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Signaling Credit-Worthiness: Land Titles, Banking Practices and Access to Formal Credit in Indonesia

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Signaling Credit-Worthiness: Land Titles, Banking Practices and Access to Formal Credit in Indonesia*

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Abstract

Many land titling programs have produced lackluster results in terms of achieving access to credit for the poor. This may reflect insufficient emphasis on local banking practices. Bankers commonly use sophisticated methods other than collateral to ensure repayment. Some methods rely on ex-ante information flows and formal land titles can improve these flows by signaling to the bank important characteristics about borrowers. Using a household survey from Indonesia, we provide evidence that formal land titles do have a positive and significant effect on access to credit and at least part of this effect is best interpreted as an improvement in ex-ante information flows. This result stands in contrast to the prevailing notion that land titles only function as collateral. Analysts who neglect local banking practices may misinterpret the observed effect of systematic land titling programs on credit access because these programs tend to dampen the signaling value of formal land titles.

1 Introduction

What are the channels through which land titles could affect access to formal credit? The standard argument posits that formalizing property rights equips landowners with collateral, opening up access to previously unavailable credit markets. In this paper, we present evidence that formal land titles could also have an ex-ante informational value by signaling credit-worthiness to the banker. Our evidence gives a richer understanding of one of the main justifications for large-scale land titling programs sponsored by the World Bank and other aid organizations, better access to credit for poor landowners. At the very least, the success of these programs depends on how local banking practices translate formal land titles into collateral. Additionally, when bankers use formal land titles in alternative ways, these programs may have unintended consequences. In particular, a systematic land titling program would likely eliminate the signaling value of possessing a formal land title.

In Indonesia, as in many developing countries, obtaining a formal title is a lengthy, bureaucratic and costly process. Therefore, having a formal land title can provide information about unobservable characteristics, such as an ability to interact within formal rules, the degree of integration into formal markets, business-minded characteristics or the condition of the asset.[1] The bank may prefer to lend to formally titled households not only because the title mitigates the bank's risk in the case of a default, but also because the title provides ex-ante information about the likelihood of compliance with the loan contract.[2]

To assess the informational role of land titles, we use the Microfinance Access and Services Survey (MASS) 2002, which was conducted by Bank Rakyat Indonesia (BRI) to evaluate households' microfinance activity and potential new markets. The survey provides disaggregated data on household economic activities, assets and loans for over 1400 households in 70 villages across 6 provinces. Since not all borrowers with a land title use it as collateral, we can separately identify the effect on loan size of having

a land title versus offering it as collateral. In fact, only 40% of those with a formal land title and formal bank loan use the title as collateral.[3] We find evidence for our explanation of formal land titles as information by considering first-time borrowers. When dealing with borrowers who have had past loans, banks have to some degree already solved their adverse selection problem. Thus, if a formal land title has a signaling role, it should be more important for borrowers without established credit histories.

We provide further support for the ex-ante informational role by taking a closer look at the banking practices used in the small-scale credit market of Indonesia. A number of factors arise in Indonesia that undermine the possibility of a legal transfer if the borrower defaults: weak legal infrastructure, political pressure, a thin land market, and the cost of foreclosure given the size of the loans involved. In the microfinance world, well-known examples such as Grameen Bank in Bangladesh and the institution we focus on in this paper, BRI, demonstrate that liquidity has already been provided to the poor by means other than formal collateral. Banks use alternative enforcement mechanisms that rely on reputation, such as lending within social networks and group monitoring, to solve both ex-ante adverse selection and ex-post moral hazard problems.[4] Using field observations and a unique mail survey of BRI unit heads that we conducted in 2004, we confirm that banks use other means to ensure repayment for the relatively small loans we are considering. Moreover, bankers report that the legal process to foreclose or collect on collateral is too costly for one-off relationships. In fact, even the process of officially registering the collateral may be prohibitively costly.

If banks use alternative enforcement methods, then the information effect observed through comparing first-time borrowers and repeat borrowers may reflect ex-post or ex-ante informational constraints.[5] We are able to distinguish between these two interpretations by focusing on a subset of borrowers that possess fixed income

salaries (by far the most common form of securitization other than land titles). Our evidence is inconsistent with the hypothesis that a formal land title plays an ex-post informational role.

Of course, land titles can also affect credit demand, and one may be concerned that observed differences between first-time and repeat borrowers reflect the impact of land titles on credit demand. We address this issue, first, by using information about demand from rejected loan applicants. Then, after correcting for selection, we evaluate how large a role land title's effect on demand could play. Second, we supplement the actual data with information about hypothetical demand and supply constructed from the MASS survey. We find that the main results can not be fully explained by the demand story.

Thus, the contribution of this paper is twofold. First, we show that land titles do matter for credit access. Second, we establish an additional important role that formal land titles play in the credit market. Namely, formal land titles reveal difficult to observe applicant characteristics to a resourceful banker. Even when alternative enforcement mechanisms are favored over those that use formal collateral, land titles can still support and develop the credit market by improving the flow of information before the loan contract is signed. Our contributions suggest that incorporating banking practices into the analysis is necessary to understand the effect of land titles on access to credit.

The rest of the paper proceeds in the following manner. In section 2, we briefly discuss the previous literature on how land titles affect access to credit. In section 3, we outline some important features of the legal and credit settings in Indonesia and discuss our mail survey of BRI Unit bankers. This section motivates our approach to the question of how land titles affect access to credit. Section 4 describes the data we use for the regression analysis, the MASS survey supplemented with village-level census data. Section 5 presents our empirical strategy and main results, including

subsection 5.4, in which we explore whether unobservable credit demand can explain our results using both actual and hypothetical data. Finally, section 6, more generally, addresses the issue of endogeneity of land title using instrumental variables, and then section 7 concludes.

2 Previous Literature

The positive effect of formal land titles on access to credit is purported to be well-established by Deininger and Binswanger [1999]. Nevertheless, in our survey of the literature, we observe mixed results. Table 1 lists previous empirical work on how land titles affect incidences of formal bank credit. The first three columns correspond to the study, the area of study and the empirical results as to whether land title had an impact on credit access. In addition, some studies consider systematic titling programs where possessing a land title can be viewed as relatively exogenous to the credit decision, while others study "sporadic" (or individually obtained) titles, which require more effort for households (shown in the fourth column). The latter is the case we are studying in Indonesia. The results reported vary considerably, reflecting that these studies took place in different countries with different sets of institutions governing the credit and land markets, and, in particular, differences in banking practices.

Table 1 here

The empirical problem of measuring the effect of possession of a land title on the probability of obtaining a formal bank loan requires separating the effects of land title on the supply of formal credit from the effects on the demand for credit. Most studies simply assume that there exists excess demand for formal credit.[6] Feder et al. [1988] lets observed credit equal the minimum of supplied credit and credit demanded but then resorts to assuming excess demand in the empirical work. Using

the MASS survey, Johnston and Morduch [2008] presents evidence that excess demand is not an appropriate assumption in our context. We address this problem by using information about the demand of those who have had rejected loan applications and measures of hypothetical demand and supply. We try to evaluate whether demand could explain the observed effect of land title. Field and Torero [2004] addresses the problem by using detailed information on different banks' requirements for loans and takes advantage of the timing of the implementation of the titling program. Using matching on observables to difference out demand, they measure the effect of land title on credit access only among banks that require title. We can not employ this method because we do not have access to a titling program in our context.

A second problem is the potential endogeneity of land title, which has not been adequately addressed in the previous literature. The problem has either been ignored or a systematic titling program has been used. In the first instance, the empirical estimates are likely to be biased. In the second instance, the results miss any direct effect of title as information. Although one could argue that titling programs often fail to produce full compliance, giving scope for an information effect, there are two reasons why this approach is unsatisfactory. First, the signal is likely to be much noisier (due to the time and cost subsidies of the program implementers). Hence, separating the information and collateral effect is more problematic than in the sporadic setting. Second, faced with imperfect compliance, the econometrician only identifies the average treatment effect of title under strong assumptions. One could accurately measure the effect of land title on access to credit by using an instrumental variable. However, the use of instrumental variables puts the randomized systematic program on equal footing as the sporadic setting where, in principle, the signal is less noisy. In general, using instrumental variables to construct unbiased estimates in order to test a signaling hypothesis is problematic. The signal is correlated with unobservables. We address this issue by using village-level instrumental variables.

Not all development experts consider titling programs to be appropriate in all situations. Customary land rights may be difficult to describe or put into an adequate formal title. Atwood [1990], in his study of sub-Saharan countries, argues that land titling can create uncertainty that undermines local relationships. In areas where customary land rights are strong and land markets are not really relevant, titling systems do little to benefit the community and may disrupt subtle societal interactions. Only in areas where rights are not well-established by the community and land markets do matter, can titling systems possibly have a beneficial role (Hoff et al. [1993]). Lanjouw and Levy [2002] shows how community relationships in an urban area can function as formal claims on assets. Hence, what we might measure is land title as a substitute for social networks. In this case, our results only hold in a local sense: first-time borrowers may simply have worse social networks. Although land title improves access to credit for this group, this may not hold for those with better social networks. Nevertheless, even if tenure security can be achieved through informal means, it is not a given that large amounts of formal credit can be accessed on the basis of this same community relationship.

3 Institutional Background

3.1 Credit Market Setting and Banking Practices

In Indonesia, there is a wide range of financial services including both private and government banks in the formal sector as well as ROSCA's, neighborhood cooperatives and money lenders in the informal sector. After the 1997 financial crisis in Indonesia, there has been much attention on creating a stable financial environment. Indonesia has an extensive rural banking system, mostly supplied by BRI, the bank most central to our study. BRI is the fourth largest bank in Indonesia with 10% of market share as measured by the total assets held by banks. The BRI Unit is the part

of the bank that deals with smaller sized loans.[7] It has over 4000 offices, reaching roughly a third of all households in Indonesia.[8]

In general, BRI units attempt to reach a part of the population that might not have had the opportunity to participate in the formal financial sector. The emphasis of the BRI Unit is small-scale in order to develop a personal relationship with the client. The BRI approach allows discretion within a set of basic rules. For example, although a loan above 20 million Rp. (approximately $2300 in 2004) must be approved by a central BRI branch, there is no one formula for accepting or rejecting loan applicants. Unit managers are allowed to rely on notions such as "trustworthiness" when granting a loan.[9] Unit managers are also encouraged to make use of progressive lending, interest refunds for timely repayment and social networks to raise the repayment rates. Successful unit managers are rewarded with more discretion and higher limits for lending without branch approval.

We conducted a mail survey of 192 BRI units across the same 6 provinces and 12 districts where the MASS household survey was conducted. Our response rate was over 60%. Most of the surveys were answered directly by the unit manager. The first point to emphasize again is that BRI units do accept forms of land documents as collateral that are not formal land titles. In our mail sample, on average only 42% of loans that are collateralized are done so with a land title certificate. However, almost 40% of all loans are not collateralized at all, instead they are guaranteed by deductions from future salary (fixed income). This is similar to what was found in the MASS survey where 33% of the 326 loans recorded at BRI units or branches were collateralized by a formal land title.

More revealing is the fact that when asked to assess the most important factor in considering whether to grant a loan, 82% indicated the character of the individual. Moreover, when determining the repayment ability of the applicant (which determines the loan size for which the applicant is eligible), the most important factor was cash-

flow (66%) followed by character (20%). Only respondent indicated that collateral was the most important factor for determining repayment capacity, and none indicated collateral as the most important factor in considering whether to grant a loan. This suggests that unit managers are aware of the informational problem and they make use of personal characteristics (including the past relationship with the bank) in the loan decision as opposed to solely relying on collateral to align incentives ex-post.

Repayment rates are very high in the BRI units, above 95% in most areas (Johnston et al. [2001]). As a policy, BRI prefers to avoid using foreclosure to enforce repayment. Foreclosure is described as a very rare event anecdotally but our survey indicates that it does happen; 37% of unit managers report having foreclosed on a client at least once.[10] Since the legal cost of foreclosure is high, we would instead expect to see forced or encouraged sales of pledged assets. In our survey, 77% do indicate encouraging clients to sell collateral in order to repay the loan at some point in the past year. The existence of encouraged sale of clients' assets may indicate that the asset is not fully transferable to the bank. BRI and other banks in Indonesia accept as collateral informal land documents that demonstrate ownership but are not legally transferable. Moreover, for individual BRI units, the cost of registering the title as collateral may outweigh its possible benefits. Formally collecting on collateral loses value when clients will themselves find a way to liquify at least part of the asset in order to maintain good relations with the bank. This suggests again that the courts are being bypassed even though the banks make use of collateral. However, the bank's reliance on a personal relationship with the client is what makes borrowing for new clients that much more difficult.

For instance, when asked how new clients find out about BRI and its services, 88% of the bankers said it was through friends and family. We also inquired about the maximum loan size available to clients without a formal land document. We find, on average, old clients would be eligible for 5 mil Rp more than new clients, a difference

which is the average value of a loan at BRI units. New clients without formal land documents face tighter credit constraints due to the practice of progressive lending and are less likely to take advantage of BRI's services unless they already know others who do so.

Our survey also asked if having a land title would increase the likelihood of success for a loan application. The answer is in the affirmative for 60% of our sample though only 29% say that having a formal land title will increase the likelihood of receiving a higher loan amount than someone without a land title.[11]

The results from our survey are consistent with the idea that having a formal land title signals useful information to the bank, especially if the titled household is a new borrower. Banks can (and do) use methods other than formal collateral to solve the moral hazard and adverse selection problems. The emphasis on the personal relationship between bank and client leads to the fact that these alternative methods may make it more difficult for first time borrowers. The use of land title as a signal goes some distance to alleviating this unfortunate outcome.

Having discussed the novel data set that we collected of BRI unit managers' banking practices, one might expect us to use this data in the regression analysis. We do not because unit managers move around quite frequently, so there is no way to match revealed banking practices to geographical locations. Instead, we use this data to motivate the interpretation of the empirical results.

3.2 Land Law in Indonesia

The National Land Administration Agency (BPN) grants titles to non-forest land.[12] Out of the 80 million land parcels on the fiscal tax register less than 27 million are on the legally titled register. In 2004, about 1.3 million new titles were registered sporadically and the total number of land parcels was estimated to be growing by more than 1 million per year.[13] The process of getting a title is both lengthy and costly.

The usual process requires a letter from the village head verifying that the land is in one's possession. The applicant then needs to have a survey conducted which requires funding the boundary markers, the survey fee, and covering all transportation costs. After this is completed, the document must be verified, mapped and finally certified. In total, the process can easily take one year. Once the certificate is obtained, a tax must be paid on the right to have a title on a piece of land (this is a one time tax; this is not a property tax or tax on the sale). Anecdotal evidence reveals there are also significant informal costs accumulated throughout the titling process. For example, the stated fee of a land certificate is around 300,000 Rp. (approximately $35 in 2004). However, when we asked what the actual fee was, respondents' answers ranged from 1 to 2 million Rp. (approximately $116-$232 in 2004).[14]

4 Data Description

For the regression analysis in this paper, we utilize data from two different sources. Our main source is the BRI MASS 2002 household survey. We use this data in all results. We also have additional village level information from a survey of village heads conducted annually by the Indonesian government, PODES 2003.[15] In the rest of this section, we will briefly describe each data set and give some summary statistics.

4.1 MASS Data

This survey consists of over 1400 households spread across 74 different villages, both rural and urban, in the provinces of West Java, East Java, West Kalimantan, East Kalimantan, Sulawesi, and Papua.[16] It slightly over-samples poor households especially in rural areas (Johnston and Morduch [2008]).

Table 2 here

Table 2 gives the percentage of titled households in each province. We exclude the landless in this table and in all results we present. For households with multiple land parcels, we label them as titled if any of their land is titled.[17] The vast majority of our sample had over 80% of their total land value either titled or documented in some way and only 53 households report no documentation at all. Other land documents refer to land deeds, customary or traditional land documents, and tax receipts as discussed earlier. In general, titled households are less common in rural areas (29%) than urban areas (65%).

Table 3 here

Table 3 gives the summary statistics for the covariates that we will be including in our estimations and other descriptive statistics. FormalBank equals one if the household reports ever having a formal loan. The per capita household income is divided by the poverty line in that province, with different cutoffs for rural and urban areas, in order to make more meaningful interpretations across very different areas.

Table 4 here

We have data on 645 distinct loans from about 575 different households. On average, formal bank loans are significantly larger than loans from other sources and the majority (74%) of reported loans are formal.[18] Of the formal loans reported, 123 went to households for whom this was their first loan. The most common formal loan use reported was working capital for an existing venture (37%) followed by home improvement (24%). We can classify 44% of the formal loans as being used for production purposes and the remaining 56% as being for consumption or other. Among the first time borrowers, the distribution is practically the same, 43% for production and 57% for consumption.[19]

In both rural and urban areas, more titled households do have formal loans (40% of titled households have had a formal bank loan compared to only 20% of other documented households), and loans securitized by formal land titles tend to be larger

on average.

Because our loan amounts are from all different years, with the majority of loans granted in 1998-2002 (only 9% of the formal loans are from the earlier 1990's), we normalized the loan amounts by converting to CPI-adjusted U.S. dollars. Table 4 breaks down the formal loan amounts by the type of security offered. First-time borrowers use the same types of securities and offer them in roughly the same proportion as the general population of borrowers.

Table 5 here

Table 5 describes in more detail the characteristics of the subset of our sample that we are particularly interested in. Panel A gives the most important personal and village-level characteristics for all households that have had a formal loan. Panel B looks at the same characteristics for households that are first-time borrowers. There are no significant differences between first-time borrowers and borrowers that have had past loans in any of these observable characteristics.[20] Therefore, any difference between the bank's relationship with a first-time borrower and its relationship with a repeated borrower cannot be attributed to differences that are observable to the econometrician.

The BRI MASS survey was conducted by BRI loan officers who work in different geographical areas than those surveyed. After they conducted each survey, the enumerators were asked to privately judge the household's feasibility as a loan candidate. They reported whether they would grant the household a loan and what the maximum loan size would be and under what terms. These questions give us a measure of hypothetical supply. That is, without considering the effects of title on credit demand, we can evaluate whether a formal land title systematically affects the amount of credit for which a household is hypothetically eligible. That these questions were answered by actual BRI loan officers adds to their validity as a measure of credit supply. In general, the maximal feasible loan amounts were comparable to actual loan amounts,

but many households that have never had a formal loan were judged feasible. The average feasible loan amount was almost 8 million Rp. or roughly CPI-adjusted US $454.

The MASS survey also asked households how much credit they would hypothetically be interested in obtaining if they had not applied for a formal bank loan. Of the 901 households that were judged feasible, 565 had never had a formal bank loan. Over half of those with no previous bank loan claimed that they have never applied because they did not want to be in debt. They were then asked a series of hypothetical questions about their possible desired loan amounts and conditions. From these questions, we can construct a measure of hypothetical demand. However, this measure of demand is somewhat problematic because even after giving a hypothetical loan amount, over 80% still insisted that they had no intention of borrowing formally. Their number one reason, given by 44%, was that they were concerned about repayment. Those that were titled were equally likely to be worried about repayment as others.

4.2 PODES 2003

Panel B of Table 3 gives the summary statistics for village level covariates from the PODES data set that we include in our estimations. These statistics are calculated using only the villages in the BRI MASS sample. Overall they are roughly comparable to the full Indonesian census of village heads. We have slightly more villages with registered councils (66% in our sample as opposed to 58% overall). RicePaddy = 1 if rice paddies are the village's primary form of income. Due to the fact that our sample is split roughly equally between urban and rural, we have less villages with rice paddies as their main source of income than the Indonesian average of 63%. Forest = 1, if the village lives near a forested area.

5 Empirical Strategy and Results

We will first consider a straightforward test that looks at how possessing a formal land title affects credit access while controlling for other observables that might also influence credit access.

$$y_i = X_i\beta + \gamma \text{title}_i + \epsilon_i \tag{1}$$

where y_i, the outcome of interest, is either an indicator variable of whether household i has ever had a formal bank loan or the total amount of formal bank loans which have been extended to household i in the recent past. X_i consists of both household and village-level covariates. When the outcome of interest is an actual outcome, not a hypothetical one, we cluster standard errors at the subdistrict level because the BRI Unit lending area is roughly a subdistrict and the BRI framework permits considerable discretion to unit managers.[21]

We have to make a further assumption on the error structure when the outcome of interest is binary and we use a Probit model. Letting y^* be the latent variable, the unobserved value to the bank of giving a loan to household i, we have:

$$\begin{aligned} y^* &= X\beta + \gamma \text{title} + \epsilon \text{ where } \epsilon \sim N(0,1) \\ y &= I[y^* > 0] \\ P(y=1|X) &= \Phi(X\beta + \gamma \text{title}) \end{aligned} \tag{2}$$

If one equates credit access with the supply of credit, then equation 1 assumes for each household the supply constraint is binding. Alternatively, access may also refer to supply or demand of credit in which case no further assumptions are necessary. However, policy analysis is problematic since we do not separately determine the effect of title on supply and demand. To this end, in subsection 5.4, we explore hypothetical

supply which we observe for all households surveyed, hypothetical demand which we observe for all households that do not have a loan, and the demanded amounts of loans for rejected loan applicants.

Next, we estimate OLS of loan amounts to compare the differences between offering a land title as security and simply possessing a land title. This step sets up the empirical test of interest to see if there is an information effect of land title. We check whether these differences vary for first time borrowers as our main empirical test of the signaling hypothesis mentioned in the introduction. We then look at whether or not the land title improves contracting under private information through ex-ante or ex-post information flows. The results of subsection 5.4 will also inform whether or not demand is driving the difference between first-time borrowers and repeat borrowers. We deal with unobservable demand directly since it is the most likely candidate to confound the results. Lastly, we address the endogeneity of land title more generally in section 6.

5.1 Formal Credit Usage

After controlling for household and village-level characteristics, we find a positive correlation between having had a formal bank loan and possessing a land title (see column (3) in table 6 for our full probit specification). Since estimation of the probit model may be sensitive to the specification, table 6 also shows specifications including just income and assets (column 1), income, assets and household characteristics (column 2), and income, assets, household and village characteristics (column 3) which we will refer to as the full specification.[22] Using the probit model we find that, at the median, becoming titled increases your probability of having a formal loan by 4.8% (with a robust standard error of 3.6%) to 7.3% (with robust standard error of 4.3%).

Since other land documents are also used as collateral, the fact that having a land

title has an effect on the probability of a household having formal credit suggests to us that either land titles could be functioning as a signal of credit-worthiness or they are a better source of collateral from either the borrower's or bank's perspective. However, conditional on having a formal loan, titled households and other documented households are equally likely to offer some type of land document as collateral (about 40% of the time).[23] Since all documented households are equally likely to offer some land document, we infer the relative benefit of offering a land document to other forms of security is similar. That is to say, if the only effect of a formal land title is as a better source of collateral, we would expect titled households to use the title comparatively more often.

In order to better understand these results, we incorporate local banking practices into the analysis and derive a direct test of the signaling hypothesis. First, we allow banks to treat first-time borrowers differently than repeat borrowers (discussed in next subsection). Second, we explore more sophisticated bankers who condition the signal of possessing a formal land title on the likelihood of possessing one in a particular village (discussed in section 6).

5.2 Loan Size - Having title vs offering it?

The key question and the main contribution of the paper is to evaluate whether having a formal title influences the size of observed formal loans, and, if so, to determine whether this is purely an effect of formal land title being better collateral. The results presented in table 7 compare least squares estimates under various specifications. Using the full set of controls, the specifications include dummy variables that indicate possession of a land title (column (1)), offering a land title as collateral (column (2)), and both possessing a land title and offering land title as collateral (column (3)). Each column contains the full specification including clustering at the sub-district level. We find that both having a land title and offering land title as security increase

the loan size on average, by 22% and 30% respectively. However, when both are included in the full regression, only offering land title as security is significant. This evidence is consistent with the hypothesis that collateral does matter for the credit market in Indonesia and formal land titles serve this function well.

We repeat the specification in column (3) for both repeat and first-time borrowers separately (column (4)). Column (4) is the crucial test for the information story. As discussed earlier, if formal land title functions as a signal it would be especially important for first-time borrowers. Only possessing a formal land title not offering the title as security obtains significance above the 95% level for first-time borrowers. Since the information problem is most apparent for first-time borrowers, our interpretation is that the bank is using the formal land title as a signal of important unobservables.

5.3 Ex-ante or Ex-post Informational Constraints?

As we discussed in the introduction, if formal land titles signal unobservable characteristics to the bank, this improves ex-ante information flows. Our empirical evidence is consistent with a formal land title signaling important information to the bank. However, this evidence is also consistent with an alternative ex-post information story that views formal land titles as supporting reputation-based contracting by making information about default more public.[24] Either mechanism predicts that first-time borrowers would experience a weaker effect of using land title as security than repeat borrowers would (first-time borrowers have less reputational capital).[25] In this section, we investigate which information story is better supported by the data.

We start by looking at the effect of offering different types of security. We note that, among formal banks, almost all loans have some security.[26] Recall that table 4 shows that the average loan size of those securitized by formal land titles is significantly larger than those securitized by other land documents.

If households have multiple kinds of security that they can offer, then we can test

whether formal land titles improve reputation-based contracting. We consider the subpopulation that has both a salary (since salary is often used to secure a loan) and land documents. The increased efficiency of reputation-based contracting should carry over to the characteristics of the loan. To see this, we employ a simple test. First, we compare whether there is a statistical difference in the means of loan characteristics by security offered for households with formal land titles. Next, we repeat this exercise for those with other land documents. By comparing those households who have salaries and formal titles with those with salaries and other land documents, we can understand the relative value of the reputation-based contracting for formal titles, while holding constant the benefit of having a fixed-income.

Of the 182 loans by households who have both a formal title and a salary, 33 of these loans were securitized by something other than the land title or the salary. Of the remaining 149 loans, 119 (79.9%) were secured with a salary and 30 (20.1%) have a formal land title as collateral. Remarkably, for those households without a formal land title, we see almost identical propensities to securitize the loan with the salary. Among the 119 loans by households that did not have a formal land title but had a salary, 29 loans were secured by something other than informal land documents or salary. Of the remaining 90 loans, 73 (81.1%) offered their salary as security and 17 (18.9%) offered their informal land documents as collateral.

In table 8, we present loan and borrower characteristics according to the type of collateral offered for both all observations and for a restricted sample that excludes those observations with missing information as well as some outliers (loan observations that claim the total payback amount is more than ten times the size of the original loan).[27] There is missing information for loan amounts, whether the loan is current and/or the total amount paid by the borrower.

Table 8 here

For titled households, we see that those who offer salary appear to be "better"

borrowers than those who offer title as security. They receive larger formal loans, have more assets, have more years of schooling, are more likely to be judged feasible are more likely to be current and have lower ratios of total payback to loan size. However, only years of schooling is significantly greater at the 5% level (see table 8, differences in bold are significant at the 5% level or greater). When we use the restricted sample, the borrowers look more similar although years of schooling is still significantly greater for those households who offer salary as security. According to the idea that formal titles improve reputation-based contracting, these borrowers should be worse. This does not mean that there is no value to formal titles in terms of improving reputation-based contracting because securing the loan with salary also may improve reputational enforcement.

In order to control for the value of offering salary as security, we also look at households with informal land documents. Essentially, the same pattern emerges. Taken together, this evidence does not support the idea that titling improves reputation-based collateral because the improved public disclosure of default should lead to better borrowers for those that choose to offer land title as security.

The systematic difference in years of schooling between those that choose fixed income contracts and those that use land titles as security does have an ex-ante information interpretation. Different contracts have different costs for a household. If households choose different contracts depending on their insurance premium and transaction costs, then, following Spier [1992], one can easily show that in the presence of asymmetric information households may choose loan contracts to signal type (in terms of ability to repay). Here, ability to repay may be positively correlated with years of schooling. Offering a land title represents a contingent contract in the Spier model. If the household is unable to repay, the bank will have to attempt to collect the security. Offering salary would correspond to a non-contingent contract. A risk-averse household can signal a superior ability to repay the loan by choosing

the non-contingent contract since this contract provides less insurance, making it particularly costly for those with an inferior ability to repay. Using this classification among households that both have outside employment and formal land title, we see separation among the types in the direction that the model would predict. Moreover, this holds no matter if we look at titled or other land documents, suggesting that the collateral value of formal land title is not too different than the collateral value of other land documents.

The alternative explanation is that the household chooses either salary or land documents according to which one is a superior form of collateral. To explore this hypothesis, we rank a household's salary and land assets by which decile each belongs. Then, we construct an indicator that tracks whenever salary is in the comparatively higher decile. We use this variable to predict choosing to offer salary as collateral. Table 9 shows the results. We see that this variable is strongly associated with offering salary as collateral.

This result could be driven by the fact that salaries and land values may be correlated. When we get the relative ranking by using predicted values from regressing both salary and land values on our controls and subdistrict dummies, the relationship between superior collateral and actual choice of collateral disappears. This is evidence that the optimal choice of collateral appears to involve more than just which collateral is worth more. It is then plausible that households take into consideration the signaling model above. Moreover, this is further evidence against the hypothesis that formal land titles are superior collateral. If formal land titles are more likely to be offered as collateral holding their relative value fixed, we might expect a negative relationship between the indicator and choice of collateral.

5.4 Assessing the Role of Credit Demand

Economists contend that possessing stronger protection of property rights increases investment incentives. The effects described in the previous subsections 5.1 and 5.2 could be at least partially due to households with land title demanding more financing, rather than shifts in credit supply due to households revealing unobservables. We attempt to address this issue in this subsection, although given our data limitations we can not rule out this possibility.

In order to give priority to actual outcomes data, we initially explore the relationship between land titles and credit demand without reference to hypothetical demand or supply. First, we look at the relationship between land titling and loan use. Here, the idea is that land titling should only affect particular investments, i.e. household or land specific investments (depending on what land is titled). We then discuss the rejected loan applicants and the reconstruction of credit demand.

Relaxing the assumption that households are supply constrained, we observe the following for all loan applicants:

$$y_i = \min(demand_i, supply_i) \tag{3}$$

For those who applied but were rejected, instead of observing y_i, we know the amount demanded directly, and, if a loan had been agreed to, the minimum in the above equation would likely be the amount supplied. Moreover, for those rejected, we know that the effect of title on the amount demanded is purely the demand effect since no supply effect can be present. Therefore we can construct the following and estimate the demand response for rejected households:

$$demand_i = X_i^{HH}\alpha + \gamma_D \text{title}_i + u_i \tag{4}$$

We can then try to understand if the demand effect could explain our results.[28]

Table 10 here

The problem is that u and ϵ (from equation 1) may be correlated. Table 10 shows the distribution of the control variables for rejected applicants. Rejected households tend to have less income, assets and education. Therefore, we employ a selection equation (not shown) yielding a probability of rejection. We use distance to the nearest bank to influence the probability of rejection since alternative enforcement methods become more difficult the farther away a household is. Distance should not affect loan amounts, given that the closest bank is likely within the subdistrict.

Another problem is possible unobservable confounding supply variables. To correct for this, we include as a regressor a variable that tracks the mismatch between supply and demand. Specifically, we condition predicted demand on the feasible amount that a loan officer would lend a household as described in 5.

$$demand_i = X_i^{HH}\alpha + \gamma_D \text{title}_i + \psi resid_i^{feasible} + u_i \tag{5}$$

where $resid^{feasible}$ comes from $supply_i^{feasible} = X_i^{HH}\beta + \gamma_F \text{title}_i + \xi demand_i + resid_i^{feasible}$.

We reestimate 5 to account for selection using distance as an excludable variable that influences the probability of rejection (again, not shown).

Finally, given that we have the feasible loan amounts judged by loan officers as well as hypothetical demand of those who did not apply for a loan, we can look at the relationship between formal land titles and hypothetical supply and demand.

5.4.1 Demand and Loan Use

The standard argument is that land titling increases incentives to invest (and, hence, credit demand) either by improving relative returns to the titled asset-specific investments or, in general, by decreasing the interest rate. In the former case, increased credit demand due to titling should lead to greater borrowing for home improvement (for titles on household plots) or consumptions loans (for investments in human cap-

ital of the household) or for production loans (for titles on farming plots). When we disaggregate the formal loans into loan type, we see that being titled has the strongest effect for working capital loan amounts, not for home improvement or consumption loans. We see that all these loans experience a weaker effect than loans for working capital, capital for a pre-existing venture. While this is consistent with the desire of households to increase investment in current ventures, possibly tied to the household, the majority of the effect on working capital loan amounts is driven by using land title as collateral. Thus, demand is unlikely driving the results because we find little evidence for land titles simply increasing credit demand.

Table 11 here

We should stress that the results on working capital loans show that land title as collateral does indeed matter. Given sample size limitations, we can not accurately verify whether the signaling effect of title is driven by repeat borrowers more likely to have ventures that require working capital. However, taken in conjunction with the evidence that is consistent with ex-ante information from section 5.3, these results on working capital loans should suggest that land title may not matter as a demand shifter.

5.4.2 Demand and Rejected Loan Applicants

Although demand (and supply) is, in general, not directly observed, we argue that we do observe the actual credit demand of the rejected loan applicants in the MASS survey (76 people reported recently having a rejected loan application). First, we point out that being titled shows no relationship to applying for a loan nor the requested loan amounts of those rejected. If having a land title influenced demand, one might expect for possessing a land title to be positively correlated with applying for a loan and with the requested loan amounts of those borrowers who were obviously supply constrained. We do not observe either relationship.

Second, if our selection equation satisfies the necessary exclusion restriction, we can derive credit demand for all borrowers from the subgroup of rejected applicants and then reestimate the effect of title on the demand of credit to see if it can explain the previous set of results. Table 12 shows that after controlling for selection and the mismatch between supply and demand (column (3)), the effect of possessing a title is not statistically significant and the estimated effect is smaller than the estimated effect of title on actual loan amounts. These results suggest that demand is not fully driving the observed relationship between land title and loan amounts.

Table 12 here

5.4.3 Hypothetical Demand and Supply

In column (1) of table 13, we run the same specification as we used for the probability of having a formal bank loan for whether a household was judged feasible. Having a title does have a positive effect on the probability of being judged feasible though not statistically significant at the 10% level. The effect on feasible loan amount is not precisely measured (possibly due to the fact that the variation in maximum feasible loan amount is smaller than the variance of actual loan amounts given).

The fact that the loan officers interview the household extensively at home suggests that the loan officers may have better observability than if the potential borrower comes to the bank. If the household doesn't have a land title then the loan officer can assess other potential sources of security more easily. Therefore, this informational advantage may make the estimates of the effect of land title weaker. This informational advantage also makes this regression suspect (since a disadvantage applies to the econometrician). We find that after instrumenting, shown in column (2), possession of a title increases the probability at the median of being judged feasible by 47%.Thus, we interpret this to mean that either loan officers recognize the value of land title as potential collateral or land title does not perfectly measure the relevant

unobservable borrower characteristics. in the latter case, the loan officer can observe and, subsequently, update the signal of possessing a formal land title.[29] .

Table 13 here

In columns (3) and (4) of table 13 we switch the dependent variable to be hypothetical demand. Again, we keep the same basic specification as in equation 1 but add as a control whether or not the household was judged feasible and an interaction term with this dummy variable and whether or not the household possesses a land title. The results show that possessing a title has no effect on the desired loan amount, neither for those judged feasible nor for those judged infeasible. If demand is driving the results, we would expect land title to have an effect on desired loan amounts. If households can correctly predict that they are feasible, then we would also expect an additional affect for those who were judged feasible. Yet, we see none of these effects.

6 Instrumenting for Title

In this section, we address the endogeneity of land title. Given the nature of our data set, this paper is necessarily more a description of economic interactions. The issue of causality likely remains but instrumental variables can still improve the empirical evidence for or against the signaling hypothesis. An additional word of caution is in order. While we want to address the endogeneity of the land title variable, we also want to capture the signaling effect. The difficulty is that land title is correlated with unobservable variables that affect credit access and some of these, in particular, personal characteristics of the borrower desirable to the bank, we would like to include in the effect of land title. However, by assumption, the typical instrumental variable estimates asymptotically reduce the "bias" resulting from the correlation between land title and any unobservable correlated with credit access to zero, removing any

signaling effect. Thus, our instrumental variables technique should allow for the effect of land title to capture its signaling role.

If a formal land title signals unobservable information about the borrower, we want to allow the coefficient on the titled variable to capture this effect, regardless of whether or not land titles have a collateral effect. This rules out using an individual level variable since an exogenous predictor of individual possession of a formal land title would put too little weight on those who should not have a title but do and those who should have a title but don't, the groups who stand most to benefit from the signaling mechanism. Moreover, using an exogenous predictor of average village possession of a formal land title can better isolate the signaling value and correct for any confounding village level variables, not captured by clustering at the subdistrict level. Importantly, we rely on the fact that we have ruled out idiosyncratic demand in the previous section so the main objection to exogeneity is not likely at the individual level.[30]

To estimate the probability of a household having had a formal bank loan, we use a bivariate probit model where we estimate the probability of a household being titled (including our instruments) and allow the error terms in the two equations to be correlated. For loan sizes, we will instrument for having a formal land title and estimate using 2SLS. We argue that whether the village is located near forested areas and whether or not rice paddies are the village's primary source of income are valid instruments. Our strategy is then to use use village level instruments for the individual level variable land title. Then, run a separate regression of our outcome variable on the residuals of the first stage.

Whether or not the village is located near forest land should be negatively correlated with the extent of titling. Forest land is not under the jurisdiction of the BPN and hence land titles can not be issued for any plots on cleared forest that have not yet been unmarked as forest. 28% of households are titled in villages near

forested areas while 53% are titled in non-forested areas. Whether rice paddies are a village's primary source of income also provides a measure of how likely a household is to be titled. Rice paddies demand irrigation which is a labor-intensive activity that can establish ownership in the eyes of the community. Considering the high cost of obtaining land titles, we would expect to see fewer titles in areas where there are other means of establishing ownership. Indeed, in rural villages, where rice paddy production is the primary source of income, the probability of being titled is 18%, contrasted with 48%, in villages where it is not. In our sample, only 3 urban villages have rice paddies as their primary source of income and these villages have 59% titled (as opposed to 66% for urban villages without rice paddy production as the primary source), suggesting that the instrument may work better in rural areas. Not surprisingly, given that you need to have a land title to be able to offer it as security, in villages with rice paddies as the primary income source, 19% of formal bank loans are securitized by land titles compared to 30% in the other villages.

Since the extent of rice paddy cultivation depends on geographical and climate characteristics, we argue that the rice paddy variable should not be correlated with the error term after we include dummy variables indicating in which subdistrict a household is located. Both of our instruments might be subject to the criticism that it is a measure of the overall development level of the village. To attempt to mitigate this problem, we always include the mean income level in the village and the village's population density in all specifications where we are instrumenting.[31]

Using hypothetical demand, we can ask how our instruments are related to credit demand. Looking at averages, hypothetical demand is significantly different for areas with and without rice paddy production as the primary source of income. When we regress hypothetical demand on rice paddy and our controls, we see a negative and significant effect on credit demand. However, when we restrict the sample to only those households (569 of 908) who have been judged feasible, the effect is no

longer significant at the 10% level.[32] Hence, those with lower demand would likely have been rejected anyway. Hypothetical demand and proximity to a forest are not statistically significantly related.[33]

After running the first stage of 2SLS, we are able to check if our instruments do explain possession of a formal land title after including other controls. For the binary outcome of whether or not a household has had a formal bank loan, our instruments always pass the test of joint significance. When restricted to only rural subdistricts, they pass the typical overidentification tests, suggesting that they do not influence credit access except via land title and we check for robustness as outlined above. However, there is evidence that the instruments do not pass the overidentification tests when run on the whole sample.

After instrumenting for the possession of title (in column (2) of table 14) with the village-level instruments, we see that the effect of land title for rural subdistricts is not statistically significant. Column (3) shows the regression of having had a formal bank loan on the residuals from the linear regression of being titled on the village-level instruments and the controls. We see that the coefficient on the variable of interest is positive and significant at the 10% level and of a greater magnitude, increasing the probability of having had a formal bank loan by 9.2 points at the mean, than in the naive results. We interpret this effect as a more direct measure of the signaling effect since the residuals more closely track the meaning of being a signal for possession of a formal land title.

Table 14 here

Our instruments do not work as well for formal loan amounts. Recall that less than a third of the sample report formal loan amounts. The instruments pass the joint significance and the weak identification tests but fail to pass the overidentification tests. The situation is worse when we restrict the sample to only rural subdistricts (reducing the sample to 191 loan observations). In both samples, the coefficient on

being titled is positive but not statistically significant. Constructing the residuals as before, we see the coefficient on the effect of interest is positive and statistically significant at the 10% level for the whole sample and positive but not statistically significant for the reduced sample. It is hard to interpret these results since it appears that at least one of the instruments is not valid. When we drop rice paddy production (in our minds, the most problematic of the two), the instrumented land title variable is negative and still not significant, as in the binary outcome, and the coefficient on the residuals is positive and significant at the 1% level, shown in columns 5 and 6 of table 14.[34] When we restrict the sample to rural subdistricts, a similar picture emerges, except that the coefficient on the residuals fails to obtain significance at the 10% level (p-value of .129).

Alternatively, our instruments may introduce some bias. If being titled increases credit demand, and areas with greater rice paddy production demand less formal credit, then the IV estimates, including the residuals, will exacerbate the bias. The same story could hold for being near a forest. However, working in our favor is the fact that this criticism does not hold for the results on hypothetical supply, and these results indicate that the naive estimates are biased downwards. In addition, there is little evidence that demand is the driving force behind the relationship between land titling and credit, possibly suggesting that even if some bias does exist, it is likely to be small.

7 Conclusion

The approach of this paper has been to tell a story about how land titles might affect an underdeveloped credit market. Our findings join two familiar stories: the Hernando de Soto story of collateral and the Muhammad Yunus story of microfinance. In the first story, formal land titles are seen as unleashing the productivity of

otherwise "dead" assets. It is a formal land title's ability to serve as collateral that transforms fixed assets into liquid ones and back again with ease. In the second story, sophisticated bankers, without the aid of formal land titles, have already unleashed these assets and more by making use of human and social capital.

In our paper, we show that formal land titles are important determinants of credit access even when bankers regularly use microfinance methods. Having a formal land title increases a household's probability of having had a formal loan and also increases the average loan size of those that do borrow. In general, the loan size is influenced by whether the title is offered as collateral. However, when looking at first-time borrowers, we see that the crucial component is merely possessing a land title and actually offering the title as collateral does not significantly increase the average loan size. We argue that these results show that both collateral and signaling play important roles in determining credit access.

This description of the credit market and land titling in Indonesia draws attention to the interaction between contracting and property institutions. In the story that we propose, bankers combine and use a variety of personal and impersonal methods in response to the lack of information and difficulty in enforcement in case of default. Banking practices and the ability of households to contract should be an important part of the economic analysis of the effect of land titles on access to credit. By incorporating local banking practices, our results help explain some more recent findings, such as Galiani and Schargrodsky [2010], that newly entitled households, as a result of a titling program, do not have better access to credit. Our evidence suggests that sporadically obtained formal land titles have an important informational component that would be missing in systematically distributed land titles.[35] Thus, these households may find it difficult to access new credit when their land titles have little to no informative content about household characteristics. Consequently, the approach of assigning formal land titles in order to open up credit markets to new borrowers may

not be as successful as policymakers might hope.

Of course, systematic titling programs may lead to changes in banking practices. BRI and other banks in Indonesia are in favor of titling programs, suggesting that there are important dynamic effects that should be considered. Additionally, personal characteristics might be signaled through alternative means, besides land titles, following a large-scale titling program. However, the emphasis on the importance of improving ex-ante information flows still remains.

Notes

1. Even if the land title has been inherited, possession of it improves incentives to learn how to interact in the formal sector.

2. There are of course reasons why land title may provide a good form of collateral. See de Soto [2000]. However, these reasons typically rely on assumptions of a well-functioning legal infrastructure as well as fairly active land markets or assume that these will develop along with widespread use of land titles. In this paper, we will by necessity focus on partial or local short term effects.

3. The other 60% is made up of mainly salary guarantees but also other land informal documents or vehicle ownership or even no security at all. For first-time borrowers with title, 42% offer it as collateral.

4. See Morduch [1999] for a thorough review of these methods.

5. As an example of an ex-ante informational effect, Bester [1985] shows that banks can screen for types with low repayment costs by offering loan contracts with higher collateral requirements at a lower interest rate.

6. Kochar [1997] argues that the existence of informal credit markets may cause the empirical data to misrepresent the extent of credit rationing; institutional credit may be accessed less because individuals' demand for credit may be satisfied by the

informal sector.

7. On a relative scale, institutions that focus on micro-lending were affected less drastically by the 1997 crisis. In fact BRI Units were profitable throughout the crisis and it is reported that the Units subsidized the Branch banks during and after the crisis (Johnston et al. [2001]).

8. BRI has been extensively studied in the microfinance literature. For more information on the history and practices of BRI, see Maurer [1999].

9. To illustrate this point, consider the following method that is often used: the loan applicant fills out a description of the condition of his/her assets, then the bank sends a bank employee to view the assets in order to make a comparison. What the bank cares about is not just the value of assets but also whether the description of the assets was honest.

10. This number seems large, leading us to believe that managers have included threats to foreclose. Formal foreclosure is a rare event in the small-scale loan market in Indonesia. However, just because foreclosure is not observed is not an argument that collateral is not at work. A simple game-theoretic framework yields a Nash equilibrium where no one defaults yet the possibility of foreclosure is real. In the Indonesian context, a threat to foreclose is unlikely to be supported by beliefs that losing ones land in case of default is a real possibility. The Indonesian legal system is a complex mix of Dutch, Muslim and customary (adat) law. Although the written law should apply across provinces, the interaction between the three traditions of law provides ample discretion for judges. With respect to foreclosure law, borrowers rather than lenders are generally favored. Foreclosure is a socially sensitive issue, and the legal practice of foreclosure in Indonesia is unpredictable and lengthy.

11. This is similar to what our empirical results show - having a formal land title makes a household more likely to receive a formal loan. Conditional on receiving a loan, having a land title has less influence on the size of the loan.

12. Land which has been designated as forest (roughly 60%) is handled by a separate Ministry of Forest.

13. World Bank Project Appraisal Document, Report No: 28178-IND (2004).

14. The current system of titling should be understood in the context of how land rights have been established previously, especially in areas where adat law is still respected. Evidence of ownership can come in a variety of forms. The most formal of these informal rights to land is a land deed or *akte*.It is a document that represents the purchase of a piece of land and is officially stamped and notarized. A less formal but perhaps locally stronger right is the *girik* or *petok* which is a use claim on land that comes from the customary law. Documents known as Letter C or D are guaranteed by the village leader and can be inherited. In the MASS survey land parcels with formal land titles are slightly overrepresented (45% of land parcels have formal land titles, 12% have *akte*, 21% have either a *girik, petok* or letter C or D, and finally 10% have only tax receipts to demonstrate ownership of that parcel). Very few households in our sample, only 4% of landowners, have no documents at all.

15. Potensi Desa Statistik or Village Potential Statistics which were actually collected in 2002.

16. Except for West Java where all households sampled were rural.

17. Alternatively we could instead use the the fraction of value of the household's total land assets that is titled. In practice this distinction is almost irrelevant in our data set because even though 352 of our households do report having more than one land or garden plot, all but 57 of these households have either all their plots titled or all untitled.

18. Of the remaining loans 14% are microbank loans and 12% are from other informal sources. The average non-formal loan amount is CPI-adjusted US$203 .

19. Among informal loans, 33% are used for production and 67% for consumption.

20. All continuous variables were tested with both the conventional t-test (allowing for the variances between the samples to differ) and with the non-parametric Wilcoxon rank sum test. A test of proportionality was used with all binary variables. There were no significant differences at the 10% level.

21. When the outcome of interest is hypothetical we choose to employ subdistrict fixed effects since the the unit manager does not have control over these outcomes.

22. Including geographical dummies at the subdistrict level instead of clustering increases the precision with which we are able to measure the effect of title, improving statistical significance. The linear probability model gives similar results.

23. Over half of all formal loans do not use any type of land document. Instead the most commonly cited security is an advance against future salaries such as fixed income or use of a guarantor.

24. By reputation-based contracting, we mean contracts that make use of relational enforcement mechanisms. For example, if a borrower defaults, on top of (or instead of) losing the collateral, one loses the opportunity to borrow from the lender and/or other lenders, relational enforcement is present.

25. Alternatively, repeat borrowers may need less public disclosure to enforce their contracts (repeated interaction). In this case, there would be a weaker effect of land title as security for repeat borrowers, which is inconsistent with our results.

26. Only 5% of formal loans are unsecuritized compared to 56% of microfinance loans and 64% of informal loans.

27. The drop in observations is primarily due to missing information although the few outliers do have considerable impact.

28. We may also use predicted demand to partially identify supply using equation 3 and 4.

29. Our instruments are discussed in section 6. The main objection to our instruments that they should be correlated with credit demand does not apply to hy-

pothetical supply. The instruments did not perform well for feasible loan amounts (perhaps because feasible loan amounts are maximal loan size and the variation is small).

30. Reverse causality is also another possible explanation; borrowing households may use the loan to fund obtaining a land title. Our results do not significantly change when we restrict attention to recent loans, and since the titling process is lengthy and there are numerous benefits to having a land title, having obtained a loan is not likely the dominant force behind deciding whether or not to obtain a land title.

31. In our full specification we also include the mean number of years that land assets are held in the village (to control for the degree of activity in the land markets).

32. Results not shown.

33. We next check for the weakness of our instruments since their presence may lead to non-normal sample distributions and potentially large bias in the IV estimates. Bound et al. [1995] show that this bias is not a small sample problem and that it is possible for OLS estimates to be more reliable in large enough samples. Several approaches have been proposed to assess the weakness of the instruments. The approach we take is the following. For linear models, we first make use of the forward and reverse-2SLS test for weak instruments outlined in Hausman and Hahn [2002]. If the forward and reverse estimates of the coefficient on the endogenous variable, land title, are relatively close we can have confidence in the more commonly used F-test on the joint significance of our instruments. Accordingly, if the first stage F statistic from the first stage of 2SLS is too small (below 10 is a commonly used cut off, as suggested by Stock et al. [2002]), we do inference following Moreira and Poi [2001] and use the Anderson-Rubin statistic to construct the proper critical value for testing the coefficient on titled since this test is robust to weak instruments. If the first stage F-statistic is large enough, we do the standard hypothesis testing of the

coefficient on titled and use the standard over-identification tests of exogeneity. In practice we report both in the following sections. Since there is much less consensus concerning the weak instrument problem in the non-linear case, we make use of the log-likelihood ratio to determine whether the unrestricted bivariate probit model is more appropriate than restricting the model to have zero correlation between the two error terms. Our interpretation of both the sign and magnitude of the correlation will reflect the observation that weak instruments may artificially inflate the correlation between the two error terms.

34. We favor the forested variable because it is less likely to indicate differences in economic opportunities than the rice paddy variable. Forested area is an official designation and does not mean that actual forests are nearby. Since the official designation prevents any land titles to be issued. Although there are regulations on use of forested land, it is not clear how enforceable they are. Thus, it is likely that many economic opportunities exist on officially designated forested land.

35. On a more positive note, this argument could suggest that even rights of exclusion to land that are not transferable might positively influence credit access if the process to receive these documents is not automatic. These types of rights are often granted instead of fully transferable land rights when governments are worried that the formalization of property rights will make it more likely for small-scale farmers to lose their land. This practice has been used in India.

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Table 1: Previous Literature

Study	**Region**	**Pos. Signif. Effect?**	**Program**
Feder et al. [1988]	Rural Thailand	Yes, especially in areas with well-developed credit markets.	Sporadic
Carter and Olinto [2003]	Paraguay	No, except for large landowners.	Sporadic
Pender and Kerr [1999]	Rural India	No	Sporadic
Lopez [1996]	Honduras	Yes	Systematic
Field and Torero [2004]	Urban Peru	Yes, for public bank loans. No, for private loans (though it did lower interest rates).	Systematic
Boucher et al. [2005]	Rural Peru	Yes, a title reduces the probability of being credit constrained.	Both
Migot-Adholla and Place [1998]	Ghana, Rwanda and Kenya	No	Both
Broegaard et al. [2002]	Nicaragua	No	Both
Galiani and Schargrodsky [2010]	Argentina	Yes, but modest	Systematic
Do and Iyer [2008]	Vietnam	No	Systematic
Foltz [2004]	Tunisia	Yes	
Barham et al. [2005]	Honduras and Nicaragua	No	Systematic
Petracco and Pender [March 2009]	Uganda	No for title but Yes for freehold tenure	Sporadic

Table 2: Percentage Titled by Province

Province	**%Titled**	**N**
West Java	.204	230
East Java	.510	204
West Kalimantan	.634	232
East Kalimantan	.391	220
North Sulawesi	.500	220
Papua	.603	204
Total	.471	1310

Source: BRI MASS 2002

Table 3: Summary Statistics

Panel A: BRI MASS 2002

Variable	**Mean**	**Std**	**Min**	**Max**	**N**
FormalBank	0.29	0.45	0	1	1310
Total Assets (approx.US$)	5920	10800	2.5	157000	1310
Income per cap/poverty line	3.31	4.74	0.01	69.8	1308
Has salary	0.41	0.49	0	1	1310
Household head's education	7.99	3.94	0	16	1310
Household size	4.44	1.73	1	13	1310
HH's age	46.3	11.9	20	87	1309
Years in village	27.8	16.1	1	99	1303
Distance to bank (km)	6.78	9.91	0	56	1297
Mean income/povline in village	3.33	2.14	0.41	13.3	74
Mean years land is held in village	17.2	6.13	3	40	74
Rural	0.58	0.50	0	1	74

Panel B: PODES 2003

Population density (adultpop/hectare)	29.7	53.1	0.06	270	74
Population Census	0.68	0.49	0	1	74
Forest	.24	.43	0	1	72
RicePaddy	0.39	0.49	0	1	74
Village has bank	0.23	0.42	0	1	74

Table 4: Average Formal Loan Amounts in U.S. Dollars (CPI-adjusted) by Security

Panel A: All formal loan amounts (U.S.$)

Security Used	**Loan Amount**	**Std**	**Min**	**Max**	**N**	**%**
Land title	650	767	3	6606	100	25%
Fixed Income	527	462	6	3979	194	48%
Other land documents	347	238	12	1239	67	17%
Other security	560	821	12	2738	19	5%
No security	257	259	31	1010	22	5%
Total	515	551	3	6606	402	

Panel B: First time loan amounts (U.S.$)

Security Used	**Loan Amount**	**Std**	**Min**	**Max**	**N**	**%**
Land title	742	600	109	2610	23	23%
Fixed Income	486	408	113	2586	44	44%
Other land documents	286	163	51	625	13	13%
Other security	253	188	42	512	7	7%
No security	230	221	31	721	12	12%
Total	472	444	31	2610	99	

Source: BRI MASS 2002

Table 5: Summary Statistics

Panel A: All Households that have a formal bank loan

Variable	**Mean**	**Std**	**Min**	**Max**	**N**
Has Land Title	0.66	0.47	0	1	376
Total Assets (approx.US)	8982	12871	2.50	138000	376
Family Income per cap/PovLine	4.67	4.67	0.14	34.8	381
Has salary	0.57	0.50	0	1	381
Household head's education	9.56	3.82	0	16	381
Distance to Bank (km)	4.5	8.32	0	55	377
Mean Income/PovLine	4.37	2.68	1.11	13.3	381
Population Density	37.5	52.6	0.11	217	381
RicePaddy	0.27	0.45	0	1	381
Forest .23	.42	0	1	381	
Population Census	0.73	0.45	0	1	381
Village has Bank	0.28	0.45	0	1	381

Panel B: Households that are first time borrowers

Has Land Title	0.66	0.48	0	1	102
Total Assets (approx.US)	8887	16655	45	138000	102
Family Income per cap/PovLine	4.44	4.51	0.14	34.8	104
Has salary	0.54	0.50	0	1	104
Household head's education	9.76	3.79	3	16	104
Distance to Bank (km)	5.54	9.80	0.05	45	103
Mean Income/PovLine	4.75	3.17	1.11	13.3	104
Population Density	31.1	46.6	0.13	217	104
RicePaddy	0.22	0.42	0	1	104
Forest .25	.44	0	1	104	
Population Census	0.70	0.46	0	1	104
Village has Bank	0.33	0.47	0	1	104

Table 6: Marginal Effects Probit

	(1)	(2)	(3)
Titled	.073*	.057	.048
	(.043)	(.039)	(.036)
Log total fixed asset value	.077***	.068***	.066***
	(.020)	(.022)	(.021)
Income per cap/poverty line	.003	.003	-.002
	(.003)	(.003)	(.003)
Has salary	.135***	.110***	.120***
	(.042)	(.041)	(.041)
Log distance to bank	-.107**	-.096*	.018
	(.052)	(.052)	(.037)
Village has bank	.027	.022	.034
	(.038)	(.038)	(.045)
years of schooling < 9		.023	.136*
		(.037)	(.072)
9 < years of schooling < 12		.037	.0004
		(.046)	(.007)
years of schooling > 12		.132*	.002**
		(.074)	(.001)
Household size		.0000478	-.001
		(.007)	(.0008)
HH head age		.002**	.028***
		(.001)	(.008)
Years in village		-.001	-.0006
		(.0008)	(.0004)
Mean income/povline in village			-.001
			(.002)
Population density			-.089*
			(.053)
Mean yrs land is held in village			.034
			(.041)
Obs.	1295	1287	1287
e(ll)	-668.397	-656.897	-637.478
e(r2-p)	.139	.15	.175

This table presents a probit model with an indicator of having had a formal bank loan as the outcome variable. Standard errors are clustered at the subdistrict level. Missing observations are due to distance to nearest bank variable. Marginal effects are reported at the median. Column 1 uses only income and asset variables as controls. Column 2 adds household characteristics and column 3 adds village level characteristics as controls.

Table 7: Loan Size - OLS

	(1)	(2)	(3)	(4)
Titled	.243**		.148	.034
	(.123)		(.118)	(.126)
Land title security		.295*	.219	.208
		(.178)	(.180)	(.211)
First-time borrower				-.300*
				(.170)
Titled first-timer				.428**
				(.180)
First-timer w/ titled collateral				.034
				(.307)
Log total fixed asset value	.072**	.080**	.072**	.069*
	(.036)	(.034)	(.035)	(.035)
Income per cap/poverty line	.050***	.048***	.048***	.047***
	(.009)	(.009)	(.009)	(.009)
Has salary	-.062	.018	-.006	.002
	(.138)	(.163)	(.158)	(.149)
Mean income/povline in village	.016	.019	.018	.019
	(.025)	(.028)	(.027)	(.024)
Population density	.0009	.001	.001	.001
	(.001)	(.001)	(.001)	(.001)
Mean yrs land is held in village	.005	.003	.004	.004
	(.011)	(.011)	(.011)	(.011)
years of schooling < 9	-.011	.017	-.002	.019
	(.195)	(.190)	(.194)	(.197)
9 < years of schooling < 12	.272*	.335**	.306**	.324**
	(.148)	(.135)	(.139)	(.131)
years of schooling > 12	.302	.400**	.359*	.378**
	(.197)	(.184)	(.184)	(.182)
Household size	.049	.046	.047	.045
	(.032)	(.033)	(.033)	(.034)
HH head age	-.007	-.004	-.006	-.006
	(.005)	(.005)	(.005)	(.005)
Years in village	-.006**	-.007**	-.007**	-.006*
	(.003)	(.003)	(.003)	(.003)
Obs.	365	362	362	362
e(r2-a)	.149	.151	.152	.154

This table presents an OLS model with log of formal bank loan amounts (in dollars) as the outcome variable. Standard errors are clustered at the subdistrict level. All columns use the same controls (with the exception of the variables of interest).

Table 8: Salaried Households and Collateral Choice

All Observations for Salaried Households with Formal Loans							
Households with land titles and salary							
Type	**Loan**	**Assets**	**Educ**	**Feasible**	**Current**	**Payback**	**N**
Offered Salary	6.04	17.89	**12.05**	.93	.98	1.28	119
Offered Land	5.66	17.63	**9.03**	.83	.93	189.12	30
Households with other land documents and salary							
Offered Salary	6.00	**16.73**	10.66	**.93**	.97	.68	73
Offered Land	5.92	**17.84**	9.24	**1**	.76	1.45	17

Note: Loan, Current, and Payback have fewer observations than N

Excluding Outliers and Observations with Missing Information							
Households with land titles and salary							
Type	**Loan**	**Assets**	**Educ**	**Feasible**	**Current**	**Payback**	**N**
Offered Salary	6.08	17.99	**12.55**	.94	.98	.65	82
Offered Land	5.88	17.83	**9.80**	.89	1	.63	20
Households with other land documents and salary							
Offered Salary	6.01	**16.72**	**10.57**	.96	.98	.68	45
Offered Land	5.94	**17.83**	**7.73**	1	.82	1.45	12

Bold indicates significantly different at 5% level

Source: BRI MASS 2002 data set

Table 9: Salaried Households and Collateral Choice: Robustness Check

Dependent Variable=Offered Salary as Collateral

	Households with land titles and salary			
Collateral Ranking	1	2	3	4
Salary Relatively Better	1.034***	.976***		
	[.247]	[.302]		
Predicted Salary Relatively Better			.360	.359
			[.235]	[.286]
Controls	No	Yes	No	Yes
Log-likelihood	-68.25	-55.80	-76.17	-60.31
N	132	132	132	132
	Households with other land documents and salary			
Salary Relatively Better	-.179	-.062		
	[.295]	[.339]		
Predicted Salary Relatively Better			-.529*	-.534
			[.294]	[.358]
Controls	No	Yes	No	Yes
Log-likelihood	-56.62	-46.31	-55.17	-45.19
N	83	83	83	83

Source: BRI MASS 2002 data set

Notes: The results are from a Probit model with an indicator for choosing to offer a fixed income as collateral as the outcome variable. The variable of interest is an indicator for whether or not the household's fixed income is relatively larger than the value of the household's land assets. Relative value refers to which, the salary or land assets, belong to the highest decile. Predicted relative value uses a full set of controls (except for total assets and income) and subdistrict fixed effects. The control variables are the full set of controls except for household's total assets and income.

Table 10: Households who applied for a loan

Status	**Desired Loan**	**Assets**	**Income**	**Educ**	**Titled**
Rejected	**5.38**	**17.22**	**3.20**	**7.63**	.58
Accepted	**5.96**	**17.76**	**4.87**	**9.71**	.64

Bold indicates significantly different at 5% level

Notes: Desired Loan is in logs of CPI-adjusted US$ with loan amounts for Accepted either as loan amount applied for or actual loan amount. Assets in logs of Rupiah. Income relative to the poverty line. Education is years of schooling.

Source: BRI MASS 2002 data set

Table 11: Loan Use

	(1)	(2)	(3)
Titled	.067		.122
	(.126)		(.125)
Land title security		-.198	-.254
		(.232)	(.229)
Working capital loan	-.302	-.415*	-.373
	(.226)	(.221)	(.230)
Titled w/ WC loan	.466*	.011	-.098
	(.254)	(.289)	(.304)
WC loan w/ titled collateral		.853***	.914***
		(.278)	(.272)
Log total fixed asset value	.076**	.076**	.070*
	(.037)	(.036)	(.036)
Income per cap/poverty line	.049***	.051***	.051***
	(.008)	(.009)	(.009)
Has salary	-.069	-.081	-.092
	(.182)	(.188)	(.185)
Mean income/povline in village	.016	.013	.012
	(.027)	(.027)	(.026)
Population density	.0009	.001	.001
	(.001)	(.001)	(.001)
Mean yrs land is held in village	.005	.002	.004
	(.011)	(.011)	(.011)
years of schooling < 9	-.052	-.028	-.033
	(.192)	(.182)	(.185)
9 < years of schooling < 12	.257*	.262*	.246*
	(.145)	(.136)	(.137)
years of schooling > 12	.307	.323*	.298*
	(.187)	(.177)	(.176)
Household size	.049	.062*	.064*
	(.031)	(.035)	(.035)
HH head age	-.007	-.008	-.009*
	(.005)	(.005)	(.005)
Years in village	-.006*	-.006*	-.005
	(.003)	(.003)	(.003)
Obs.	365	362	362
e(r2-a)	.157	.171	.171

This table presents an OLS model with log of formal bank loan amounts (in dollars) as the outcome variable. Standard errors are clustered at the subdistrict level. All columns use the same controls (with the exception of the variables of interest).

Table 12: Demand Derived from Rejected Applicants

	OLS	Heckman	Heckman
	(1)	(2)	(3)
Titled	-.268	-.197	-.202
	(.385)	(.571)	(.455)
Log total fixed asset value	.465**	.420	.555
	(.187)	(.800)	(.566)
Income per cap/poverty line	.125*	.151	.137
	(.070)	(.123)	(.094)
Has salary	-.367	-.356	-.125
	(.367)	(1.605)	(1.128)
Mean income/povline in village	-.175*	-.063	-.122
	(.094)	(.357)	(.254)
Population density	.003	.009	.009
	(.004)	(.010)	(.008)
Mean yrs land is held in village	-.075*	.072	.044
	(.045)	(.261)	(.188)
years of schooling < 9	1.083*	1.261	1.103
	(.599)	(1.887)	(1.410)
9 < years of schooling < 12	.556	.610	.447
	(.394)	(1.131)	(.804)
years of schooling > 12	2.073*		
	(1.080)		
Household size	.116	.023	.079
	(.099)	(.505)	(.364)
HH head age	-.010	-.091**	-.093***
	(.019)	(.041)	(.031)
Years in village	.007	.040	.039
	(.014)	(.034)	(.026)
resid-feasdolcpi			-.003
			(.004)
Obs.	68	383	383
e(r2-a)	.247		
e(lambda)		-2.606	-1.888

Column 1 of this table presents an OLS model with log of formal bank loan application amounts (in dollars) as the outcome variable for only those applicants who were rejected. Columns 2 and 3 use a Heckman selection model using distance to the nearest bank as the selection variable. Column 3 adds as a control the residuals from a regression of hypothetical supply loan amounts on all the controls (including titled). Robust standard errors are reported and all columns use the same controls.

Table 13: Hypothetical Supply and Demand

	Hypothetical Supply Dep. Var.=Feasible		Hypothetical Demand Dep. Var.=Desired Loan Amt		
	OLS	IV	OLS	OLS	Heckman
	(1)	(2)	(3)	(4)	(5)
Titled	.178	.544*	-.067	-.0006	-.113
	(.121)	(.279)	(.086)	(.140)	(.146)
Feasible				.523***	.070
				(.096)	(.120)
Titled and feasible				-.114	-.066
				(.149)	(.169)
Log total fixed asset value	.388***	.307***	.408***	.338***	.246***
	(.050)	(.063)	(.047)	(.045)	(.047)
Income per cap/poverty line	.098***	.101***	.041***	.038***	.049***
	(.022)	(.024)	(.009)	(.009)	(.010)
Has salary	-.470***	-.645***	-.215***	-.158**	-.598***
	(.107)	(.092)	(.080)	(.078)	(.098)
years of schooling < 9	.154	.027	.291***	.260***	.375***
	(.123)	(.109)	(.090)	(.090)	(.102)
9 < years of schooling < 12	.363***	.267**	.263***	.222**	.354***
	(.125)	(.123)	(.091)	(.091)	(.109)
years of schooling > 12	.761***	.597***	.539***	.454***	.433**
	(.213)	(.221)	(.154)	(.154)	(.205)
Household size	.014	.011	.048**	.048**	.124***
	(.027)	(.025)	(.021)	(.020)	(.025)
HH head age	.001	-.003	-.005	-.006*	-.009**
	(.004)	(.004)	(.003)	(.003)	(.004)
Years in village	-.006*	-.003	-.004	-.003	-.0009
	(.003)	(.003)	(.003)	(.003)	(.003)
Mean income/povline in village	.034	.019	-.0008	-.010	.011
	(.035)	(.025)	(.031)	(.032)	(.028)
Population density	-.003	-.002**	-.002*	-.001	.0008
	(.002)	(.001)	(.0009)	(.0009)	(.0008)
Mean yrs land is held in village	.005	.003	.019*	.018*	-.026***
	(.015)	(.009)	(.011)	(.011)	(.008)
Obs.	1241	1280	907	897	1135
e(ll)	-588.246	-1301.084	-1180.47	-1147.844	-1646.689
e(r2-p)	.263				

This table presents a probit model with an indicator of being judged a feasible borrower as the outcome variable in the first and second columns. The second column uses household head's age, a village indicator of rice production and a village indicator of forested area as instruments for being titled. The first column is run with fixed effects at the subdistrict level and both report robust standard errors. The third and fourth columns are OLS models with log of desired loan amounts (in dollars) as the outcome variable. The fourth column adds as a control whether or not the household was judged feasible to borrow. Full set of controls are used in all columns.

Table 14: Village-level Endogeneity

	Dependent Variable					
	T	FB	FB	T	FLA	FLA
	OLS	Biprobit	OLS	OLS	2SLS	OLS
Village IV Titled		-.124			-.094	
		(.334)			(.673)	
Residual Titled			.092*			.255**
			(.052)			(.125)
Forest	-.198***			-.227**		
	(.061)			(.115)		
Ricepaddy	-.323***					
	(.108)					
F-stat for IV	14.39			3.89		
Joint sign. of controls at 1% level	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	777	777	777	365	365	365
R^2	.289		.235	.283	.158	.18
e(ll)	-366.4	-689.0	-345.2	-198.0	-483.5	-478.8
e(rho)		.253				

In this table, the dependent variables are T standing for titled, FB for formalbank and FLA for formal loan amounts in logs of CPI-adjusted US dollars. This table presents three different IV models. The first is a biprobit model with an indicator of having had a formal bank loan and possessing a land title as the outcome variables, shown in column 2. The second model uses the residuals from predicting possession of a formal land title using village-level instrumental variables as the variable of interest, shown in column 3. These results for these models are only presented for the restricted sample containing rural villages. The full set of controls are used. Column 1 presents the "first-stage" for both of these models. Column 5 presents the 2SLS estimates for formal loan amounts, the third IV model and column 6 presents the fourth model, analogous to the second model. All formal loans are used. Distance to nearest bank and village bank controls have been excluded, but otherwise a full set of controls is included. Column 4 presents the "first-stage" for both of these models. Standard errors are clustered at the subdistrict level. Marginal effects are not reported for the Biprobit model.