likely to be correlated across household members, such that the likelihood that any household member is at home in a given moment is increasing with family size at a decreasing rate. Taking into account all of the relevant interactions, the estimated average program effect from equation 4.1.2 is $[\beta_5 + \beta_7(\text{mean \# program periods}) + \beta_{11}(\text{mean residential tenure}) + \beta_{16}(\text{mean household size}) + \beta_{17}(\text{mean HH size})^2]$.

The set of regressors contained in X_i is common to all regressions in the empirical section, and includes controls for the number of working-aged household members, city fixed effects, lot size and residential tenure, as well as a constant. In addition, X_i includes the following demographic controls: sex, age, education and degree level of household head; number of household members, number of school-age children, number of babies (ages 2—4), fraction of adults that are male, fraction of adults that are immigrants (born outside of province), and number of members age 70 and older; size of property, household residential tenure, whether the property has indoor plumbing, whether the property was acquired by invasion, and whether the property was inherited; whether the dwelling lies within walking distance of the nearest primary

school, secondary school, bus stop, public phone, and public market, and this indicator interacted with walking time to each locale; and whether the neighborhood has a local bus stop/market/public phone/primary and secondary school currently and whether each of these existed two years ago, and whether the neighborhood has a government school, child, food or general social assistance program. Precise variable definitions are provided in Appendix 3.

All regressions also include a set of dummy interactions between cities and program entry, and between cities and pre-program title status. The inclusion of these interactions absorbs potential regional variation in program implementation and regional differences in informal property institutions that could be driving relative differences in program impact between titled and untitled residents. It is arguable that the inclusion of such a wide set of demographic controls amounts to over-controlling. However, the proceeding results are robust to the exclusion and inclusion of a wide variety of right-hand-side variables. Finally, all estimates are adjusted to account for the sample clusters and strata, the standard errors derived from the Huber-White robust estimator for the variance-covariance matrix.²³

5 Empirical Results

5.1 Program Effect on Tenure Security

The first set of empirical results presented in Tables 1-3 concerns the direct impact of formal property titles on household tenure security. Property formalization will only alter individual and community time use patterns if becoming a titled property owner actually changes households' perceived probability of eviction. Survey data on household perceptions of eviction likelihood and experiences with receiving a title are therefore informative for verifying the presumed relationship between title acquisition and tenure security.

I begin by studying survey responses to the question, "What do you think are the main benefits of receiving a property title?" reported separately for titled and untitled households in Table 1. The pattern of responses illustrates that, although the primary motivation for land titling programs is commonly perceived to be improved credit access, higher collateral value of land is not the most common benefit associated with property titles among squatters. Instead, 75.5% of respondents in titled and 73.9% of respondents in untitled households cite tenure security as a

²³ For a description of the technique used to estimate standard errors, see Chapter 2.2 of Deaton (1998).

benefit of obtaining a property title – roughly twice the number that cite collateral. This evidence supports the assumption that informal tenure arrangements are not perfect substitutes for formal property rights in terms of lowering eviction likelihood in this setting. Hence, an important component of the cost of informality to households is the perceived risk of losing land.

Table 2 explores the following indicators: whether the household reported experiencing a change in tenure security with the acquisition of a property title, and whether eviction is considered “very likely” or “very unlikely.” Table 2 presents simple DID estimates based on the methodology described in the previous section. In particular, the difference between early and late program neighborhoods for squatter households is compared to the difference for households already in possession of a property title prior to the program. The means in column 1 indicate that squatters in program neighborhoods are significantly more likely to report an improvement in tenure security than are titled households. Similarly, columns 2 and 3 reveal that squatters that have participated in the titling program also report significantly lower current levels of eviction likelihood than do titled households living in program neighborhoods.

In addition to asking respondents whether they experienced a change in tenure security with titling, the survey also collected information on the source of the change in tenure security. Table 3 presents tabulations of the responses separately for COFOPRI title-owners and households that acquired a title prior to the program. Whereas the responses are not well defined in the sense that they include such vague answers as “value of a legal document,” it is worth noting that the most common response among those households that report a positive change in tenure security is that property titles “give tranquility” or peace of mind and reduce conflicts over land.

5.2 Program Effect on Time Spent Inside the Home

To connect the patterns in tenure security with patterns of household time allocation, I first look in the data for direct evidence that members of squatter households in program areas are spending more time at home and inside the community guarding property. Strong support for this hypothesis comes from a survey question conveniently included on the home security module, in which survey respondents were asked whether their home has a list of security measures, including a person at home protecting the property. The question does not collect

information on the number of people or amount of time spent at home, nor does it identify whether individuals are staying home for that reason only. Nonetheless, a DID probit analysis of the binary indicator shows significant differences among squatter households pre- and post-program, presented in Table 4.²⁴ Results from these estimates indicate that, in neighborhoods in which the titling program has entered, pre-program squatters are significantly less likely to keep a person at home guarding property. Not surprisingly given that the variable does not distinguish between keeping a person home for several hours a day versus a few hours a week, this measure depends heavily on the number of household members. For the average squatter household with four working-age members and fifteen years of residence, the marginal effect of two-years with a property title is approximately -0.37 . In other words, the household is 45% less likely to report keeping a person at home to guard property. For households with more than four working-age people, there is no discernable difference, as all of these households are substantially likely to report having someone at home.

A separate outcome related to the time family members spend inside the home is whether or not members of a household participate in market work at home, explored in Table 5a. In the sample, 24.3% of households report running a business from home.²⁵ If in-home work increases tenure security in the absence of a property title, either by directly protecting the residence or by facilitating participation in community groups, the marginal value of in-home work should fall when formal property rights are secured and there is no longer a security incentive to stay at home. As a result, newly unconstrained decision makers will have an incentive to allocate resources more efficiently by moving production outside of the home or by finding work with an outside employer. The probit estimates presented in Column 1 of Table 5a support these predictions. The marginal effect implied by the coefficient on the interaction term between squatter and program periods is an 11.6 percentage point reduction in the likelihood of using the residence as a source of economic activity for the average squatter household with two program periods – implying a reduction in the rate of home business activity of approximately 47%.

²⁴ Unlike the simple mean differences presented in Table 3, to obtain precise estimates of household time use patterns the estimates in Table 4 control for demographic differences between households and regional factors that could confound measures of program impact.

²⁵ The exact survey question was: “Do you participate in some economic activity within your home or use part of your property as a source of economic activity?”

Furthermore, as reported in column 2, a significantly higher fraction of households in titled neighborhoods that work at home say they would prefer to work outside of the home. Responses to a separate survey question indicate that the most common reason for working inside among households in both program and non-program neighborhoods *that would prefer to relocate* is financial constraints. Given that credit constraints are likely to be even larger among untitled households, it is striking that a greater fraction of titled households would rather move business activities outside the home. This suggests that there is indeed a shift in preferences away from working at home when households obtain formal property rights.

Consistent with this story is the pattern of tenure security among home business owners presented in Table 5b. Here, the perceived tenure security of households that use their residence for entrepreneurial activities is compared with the security of all other residents in program and non-program neighborhoods. The mean differences reveal that, in titled neighborhoods, tenure security is negatively correlated with home business activity as economic theory would predict.²⁶ In contrast, the opposite relationship is observed in untitled neighborhoods, among which home business owners report significantly lower levels of tenure security than other residents.

5.3 Program Effect on Time Outside of the Home

The next set of results explores survey data on individual household members' time use in work and leisure activities outside the home. Here the idea is that, if lack of a property title leads household members to spend more time at home, once the titling program reaches an area, the hours untitled households spend on activities unrelated to security provision, including employment and other recreational activities outside the home, should rise and approach that of title-holders.

5.3.1 Labor Supply

Table 6 presents the coefficient estimates of interest from model 4.1.2 where the dependent variables are total weekly hours of work per household, fraction of household

²⁶ In other words, in the absence of a security incentive to stay home, risk of eviction should reduce the incentive to invest in entrepreneurial activities inside the home.

members in the labor force, and child labor force participation.²⁷ As described in the previous section, the estimates in Table 6 allow the program effect to vary by time since program entry, length of residence and family size. In column 1 of Table 6 the implied marginal effect of residing in a program neighborhood is a 12.3 increase in total household labor force hours per week. Although the estimated effect on the average squatter household is insignificant, the estimates indicate that the size of response depends heavily on household type. For “new” households and households with few working-age members, the program effect is relatively large: a squatter household with three working-age members and only ten years of residential tenure is predicted to experience a 23.2 hour per week increase in labor force hours. Column 2 reveals that added workers account for a significant portion of the change in family labor supply resulting from the titling program. When the same regressions are run on household labor force participation rates, we observe an implied 8.3 percentage point increase in the number of working-age household members who are employed or searching for work for households with three working-age members and ten years of residential tenure.

Column 3 employs the same DID strategy in a probit model where the dependent variable is a dummy indicator of whether any household member under age 16 is reported as working more than five hours per week. As discussed in the previous section, an increase in formal property rights is predicted to generate a decrease in the amount of child employment if children have a comparative advantage in market work relative to home security or neighborhood enforcement.²⁸ Once again, while the mean program effect is insignificant, there is a significant effect of property titling on households with fewer than four working-age members and few years of residence. As reported in the last row of the table, for households with three working-age members, the implied marginal effect of titling is large, indicating a 2.6 percentage point change in the fraction of children working, where the mean is 7.8%.

²⁷ In total, 99 households were dropped from the analysis due to missing labor supply information (a household was considered to have missing weekly hours data if it had one or more members who reported having worked in the prior week *and* who had positive reported values of either hours worked per day or days worked per week and missing values of the other variable), 31 households had missing data on property size and/or local elementary school facilities, 20 households were excluded in two clusters in which program entry does not match institutional data on regional program timing, and 8 were excluded because all members were reported as over the age of 80, leaving a total of 2592 households.

²⁸ I estimate a binary model rather than modeling the marginal effect on child labor hours due to the fact that the majority of families report no child labor hours, necessitating a limited dependent variable model with more stringent functional form assumptions.

5.3.2 Leisure Activities Outside of the Home

While the above results provide strong evidence of a relationship between formal property titles and labor hours, looking at employment hours alone gives no direct indication as to the mechanism by which an increase in property rights encourages people to work. The model in Section 2 predicts this response by assuming that the value of leisure falls when formal property rights are secured since leisure hours can contribute to the production of informal tenure security. Hence, an important piece of supporting evidence for this hypothesis comes from studying data on household members' monthly participation in organized activities outside the home. Just as with labor supply, if staying at home increases tenure security, members' participation in such activities should also be hindered by an unwillingness to leave the home.

Information on non-work activities outside of the home comes from a survey question regarding all household members' participation in "groups, organizations or associations" that include the following: women's groups, political groups, youth groups, religious organizations, sports, financial groups, unions, craft or producers' organizations, and cultural associations. Although these activities do not encompass all potential leisure activities outside the home, results from a separate survey question, asked only of the respondent, indicate that most other free time is in fact spent inside the home.²⁹ According to these data, 87.8% of respondents' leisure hours are spent inside the home, where the four most common activities are resting, reading/watching television/listening to music, doing household chores and spending time with family. Furthermore, the most common outdoor leisure activities from these data are religious activities and sports, which account for 86% of outside "free time hours." Both of these activities are included in the Table 7 data on time in "groups, associations and organizations."

The DID estimates presented in Table 7 reveal that squatters' time spent participating in outside leisure activities also increases with program participation, demonstrating that members of squatter households in program areas are not only working significantly more hours, but also are spending more leisure time outside of the home post-program. Columns 1 and 2 indicate that

²⁹ In particular, respondents in all households were asked whether they have free time, how many hours per week, and how they spend it. Unfortunately, these data are not useful in exploring the program effect on leisure hours without strong assumptions regarding the selection of respondents across program and non-program neighborhoods.

both the probability of participation in any outside leisure activity and the number of days per month of participation rise significantly for squatters in program neighborhoods, such that newly titled households spend almost twice as much time outside the home in these activities. This result is particularly surprising given that total leisure hours fall with the titling program, providing additional support for the hypothesis that untitled households are constrained in their ability to participate in *all* activities outside the home. Hence, the results in Table 7 provide evidence that the labor supply response to obtaining a property title is not driven by a lower demand for outside leisure activities associated with receiving a property title.

Columns 3 and 4 divide reported time spent in outside organizations into participation in “neighborhood groups” and all other organizations. In the survey, neighborhood groups were defined as those in which “neighbors come together to complete a project that would benefit all members of the community or to resolve a problem that is affecting everyone.” Unlike other activities in the data, because these organizations frequently are responsible for land-related projects and neighborhood security, I expect the change in household participation in neighborhood groups to be lower among titled households. Indeed, columns 3 and 4 of Table 7 reveal that the entire program effect on time allocated to outside activities is concentrated among activities other than public goods provision through neighborhood groups. In fact, column 7 indicates that the estimated change in days per month in neighborhood groups among those households that do participate in some activity outside the home is negative and significant. This pattern is consistent with a scenario in which newly titled households are substituting non-work hours in neighborhood public projects for time spent in private organizations and activities when they attain greater formal property protection. This would arise if incorporation into the formal property system provides households with greater access to state-provided public goods, including property rights enforcement.

It is also interesting to note from the Table 7 data that not only does participation in property-related neighborhood groups fall with titling, but so does participation in infrastructure-related neighborhood groups. Since there are no significant differences in the level of public service provision in program and non-program areas, this does not appear to be driven by a shift in supply. A more likely explanation is that investment in community infrastructure serves to increase community tenure rights in informal neighborhoods, so demand for these investments falls with formalization whereas demand for investment in individual housing units rises.

5.4 Community-Level Evidence

The last set of results examines the trend in property-related neighborhood and state-level governance across program and non-program areas. As documented in de Soto's (1990) groundbreaking work on urban informality in Peru, property protection in squatter communities frequently involves neighborhood-level governance. The level of land-related governance presumably should fall when these communities are incorporated into the formal property system.

5.4.1 Neighborhood Organizations

I first look at differences between program and non-program neighborhoods in the existence of any type of local public goods provision. These data come from a survey question in which respondents were asked “In your community, have neighbors come together to complete a project that would benefit all members of the community or to resolve a problem that is affecting everyone,” irrespective of whether individual household members participate in such projects. Households that responded positively were asked to describe up to two types of neighborhood organizations that are present in the community. Households with COFOPRI titles were also asked about the existence and nature of neighborhood organizations *before* the titling program. Table 8 presents means from these data separately for program and non-program neighborhoods, which reveal that program areas were significantly less likely to have any type of neighborhood organization than were non-program areas at the time of the survey. The last two rows of the table present means for the two most common types of local group: land and property governance – including “reallocation of land, neighborhood security and [informal] titling” – and projects related to public utilities and infrastructure provision. Here we see a dramatic difference in the presence of titling and land allocation related organizations as well as significantly fewer infrastructure-related neighborhood groups. These data provide evidence that land formalization indeed shifts key institutional responsibilities away from local communities as well as local households.

This pattern is hardly surprising given the results of Table 7, which indicate that land titling is associated with fewer household participation hours in neighborhood groups. More striking is the fact that early and late program neighborhoods report an approximately identical frequency of neighborhood organizations *before* the program, as evidenced by a comparison of column 1 and column 3 in Table 8. This pattern strengthens support for a causal interpretation of land titling and neighborhood public goods provision.

5.4.2 Law Enforcement

The evidence from individual labor and community organization hours suggests that time use patterns change in response to heightened residential security. If the driving force behind this behavioral response to land titling is indeed that the role of property protection is shifted from individual household members to the state, we would expect a corresponding change in the presence and activities of law enforcement agencies in local communities. In other words, if there is no increased property protection by government agencies, there is no reason for households and communities to feel more comfortable abandoning this role.³⁰

Unfortunately, the only information on the role of legal and judicial institutions comes from a survey question regarding household experiences with various state institutions, including the police force and judicial system. In particular, respondents were asked whether they have utilized the services of either institution, and then asked to rank the quality of the service on a scale from one to five, where five is “very good” and one is “very bad.” Table 9 presents the coefficient estimate on the indicator of residing in a program neighborhood from probit and regression estimates of these outcomes. The estimates reported in columns 1 and 3 reveal no difference in the use or quality of police services. While this is somewhat surprising given that individual property rights of titled households are presumably enforced by local police, it is important to keep in mind that the available data do not provide information on the level of police presence in the neighborhood, which could be sufficient to deter actual police interventions. Likewise, the expectations of police enforcement of formal property rights could

³⁰ It is worth keeping in mind, however, that households in titled neighborhoods do in fact report a higher perceived level of tenure security, as indicated by the estimates in Table 2. Even if actual security does not change, the difference in expectations is sufficient to influence their behavior in the short run.

be sufficient to deter the number of property-related crimes and thereby reduce the demand for police services.

In contrast, the probit estimate of column 2 indicates that households in titled neighborhoods have a significantly higher likelihood of going to court, although the difference across titled and untitled neighborhoods in the quality of judicial services is again close to zero and insignificant. While the reason for judicial intervention is unobservable, the pattern is consistent with increased access to legal resources among property owners and a corresponding reduction in the demand for informal property protection.

6 Conclusions

In recent years, a handful of policy initiatives have arisen to address tenure insecurity of untitled residents by promoting formal property institutions in urban slums of several developing countries.³¹ Obtaining accurate measures of the micro-economic impact of titling programs is especially important at this juncture given the resources devoted to such reforms. While cost-benefit analyses suggest that governments are more efficient suppliers of property rights, these claims tend to ignore actual quantifications of the immediate cost to households of individual property protection. Accurately measuring the return to property formalization requires adequate attention to the cost of informality, including the time allocated to informal property protection at both the household and the community level.

By studying the relationship between exogenous acquisition of land title and household time allocation inside and outside the home, this paper has provided new evidence on the value of formal property institutions to urban squatters in developing countries. My results indicate that urban land titling efforts lead to substantial changes in the pattern of time allocated to guarding property or participating in neighborhood groups by previously untitled households. Individuals in titled households spend fewer hours inside the home guarding property and engaging in home-based entrepreneurial activities, and a greater number of hours in both employment and private leisure activities outside the home. Furthermore, participation in informal neighborhood projects falls whereas household experiences with formal government agencies rise in titled communities.

³¹ For an overview, see “Land, Security, Property Rights and the Urban Poor: Twenty Five Years of World Bank Experience.” World Bank Briefing Note 8. 2001.

These results provide empirical support for the anecdotal evidence that untitled squatters commonly attain informal rights by taking time off work to participate in such activities as guarding their property, participating in community groups and filing administrative claims for formalization. As indicated by these results, the time costs and distortions in optimal time allocation that result from informal tenure arrangements constitute critical components of the cost of informality and hence important indicators of the value of urban property institutions in a given setting.

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Table 1: What do you think are the main benefits of a property title?

	<i>Untitled</i>	<i>Titled</i>
	<i>(N=1064)</i>	<i>(N=1674)</i>
Tenure security	75.53	73.92
Collateral	37.54	39.68
Access to public utilities	2.81	3.35
Other	1.67	2.44
None	0.38	1.46

Note: Multiple responses allowed.

Table 2: Perceived Tenure Security, Difference in difference estimates

	(1)		(2)		(3)	
	Improvement in tenure security with last title?		Do you consider dwelling currently at risk of eviction/invasion?		Do you consider dwelling currently very secure from eviction/invasion?	
	<i>Pre-program squatter</i> (N=559)	<i>Pre-program titled</i> (N=1921)	<i>Pre-program squatter</i> (N=559)	<i>Pre-program titled</i> (N=1921)	<i>Pre-program squatter</i> (N=559)	<i>Pre-program titled</i> (N=1921)
<i>No titling program</i>	0.000 (0.000)	0.586 (0.012)	0.433 (0.023)	0.181 (0.011)	0.148 (0.026)	0.333 (0.012)
<i>Titling program</i>	0.674 (0.029)	0.657 (0.019)	0.157 (0.019)	0.093 (0.013)	0.379 (0.030)	0.377 (0.020)
Difference	0.674 (0.037)	0.071 (0.023)	-0.276 (0.030)	-0.088 (0.017)	0.232 (0.040)	0.044 (0.024)
Difference in difference		0.603** (0.045)		-0.188** (0.035)		0.188** (0.046)

* Statistically significant at 5% level.

Notes: Standard errors in parentheses. Only eligible HHs (according to residential tenure) included. Change in tenure security comes from survey question: "Did the last property document you obtained affect the security of your residence?" asked only of households with property documents. Other data based on responses to survey question, "How secure do you consider your property?" Respondents could report: (1) Very secure, I do not believe that it will be taken; (2) Secure; (3) Not so secure, I believe that in any moment it could be taken; (4) Not at all secure, I believe that it is very probable that at some moment it will be taken. Households classified as "at risk" if they answer (3) or (4).

Table 3: Why did a property title improve the security of your residence?

	<i>COFOPRI</i>	<i>Other title</i>
	<i>(N=569)</i>	<i>(N=1271)</i>
Gives tranquility/reduces conflict	37.57	47.80
Value of legal document	27.01	21.82
Access to credit	8.23	4.07
Provides guarantee	5.19	2.61
Better to have more than one document	0.18	0.00
Allows me to make housing improvements	0.00	0.16
<u>No improvement</u>	21.82	23.53
<i>Why not?</i>		
Already secure	12.88	8.14
Debt	2.33	5.86
Property in insecure neighborhood	0.54	2.12
Institution not effective	0.18	1.06
Process incomplete	0.36	0.00
Wrong type of title	0.36	0.33
Other	5.19	6.02

Table 4: Whether HH Member Stays Home for Security

<i>(N=2368)</i>	
Number working-age members	0.041 (0.06)
Squatter*program	-0.514 (0.62)
Squatter*program periods	-0.562 (0.161)**
Squatter*program* tenure	0.011 (0.02)
Squatter*program* working-age members	0.429 (0.177)**
(Squatter*program* working-age members) ²	-0.038 (0.018)**

<i>Implied program effect: † N=4, T=15</i>	<i>-0.365 (0.176)**</i>

** Significant at the 0.01% level.

† Implied program effect evaluated at N number of working age HH members, T years of residential tenure and median number of program periods (2).

Notes: Probit estimate, marginal effects reported. Dependent variable is dummy indicator of whether HH reported that one or more members stay home to provide security. Standard errors are in parentheses. Regression includes controls for city, size of property, residential tenure, household composition, number of rooms in dwelling, number of floors in dwelling, whether home business in residence, and age and sex of HH head. Robust standard errors account for sample clustering and stratification. Only eligible HHs (residential tenure pre-1995) are included.

Table 5a: Economic Activity in Residence

	(1) (N=2297) <i>Economic Activity in Residence</i>	(2) (N=592) <i>Would prefer to work outside of home</i>
Squatter	0.008 (0.093)	0.458 (0.184)**
Program	0.176 (0.107)	-0.041 (0.154)
Squatter*program periods	-0.182 (0.091)*	0.452 (0.293)**
<i>Implied marginal change</i>	<i>-0.116</i>	<i>-0.177</i>

* Significant at the 0.01% level. ** Significant at the 0.05% level.

Notes: Binomial probit estimates, standard errors in parentheses. All regressions control for city, size of property and residential tenure of HH, and columns 3-4 include all relevant intermediate interactions of HH tenure and size. Robust standard errors account for sample clustering and stratification. Ineligible HHs (residential tenure pre-1995) and HHs with missing values for working members are excluded. Demographic controls include: sex and age of HH head; # HH members, # of school-age children, # of babies, percentage male and percentage immigrants; whether indoor plumbing, whether property acquired by invasion and whether inherited lot; and whether neighborhood has municipal services, electrical infrastructure, whether local bus stop/market/commissary/primary and secondary school two years ago, whether neighborhood has school assistance program, cluster average walking distance to local primary school, and cluster average walking distance to bus stop.

Table 5b: Do you consider dwelling currently very secure from eviction/invasion?

	(1) (N=1616) <i>Program</i>	(2) (N=837) <i>No Program</i>
Economic activity in Residence	0.719 (0.063)	0.329 (0.055)
No Economic Activity in Residence	0.599 (0.041)	0.303 (0.073)
Difference	-0.120 (0.040)	-0.026 (0.024)
Difference in difference		0.094 (0.036)**

** Significant at the 0.05% level.

Notes: Regression-controlled means, standard errors in parentheses. Robust standard errors account for sample clustering and stratification. Ineligible HHs (residential tenure pre-1995) and HHs with missing values for working members are excluded. All regressions control for city, size of property, sex of HH head, literacy and education level of HH head, # HH working-age members, and # of babies age 2-4.

Table 6: Household Labor Supply, Difference in Difference Estimates

	(1)	(2)	(3)
<i>(N=2379)</i>	Household Weekly Hours in Labor Force	Fraction of Household Members in Labor Force	Child Labor Force Participation
Number working-age members	9.25 (6.45)	-0.095 (0.018)**	0.282 (0.111)**
Squatter*program	58.33 (26.04)*	0.207 (0.156)	-4.475 (1.680)**
Squatter*program periods	16.4 (5.37)**	0.058 (0.020)**	0.471 (0.323)
Squatter*program* tenure	-1.12 (0.56)*	-0.000 (0.003)	0.012 (0.028)
Squatter*program* working- age members	-29.09 (11.66)*	-0.112 (0.060)	1.119 (0.542)*
(Squatter*program* working- age members) ²	3.39 (1.31)*	0.011 (0.005)*	-0.085 (0.047)
<hr style="border-top: 1px dashed black;"/>			
<i>Implied program effect:[†]</i>	<i>12.27</i>	<i>0.046</i>	<i>-0.713</i>
<i>N=4, T=15</i>	<i>(7.98)</i>	<i>(0.029)</i>	<i>(0.370)</i>
<i>(Marginal effect)</i>			<i>0.24</i>
<i>Implied program effect:</i>	<i>23.23</i>	<i>0.083</i>	<i>-0.823</i>
<i>N=3, T=10</i>	<i>(7.97)**</i>	<i>(0.038)*</i>	<i>(0.401)*</i>
<i>(Marginal effect)</i>			<i>0.26</i>

[†] Implied program effect evaluated at N number of working age HH members, T years of residential tenure and median number of program periods (2).

* Significant at the 0.05% level. ** Significant at the 0.01% level.

Notes: Columns 1-2 are OLS regressions. Column 3 is binomial probit estimation where dependent variable is dummy indicator of whether HH members ages 5-16 report working more than 5 hours/week. Standard errors in parentheses. All regressions control for city, size of property and residential tenure of HH. In addition, estimates include all relevant intermediate interactions of HH tenure and size. Robust standard errors account for sample clustering and stratification. Ineligible HHs (residential tenure pre-1995) and HHs with missing hours or days values for working members are excluded. Demographic controls include: sex, age, literacy and degree level of HH head; # HH members, # of school-age children, # of babies, fraction male, fraction immigrants, and # members 70 and older; whether indoor plumbing, whether property acquired by invasion, and whether inherited lot; whether dwelling lies within walking distance and this indicator interacted with walking time to nearest primary school, secondary school, bus stop, public phone, and public market; and whether neighborhood has local bus stop/market/public phone/primary and secondary school currently and for last two years, and whether neighborhood has school, child, food or social assistance program.

Table 7: HH Participation in Outside Activities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	HH participation in outside activities	HH days per month of participation	HH days per month in neighborhood groups	HH days per month in other activities	HH days per month given participation	HH days per month in neighborhood groups given participation	HH days per month in other activities given participation
	<i>(N=2314)</i>	<i>(N=2314)</i>	<i>(N=2314)</i>	<i>(N=2314)</i>	<i>(N=447)</i>	<i>(N=447)</i>	<i>(N=447)</i>
Number working- age members	0.000 (0.013)	0.153 (0.116)	0.001 (0.006)	0.152 (0.111)	1.00 (0.33)**	1.00 (0.33)**	0.005 (0.014)**
Squatter	0.022 (0.028)	0.042 (0.185)	-0.038 (0.028)	0.081 (0.169)	4.23 (1.14)**	4.59 (1.12)**	0.353 (0.053)**
Program area	0.024 (0.061)	0.384 (0.439)	-0.001 (0.028)	0.386 (0.425)	-10.06 (1.64)**	-11.00 (1.73)**	0.932 (0.159)**
Squatter*program	0.063 (0.016)**	0.543 (0.164)**	0.022 (0.019)	0.521 (0.178)**	4.12 (0.84)**	4.25 (0.86)**	-0.136 (0.066)**

* Significant at the 0.01% level. ** Significant at the 0.05% level.

Notes: Column 1 is probit estimate and columns 2-7 are OLS regressions, standard errors are in parentheses. Robust standard errors account for sample clustering and stratification. Only eligible HHs (residential tenure pre-1995) are included. Control variables in each estimate correspond to set of regressors in notes to Table 6.

Table 8: Neighborhood Organizations, Sample Means

	(1) Non-program area <i>(N=1799)</i>	(2) Program area <i>(N=939)</i>	t_{Δ}	(3) Program area pre-program <i>(N=572)</i>
Whether neighborhood has organization [†]	0.599	0.423	8.68	0.602
<u>Type of organization</u>				
Infrastructure (roads/parks/electricity/water)	0.471	0.345	6.30	0.470
Titling/land allocation/property security	0.191	0.088	7.00	0.176
Sports	0.028	0.020	1.30	0.027

[†] Data comes from survey question, “In your neighborhood, have neighbors come together to complete a project that would benefit all members of the community, or to resolve a problem that is affecting everyone? If yes, what type?” Only households with COFOPRI titles are asked about the existence of community organizations both before and after the titling program. Hence, the responses in the column 3 come from titled households only.

Note: Respondents could report one or two types of community organizations.

Table 9: Relationship with Law Enforcement Agencies

	(1)	(2)	(3)	(4)
<i>(N=2382)</i>	<i>(demographic characteristics, city*program years, and city*initial rights)</i>			
	Had Experience using Police Services	Had Experience with Judicial System	Quality of Experience with Police	Quality of Experience with Judicial System
Program neighborhood	0.098 (0.082)	0.300 (0.092)*	-0.107 (0.183)	-0.032 (0.169)
<i>Implied marginal change</i>	<i>0.038</i>	<i>0.085</i>		

* Significant at the 0.05% level. ** Significant at the 0.01% level.

Notes: Columns 1-2 are binomial probit estimates, columns 3-4 are OLS regressions where dependent variables is 5-level index of quality of services. Standard errors in parentheses. All regressions control for city, size of property and residential tenure of HH, and columns 3-4 include all relevant intermediate interactions of HH tenure and size. Robust standard errors account for sample clustering and stratification. Ineligible HHs (residential tenure pre-1995) and HHs with missing values for working members are excluded. Demographic controls include: sex and age of HH head; # HH members, # of school-age children, # of babies, percentage male and percentage immigrants; whether indoor plumbing, whether property acquired by invasion and whether inherited lot; and whether neighborhood has municipal services, electrical infrastructure, whether local bus stop/market/commissary/primary and secondary school two years ago, whether neighborhood has school assistance program, cluster average walking distance to local primary school, and cluster average walking distance to bus stop.

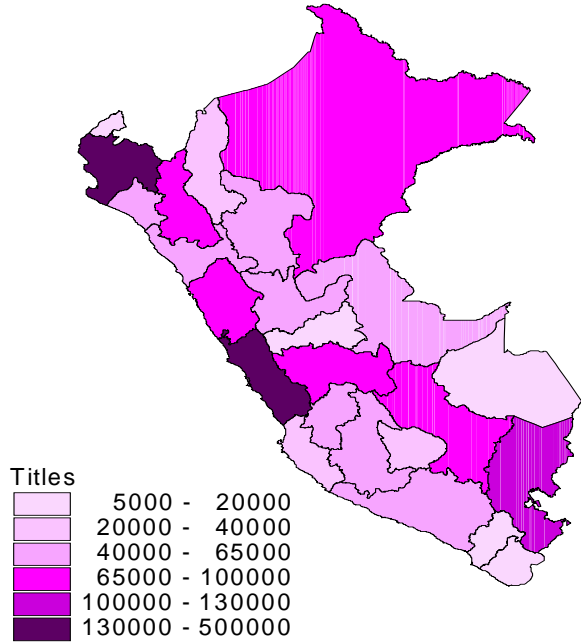
Appendix I. Sample Means

	<u>Pre-program squatter households</u>			<u>Pre-program titled households</u>			
	<i>(N=668)</i>			<i>(N=2082)</i>			
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)	(3)
	<i>Program</i>	<i>No Program</i>	$ t_{\Delta} $	<i>Program</i>	<i>No Program</i>	$ t_{\Delta} $	$ t_{\Delta}^2 $
Female head of HH	0.232	0.259	0.74	0.223	0.247	1.13	0.09
Age of HH head	46.79	47.63	0.62	50.49	50.78	0.29	0.37
HH size (# members)	5.059	5.178	0.71	5.368	5.603	1.87	0.56
Number of rooms in dwelling	3.19	3.527	2.49	3.74	3.982	2.32	0.61
Lot size (m2)	170.6	210.0	1.49	197.7	208.5	0.59	0.9
Highest grade head	4.633	4.716	0.66	4.77	4.646	1.45	1.39
Residence acquired by invasion	0.27	0.202	1.24	0.22	0.213	0.2	1.08
Age of dwelling	17.5	17.71	0.16	21	19.12	1.8	1.48
HH adult literacy rate	0.854	0.861	0.53	0.877	0.867	1.2	1.08
Plumbing	0.734	0.653	1.5	0.839	0.829	0.28	1.35
Light	0.948	0.893	1.9	0.978	0.944	2.81	0.83
Municipal service (water)	0.792	0.814	0.41	0.892	0.898	0.18	0.33
HH monthly expend. (S/)	558.7	544.8	0.52	587.6	567.4	0.86	0.19
Whether HH saves	0.08	0.068	0.54	0.075	0.095	1.3	1.24
Number of members moved/left HH	1.453	1.325	0.65	1.709	1.609	0.71	0.12
Number of members born in province	7.053	7.395	2.02	6.571	6.661	0.05	1.67

Notes: Columns 1c and 2c report the t-statistics of the difference between columns 1a and 1b, and 2a and 2b. Column 3 reports the t-statistic of the difference in difference.

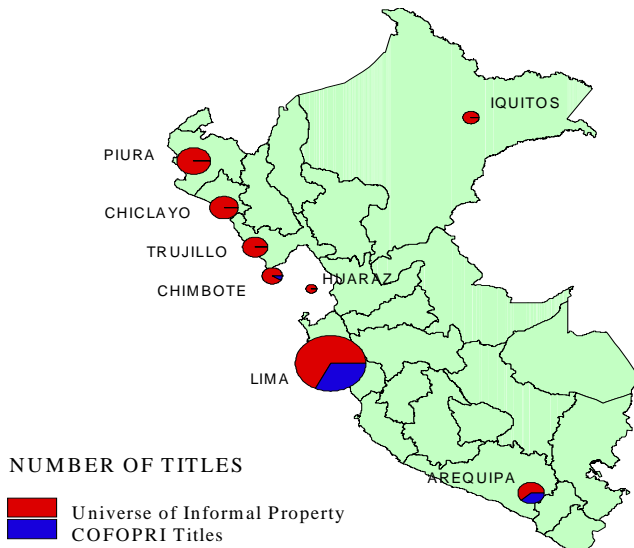
Appendix 2: Map of Program Areas and Untitled Population

NUMBER OF UNTITLED PROPERTIES
(NATIONAL LEVEL)



Source: ENAHO TRIMESTER II, 1998

PROGRAM UNIVERSE OF INFORMAL PROPERTY AND
LEVEL OF FORMALIZATION VIA COFOPRI



Appendix 3: Definitions of Variables

BABIES: Number of household members aged 2-4.
EDUCATION OF HH HEAD: Four dummies indicating whether or not household head completed primary school, high school (common or technical), non-university post-secondary, and university.
FIRST OWNERS OF DWELLING: Household members built or were first to reside in dwelling.
INHERIT: From question, “How did you obtain this property?” respondent answered, “By inheritance.”
INVADED PROPERTY: From question, “How did you obtain this property?” respondent answered, “By invasion.”
INVADED DWELLING: From question, “What type of ownership claim does the household have with respect to the dwelling?” respondent answered, “Invaded or de facto.”
LITERACY OF HH HEAD: Household head can read and write.
LOT SIZE: Property size in square meters. Interviewers instructed to verify if possible size recorded on property deed/title.
MARITAL STATUS OF HH HEAD: Dummy indicator of whether or not household head reports either being married or having a domestic partner.
MEMBERS: Total number of people who “habitually live and share meals” in the household.
NEIGHBORHOOD HAS FAMILY PLANNING PROGRAM: Someone in cluster reports participating in or benefiting from a family planning program.
NEIGHBORHOOD HAS CHILD WELFARE PROGRAM: Someone in cluster reports participating in or benefiting from Vaso de Leche, Wawa Wasi, PRONEI, or public day care program.
NEIGHBORHOOD HAS FOOD ASSISTANCE PROGRAM: Someone in cluster reports participating in or benefiting from school lunch, community kitchen, PANFAR, food-for-work, church kitchen, or food donation program.
NEIGHBORHOOD HAS SCHOOL ASSISTANCE PROGRAM: Someone in cluster reports participating in or benefiting from literacy program, school insurance program, or INFES.
NEIGHBORHOOD HAS GENERAL SOCIAL ASSISTANCE PROGRAM: Someone in cluster reports participating in or benefiting from FONCODES, VAN, or other type of assistance program.
NEIGHBORHOOD HAS [] FACILITY: Anyone in cluster unit reports that neighborhood currently has primary school/secondary school/job training center/bank/paved road/bus stop/soccer field/public phone/post office/market/commissary/cinema/health center reasonably in reach.
NEIGHBORHOOD HAD [] TWO YEARS AGO: Anyone in cluster unit reports that neighborhood had current nearest [] facility for at least two years.
NEIGHBORHOOD WALKING INDICATOR TO []: Whether or not anyone in neighborhood reports walking to [] facility.

NEIGHBORHOOD AVERAGE WALKING DISTANCE TO []: The mean commuting time to [] among people in cluster who claim to walk there.
NEIGHBORHOOD LACKS INFRASTRUCTURE: From question: Which services does community lack? Answer: Public infrastructure (roads/bridges).
NEIGHBORHOOD HAS NO MUNICIPAL SERVICES: How does HH generally dispose of trash? Municipal trash services. Year of municipal service? Pre-1995.
NEIGHBORHOOD HAS NO PUBLIC WATER SYSTEM: From question: Which services does community lack? Answer: Public water system.
PERCENT IMMIGRANTS: Percentage of HH members who report being born in the province.
PERCENT MALE: Percentage of HH members age 5-70 (working-age) who are male.
[INDOOR] PLUMBING: From question, “Where does this HH get water?” Answer: “Faucet inside home.” From which year public system? Before 1995.
PROGRAM: More than one household in cluster reports having a COFOPRI title.
PROGRAM PERIODS: One of four program periods determined by cluster year of program entry. Year of entry defined as the cluster minimum year of COFOPRI title greater than 1992. If minimum year less than 1992, second minimum year is used. Lengths of program periods described in Section 4.2.
SCHOOL-AGED KIDS: Number of children ages 5-10 (primary school age).
SENIORS: : Number of household members over age 70.
SHOCK: Household reports experiencing economic shock unrelated to titling program during past year.
SQUATTER: A household is identified from my data as a squatter household in the following manner: Treatment Group “squatter”=1 if: (1) current squatter, identified by (a) “title type”= “No title”; or (b) “title type”= “Other documents” and “registered”= “no”; or (c) “title type”= “Legal settlement” and “registered”= “no”, OR (2) pre-program squatter, identified by “title type”= “COFOPRI”, and (a) “old title type”= “No title” or (b) “old title type”= “Other documents” and “previously registered”= “no” or (c) “old title type”= “Legal settlement” and “previously registered”= “no” Control Group “squatter”=0 if: (1) current title-owner in non-program area, OR (2) pre-program title-owner in program area
TENURE: Residential tenure is defined as the earliest year in which a resident reports having lived in the house. Data comes from information collected on each household member, including which year they moved (or were born) into house.
WALKING INDICATOR TO [] FACILITY: Whether or not household reaches [] facility by walking.
WALKING DISTANCE TO [] FACILITY: HH walking time to reach [] facility.
WORKING-AGE MEMBERS: Number of household members between ages 5 and 70.

Appendix 4: Legislative Guidelines for Peruvian Urban Property Reforms

Legislative Decree N° 803: Law to Promote Access to Formal Property

Supreme Decree N° 009-99-MTC: Approves the unique text in the Law to Promote Access to Formal Property

Supreme Decree N° 014-98-MTC: Approves the establishment of the Commission for Formalizing Informal Property (COFOPRI)

Resolution N° 261-97-MTC-15.01: Approves the organizational rules and functions of COFOPRI

Supreme Decree N° 020-99-MTC Approves the legislation governing administrative processes under COFOPRI