

# Do Property Titles Increase Credit Access Among the Urban Poor? Evidence from a Nationwide Titling Program

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The collateral value of landholdings is generally assumed to increase with ownership rights, thereby improving credit access among landholders. In situations of poverty, however, this outcome is uncertain because of other significant barriers to lending.

To test whether proof of property ownership promotes the use of low-income housing as collateral, we evaluate the impact on credit supply of obtaining a property title through a land-titling program in Peru. By directly observing whether loan applicants are requested to provide collateral, we can isolate the effect of property titles on credit supply from their effect on demand by comparing loan approval rates when titles are requested to rates when they are not. Our results indicate that property titles are associated with approval rates on public sector loans as much as 12% higher when titles are requested by lenders and no relationship between titles and approval decisions otherwise. In contrast, there is no evidence that titles increase the likelihood of receiving credit from private sector banks, although interest rates are significantly lower for titled applicants regardless of whether collateral was requested.

The failure of commercial banks to increase their rate of lending to households that obtain property titles through government programs has important implications for the potential effects of property reform on economic growth and poverty reduction. One explanation for this failure is that titling programs reduce banks' perceptions of their ability to foreclose. This is supported by data from Peru indicating that individuals with title have *less* fear of losing property in cases of default.

## 1. INTRODUCTION

A large body of work has documented extensive credit rationing in developing countries, whereby low-income households are excluded from the formal banking sector.<sup>1</sup> The widespread inability of small and informal borrowers to provide secure collateral for loans – generally a necessity in formal credit markets – is a critical barrier to access,<sup>2</sup> and

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<sup>1</sup> In this context, credit rationing refers to non-price rationing, such that asymmetric information and enforcement costs prevent price from serving as the market-clearing mechanism, in turn creating a disequilibrium of excess demand in the credit market (Stiglitz and Weitz (1981). For an overview of this literature, see Hillier and Ibrahim (1993) and Jaffee and Stiglitz (1990).

<sup>2</sup> According to Berger and Udell (1990), around 70% of all commercial and industrial loans in the US are secured with collateral. Meanwhile, “Lack of collateral satisfactory to banks has almost always been a constraint on disbursement of World Bank SME lines of credit” (Balkenhol *et al.* (1995)).

the large percentage of untitled property in much of the developing world is a frequently cited contributing factor (Holden (1997)). While land is advantageous as collateral because it cannot be removed and does not easily devalue, it is widely believed that many borrowers face credit barriers for lack of formally documented ownership rights.<sup>3</sup>

Consistent with this notion, government land-titling programs are thought to be critical in improving access to credit among the poor, and wide scale land-titling has become a popular policy prescription for reducing credit constraints in developing countries (Binswanger *et al.* (1999)). Nevertheless, property titles are not necessarily sufficient to transform modest landholdings into viable collateral for commercial loans. Use of titles to securitize loans may fail in impoverished settings because transaction costs involved – such as those associated with collateral processing, foreclosure and resale – are sizable compared with the average loan sought. Such costs are even higher when political or legal factors impede repossession of property (Deninger *et al.* 1993). Even when foreclosure is feasible, a high degree of mistrust often exists among lenders as to the validity of ownership documents, and the cost of verification may be prohibitively high even in the context of a formal property system. If poor households are “transactions-cost rationed” in formal credit markets, the lower default risk brought about by collateral provision may be insufficient to facilitate access to loans. Indeed, past research has found the impact of rural titling programs on credit supply and investment demand to be strongly size-differentiated, rationing small producers out of the credit market even when they have titled collateral (See Carter and Olinto (1997)).

Hence, in an era of land-titling reform motivated by credit market improvements, a key question is whether the distribution of property titles in fact enables lenders to profitably use low-income housing as collateral. This paper examines this question by analyzing lender responses to changes in formal ownership rights brought about by a nationwide titling program in Peru, under which more than 1.2 million property titles were distributed to urban households. The staggered timing of the program combined with the collection of cross-section micro-data after partial implementation enables a comparison of households in neighbourhoods already served by the program with households in neighbourhoods yet to be served. In this way, we assess the effect of obtaining a property title on the likelihood of a household receiving a bank loan.

One contribution of the paper is to examine credit market effects of land titling in an urban setting, whereas the existing empirical work on property titles and credit has focused on rural markets (See Feder *et al.* (1988); Alston *et al.* (1996); Lopez and Romano (2000); Carter and Olinto (1997); Atwood (1990); Carter and Wiebe (1990); Migot-Adholla *et al.* (1991); Christensen *et al.* (1993)). Titling programs arguably have larger potential impact on credit supply in urban settings where geographic barriers play a minimal role in transactions costs, an increasingly relevant distinction as the number of urban titling programs rises.

A second contribution of the paper is to make use of the natural experiment provided by the Peruvian program to address concerns over the endogeneity of tenure

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<sup>3</sup> Furthermore, in many countries, including Peru, legal barriers restrict the use of movable property as collateral, such that real estate is the only viable form of security interest (Fleisig and de la Pena (1996)).

status that arises in measuring the collateral value of property titles by comparing titled to untitled households. In particular, any relationship between legal ownership and credit access may reflect spurious correlation between strong property institutions and well-functioning credit markets. Similarly, the decision to title property may be a function of property values or the perceived collateral value of titled land (direct evidence of this is provided by Miceli *et al.* (2001)). For instance, households may have a tendency to seek property titles in communities where loan transactions are less costly based on external factors such as an adequate local property registry that facilitates title verification or a local court known to uphold loan contracts. If tenure status is endogenous to land values or financial markets, the collateral value of titled properties will overstate the gains to titling untitled properties. The Peruvian program, in which all households were “assigned” property titles irrespective of demand, helps isolate the causal effect of property titling on credit market outcomes by reducing these endogeneity concerns.

A second issue complicating empirical work on property rights and credit supply is the fact that land titling efforts have the potential to affect not only the supply but also the demand for credit, which is also a function of ownership rights since tenure security influences incentives to undertake land-related investments (see Besley (1995)). Because of this complexity, the majority of past work has focused on changes in the demand for credit or changes in the total amount of borrowing associated with improved ownership rights. Hence, a final contribution of this paper is to isolate the role of property titles in increasing credit supply from their effect on demand by using extensive micro-data on the loan approval criteria used by banks, including all documentation and information households were asked to provide in loan applications. This allows relatively precise reconstruction of the information set on which banks based approval decisions, legitimizing a selection on observables model to account for potential changes in the composition of titled and untitled loan applicants arising from changes in demand.

More importantly, within the set of reported screening criteria, we can observe whether property titles were used in each loan approval decision. Clearly, improved access to credit resulting from changes in the collateral value of land can only occur among the 60% of applications in which potential borrowers are asked for proof of property ownership. Meanwhile, any difference in approval rates of titled relative to untitled households that is independent of banks’ use of property titles to screen prospective borrowers can be attributed to unobservable changes in the applicant pool arising from changes in demand.

Our estimates indicate that titling programs lead to a limited reduction in overall credit rationing and financial market inequalities for the urban poor. In particular, households with no legal claim to property are 9–10 percentage points less likely to secure a loan from a public-sector bank for housing construction materials. Importantly, the effect is concentrated entirely among applicants asked to provide a title as collateral, providing evidence that the observed program effect arises from the increased collateral value of property. Meanwhile, we find no effect of formal property ownership on approval rates of private sector lenders. However, conditional on receiving a loan, titled households face private sector interest rates an average of 9 percentage points lower. Since the measured effect is independent of banks’ reported use of titles in loan

transactions, the program effect of titling on private sector interest rates appears to operate through the signalling value of property ownership rather than by increasing the fraction of debt securitized with collateral.

The failure of commercial banks to expand credit to new property owners has important implications for the potential influence of property reform on economic growth and poverty reduction. One explanation for this finding is that titling programs may actually reduce banks' ability to repossess property, which is supported by data from Peru indicating that individuals with title have *less* fear of losing property in cases of default. This suggests that one reason that titling programs may fail to reduce credit constraints is because they unavoidably signal to lenders that a government prioritizes housing for the poor, and hence is more likely to side with borrowers in enforcing credit contracts.

## 2. THEORETICAL ISSUES

Loans contracts are characterized not only by interest rates but also by non-price elements including collateral (as emphasized by Baltensperger (1976)).<sup>4</sup> Poor borrowers are frequently denied access to loans because they lack adequate collateral to offer the lender as a warranty for their loan and also because of high costs of monitoring and processing relative to the magnitude of loans requested (Hoff and Stiglitz, 1990). Formal treatment of the link between property rights and credit supply is provided by Besley (1995) and Feder (1985). The principal argument is that formal property titles encourage the use of land as collateral by lowering the risk of loss, the costs involved in verifying ownership and the costs of foreclosure in the case of default, thereby reducing the effective leverage ratio and increasing the net collateral value of land. In competitive markets with full information, improved access to collateral reduces the risk premium, and hence the interest rate, on lending. Meanwhile, in the presence of information asymmetries, the use of collateral can eliminate credit rationing by reducing agency problems.

To motivate the empirical question, we consider the model of Bester (1985), in which collateral is used in conjunction with the interest rate to achieve separation of risk types and eliminate credit rationing. Here, a banker faces a heterogeneous distribution of potential borrowers represented by an unobservable risk parameter,  $\theta$ , such that  $\theta \in \{\theta_a, \theta_b\}$ , where  $\theta_b$  is a higher risk type than  $\theta_a$ . The fact that the banker is unable to identify types will lead him to screen borrowers by offering a menu of contracts  $\gamma_i = (R_i, C_i)_{i \in I}$  specified with interest rate  $R_i$ , and collateral requirement  $C_i$ , and constructed such that each type of borrower will choose a specific type of contract.<sup>5</sup> *A priori* two types of Nash equilibria can be obtained: a separating equilibrium in which different types of borrowers choose different types of contracts, and a pooling equilibrium in which both types choose the same contract. In this paper, we test whether

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<sup>4</sup> The theoretical function of collateral in lending is discussed extensively by Binswanger et al. (1985), Barro (1976), Benjamin (1978), and Plaut (1985).

<sup>5</sup> As discussed by Bester, the result depends on the correlation of borrowers' preferences and risk type.

a land titling program, under which borrowers shift from not having to having collateral, induces a separating equilibrium (see Bester (1985) for the formal proof of this equilibrium).

In the absence of collateral, quantity rationing will occur if adverse selection inhibits lenders from raising the equilibrium interest rate. However, as shown in Bester's model, if the equilibrium exists, no credit rationing will occur post-reform because property titles enable banks to use contracts with different collateral requirements as a screening mechanism to separate low-risk from high-risk borrowers.<sup>6</sup> Hence, for beneficiaries of the titling program, the shift from an equilibrium with only one set of price characteristics to another in which contracts include non-price components may prevent or reduce rationing. Clearly, this result depends fundamentally on the degree to which land titling promotes the use of property as collateral. Perfect sorting without rationing may be unattainable if titled borrowers still face a binding constraint on the amount of collateral they can provide (e.g. if the value of property does not exceed the transaction costs involved in processing loans).

In addition, land titles may have value in loan transactions other than their use as collateral. First, titled property owners may be offered more credit because of the household's higher expected wealth from reduced risk of expropriation. If lenders use wealth as a signal of default risk, titling may give rise to an alternative separating equilibrium in which *all* titled borrowers are considered less risky irrespective of type, lowering the collateral and interest rate requirements on all equilibrium contracts even when loans are not collateralised with property.<sup>7</sup> Secondly, land titles may influence other borrower characteristics that determine credit-worthiness, most notably employment. In particular, if ownership rights increase household labour hours as found in Field (2004), the corresponding increase in wage income could improve applicants' access to credit. In this paper we ignore the indirect influence of ownership rights on credit-worthiness via changes in employment in order to concentrate on the direct effect of titling on banks' use of property as collateral. Hence, the results provide a lower bound estimate of the total effect of the land-titling program on credit access.

### 3. EMPIRICAL METHODS

This research examines the Peruvian government's recent series of legal, administrative and regulatory reforms aimed at promoting a formal property market in urban squatter settlements. 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 embarked on an innovative nationwide program whose goal was "rapid conversion of informal property into securely delineated

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<sup>6</sup> We are for simplicity ignoring the possibility of equilibrium credit rationing, in which a borrower's demand for credit can be turned down even if the borrower is willing to pay the entire price and non-price elements of the loan contract. For a discussion, see Baltensperger (1978).

<sup>7</sup> Barham et al. (1996) note that lenders may use wealth to assess borrowers' risk, because "repayment capacity under a negative income shock is likely to be lower for [poor] borrowers because of their inability to suppress consumption to meet loan repayments and ... inability to establish a diversified asset portfolio."

land holdings by the issuing and registering of property titles” (World Bank, 1992). Implementation involved area-wide titling, in which project teams entered one neighbourhood at a time, moving contiguously within cities until all informal settlements had been reached (World Bank, 1998). While the old process of acquiring a property title was prohibitively slow and expensive, the new process was virtually free and extremely rapid (see Field (2004) for an overview). Eligibility for program participation required title claimants to verify pre-1995 residency on eligible public properties, generally using informal title documents from local registries, post-dated mail, utilities bills or signed sales documents. As a result of the reforms, roughly 80% of the country’s eligible residents became nationally registered property owners, affecting approximately 6.3 million individuals.<sup>8</sup> As target households were living in the range from just above to below the poverty line, the value of residences titled through the program was relatively low: In Lima, a comparison of titled and untitled households showed that, on average, untitled lots were roughly 40% smaller than lots titled prior to the intervention.

### 3.1 Data

To study the effect of titling on credit access we use survey data from March 2000 containing 2,750 randomly sampled households from the program’s target population. The survey was modelled after the World Bank’s *Living Standards Measurement Surveys (LSMS)*. In addition to capturing detailed information on household and individual characteristics, the survey collected an extensive array of self-reported data on all loan applications submitted by the sample households between 1997 and 1999, including bank requirements and terms of loans provided.

To tackle the question of whether improvements in land rights reduce credit rationing, our empirical analysis employs a quasi-experimental set-up that ideally mimics an experimental design with treatment and control groups. Because the survey was conducted approximately one-third of the way into the program’s implementation, roughly 60% of surveyed households belonged to neighbourhoods not yet served by the program. Hence, the treatment group is composed of 536 households that have already participated in the program, and the control group comprises households that have not.<sup>9</sup> The control group is further refined to include only the 1,180 households that eventually received a registered property title through the program (the other 1,034 discarded households already had a title prior to the program).

Table 1 provides descriptive statistics on the sample population, which allow an informal check for random assignment of program timing. As the sample means indicate, there is very little variation in demographic characteristics across program and non-program neighbourhoods. In contrast, households with titles exhibit substantially

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<sup>8</sup> By December 2002, 1.64 million lots had been formalized and 1.21 million titles granted, the vast majority of which took place between 1998 and 2000.

<sup>9</sup> In the results presented in the paper, as opposed to an intent-to-treat (ITT) analysis, households in titled neighbourhoods that have not yet received a title are excluded, presenting a potential bias in the comparison of experimental groups if ability to secure a title is related to credit-worthiness. Estimates not presented here reveal that the magnitude and significance of results are robust to an ITT model.

**Table 1: Summary statistics**

	<i>All households</i>			<i>Households requesting a formal loan</i>		
	<u>Untitled</u>	<u>Titled</u>	<i> t<sub>Δ</sub> </i>	<u>Untitled</u>	<u>Titled</u>	<i> t<sub>Δ</sub> </i>
<i>N:</i>	1,180	536		470	253	
<b><u>Characteristics of household</u></b>						
Number of working-age members	4.22	4.15	0.64	4.25	4.25	0.00
Number of members	5.30	5.28	0.23	5.45	5.50	0.32
Number of children aged 5 to 11 years	0.87	0.88	0.08	0.99	0.98	0.12
Number of children aged 12 to 16 years	0.64	0.59	1.25	0.67	0.65	0.27
HH head is female	0.23	0.23	0.15	0.18	0.20	0.75
Age of HH head	48.13	48.68	0.83	46.27	46.58	0.34
HH head is literate	0.93	0.93	0.19	0.95	0.95	0.14
HH head had no schooling	0.06	0.05	0.68	0.04	0.04	0.06
HH head attended high school	0.45	0.43	0.71	0.48	0.49	0.35
HH head's attended post-secondary school	0.07	0.06	0.71	0.09	0.08	0.75
HH head's monthly wage	635.21	575.06	0.94	689.52	573.85	0.96
HH employment days per year	460.72	490.56	1.65	458.42	485.66	1.00
Total monthly HH consumption	546.58	548.20	0.10	573.76	574.31	0.03
Bi-monthly HH food expenditures	189.21	190.44	0.23	194.88	195.08	0.03
HH education expenditures, per year	417.38	403.67	0.48	469.03	439.64	0.75
Whether HH has savings	0.08	0.08	0.21	0.10	0.09	0.48
Whether HH is extremely poor	0.27	0.25	1.00	0.20	0.19	0.60
<b><u>Characteristics of residence</u></b>						
Whether HH rents part of residence	0.03	0.03	0.02	0.03	0.04	0.49
Years of residence	1982.70	1981.40	1.31	1984.20	1982.70	1.40
Whether HH has a telephone	0.20	0.18	0.78	0.23	0.20	1.10
Whether HH has a home business	0.24	0.26	0.65	0.29	0.32	0.83
Income from home business	332.79	279.00	1.59	335.50	256.40	1.51
Average distance to formal lender	3.82	4.11	1.24	4.65	4.85	0.64
Closest bank two years ago	0.95	0.96	0.50	0.94	0.95	0.52
District number of bank branches per capita	0.03	0.03	0.08	0.03	0.03	0.65
District amount of deposits per capita, soles	1059.97	926.75	0.91	943.27	863.94	0.75
District number of ATMs per capita	0.04	0.04	0.49	0.04	0.04	0.59
<b><u>HH lending behavior</u></b>						
Would accept a formal sector loan	0.60	0.73	5.11	-----	-----	-----
Asked for a formal sector for loan	0.40	0.47	2.87	-----	-----	-----
Requested an informal loan	0.12	0.11	0.64	0.17	0.14	1.12
<b><u>HH housing improvements</u></b>						
Housing improvements made, 1997–99	0.46	0.56	2.83	0.63	0.76	2.95
HH improvements financed with formal credit	0.18	0.30	4.09	0.42	0.60	3.70
Housing improvements made, ever	0.75	0.83	2.62	0.87	0.95	3.06
Asked for a construction loan, ever	0.37	0.51	4.94	0.60	0.74	3.46

Note: Observations are households. HH indicates household head. Cells contain sample means as reported in the 2000 COFOPRI Baseline Survey.

different patterns of borrowing and housing investment behaviour than those without titles – presumably a reflection of greater demand for investment associated with higher tenure security. In particular, titled households are 10% more likely to have undertaken housing improvements in the two years prior to the survey and 8% more likely to have

made improvements to the house at some point in the past.<sup>10</sup> Of households that engaged in housing improvements between 1997 and 1999, titled households are 15% more likely to finance improvements through formal loans, and this difference is statistically significant (See Field (2004) for a detailed description of investment responses to the program).

Correspondingly, formal credit demand also increases significantly, measured as the share of households applying for formal loans (18 percentage points higher for households with titles) and the share reporting willingness to accept a loan from a formal lender (14 percentage points higher for households with titles). Regression estimates of the effect of property titles on formal credit applications that control for observable household characteristics are reported in Appendix 1. These estimates indicate that household traits account for more than half the difference in loan application rates between titled and untitled households. Columns 3 and 4 of Table 1 compare observable characteristics of loan applicants with and without titles, the sub-population used in our analysis. While differences exist in the demand for credit associated with property ownership, observable differences between untitled and titled households are even smaller with respect to almost every demographic characteristic. This indicates that marginal applicants (those encouraged to apply for a loan in response to receiving a title) are similar in observable characteristics to unconditional applicants (those who would have applied for a loan in the absence of the program).

### 3.2 *Aggregate level of credit rationing*

Before attempting to differentiate supply- and demand-side effects, we first explore whether changes in the demand for loans are accompanied by changes in the aggregate level of credit rationing. If the net change in the level of borrowing exceeds the increase in demand for loans associated with the titling program, we can conclude that credit access has also improved post-reform. Following the definitions of Feder et al. (1991), we classify sample households as fully constrained, partially constrained or unconstrained (price rationed) in formal credit markets. Households with a 100% rejection rate on loan applications between 1997 and 1999 are classified as fully constrained, households who received a lower amount than requested are classified as partially constrained and households with a 100% approval rate on loans at the amount requested are classified as unconstrained.

Rather than inferring demand from loan requests, we follow Barham *et al.* (1996) and construct a measure of latent demand among non-borrowing households. Survey households that do not request loans were asked whether they *would have* accepted credit from several different sources. Households that do not apply for formal loans but report they would accept credit from at least one of these sources are assumed to have self-sorted out of the credit market, and are classified as ‘fully quantity rationed.’ Households reporting that they would not accept credit from any formal source are further subdivided

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<sup>10</sup> This difference persists in regression-controlled means accounting for years of residential tenure, indicating that the difference is not simply a result of treatment group members living in newer neighbourhoods and thus being more likely to have engaged in housing improvements in recent years.



on the basis of their reasons for refusing loans from formal sources. Those whose reason is fear of losing collateral are classified as ‘fully risk-rationed,’ while those whose reason is anything other than fear of losing collateral are classified as price rationed on the presumption that they have zero demand at the available interest rate.

Interestingly, the pattern of credit rationing indicates that identical shares (34%) of households both with and without titles are fully rationed out of the credit market, meaning they either apply for credit and are rejected or do not apply but would accept a loan if it were offered (Table 2). Meanwhile, the data suggest clear differences in the pattern of credit demand between titled and untitled households. In particular, the share of households that either applies for or would accept a loan rises from 60 to 73 percent, while the share that actually applies rises from 40 to 47 percent. All of the increase in loan applicants is absorbed under the category of partially rationed households (received less than they requested). Since the increase in the share of households that receive loans approximates the increase in the share that apply for loans, we cannot automatically infer that the rise in demand for credit was accompanied by a change in supply. Instead, it is possible that the increased share of loan applicants comes entirely from the population who were previously credit-worthy but unwilling to borrow. In other words, there is either perfect self-sorting among new credit market entrants or corresponding improvements in the availability of credit to households with property titles.

**Table 2: Degree of credit rationing**

	<u>Did not apply for a loan</u>			<u>Applied for a loan</u>		
	<i>Would not accept</i>		<i>Would accept</i>	<b>fully</b>	<b>partially</b>	<b>price</b>
	<b>price</b>	<b>quantity</b>	<b>quantity</b>	<b>quantity</b>	<b>quantity</b>	<b>price</b>
	<b>rationed</b>	<b>rationed:</b>	<b>rationed:</b>	<b>rationed:</b>	<b>rationed</b>	<b>rationed</b>
	<i>too expensive</i>	<i>risk-rationed</i>	<i>self-sorting</i>	<i>rejected</i>	<i>received some</i>	<i>received all</i>
Untitled	0.290	0.110	0.202	0.026	0.142	0.231
<i>N:</i>	342	129	239	31	167	272
COFOPRI title	0.203	0.069	0.256	0.015	0.218	0.239
<i>N:</i>	109	37	137	8	117	128
<i>t<sub>A</sub></i>	3.79	2.62	-2.47	1.46	-3.98	0.38

*Pearson  $\chi^2 = 36.72$*

Notes: Universe is all households in sample, and outcomes pertain to formal sector loan applications over the past three years. Among households that applied for at least one loan from a formal source, those that receive no credit are classified as ‘fully quantity rationed’, those that some but less than all of what they requested are classified as ‘partially quantity rationed’, and those that received all the credit they requested are classified as price rationed. Households that do not apply for formal loans over the reference period but report they would have accepted credit from at least one source are classified as ‘fully quantity rationed.’ Among households reporting that they *would not* accept credit from any formal source, those whose reason is fear of losing collateral are classified as ‘fully risk-rationed,’ while those whose reason is anything other than fear of losing collateral are classified as price rationed.

One notable observation is that, whereas tenure security is generally thought to give rise to an increased fear of losing property, in these data a significantly smaller share of titled households is risk-rationed in the credit market (Table 2). This pattern suggests that perceived risk associated with collateral use is *negatively* related to ownership rights among urban households, which would be the case, for instance, if banks had greater repossession rights over property documents that were not fully registered. This result suggests an important reason the program may have failed to improve credit market functioning: In contrast to standard predictions about gains from property formalization, changes in property institutions accompanied by increased protection of homeowners from collateral loss would *reduce* rather than increase banks' willingness to securitize loans with property. Titling programs will only improve credit markets if rights are strengthened for any formal claim on property – including banks' right to foreclose when a client defaults on a loan. Because of the political climate surrounding land titling, it is very possible that repossession is actually more costly for banks post-reform.

### 3.3 *Household borrowing behavior*

Table 3 presents categories of formal credit available to households in the sample and the share of loan applications to each source. The columns on the right-hand side compare the credit sources of households with and households without titles. Here we observe three main categories of banks participating in formal credit markets in urban Peru. According to the survey, the most important source of credit – constituting 35% of all loan applications and 45% of all formal loan applications – is the public-sector Materials Bank (MB), which has historically been one of the largest lending institutions in Peru. Since 1980, MB has targeted in-kind loans of housing construction materials to urban populations living in settlements, housing cooperatives and popular housing associations – the urban titling program's exact target population.<sup>11</sup> The maximum loan amount is roughly \$5,000, loans are relatively long-term (up to 15 years) and the bank's official guidelines maintain effective annual interest rates of between 7% and 9% on all loans.<sup>12</sup> Households in this sector are highly dependent on MB for construction materials. Among all households that financed improvements with credit obtained between 1997 and 1999, 73.3% were from MB.

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<sup>11</sup> These two government programs, however, operate independently, such that there is no explicit relationship between neighbourhoods targeted for program intervention and MB operations (personal interview with Daniel Andaluz, 14 August 2002, COFOPRI office, Lima, Peru).

<sup>12</sup> While MB functions somewhat as a government relief plan, loan approval is not automatic. To qualify for a loan, the bank's guidelines stipulate that the borrower have a minimum monthly family income equivalent to five times the estimated monthly payment, and borrowers may be asked to provide a co-signer. The bank guidelines also state that all loans involve a lien on the house as collateral for the loan, although a registered mortgage on land is not required. In this sense, all MB loans in theory involve 'inside' collateral, such that, in case of default, control of the construction project and ownership of depreciated assets go to the lender. In cases in which land mortgages backed by a registered property title are used in place of lien mortgages, the loan is additionally securitized with 'outside' collateral. See Chan and Kanatas (1985) for a discussion of these concepts. Official guidelines are reported online at [www.banmat.org.pe](http://www.banmat.org.pe). As Banerjee and Duflo (2002) point out, it is unclear how closely banks follow guidelines.

**Table 3: Allocation of applications across sources of credit**

	<i>All</i>	<i>Untitled</i>	<i>COFOPRI</i>	<i>[t.d.]</i>
<i>N:</i>	<i>1066</i>	<i>712</i>	<i>354</i>	
Materials Bank	0.351	0.310	0.435	2.83
Other formal lender	0.233	0.254	0.189	2.07
Commercial supplier	0.209	0.221	0.187	1.09
Informal lender	0.207	0.215	0.189	0.88
<i>Total:</i>	<i>1.000</i>	<i>1.000</i>	<i>1.000</i>	
<b><u>Composition of other formal lenders</u></b>				
Commercial bank (fully regulated)	0.544	0.525	0.597	1.01
Savings and loan organization (fully regulated)	0.327	0.343	0.284	0.88
Credit cooperative (fully regulated)	0.057	0.061	0.045	0.48
EDPYME (semi-regulated)	0.012	0.011	0.015	0.25
Nongovernmental organisation/village bank (unregulated)	0.060	0.060	0.059	0.03
<i>Total:</i>	<i>1.000</i>	<i>1.000</i>	<i>1.000</i>	
<b><u>Composition of informal lenders</u></b>				
ROSCA	0.329	0.358	0.253	1.54
Local moneylender	0.092	0.079	0.121	0.97
Family	0.222	0.237	0.195	0.67
Friend	0.130	0.135	0.121	0.36
Street vendor	0.227	0.191	0.311	1.98
<i>Total:</i>	<i>1.000</i>	<i>1.000</i>	<i>1.000</i>	

Note: Observations are loan applications, not applicants. Individuals have up to 6 loan applications each, with a sample average of 1.33 loans per applicant. COFOPRI indicates the Committee for the Formalization of Private Property; EDPYMES, 'Entities for the Development of Small and Micro Enterprises'; and ROSCA, 'Rotating Savings and Credit Association'.

Among other creditors, loan applications are fairly evenly divided between supplier or store (hire purchase) credit, credit from other private-sector lenders and informal credit.<sup>13</sup> In-kind loans from retailers or wholesale suppliers (henceforth, supplier credit), which take the form of inputs or merchandise advanced as credit, constitute 21% of loan applications from sample households. Supplier credit is available through stores specializing in selling consumer electronics and home appliances directly to clients on a credit basis, and is generally offered interest free or at very low interest rates, but for short periods of time (Dunn (1999)). In addition, the prices of goods supplied on credit are often considerably higher than the prices for cash purchases in wholesale or retail markets. Thus, the implicit real interest rates are likely to be high.<sup>14</sup> However, because interest costs are built into lease payments, *reported* interest rates on supplier credit are extremely low and often zero. Furthermore, supplier loans have close to a 100% approval rate, likely due to the fact that the good being supplied can easily be repossessed (i.e. full inside collateralization) As a result, property titles are rarely used as collateral to obtain supplier credit, so land titling should have little impact on the supply of inputs or merchandise advanced as credit.<sup>15</sup>

<sup>13</sup> Because of the importance of utilizing data on bank loan requirements, our formal analysis excludes the informal credit market, where unobservable factors are much more likely to determine credit access.

<sup>14</sup> For instance, Barham *et al.* (1996) found store credit in Guatemala provided at a 7% premium.

<sup>15</sup> In the survey data, property titles were used in only six loan applications.

Other private-sector financial institutions include commercial banks and savings and loan organizations, including commercial micro-finance lenders such as MiBanco, credit cooperatives, ‘Entities for the Development of Small and Micro Enterprises’ (EDPYMEs), village banks, and nongovernmental organisations (NGOs). With the exception of village banks and NGOs, these institutions are regulated by the national bank superintendency.<sup>16</sup> Since our data contain very few applications to semi-regulated or unregulated lenders, all private-sector lenders are grouped together in our empirical analysis and estimates run on the pooled sample along with a dummy indicator for type of institution. While sample size prevents us from studying separately the impact of a title on non-regulated lenders, the results are robust to excluding village banks and NGOs.

Meanwhile, since MB and supplier credit lending practices are much different from those of private-sector financial institutions, we separate formal loan transactions into these three categories throughout the analysis. Most importantly, the nature of credit rationing is likely to be distinct in the market for MB loans for two reasons. First, because MB is designed to reach low- to middle-income households, local branches are positioned and bank administrators accustomed to operating in these neighbourhoods. Therefore, low-income households are less likely to be transaction-cost rationed for MB loans. Secondly, because MB loans are for housing construction, loan amounts are larger on average and have a lower variance than loans from other institutions. The lending practices of MB are also distinct in that they potentially entail substantial project monitoring. Not only are construction materials purchased by the bank, but prospective borrowers must present a certified building plan when applying, and construction projects are at least minimally overseen by bank field representatives.

Significant differences in loan application behaviour exist between titled and untitled households (Table 3). In particular, households with titles are much more likely to request both public- and private-sector loans, while the share of loans sought from stores and informal sources does not vary by ownership status. In terms of differences in credit applicants to each type of lender, on average, MB loan applicants have lower socio-economic status, evidenced by their lower education levels, higher share of female household heads, lower wage incomes, higher education expenditures per year, higher share of extremely poor households and lower income from entrepreneurial activities (Table 4). It is worth noting that, despite having virtually equivalent monthly wage income, applicants to commercial banks have higher monthly spending in all categories of consumption, likely because they spend less of their earned income on housing investment. In addition, the last three rows in Table 4 reveal that mean loan approval and interest rates are distinct across types of lenders.

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<sup>16</sup> In Peru, the interest rate on regulated private-sector loans is unconstrained by the government. EDPYMEs represent an intermediate stage between unregulated credit organizations and regulated banks. See Nexus (1998) for a description of the rules for EDPYMEs.

**Table 4: Applicant summary statistics, by type of lender**

	<i>Commercial bank</i>			<i>Materials Bank requests</i>			<i>Supplier/store credit</i>		
	<u>Untitl</u>	<u>Titl</u>	t <sub>Δ</sub>	<u>Untitl</u>	<u>Titl</u>	t <sub>Δ</sub>	<u>Untitl</u>	<u>Titl</u>	t <sub>Δ</sub>
<i>N:</i>	158	60		220	154		148	62	
<b>Characteristics of household</b>									
Number working-age members	4.32	4.63	1.09	4.20	4.18	0.11	4.19	4.08	0.39
Number of members	5.35	5.77	1.31	5.49	5.40	0.41	5.43	5.61	0.61
Number children aged 5 to 11 years	0.84	0.95	0.84	1.08	0.95	1.18	1.01	1.19	1.35
Number children aged 12 to 16 years	0.62	0.78	1.27	0.74	0.62	1.32	0.59	0.63	0.25
HH head is female	0.13	0.13	0.01	0.17	0.19	0.43	0.26	0.27	0.17
Age of HH head	47.32	46.58	0.47	45.19	45.85	0.42	46.55	48.52	0.76
Household head is literate	0.97	0.98	0.70	0.95	0.95	0.20	0.93	0.90	0.57
HH head attended primary school only	0.23	0.18	0.86	0.32	0.40	1.49	0.35	0.39	0.44
HH head attended high school only	0.47	0.68	2.92	0.50	0.44	1.19	0.43	0.47	0.55
HH head's attended post-secondary school	0.12	0.13	0.26	0.07	0.07	0.05	0.11	0.06	1.04
HH head's monthly wage	762.96	631.19	1.00	784.79	650.35	0.78	603.10	584.41	0.36
Total monthly HH consumption	630.36	691.14	0.75	519.00	523.64	0.15	604.75	590.03	0.35
Monthly HH food expenditures	212.24	214.75	0.20	179.51	186.95	0.75	207.50	207.17	0.02
HH education expenditures, per year	683.92	573.60	0.66	371.67	400.38	0.62	432.06	455.74	0.36
Whether HH has savings	0.09	0.15	0.87	0.07	0.09	0.53	0.16	0.07	1.78
Whether HH is extremely poor	0.08	0.07	0.23	0.28	0.23	0.89	0.19	0.15	0.15
<b>Characteristics of residence</b>									
Whether HH rents part of residence	0.03	0.02	0.70	0.05	0.02	1.47	0.01	0.10	2.26
Whether HH has a telephone	0.32	0.30	0.26	0.16	0.19	0.56	0.19	0.10	1.57
Whether HH has a home business	0.32	0.38	0.85	0.28	0.29	0.15	0.24	0.34	1.05
Income from home business	434.47	391.33	0.37	280.83	260.80	0.37	293.69	111.03	3.80
Average distance to formal lender	5.23	6.03	1.33	4.19	4.35	0.43	5.33	5.44	0.17
Closest bank two years ago	0.94	1.00	2.62	0.95	0.95	0.16	0.93	0.95	0.36
<b>HH lending behavior</b>									
Number of loans requested	1.34	1.38	0.41	1.21	1.13	1.56	1.37	1.32	0.49
Requested an informal loan	0.11	0.15	0.74	0.10	0.08	0.70	0.32	0.31	0.23
Loan offered, whether any	0.90	0.87	0.66	0.91	0.99	3.40	0.99	1.00	1.00
Average difference in credit amount	-117.7	-242.8	0.72	-184.5	-543	1.02	-0.16	-0.07	1.01
Size of loan, Soles	2,773.30	2,414.30	0.65	3,702.60	3,768.46	0.24	456.60	266.60	1.34
Interest rate, percent	0.32	0.20	3.76	0.09	0.07	1.35	0.03	0.02	1.23

Note: HH indicates household head. Cells contain sample means as reported in the 2000 COFOPRI Baseline Survey.

Because the sample sizes are small, very few significant differences are observed between titled and untitled households *within* each category of loan. One notable difference is that, among applicants for private-sector loans, households without titles are relatively more educated, while in the pool of MB applications, applicants without property titles are less educated. For all education categories, these differences in differences are statistically significant. With respect to loan application outcomes, the mean differences in approval and interest rates indicate that a higher share of titled applicants receive MB loans and that applicants with titles are offered lower average interest rates on private-sector loans.

### 3.4 Econometric model

We attempt to measure the collateral value of land titles by modelling the outcomes of individual credit applications. Inference about the impact of a property title on a loan applicant's probability of approval involves speculation about what the applicant would have experienced in the absence of a title. The simplest of such models is:

$$y_{ij} = \alpha_i + \gamma d_j + \varepsilon_i,$$

where  $y$  is application outcome,  $j$  is the index for the control group ( $j=0$ ) and the treatment group ( $j=1$ ),  $d_j$  is 1 if the household has a title and zero otherwise, and  $\gamma$  is the treatment effect of having a title. The no-treatment counterfactual is assumed to obey an additive model, while the treatment effect is constant:

$$y_{i0} = \alpha_i + \varepsilon_i, \text{ where } y_{i1} - y_{i0} = \gamma \text{ and } E[y_{i0}] = \alpha_i. \quad (1)$$

Equation (1), which states that the only reason access to credit changes in the treatment group is titling, is required for identification.

As discussed in Section 1, in non-experimental data, having a land title will generally *not* be independent of potential outcomes since both the decision to obtain a title and the decision to apply for a loan are likely to be correlated with the local lending environment or with property values. The fact that property titles were assigned in our data in a quasi-experimental fashion independent of household demand for tenure security or credit reduces concern over the endogeneity of tenure status. Nonetheless, the large apparent changes in investment demand raise concern over heterogeneity in the pool of loan applicants even if program participation is as good as random. If receiving a title encourages an individual to apply for a loan, titled and untitled applicants will not be comparable in every respect other than the title even if titled and untitled individuals are, so Equation (1) is violated. The direction of bias will depend on whether marginal applicants are more or less credit-worthy than unconditional applicants. A comparison of observables (Table 1) suggests that the two groups are equivalent, lending confidence to our ability to identify treatment effects using untitled applicants as a control group.

To further isolate the effect of changes in the collateral value of land from changes in the pool of applicants, we make use of detailed survey data on the information and documentation used by banks in the screening process of each loan application.<sup>17</sup> This allows us to identify the full set of household characteristics relevant to each particular approval decision. Since loan approval decisions are made by formal lenders on the basis of some finite set of observable characteristics of the applicant,  $X_i$ , in theory loan approval outcomes depend only on  $X_i$  and treatment (having a land title). In this case, the additive model applies conditional on  $X$ , and identification requires:

$$E[y_{i1} - y_{i0} | X] = \gamma(X) \text{ and } E[y_{i0} | X] = \alpha_i(X). \quad (2)$$

This supports estimating the effect of a title in the approval of loan application  $i$  with the following Probit model:<sup>18</sup>

$$Pr(\text{approval})_i = \beta_0 + \beta_1(\text{title}) + (\Pi_i X_i)\alpha + e_i. \quad (3)$$

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<sup>17</sup> All households that applied for a loan – regardless of whether the loan was approved – were asked to report the complete set of documents and information, including property titles, requested by the bank.

<sup>18</sup> For households with multiple applications to one type of bank, we use only the most recent application and control for the whether the household applied previously for other formal loans from that category of bank. Robust standard errors are used throughout to account for survey clusters and strata.

where  $X_i$  is a  $k$ -dimensional vector of applicant characteristics and  $\Pi_i$  is a  $k \times k$  diagonal matrix containing along the diagonal indicators of whether the bank used each characteristic ( $x_1, \dots, x_k$ ) in its approval decision for application  $i$ . As long as the set of criteria reported by households reasonably captures the information set on which lenders base their approval decisions, the average treatment effect of a property title will be identified by  $\beta_1$ . In other words, even if differences in the demand for credit generate differences between treatment and control applicants, given sufficient information on household characteristics observed by banks at the application stage and the bank's approval algorithm, unconfoundedness is likely to hold conditional on  $X_i$ .

Furthermore, even if there are remaining differences in credit-worthiness observable to banks but not captured by the reported criteria, these should be absorbed by the difference in approval rates between titled and untitled households among the approximate 50% of lenders that do not request a title. Hence, we also estimate:

$$Pr(\text{approval})_i = \beta_0 + \beta_1(\text{title}) + \beta_2(\text{title} * \text{title used in screening}) + (\Pi_i X_i) \alpha + e_i. \quad (4)$$

The coefficient estimates on the indicator of whether the applicant acquired a property title through the program,  $\hat{\beta}_1$ , and the indicator of whether he was asked to provide a title in the loan transaction,  $\hat{\beta}_2$ , provide inference on the treatment effect of titling. If loan approval rates are higher among the treatment group because property titles are used as collateral, the treatment effect will be fully concentrated among applications in which a title was used, so  $\hat{\beta}_1$  will be zero. Conversely, if differences in loan approval rates across experimental groups reflect unobservable differences between treatment and control applicants, approval will be independent of collateral requests, so  $\hat{\beta}_2$  will be zero.

Personal identity documents, property titles, wage receipts, co-signer information, reported self-worth and utilities receipts are the most common loan application requirements (Table 5).<sup>19</sup> In total, 51% of loan applicants, comprising roughly equal proportions of applicants with and without titles, are asked to provide a property title prior to the loan approval decision. In less than 10% of applications, banks require other documentation of repayment capacity, including lending group membership, rental contracts, tax receipts, and business registration or professional license documents. Only a handful of banks ask for bonds or collateral in the form of household goods.

To account for these requirements, our empirical estimates control for a number of relevant household characteristics available from the survey. It is important to note that, while several of these variables are potentially endogenous to program participation, in order to isolate the partial derivative of titling on banks' use of collateral it is necessary to account for simultaneous changes in other household characteristics relevant to loan approval decisions. In this sense, we do not measure the net effect of land titling – or total derivative of ownership status on credit access –, which includes indirect channels such

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<sup>19</sup> A potentially important source of missing data is the category of loan requirements labelled 'other,' in which the exact requirement was not specified by the household. To correct for this missing information, we include a larger set of potentially relevant household characteristics that might fall under this category.

as employment, and would presumably be larger than the partial derivative. However,  $(\beta_2 - \beta_1)$  sheds light on the size of the difference between total and partial derivatives.

**Table 5: Loan requirements**

<i>Credit source:</i>	<u>Materials bank</u>	<u>Commercial bank</u>	<u>Supplier credit</u>
<i>N:</i>	614	548	266
Nothing, just reputation	0.003	0.106	0.652
Personal identity document	0.982	0.821	0.303
Property title	0.599	0.429	0.113
Utilities bill	0.503	0.454	0.175
Reported self-worth	0.375	0.299	0.075
Co-signer	0.345	0.285	0.132
Wage receipt	0.269	0.347	0.145
Other	0.246	0.179	0.087
Solidarity group membership	0.083	0.020	0.019
Promisory note	0.072	0.089	0.071
Business registration documents	0.031	0.078	0.011
Household items (collateral)	0.016	0.040	0.009
Bond	0.016	0.038	0.004
Tax receipt	0.015	0.051	0.004
Operating license	0.015	0.041	0.000
Rental contract	0.003	0.003	0.000

Notes: Data come from survey question asked of all loan applications reported in the 2000 COFOPRI Baseline Survey (1428) regarding each piece of documentation required by the bank for a given loan application, whether or not loan was approved.

To capture wage income, we control for total monthly household wage income, monthly earnings of the highest wage earner, whether the highest wage earner is self-employed, whether the worker with highest number of working hours is self-employed, monthly earnings of the highest contracted employee and the share of household wages from the contracted employment. We separate self- and contract-employment from non-contract employment because commercial banks may only accept formal wage receipts, although households are likely to report all wage income. To capture the reported self-worth of the loan applicant, we control for the value and age of the property, whether the household is engaged in entrepreneurial activity, monthly income from household entrepreneurial activity, whether the business has a registered tax number, whether the household rents part of their residence and the total amount of other outstanding formal debt incurred between 1997 and 1999.

To account for household utilities bill requirements, we include information on whether or not the household paid an electricity, water, or telephone bill the month before the survey, along with amounts paid for each. To address the remaining loan requirements, we incorporate information on whether the household rents part of its residence, and whether any household member belongs to a community financial group. Capacity to provide a co-signer is proxied by the number of adults in the household and



sex of the household head. Finally, capacity to provide a property document is indicated by whether the household is a member of the treatment group (and therefore has a property title), along with whether the household has an unregistered property document.

We also include in the empirical model basic pieces of household information that are possibly relevant to loan application decisions but not recorded as official screening criteria. These include: sex, age, literacy and education levels of household head; whether the household reports experiencing an economic shock in the past year; and whether the household previously applied for a loan from the same category of institution, the year of the loan application, the intended use of loan funds and the distance from the lender to the household. The last characteristic is relevant for transaction costs of the bank, while loan history could be important if use of collateral decreases with length of relationship with the bank (Berger and Udell (1995) provide evidence of this).

The intended use of loan funds is relevant only for applications to private-sector lenders because loans to MB are uniformly intended for housing construction, while supplier credit is used solely for the purchase of consumer goods. Among applications to private-sector banks, the purpose of the loan is important primarily for identifying risk associated with entrepreneurial credit. Among these loan applications, 34.3% are intended for housing construction, 38.7% for entrepreneurial activity, 8.1% for emergency needs, 2.0% for household goods, 1% for land purchases and 16.1% for other consumption. In the market for private-sector loans, there is little difference between households with and without titles in the composition of loan uses (Table 6).

**Table 6: Distribution of loan uses and approval rates by loan use**

	<i>All loan applications</i>				<i>Private sector loan applications</i>		Approval rates	
	<i>All loan applications</i>		<i>Private sector loan applications</i>		<i>All loan applications</i>			
	<u>Untitled</u>	<u>COFOPRI title</u>	<u>Untitled</u>	<u>COFOPRI title</u>	<u>Untitled</u>	<u>COFOPRI title</u>		
<i>N:</i>	526	276	158	60	526	276		
Household items	0.073	0.076	0.017	0.030	1.00	1.00		
Housing construction	0.445	0.545	0.359	0.328	0.89	0.95		
Entrepreneurial activity	0.153	0.083	0.381	0.403	0.90	0.89		
Emergency	0.071	0.043	0.088	0.060	1.00	1.00		
Other	0.260	0.253	0.155	0.179	0.98	0.98		

Notes: Observations are loan applications, not applicants. Distribution of credit use across loan applications to all lenders and private sector lenders only (excludes Materials Bank applications only).

By including only application-specific lending criteria (via  $\Pi$ ), Equations (3) and (4) assume that the only household information relevant to the approval decision is that which was reported by the household as required documentation. This model is appropriate only under the strong assumption that households report all information used by banks and that banks do not make use of information that was not requested. Another possible specification is to include all potentially relevant information regardless of a particular bank's reported screening criteria by adding a term  $\phi X_i$  to the regression equation. In light of the small sample sizes for each type of loan, the regression estimates follow the parsimonious specification.

Given that additional information might be used but not reported and the fact that a non-trivial number of loans involved unspecified ‘other’ information (Table 5), we also estimate program effects using non-parametric matching techniques, which impose fewer constraints on the total number of covariates included in the model. In particular, titled households are matched to untitled households on the basis of the propensity score, defined as the conditional probability of having a title,  $P(X_i, \Pi_i) = \Pr(D_i = 1 | X_i, \Pi_i)$ .  $P(X_i, \Pi_i)$  is calculated by performing a logistic regression of  $X_i$  and  $\Pi_i X_i$  on program participation.<sup>21</sup> Propensity scores balances the distributions of covariates in  $X$  between program participants and non-participants based on the similarity of their predicted probabilities of participation (Rosenbaum and Rubin 1983).<sup>22</sup> The main advantage to propensity score matching is to capture possible non-linearities in treatment effects and control variables without increasing the dimensionality of the problem. Since lending decisions involve potentially complex interactions among observable borrower characteristics, it is arguably erroneous to impose a parametric functional form linking program participation to outcomes (see Jalan and Ravallion (2003)).

There are several ways to construct estimators based on the propensity score. Kernel matching compares each treated individual with a kernel-weighted average of all comparison observations, with the weights assigned according to the propensity score. In our estimates, the kernel matching estimator is given by a Gaussian kernel function, and standard errors are obtained by bootstrapping. We also present results from random-draw, nearest-neighbour and stratified matching procedures for robustness (see Ichino (2002) for a description of these methods).

Among the pool of approved loans, we also examine differences across treatment and control groups in the interest rate, size of loan obtained, and difference between amount requested and amount received. The corresponding ordinary least squares (OLS) and matching estimates are presented alongside the loan approval estimates. Clearly, loan terms and approval are determined simultaneously; however, data limitations prevent us from estimating a joint model of application outcomes that would give precise estimates of lenders’ interest rate and quantity responses to property titles. Instead, we examine differences in average loan terms in order to help distinguish between competing explanations for differences in approval decisions across titled and untitled households.

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<sup>21</sup> There are several procedures for matching on the propensity score; see Heckman *et al.* (1998) for a good review. Here we estimate the propensity score with predicted values from a Probit model. We did not find significant differences in the distribution of covariates within strata.

<sup>22</sup> Rosenbaum and Rubin (1983) show that if the  $D_i$ ’s are independent over all  $i$ , and outcomes are independent of participation given  $x_i$ , then outcomes are also independent of participation given  $P(x_i)$ , just as they would be if participation was random. In other words, the strong ignorability assumption holds and differences in the outcomes between the control group and the participants can be attributed to the program.

## 4. RESULTS

### 4.1 Materials Bank loans

A strong, positive relationship exists between the likelihood that a loan request to MB is approved and the household having received a property title from the program (Table 7). The Probit estimate in column 1 of Table 7 indicates a 4.6 percentage point increase in the likelihood that a loan application to MB is approved, implying a reduction in the rejection rate of nearly 50%. Further, when the treatment effect is combined with whether the document was requested by the bank, in column 2 of Table 7, we observe that the entire treatment effect is concentrated among households that were asked to provide a title. Among the 40% of MB loan applicants that were not asked to provide property titles with loan applications, the treatment effect is close to zero and insignificant – a strong indication that the availability of property titles is indeed responsible for the increase in the loan approval rate associated with having a title.

**Table 7: Ordinary least squares (OLS) regressions, outcome of formal credit applications**

<i>Dependent variable:</i>	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Offered	given applied	Interest rate offered, given received	Amount received, given received			Difference between amount asked and amount received	
<b><u>Materials Bank loans</u></b>								
COFOPRI title	0.046 ** (0.010)	0.012 (0.030)	-0.010 (0.015)	-0.015 (0.019)	-216.26 (219.05)	-113.14 (307.40)	399.59 (480.49)	-215.51 (470.70)
Property documents required COFOPRI title		0.057 ** (0.024)		0.011 (0.022)		-325.03 (403.03)		1,342.86 (992.63)
<b><u>Other formal loans</u></b>								
COFOPRI title	0.002 (0.061)	0.000 (0.017)	-0.085 * (0.041)	-0.102 * (0.048)	-25.83 (684.69)	614.71 (715.37)	54.02 (119.31)	247.82 (192.12)
Property documents required COFOPRI title		-0.062 (0.082)		0.038 (0.063)		-1435.43 (1,107.56)		-434.31 (260.27)
<b><u>Supplier loans</u></b>								
COFOPRI title	0.000 (0.000)	0.000 (0.000)	-0.008 (0.014)	-0.006 (0.013)	110.35 (100.57)	106.50 (102.35)	0.128 (0.304)	0.084 (0.309)
Property documents required COFOPRI title		0.000 (0.000)		-0.014 (0.147)		192.87 (858.88)		2.210 (2.59)

Notes: HH indicates household; \*, significance of less than 1%; and \*\*, significance of less than 5%. Data in the first two columns are Probit estimates; data in all remaining columns are OLS regressions. Standard errors are shown in parentheses. Robust standard errors account for sample clustering and stratification. Demographic controls include the following: age, literacy, and education of HH head; whether residence is used as a source of economic activity; total monthly household wage income; monthly earnings of highest wage earner; whether highest wage earner is self-employed; whether the worker with the highest working hours is self-employed; monthly earnings of highest contracted employee; share of household wages from contracted employment; self-reported sale value and age of property; whether household is engaged in entrepreneurial activity and monthly income from HH entrepreneurial activity; whether business has a registered tax number; whether HH rents part of residence; total amount of other outstanding formal debt during 1997–99; whether HH paid for electricity, water or telephone in the previous month and amounts paid for each; whether HH member belongs to local financial group; number of adults; dummy indicating HH has an additional type of unregistered property document; whether an economic shock occurred in the past year; whether HH previously applied for a loan from the same category of institution and year of the loan application; intended use of loan funds; distance from the lender.

The estimated treatment effect of property ownership on MB loan approval rates from propensity score matching suggests an even larger improvement in approval rates of between 9 and 10 percentage points (Table 8). The difference between the estimated treatment effects from Probit and propensity score models is likely related in part to the exclusion of treatment group members who have no well-defined match among the control group (approximately 10% of households fall outside the region of common support). These unmatched households with property titles represent those that would not have applied for a loan in the absence of the program, which suggests that marginal applicants are characterized by below average approval rates based on some non-linear combination of observables.

**Table 8: Propensity score estimates**

<i>Matching method:</i>	(1) kernel matching	(2) nearest neighbor	(3) stratified matching
<b><u>Materials bank loans</u></b>			
Loan application approved	0.094 ** (0.028)	0.104 ** (0.036)	0.093 (0.029)
Loan amount offered, Peruvian soles	-328.66 (364.00)	-121.67 (392.05)	119.63 (287.17)
ifference in amount requested and received, Peruvian soles	-656.09 (1,072.25)	36.75 (692.64)	-127.52 (596.32)
Interest rate, %	-0.017 (0.012)	-0.021 (0.016)	-0.017 (0.014)
<b><u>Other formal loans</u></b>			
Loan application approved	0.047 (0.036)	0.036 (0.123)	0.051 (0.036)
Loan amount offered, Peruvian soles	1,494.3 (1,708.0)	789.0 (1,200.5)	1,654.0 (1,744.2)
ifference in amount requested and received, Peruvian soles	108.34 (65.08)	126.53 (61.29)	79.37 (105.20)
Interest rate, %	-0.087 * (0.043)	-0.101 * (0.047)	-0.097 (0.041)
<b><u>Supplier loans</u></b>			
Loan amount offered, Peruvian soles	-258.8 (190.43)	36.29 * (18.55)	-166.5 (98.6)
ifference in amount requested and received, Peruvian soles	-0.268 (0.22)	-0.196 (0.50)	-0.092 (1.02)
Interest rate, %	-0.029 * (0.009)	-0.005 (0.004)	-0.020 (0.022)

Notes: \*\* indicates significance of less than 1%; \*, significance of less than 5%. Demographic controls are the same as those listed in the notes to Table 7. Nearest neighbor matching in column 2 is based on a random draw. The kernel matching estimator is given by a Gaussian kernel function, and standard errors are obtained by bootstrapping. Bootstrapped standard errors are shown in parentheses.

In contrast to the loan approval outcome, the provision of a property title appears to have little effect on MB interest rates and loan amounts in both the Probit and propensity score estimates. Although the mean interest rate is nearly two percentage points lower for titled borrowers, the difference is not significant.

#### 4.2 *Private-sector loans*

Based on the regression estimates in Table 7, the effect of property titles on the market for loans from private-sector lenders is distinct from the market for MB loans. In column 3, the estimated treatment effect from the Probit estimate indicates that the likelihood of loan approval does not change with ownership status. The propensity score results are larger than the regression estimates but insignificant (Table 8). In addition, the average size of private loans to households with titles is around 50% larger, although the point estimate of the difference is not statistically significant. There are no significant or consistent differences between households with and without titles in the difference between amounts requested and received.

Meanwhile, all estimates from Tables 7 and 8 indicate that, conditional on approval, property owners face interest rates that are on average 8–10 percentage points lower than the interest rates offered households without titles. This implies a reduction in the mean private-sector interest rate from 27% to 18%. However, the treatment effect on interest rates does not appear to operate through collateral provision, as indicated by the small and statistically insignificant estimate of  $\beta_2$ , the coefficient on the interaction between treatment and the indicator of title requirement in column 4 of Table 7.

#### 4.3 *Supplier credit*

As expected, given that the supply of store credit is relatively unconstrained and prices are poorly captured by reported interest rates, we observe little evidence of a treatment effect of property titling on supplier credit. The regression estimates reported in the last two rows of Table 7 find close to zero and insignificant effects of titling on all outcomes. In the propensity score estimates, we observe measurable effects on the interest rate and average loan size with certain matching methods, but neither the sign nor significance of either result is robust to alternative matching techniques.

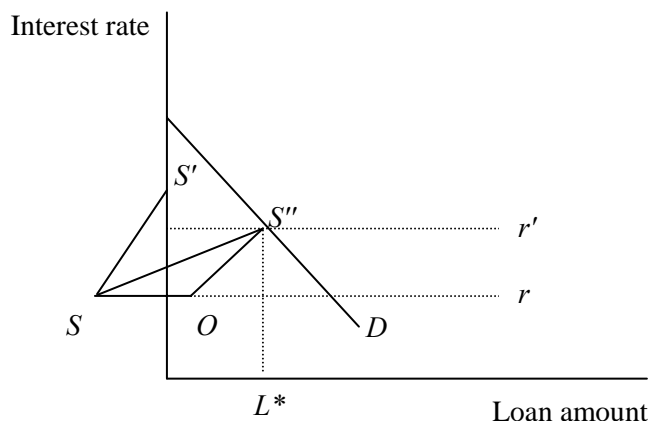
### 5. DISCUSSION

In the case of MB credit, the absence of a large effect of property titling on interest rates, conditional on receiving a loan, is not surprising because interest rates on MB loans are regulated by the government to fall within a range of two percentage points. The situation of MB is analogous to a credit market model in which the bank is constrained by moral hazard issues from raising the interest rate above a certain level (which may well be the rationale behind the regulation), inducing quantity rationing of MB loans to exclude applicants that cannot provide sufficient collateral or surpass a certain level of default risk. In this market, collateral serves to reduce credit rationing by increasing the share of loans that are free of risk to the lender.

The absence of a strong relationship between loan size and ownership status is also not surprising given that loan amounts are also imprecisely restricted by MB lending rules, which state that amount is limited by the “particular construction needs of the household.” Alternatively, differences in risk level inferred by the bank but not captured by the covariates could be responsible for the change in approval probability but not loan

amount. As described by Wette (1983), in the presence of interest rate regulation, increased use of collateral could generate adverse selection effects, depicted in Figure 1.

**Figure 1.**



Here, the supply curve originates in the negative orthant to reflect the fact that the collateral value of titled property, net of transaction costs, may be negative when land values are low. In this scenario, prior to the titling program, low-risk applicants face the supply curve  $SS''$ , while high-risk applicants face  $SS'$ , so only low-risk types are awarded loans of  $L^*$  at interest rate  $r'$ . When both types can provide collateral, the aggregate supply curve becomes  $SOS''$ . Here, more loans are awarded (since both types enter the market), but the average loan amount remains at  $L^*$  and the interest rate stays fixed at  $r'$ . Hence, if MB applicants are considered riskier, on average, post-reform because the availability of collateral induces high-risk types to enter the market, a higher share of titled applicants will receive loans, but the average interest rate and loan amount could remain unchanged.

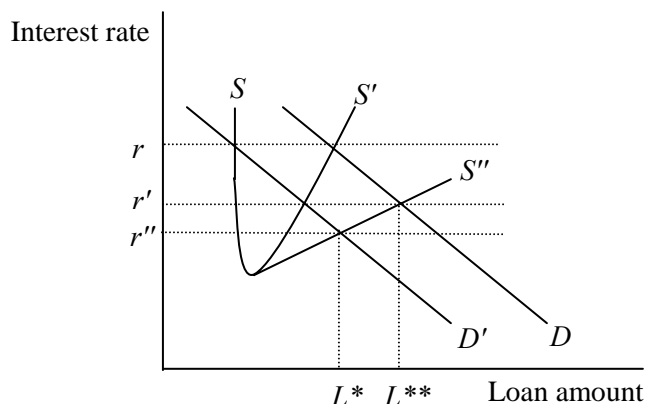
The results for private-sector lenders are more ambiguous because private-sector interest rates do not appear to depend on whether banks use property as collateral. As mentioned in Section 2, one possibility is that private-sector banks do not find it profitable to use land as collateral (*i.e.* the expected effective leverage ratio of capital is non-positive), but that they do infer lower default risk from ownership rights and possession of a property title. This situation is illustrated in Figure 2.

In this figure, no portion of the supply curve is flat, indicating that banks are not using property titles to securitize loans.<sup>23</sup> However, because banks infer lower default risk from the existence of a property title, the supply curve for program participants shifts from  $SS'$  to  $SS''$ , lowering the interest rate available to titled borrowers from  $r$  to  $r'$ . In this scenario, the average loan size also shifts outward from  $L^*$  to  $L^{**}$ , which is inconsistent with our findings. However, if the demand curve is steeply sloped, the change in quantity demanded will be small relative to the change in the interest rate. Another possibility is

<sup>23</sup> The downward-sloping portion of the supply curve reflects the fact that effective interest rates are higher for small loans due to costs involved in processing and monitoring loans.

that, given the increased availability of low-interest MB loans for titled households post-reform, the demand curve for more expensive commercial loans shifts inward for program households ( $D$  to  $D'$ ). In this case, the interest rate would fall even further to  $r''$  while the change in quantity demanded at the new interest rate is ambiguous.

**Figure 2.**



Unfortunately, with available data can not distinguish between these two stories, nor can we fully rule out the possibility that results are driven by unobserved heterogeneity in local financial markets. While the result on the private-sector interest rate is robust to controlling for observed heterogeneity among private-sector lender types, clearly the pattern could still be driven by unobservable differences in the lending practices or the level of competition among financial institutions available to households with and without titles. However, measures of aggregate banking activity from 1994 and nation-wide censuses of financial activity from 1996, including the number of automated teller machines (ATMs) and the number of bank employees by district show no differences in local financial sector development (Table 2).

An important issue that emerges from our findings is the discrepancy in lending strategies across public- and private-sector banks. Given the strict lending practices of MB, it is not surprising that MB's interest rate response differs from private-sector banks. Unable to separate the market according to risk, MB must limit the amount of credit it provides to the households without titles, evidenced by the lower approval rates to this population. In particular, since the pool of applicants for MB loans is on average more vulnerable to income shocks, it is likely that MB faces greater moral hazard and enforcement constraints that make it unprofitable to adjust the interest rate, whereas commercial banks that screen out a greater portion of applicants and therefore service a less risky pool of borrowers have more interest rate flexibility. Furthermore, it is unlikely that possessing a property title offers additional information on the default risk of MB borrowers given that they are all borrowing for housing construction, and the bank could reasonably infer the same tenure security and low likelihood of eviction from the household's decision to invest in immobile assets.

It is less clear why private-sector lenders would not make use of property titles as collateral if MB finds it profitable to do so. One explanation is that quantity rationing will

generally be size-biased because the net profit on small loans is lower, making collateral cost-effective only for relatively large loans. Since MB loans are all for housing construction and tend to be fairly large – the mean amount is roughly \$1,421 – and the variance in loan size is small, a larger number of loans from non-MB sources will be rejected because of the transaction costs involved. Collateral provision, which only increases loan transaction costs, cannot eliminate this type of quantity rationing.

Another explanation is that commercial banks perceive the transaction costs involved in using land titles as collateral to be higher post-reform. As mentioned in Section 3.2, data on households' fear of losing property suggests that titled applicants perceive foreclosure to be less likely, and commercial lenders may have the same perception in a political climate that prioritizes housing for the poor. Ironically, property titling programs might actually reduce banks' ability to foreclose because they unavoidably send the message that governments will side with poor borrowers in enforcing credit contracts. This could be different for a public-sector bank such as MB if it has inside information regarding the extent to which the government is willing to enforce property collection in the case of default.

A final possibility is that, as a public-sector institution, MB subsidizes its clients and is not, in fact, making profit-maximizing lending decisions. Furthermore, MB may be characterized by higher corruption or misuse of funds for political gain. Indeed, early reports of high default rates among MB borrowers suggest that loans may be distributed according to other criteria.<sup>24</sup> Long-run information on the profitability of MB loans and private-sector lending strategies is needed to disentangle these competing stories.

## 6. CONCLUSION

Despite the distribution of over 1.2 million property titles to urban squatters, our results indicate that credit rationing is still a key feature of the micro-lending environment in urban Peru. In particular, post-reform, a full 34% of households with titles remain fully rationed out of the formal credit market. These results shed light on the potential impact of titling efforts on financial market integration and development in poor urban communities worldwide. Although property titles are associated with a small reduction in formal-sector credit rationing, it appears that titling efforts will not automatically make collateral-based lending viable for the majority of formal-sector credit applicants.

Most notably, our estimates suggest that the bulk of the reduction in credit rationing associated with the Peruvian titling program can be attributed to one particular lending institution, the publicly funded Materials Bank, which supplies in-kind loans of housing materials. Meanwhile, access to credit from private-sector lenders appears unaltered by titling. While there are a number of possible explanations, one compelling piece of evidence indicates that titling may have reduced banks' ability to foreclose.

The fact that credit access for construction materials improves with ownership rights is important insofar as it helps meet the increased demand for housing investment

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<sup>24</sup> In conjunction with the increase in default, the bank's own financial assessment, (Banco de Materiales, "Evaluacion a Junio 2003") suggests operating losses and declining profitability for 2002.



that accompanies improvements in tenure security. In this manner, greater access to MB loans should reduce the dampening effect on other types of investment that will result if demand for construction materials rises while households remain credit constrained in financing home improvements (See Carter and Olinto (2003) for a formal presentation of this relationship). However, given that access to loans for purposes other than housing does not appear to have changed with ownership status, households with titles, post-program, will still be unable to rely on credit as a source of consumption insurance. This is exaggerated by the fact that MB loans are in-kind transfers and hence not fungible in case of unexpected changes in consumption needs.

Perhaps more importantly, property titling does not appear to assist poor households finance micro-enterprise activities. This pattern is clearly illustrated in Table 6, which presents the loan approval rates for households with and without titles according to the designation of credit. Consistent with the regression and matching estimates, we see that the entire improvement in loan approval rates is concentrated among construction loans, while all other categories of credit use have nearly identical approval rates for titled and untitled households. The means in the table indicate that liquidity constraints are still binding on entrepreneurial loans for titled households. Given that collateral-based wealth is an important determinant of small business formation in other settings (Black *et al.* 1996), land titling will have no effect on socially inefficient allocations of entrepreneurial activity across socio-economic groups if post-reform titled property cannot serve as collateral. As a result, the growth implications of titling programs may be greatly overstated.

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**Appendix 1: Ordinary least squares (OLS) regressions, whether applied for credit from particular source**

	(1)	(2)	(3)	(4)	(5)
<i>Universe:</i>	All formal loans	Construction	Materials	Supplier loans	Other formal loans
COFOPRI title	0.067 * (0.032)	0.099 ** (0.027)	0.104 ** (0.028)	0.060 * (0.028)	0.034 (0.027)

Notes: Linear probability estimates, dependent variable in all columns in binary indicator of whether applicant applied for credit from each formal lender type. Similar results are obtained from Probit estimation. COFOPRI indicates whether individual received a property title through the government program, the Committee for the Formalization of Private Property; \*, significance of less than 1%; and \*\*, significance of less than 5%.