PROPERTY RIGHTS AND INVESTMENT IN URBAN SLUMS

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Abstract
This paper examines the effect of changes in tenure security on residential investment in urban squatter neighborhoods. To address the endogeneity of property rights, I make use of variation in ownership status induced by a nationwide titling program in Peru. In a difference-in-difference analysis, I compare the change in housing investment before and after the program among participating households to the change in investment among two samples of nonparticipants. My results indicate that strengthening property rights in urban slums has a significant effect on residential investment: the rate of housing renovation rises by more than two-thirds of the baseline level. The bulk of the increase is financed without the use of credit, indicating that changes over time reflect an increase in investment incentives related to lower threat of eviction. (JEL: O12, O18, P25, P26)

1. Introduction

Economic theory predicts a straightforward relationship between individual property rights and incentives to invest in land. As demonstrated formally in Besley (1995), security of tenure increases the marginal value of irreversible investments. As a result, insecure ownership rights in rural settings imply a distortion in the composition along with the level of agricultural investment. Because farmers substitute away from investments with longer-run yields, inputs such as soil-replenishing fertilizers and crops such as trees are underprovided. A number of authors have found empirical evidence supporting these predictions.[1]

This paper examines empirically whether the established relationship between farm investment and tenure security extends to residential investment in urban

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1. Many authors including Feder et al. (1998), Besley (1995), Banerjee, Gertler, and Ghatak (2002), and Alston, Libecap, and Mueller (1996) provide evidence that lack of property title affects agricultural investment demand Other work, such as Migot-Adholla, Hazell, and Place (1991); Place and Otsuka (2002), and Pinckney and Kimuyu (1994) detect little impact of titling on investment. The mixed results are commonly attributed to the difficulty of addressing endogeneity of tenure status.

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areas. Analogous to the rural setting, fear of eviction in urban squatter communities implies discounted returns to investment in housing and infrastructure. As with farm inputs, the quality and not just quantity of housing is predicted to rise with expected duration of tenure, implying that long-run effects on investment of weak property rights are particularly costly for urban neighborhoods threatened by natural disasters such as earthquakes and floods.

While the theoretical relationship between property rights and investment extends naturally to an urban setting, two potentially important distinctions are worth noting. First, unlike rural sharecropping situations characterized by contractual arrangements between landowners and tenants, most urban squatters reside illegally on public land such that investment incentives are unambiguously distorted by tenure insecurity.[2] On the other hand, the reverse effect of investment on household or community tenure security may be stronger in urban than in rural areas due to the political nature of ownership rights. For instance, governments may be less likely to evict communities with sufficient residential infrastructure or community leaders may reallocate property on the basis of household investment, both of which imply a negative effect of titling on investment.

To assess the net effect of these forces, I examine the impact of a nation-wide titling program in Peru in which 1.2 million property titles were distributed to urban squatters on public land. Using panel data on ten categories of housing renovations ever made by a sample of program beneficiaries, I study the effect of increases in tenure security arising from the acquisition of a property title on the rate of residential investment before and after the program. The natural experiment provided by the titling program is valuable for addressing endogeneity concerns that typically arise in comparing titled and untitled households. That is, the tenure status of a given household is generally a function of time-varying demand for legal protection, which is likely to be related to factors influencing housing investment. The Peruvian reform in which households were "assigned" property titles irrespective of demand helps isolate the causal effect of titling on behavior by providing a source of quasi-random variation in ownership status. To control for potentially confounding time trends, I calculate difference-in-difference (DID) estimates of the program effect using two comparison groups of nonparticipating households.

My results indicate that strengthening tenure security through property formalization in urban squatter settlements has a large positive effect on investment. Land titling is associated with a 68% increase in the rate of housing renovation within only four years of receiving a title. The nature of investment is limited to small renovations as opposed to housing additions. Although past studies report

2. In a sharecropping scenario, property owners have an incentive to provide investments with long-run yields and simply charge tenants a higher rental price, so that investment decisions will not necessarily be distorted by tenants' insecurity.

some improvement in credit access associated with the titling program, my analysis suggests that greater incentives to invest associated with lower threat of eviction are responsible for a significant portion of the change. In particular, there is also a significant increase in renovations financed out-of-pocket and in total investment among nonborrowing households. In fact, demand for construction loans unambiguously rises much faster than supply, suggesting that the investment effects are significantly muted by binding credit constraints.

2. Data and Empirical Methods

In 1996, the Peruvian government issued a series of legal, administrative, and regulatory reforms aimed at promoting a formal property market in urban squatter settlements (World Bank 1998). Prior to the reforms, obtaining a property title for a Peruvian household was hampered by lengthy bureaucratic procedures and prohibitive fees. As a result, more than a quarter of Peru's urban population had no formal property title (World Bank 1997). While the old process of acquiring a title was expensive and slow, the new process was virtually free and extremely rapid.[3] Program implementation involved areawide titling, in which project teams moved from neighborhood to neighborhood within cities. To receive a title, claimants were required only to verify residence on eligible public properties predating the start of the program.

By December 2001 nearly 80% of the country's previously untitled residents eligible for program participation were nationally registered property owners, affecting approximately 6.3 million individuals primarily living below the poverty line. Most importantly, past research indicates that the program had a significant effect on perceived tenure security: 67% of title recipients report a large change in tenure security with the acquisition of formal ownership rights. As a result, the intervention provides a useful opportunity to evaluate the influence of tenure security on urban investment.

2.1. Difference-in-Difference Estimates

My empirical analysis of household responses to changes in property rights uses cross-sectional survey data collected in May 2000, midway through the program. The data consist of 2,750 households sampled from the universe of all residences identified in the 1993 census as eligible for program intervention. At the time of the survey, roughly 60% of these households lived in neighborhoods in which the titling program had begun and 40% were awaiting intervention.

3. See Field (2003) for an overview of the titling process.

To identify the impact of receiving a property title on residential investment, I focus on households living in neighborhoods that were reached by the program between 1996 and 1999.[4] The survey instrument includes questions on ten types of housing improvements, including whether each type of renovation was undertaken, how it was financed, and the year in which it was carried out.[5] With the latter I separate investments into those completed in 1994 or 1995 and those completed in 1999 or 2000 in order to estimate the simple difference in the rate of residential investment before and after the program.

Because changes over time in the level of housing investment may reflect a natural increase in the propensity to renovate or other confounding time trends, I also make use of two comparison groups to calculate DID estimates of the program effect on investment. The first comparison group consists of future program beneficiaries residing in neighborhoods that were reached after 1999. The DID estimate is the change in the rate of housing investment before and after the first wave of the program in early and late neighborhoods. In the absence of area-specific time trends correlated with program timing, the DID will consistently estimate the effect of the titling program and be robust to time consistent differences across early and late program neighborhoods. Unlike many artificial control groups, these data have the advantage that all sample members live in areas that were initially targeted for program intervention, increasing confidence in the comparability of treated and untreated households. While the comparison may be contaminated by program timing bias, available information on program timing suggests that it was largely exogenous to the economic environment of neighborhoods (Field 2003).[6]

As a robustness check against area-specific time trends, the second approach restricts attention to households living in early program neighborhoods. In this alternative DID analysis the control group consists of households in the same neighborhoods as the treatment group who did not benefit from the program because they already possessed a registered property title prior to intervention.[7] The simple idea underlying this distinction is that the tenure security effect of titling disproportionately benefits households with weak ex ante property claims. The program effect will be identified as long as unobservable differences in the

4. Program data provided by the COFOPRI office provides information on the neighborhood timing of program intervention. Since households in these neighborhoods may or may not actually have received a government title, I employ an intent-to-treat analysis in which all households in program neighborhoods are considered treated. See Field (2003) for details.

5. Specific categories of improvement are listed on the last ten rows of Table 1. The survey data do not provide direct information on household expenditures on home improvements.

6. The influence of nonrandom city timing is resolved by including city fixed effects in the regression estimates, so all uncertainty about program timing bias concerns the order in which project teams entered neighborhoods within cities.

7. Nonbeneficiary households are identified from detailed survey data on the title history of each residence. See Field (2003) for a description of the ways households obtained property titles in the era before the recent titling effort.

behavior of squatters and nonsquatters are constant over time. Both estimates are captured by the following equation, where I_{it} is household i's investment in time t:

$$I_{it} = \beta_0 + \beta_1(\mathit{after})_{it} + \beta_2(\mathit{treated})_i + \beta_3(\mathit{after} * \mathit{treated})_{it} + \alpha' X_{it} + e_{it}$$

The variable *treated* indicates whether the household lives in a neighborhood that has been reached by the program and X_i is a vector of demographic controls.[8] The coefficient on the interaction between *after* and *treated*, β_3 is the estimated program effect, which provides a measure of the conditional (on X_i) average change in investment by treated households.

Table 1 provides descriptive statistics on the sample population. A comparison between households in early and late neighborhoods reveals few differences in observable household characteristics unrelated to housing (columns 1 and 2). Meanwhile, the fraction of households undertaking home improvements over the past two years is 50% higher in titled neighborhoods. The comparison across program beneficiaries and nonbeneficiaries (columns 1 and 3) suggests that the difference in investment does not reflect neighborhood-level variation. While there are notable differences in household characteristics between columns 1 and 3, comparisons between treatment households and both control groups indicate approximately the same difference in postprogram investment.

Figure 1 shows the trend over time in the number of housing renovations per year among households that participated in the titling program between 1996 and 1999 and the comparison group of nonparticipants. The graph indicates a clear divergence in the rate of investment coinciding with the year in which the titling program began (1996).

3. Results

3.1. Total Investment

Table 2 presents DID estimates of these effects controlling for a wide range of household observable characteristics and city fixed effects. In columns 1a–c the dependent variable is the number of reported housing renovations; in columns 2a–c the dependent variable is a binary indicator of any renovation; and in columns 3a–c the dependent variable is an indicator of any housing addition.[9] Columns 1–3a present the simple differences, columns 1–3b present the DID estimates with late program control group, and columns 1–3c present the DID estimates with nonbeneficiary control group.

8. Vaiables included in X_i are listed in the notes to Table 1.

9. Throughout the analysis the reference period for investment is two years.

TABLE 1. Summary statistics.

	(1) Beneficiaries in program neighborhoods	(2) Beneficiaries in nonprogram neighborhoods	$t_{\|\Delta\|1,2}$	(3) Nonbeneficiaries in program neighborhoods	$t_{\|\Delta\|1,3}$
	($N = 400$)	($N = 253$)		($N = 1190$)	
Home characteristics					
Age of residence	17.54	17.70	0.18	20.70	5.29
Number rooms/pers	0.76	0.82	1.21	0.83	2.10
Number bedrooms/pers	0.49	0.53	1.26	0.52	1.54
Indoor plumbing	0.69	0.68	0.25	0.83	5.98
Electricity	0.93	0.90	1.23	0.96	2.24
Lot size (m2)	170.50	227.67	2.24	205.74	1.66
Straw roof	0.20	0.22	0.68	0.14	2.98
Dirt floor	0.42	0.56	3.54	0.32	3.65
Household characteristics					
Number members	5.09	5.18	0.52	5.45	2.76
Sex head	0.26	0.21	1.60	0.24	0.82
Wage income	568.30	573.20	0.09	737.50	0.79
Format credit	285.06	292.37	0.84	312.56	0.39
Reported renovations 1999–2000	0.26	0.17	2.62	0.18	3.16
New walls	0.10	0.05	2.66	0.06	3.17
New roof	0.09	0.05	1.69	0.07	0.79
New floor	0.07	0.03	2.28	0.07	0.62
Electricity	0.04	0.02	0.95	0.01	2.99
Plumbing	0.06	0.03	1.49	0.03	2.18
Painted walls	0.04	0.04	0.16	0.03	0.72
Others	0.03	0.02	1.33	0.01	2.01
Reported additions 1999–2000	0.05	0.04	1.11	0.05	0.07
Added story	0.01	0.00	0.56	0.01	0.85
Added bedroom	0.03	0.02	0.78	0.03	0.31
Added other room	0.02	0.02	0.22	0.02	0.16

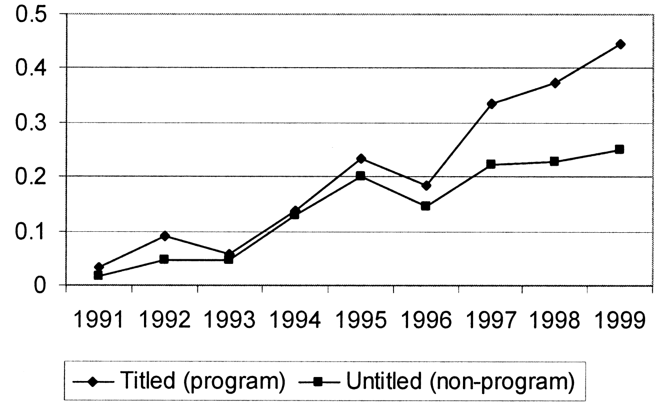

FIGURE 1. Annual housing renovations.

TABLE 2. Housing investment and property titling.*

	Number of annual additions or renovations**			Any renovation within two-year period***			Any addition within two-year period		
Universe:	Beneficiaries in program areas (1a)	Program areas only (1b)	Beneficiaries only (1c)	Beneficiaries in program areas (2a)	Program only (2b)	Beneficiaries only (2c)	Beneficiaries in program areas (3a)	Program areas only (3b)	Beneficiaries only (3c)
After program	0.275 (0.075)**	0.093 (0.040)*	0.083 (0.079)	0.126 (0.033)**	0.036 (0.021)	0.047 (0.043)	0.020 (0.008)**	0.031 (0.007)**	0.003 (0.008)
Beneficiary		−0.108 (0.040)**			−0.060 (0.021)**			−0.010 (0.010)	
Beneficiary* After Program		0.182 (0.078)*			0.089 (0.042)*			0.007 (0.015)	
Program area			0.021 (0.059)			0.009 (0.038)			−0.007 (0.009)
Program area* After program			0.192 (0.095)*			0.080 (0.037)*			0.018 (0.017)
Household-period obs	800	3180	1306	794	3166	1300	756	3026	1300

Notes: *Huber–White robust standard errors adjusting for sample clusters and strata are reported in parentheses: *indicates 5% significance level, **indicates 1% significance level. Marginal effects from probit estimates are reported in Columns 2a–3c. All Table 2–4 regressions include the following set of time variant control variables: family size, number of children, and income shock; and the following time invariant controls: city dummies, age head, sex head, education head, head literate, lot size, residential tenure, distance to nearest paved road, whether community has security guards, whether community has water, and whether six different community services are within walking distance to residence.

**Specific renovations and additions are listed in Table 1.

***Period data reflect home improvements undertaken 1994–1995, before the titling program began, and 1999–2000, after households in the treatment group had been reached by the program.

All column 1 estimates indicate a substantial change in total investment following the titling program. While the naive estimate in column 1a suggests that property titling is associated with a near doubling of the rate of residential investment over just four years, columns 1b and 1c indicate that much of this is driven by common time trends. Both DID estimates associate land titling with a 68% increase in the number of housing renovations.[10] The fact that the magnitudes of the DID estimates are similar for two very different control groups lends credibility to the identification strategy.

According to columns 2 and 3, investment is limited to smaller renovations as opposed to housing additions. This could reflect a number of local market features. For instance, space constraints may limit expansion in urban neighborhoods or credit constraints may limit costlier investments. Alternatively, in the absence of formal property rights, households may have incentive to claim land by constructing larger residences.

3.2. Change in Ability versus Incentive to Invest

Property titles presumably increase not only the incentive but also the ability to invest by raising the collateral value of land. Indeed, past research indicates some improvement in the supply of credit for housing materials associated with the Peruvian titling program (Field and Torero 2003). Hence, it is feasible that the observed increase in the rate of residential investment is driven entirely by greater lending opportunities for titled households.[11] Two pieces of evidence from the data confirm that there is an independent effect of tenure security on incentives to invest. First, by distinguishing home improvements financed with credit from those financed out of pocked (OOP), I can test whether property titling is also associated with an increase in OOP investment. As long as improvements in credit supply are not tied to categories of consumption other than building materials and the cost of credit transactions is constant across types of purchases, an increase in credit supply will not alone lead to an increase in OOP expenditures on housing investment.[12] Isolating the effect of titling on investment among nonborrowing households provides a related test. Roughly 60% of households report no formal

10. Similar results area obtained using a dummy indicator of any home improvement as the dependent variable to calculate the change in probability of residential investment.

11. While tenure security has an unambiguous positive influence on the expected returns to housing investment, if the opposite is also true (investment increases tenure security), there may be a zero net effect of land titling on investment incentives.

12. Past research indicates that the first assumption is valid. Field and Torero (2003) find that, in the year 2000, the only financial institution that had responded to the titling program in terms of securitizing loans with the new titles was the national bank for housing construction. For instance, there are no instances in the data of entrepreneurial loans securitized with new property titles.

TABLE 3. Out-of-pocket housing investment and property titling.+

	Number of annual additions or renovations financed OOP++ All households			Number of annual additions or renovations Nonborrowing households		
Universe:	Beneficiaries in program areas (1a)	Program areas only (1b)	Beneficiaries only (1c)	Beneficiaries in program areas (1a)	Program areas only (1b)	Beneficiaries only (1c)
After program	0.193 (0.059)**	0.068 (0.028)*	0.043 (0.062)	0.193 (0.082)*	−0.031 (0.043)	−0.050 (0.069)
Beneficiary		0.005 (0.049)			−0.180 (0.044)*	
Beneficiary* After program		0.149 (0.084)			0.224 (0.088)*	
Program area			−0.058 (0.032)			−0.045 (0.066)
Program area* After program			0.124 (0.060)*			0.243 (0.107)*
Household-period obs	800	3180	1306	508	1912	826

Notes: + Huber–White robust standard errors adjusting for sample clusters and strata are reported in parentheses; * indicates 5% significance level, ** indicates 1% significance level.
++ Specific renovations and additions are listed in Table 1.
+++ Period data reflect home improvements undertaken 1994–1995, before the titling program began, and 1999–2000, after households in the treatment group had been reached by the program.

credit over the past three years.[13] If the investment effect is robust to limiting the sample to nonborrowers, we can conclude that the change in investment is not driven entirely by improved access to credit.

Table 3 reports both sets of results. Here we observe that OOP investment rises by more than two-thirds the amount of total investment and the effect is significant. Similarly, the change in investment among nonborrowing households is nearly identical in magnitude to that of borrowing households. Both results suggest that greater incentives to invest are at least partly responsible for the observed increase in the rate of housing investment.

Separate survey data on desired housing improvements and credit provide direct evidence on the incentive effects of property titling that are not confounded by changes in credit supply. In particular, the survey asks respondents that made no improvements to their homes to identify the reasons for not doing so. With these data, I construct an indicator of investment demand that is equal to one if the household made any investment or did not renovate their home on account of

13. The questionnaire does not ask households about credit prior to 1997. However, including households with credit available for preprogram investments does not reduce the validity of the test, since these particular households experience no increase in credit between 1994 and 2000.

TABLE 4. Investment demand and property titling.+

	Any credit desired?++	Any desired addition or renovation since moving in?+++	Credit requested for housing improvements++++	Credit received for housing improvements
	(1)	(2)	(3)	(4)
	($N = 756$)	($N = 800$)	($N = 1271$)	($N = 1271$)
Program neighborhood	0.069	0.042	36.90	−57.80
	(0.032)*	(0.020)*	(158.20)	(230.20)
After program			−43.10	−32.50
			(168.40)	(286.50)
Program neighborhood*			431.90	229.80
After program			(185.9)*	(282.21)

Notes: +Huber–White robust standard errors adjusting for sample clusters and strata are reported in parentheses; * indicates 5% significance level. ** indicates 1% significance level. Marginal effects from probit estimates are reported in columns 1–2.
++Households that did not renovate due to lack of financial resources are counted as desiring renovations.
+++Households that did not apply for loans because they did not believe they would receive it were counted as desiring credit.
++++All credit applications over past two years, where primary purpose of loan is residential improvements.

financial constraints. A similar measure of demand is constructed from the survey module on credit applications, in which households that did apply for credit are asked to identify the reasons for not doing so. A binary indicator of credit demand assigns households a value of one if they either applied for formal credit or reported not applying because they believed they would not receive it.

Table 4 reports the marginal effects from probit estimates of these two dependent variables on an indicator of whether a household lives in a program neighborhood and full set of controls.[14] The estimate in column 1 indicates that beneficiary households in program neighborhoods are 12% more likely to desire credit and 8% more likely to desire any home improvements since moving in than are households in neighborhoods not yet reached by the program.

Longitudinal information on the amount of credit requested for housing improvements reveals the same story. In column 3, the dependent variable is the amount of credit requested for the purpose of residential renovations during a two-year period. Credit requests are divided into requests made in 1997 or 1998, and requests made in 1999 or 2000, and only households reached by the titling program in 1998 or 1999 are included in the treatment group. The DID estimate in column 3 indicates that credit requested for home improvements increased 110% over the interval for households that were treated during the interval. Interestingly,

14. Since there is no longitudinal information on why households did not apply for credit or did not make home improvements, I am unable to employ the same DID analysis as before.

data on credit received for home improvements indicates that changes in supply were insufficient to meet the change in demand. In particular, while the amount of credit demanded for home improvements more than doubles, credit received for the same purpose only increased by 49%.

4. Conclusions

The collection of evidence presented in this analysis suggests that strengthening property rights in urban slums leads to a significant increase in the rate of residential investment. The magnitude of the implied effect is more than two-thirds of the baseline level. The fact that both OOP expenditures on home improvements and the rate of investments among nonborrowing households also rise significantly with titling indicates that changes do not operate exclusively through greater access to credit. Furthermore, survey data on household access to loans for home improvements suggest that the effect of tenure security on investment incentives is even greater than realized levels of renovation imply. This implies that property reforms can be productivity-enhancing in urban settings even when credit constraints are not binding.

The study fills an important gap in the literature on property rights and investment. While a sizable empirical literature investigates the relationship between tenure status and agricultural investment, the influence of tenure security on urban investment has received little attention.[15] Meanwhile, an estimated 10% of the global population is housed in urban squatter settlements where tenure security is very low. The issue also has significant policy relevance in light of the growing number of urban land titling programs in developing countries. Despite the absence of empirical evidence, property titling is increasingly considered a critical instrument for generating investment in urban slums (Binswanger and Deninger 1999). My results lend empirical support to this motivation.

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15. An exception is Hoy and Jimenez (1997), who provide empirical evidence that tenure security increases local public goods provision in Indonesia.

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