Selected Paper prepared for presentation at the American Agricultural Economics

Association Annual Meeting, Providence, Rhode Island, July 24-27, 2005

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

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May 16, 2005

Abstract

It is often suggested that the poor are credit-rationed due to their lack of formal collateral. Using a household survey from Indonesia, we estimate the impact of having a land title on formal credit access. Adopting an instrumental variable approach, we find that having a formal title significantly increases a household's probability of ever having had a formal loan and the observed loan amount. Why land titles increase access to credit is still not clear. Incorporating data from a unique survey of bankers in Indonesia, we will argue that possessing a formal title increases a household's incidences of formal credit not because the value of the title as collateral but because of what possessing a title signals about the household to the banker. We apply a simple model of contract choice to show how title can act as an indirect signal.

1 Introduction

What are the channels through which land titles could affect access to formal credit? Policymakers have long argued that formalizing property rights equips a landowner with a collateralizable asset. In fact, one of the main justifications for large-scale land titling programs that have been sponsored by the World Bank and other aid organizations is that they increase access to credit for the poor. The idea behind formal land title as collateral is that there exists the possibility of a legal transfer of the pledged asset if the borrower defaults. If creditors cannot feasibly foreclose then we would not expect formal titles to directly affect credit access. But formal land titles may have informational value to the banker above and beyond their value as collateral. Possessing a title could signal credit-worthiness. In Indonesia, as in many developing countries, obtaining a title is a lengthy and costly process. Therefore having a title provides information about unobservables such as one's degree of integration into the formal system. The central argument of this paper is that land titles in Indonesia are important not because they serve as collateral but because of what they signal about the loan applicant.

To make this argument we need to answer two further questions. First, what is title signaling to banks? And second, why is it that land titles as collateral are not crucial for determining the credit supply in Indonesia? Land titles could signal the ability to interact within formal rules, business-minded characteristics or the condition of assets. To answer the second question we will use three observations relevant to banking practices in Indonesia. First of all, land can serve as collateral without being titled. Households can use informal land documents that demonstrate ownership but are not legally transferable. Second, even having a land title does not necessarily guarantee its full transferability. And third, collateral itself may not be the most important determinant of either receiving the loan or the size of the loan. We should note that there are of course reasons why land title may provide a good form of collateral.[1] However these reasons typically rely on 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.

Drawing from field observations and a mail survey of Bank Rakyat Indonesia (BRI) unit heads that we conducted, we argue that for the relatively small loans we are considering the legal process to foreclose or collect on collateral is too costly. In fact even the process of officially registering the collateral is too costly.[2] Consequently banks use other means to ensure repayment. Also not all formal loan applicants with title use it as collateral. Therefore we can separately identify the effect of having a title versus offering it as collateral

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[1]See deSoto [2000].

[2]Obviously we must emphasize that just because foreclosure is not observed very often is not an argument that collateral is not at work. A simple game-theoretic framework will yield a Nash equilibrium where no one defaults yet the possibility of foreclosure is real.

on the formal loan amount received. The effect of the possession of title is positive and significant while the difference between offering an informal land document as security in place of a land title is not statistically significant. This suggests to us that the signaling role of title is more important than its collateral role. At first glance one might be surprised that possessing a land title and not offering it as collateral is not considered a bad sign by the bank.[3] Obviously one must ask what people are offering as security in place of the title. The answer in our data is that in the place of contingent contracts securitized by collateral, households choose to take fixed income non-contingent contracts that are guaranteed by salary.

The possession of a land title permits signaling via contract choice. Although our main argument does not rely on title functioning as an indirect signal, we present this application of contract theory for several important reasons. The detailed data set that we have allows us to ask this relatively specific and to our knowledge unexplored question. And, most importantly, it illustrates in a simple way that even though land title is not essential to receiving a loan, land title can still provide relevant information in the loan decision. It is precisely the fact that a household has a title but does not offer it as security that reveals information.

We adopt the model in Spier [1992] to explain this situation. Contracts with fewer contingencies provide less insurance to a risk averse household but avoid transaction costs. Choosing a non-contingent contract could indicate transactions costs outweigh the insurance premium for a given household. In the model we present, choosing a non-contingent contract is also how the household signals its good qualities over a certain range of transaction costs. We will show that among the population that is both titled and has outside employment (and therefore is eligible for a fixed income loan), those that choose fixed income loans have better observable characteristics such as educational attainment.

We use the Microfinance Access and Services Survey (MASS) 2002 which was conducted by BRI in order to evaluate households' microfinance activity and potential new markets. Indonesia has an extensive rural banking system mostly supplied by BRI Units. The survey provides disaggregated data on household economic activities, assets and loans for over 1400 households in 70 villages across 6 provinces. Most importantly for each loan reported it includes what was offered to securitize the loan. These households have not been affected by any large-scale systematic land titling program. Therefore the household choice to title depends on certain unobservable characteristics, such as proclivity for business, that may also be associated with the desire to obtain formal loans. Also, the sophistication of credit markets may be correlated with the sophistication of the titling system. For these reasons title is likely to be endogenous.

The endogeneity of land title has not been adequately addressed in the previous liter-

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[3]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.

ature. Either the problem is ignored or a systematic titling program is used. In the first instance, the empirical estimation results will most likely be biased. In the second instance, the results will miss any effect of title as a signal. To accurately measure the effect of land title on access to credit we will make use of the instrumental variables technique. Our instrumental variables come from Potensi Desa 2003 (PODES 2003), a government survey of Indonesian village heads, and the MASS survey. We use this village-level information to supplement our household survey. The two instrumental variables that we will consider are the head of household's age and whether rice paddies are the village's primary form of income. Using a bivariate probit model, we estimate the effect of title on the probability of having had a formal bank loan. We find that at the sample median the marginal effect of having a title on the probability of having had a formal bank loan is 21% (with a robust standard error of 10%). We also estimate the effect on loan amounts using a Heckman selection model and find that possessing a land title increases the predicted loan amount, although the effect is not as large as one might expect.

Implications for policy on land titling programs can now be considered. If land titles influence banks through what they signal about the household then the benefits of a large-scale systematic titling program on access to credit are less than when effect of title is via collateral. This is assuming that systematic titling programs do not lead to large changes in banking practices.

This rest of the paper precedes as follows. In the next section we briefly discuss the previous literature that has looked at the question of how land titles affect access to credit. Then we outline some important features of the legal and credit settings in Indonesia which motivate our approach to this question. In section 4 we present the implications of a simple contracting and signaling model. Section 5 describes our data sets, section 6 presents our empirical results and section 7 concludes.

2   Previous Literature

The positive effect of title 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. As noted in our introduction, we believe we are the first paper to try to instrument for title without using a land titling program as the instrument. Table 1 lists previous empirical work on how land titles affect incidences of formal bank credit. In some ways it is not so surprising to be finding contradictory results. After all, these studies took place in different countries with different sets of institutions governing the credit and land markets. Also some studies consider systematic titling programs where possessing title can be viewed as relatively exogenous to the credit decision and others study "sporadic" (or individually obtained) titles which require more effort for households. This is the case we are studying in Indonesia.

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
Migot-Adholla and Place [1998]	Ghana, Rwanda & Kenya	No	Both
Broegaard et al. [2002]	Nicaragua	No	Both

The empirical problem of measuring the effect of possession of title on the probability of obtaining a formal bank loan is not as straightforward as it might appear. First of all, there is the necessity of separating the effects of title on the supply of formal credit from the effects on the demand for credit. Feder et al. [1988] was the first to identify this problem. They model it by letting observed credit equal the minimum of supplied credit and credit demanded but resort to assuming excess demand in their empirical work. Field and Torero [2004] solve the problem by using detailed information on different banks' requirements for loans. They measure the effect of title on credit access only among banks that require title. In this way, they control for the possibility that having a title will increase demand for credit. Most studies simply assume that there exists excess demand for formal credit which as Kochar [1997] argues is far from ideal. She stresses that 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.

We also should mention that not all development experts consider titling programs to be appropriate in certain situations. Customary land rights may be difficult to describe or put into an adequate formal title. Atwood [1990] argues that land titling can create uncertainty which undermines local relationships in his study of sub-Saharan countries. 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 the subtle societal interface; 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]). In an urban

area, Lanjouw and Levy [2002] show how community relationships can substitute for formal claims on assets. This is of particular concern in our context due to the strong adherence to adat law in certain areas of Indonesia. Nevertheless, even if tenure security can be achieved through informal means, it is hard to imagine that large amounts of formal credit can be accessed on the basis of this same community relationship.[4]

3   Institutional Background

Land Law in Indonesia

The Indonesian legal system is an interesting mix of Dutch, Muslim and customary (adat) law. Although the written law should apply across provinces, the complicated 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.

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*.[5] 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.

The National Land Administration Agency (BPN) grants titles to non-forest land.[6] Out of the 80 million land parcels on the fiscal tax register less than 27 million are on the legally titled register. Currently about 1.3 million new titles are registered sporadically each year and the total number of land parcels is estimated to by growing by more than 1 million each year.[7] 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 and a survey with the applicant funding the boundary markers, the transportation costs and the survey fee. Transportation costs are not trivial. 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 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.

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[4]Though perhaps it is not that hard given the continual rise of micro-credit and group lending.

[5]It is a document that represents the purchase of a piece of land and is officially stamped and notarized.

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

[7]World Bank Project Appraisal Document, Report No: 28178-IND (2004)

For example, the stated fee of a land certificate is around 300,000 Rp. (approximately $33), but when we asked what the actual fee was we received answers ranging from 1-2 million Rp. (approximately $111-$222).

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 and ROSCA's, neighborhood cooperatives and money lenders in the informal sector. The bank that we focus on, BRI, is the fourth largest bank in Indonesia with 10% of market share as measured by the total assets held by banks. It also has the most extensive banking presence in rural areas. The primary BRI bank in these areas is the BRI Unit, a bank that deals with smaller sized loans. 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 BRI Units 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, a loan above 20 million Rp. (approximately $2200) must have approval from its central BRI Branch but there is no one formula for accepting or rejecting loan applicants. Successful unit managers are rewarded with more discretion and higher limits for lending without branch approval. Unit managers are allowed to rely on notions such as "trustworthiness" when granting a loan.

We conducted a mail survey of 192 BRI Units across the same 6 provinces and 12 districts where the MASS 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 other forms of land documents as collateral besides just official land titles. In our mail sample, only 42% of loans that are securitized by some form of collateral are done so with a land title certificate. But 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 the most important factor in considering whether to grant a loan 82% answered the character of the individual. Moreover, when determining the repayment ability of the applicant (which determines the loan size that the applicant is eligible for), the most important factor was cash-flow (66%) followed by character (20%). Collateral appeared as an answer only once as the most important factor for determining repayment capacity and never as the answer to the most important factor in considering whether to grant a loan at all. 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

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

60% of our sample though only 29% say that having land title will increase the likelihood of receiving a higher loan amount than someone without a land title.[9]

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 might expect to see instead 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. In order to maintain the relationship with the bank the client finds a way to liquify at least part of the asset. This suggests again that legally titled collateral might not be that important since the courts are being bypassed.

Drawing on the microfinance literature (Morduch [1999]) we identify several repayment mechanisms that seem to be at work in place of collateral: dynamic incentives, one-time incentives and monitoring. BRI Units promote repayment through progressive lending, the practice of allowing clients to borrow larger amounts in the future if they repay on time.[11] Another method is to offer one-time interest payment rebates for those customers that repay on time.[12] Also the supply of credit can be limited to members of a social network in order to lessen monitoring costs (assuming that the social network naturally monitors its members to some degree). When asked how new clients find out about BRI and its services, 88% of the bankers said it was through friends and families. By signaling characteristics associated with desire or ability to repay or membership of a social network, the client can help solve the information and the incentive problem. These methods show that banks can (and do) use methods other than collateral requirements to solve the moral hazard and adverse selection problem.

These mechanisms have costs that may seem higher than the expected costs of the self-help methods of collecting collateral common in the US.[13] Given the legal premise for self-help methods in Indonesia is weak, it is unclear that formal collateral will have lower enforcement costs than the methods described above. The important distinction between the two is that these costs do not depend on the ex-post collection or seizure of assets in case of default. In fact, default is often hard to characterize when these repayment mechanisms

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[9]This is similar to what our empirical results will show - having a land title makes a household more likely to receive a formal loan but conditional on receiving a loan, a title has less influence on the size of the loan.

[10]This number actually surprises us; we would have asked for how many times in past few years but we were almost sure that answer would be zero.

[11]We find that the mean of the reported maximum loan given to repeat customers without formal title but with some land document is twice that of new customers with a similar land document.

[12]See Robinson [2001].

[13]For example, in the case of repossession a bank could hire someone to non-violently seize the collateral in order to resell without going to court.

are in place. Often debt-rescheduling is preferred to a declaration of default for a variety of social and economic reasons.

4 Signaling with Contract

As we discussed in the introduction titles can signal for unobservable characteristics. In this section we show that title can also signal type indirectly through the choice of loan contract. Different contracts have different costs for a household. Households may choose different contracts depending on their insurance premium and transaction costs. Following Spier [1992], we show that in the presence of asymmetric information households may choose loan contracts to signal type.

Basic Set-up of Model

A risk averse household owns a productive asset that with a fixed loan size, L, yields a stochastic output $Q \in \{Q_L, Q_H\}$ with $Q_L < Q_H$. A household is a good type, g, with probability $\pi \in (0,1)$ or a bad type, b, with probability $1 - \pi$. Let $p_i = P(Q_H | type\ i)$ and $p_g > p_b$. The household has expected utility $V(x) - y$ where x is the household's income and y are the transaction costs. $V' > 0$ and $V'' < 0$ as usual.

The bank is a risk-neutral player in a competitive market (zero profits) that knows π but cannot observe the type of the household. The bank's opportunity cost of lending L is L_o. A court can verify Q but not type, hence contracts can depend only on Q. A contract is considered contingent if the repayment schedule, $\{R_L, R_H\}$, depends on realization of output and non-contingent if $R_L = R_H$. Contingent contracts incur an ex-ante cost of k paid by the household.

We think of ex-ante transaction costs as being both the drafting costs, psychological costs and expected costs of avoiding default. The distinction between ex-post costs and expected ex-ante costs is one of verification. If the bank or the household must pay to verify the actual outcome given a dispute, then the renegotiation process must be taken into account in the contract selection decision. As mentioned in the previous section, in our setting banks tend to use enforcement mechanisms that avoid verification costs such as denial of future access. That is not to say that the threat of going to court is not real. Only that it is unlikely that verification costs play a significant role for small-sized loans.[14]

Timing of the game is as follows. Households observe their type and pick contract $\{R_L, R_H\}$ from among the bank's menu of contracts (that leave it indifferent between offering the loan or not). If the household refuses all loans then it gets nothing and the bank gets L_o. If they pick one then the bank gives the household the loan of size L after which

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[14]Spier models the case when verification costs are paid by the household. She gets a similar result that in the presence of asymmetric information. Good types signal their type with a non-contingent contract over a range of verification costs. In our case, the important difference between enforcement costs that require verification is that verification costs are paid by the bank.

Q is realized. We assume that there exists costless verification of the contract. So the household will never repays more than the true amount and the bank refuses any offers that in expected terms give it less than its outside option.

Assuming that the household picks the contract $\{R_L, R_H\}$ then given p and k the household's expected payoff is:

$$E_Q V(R_L, R_H; p, k) = \begin{cases} pV(Q_H - R_H) + (1-p)V(Q_L - W_L) - k & \text{if } R_H \neq R_L \\ pV(Q_H - R_H) + (1-p)V(Q_L - W_L) & \text{if } R_H = R_L \end{cases} \tag{1}$$

Full information

Suppose the bank observes the household's type and therefore p, then the full-information contract that the household chooses is the solution to

$$\max_{R_H, R_L} E_Q V(R_L, R_H; p, k)$$

subject to

$$pR_H + (1-p)R_L \geq L_o \tag{2}$$

Clearly the best non-contingent contract the household can choose is $\{L_o, L_o\}$ and the best contingent contract is one that fully insures the household and leaves the bank indifferent, i.e.

$$\{R_L, R_H\} = \{L_o - p(Q_H - Q_L), L_o + (1-p)(Q_H - Q_L)\} \tag{3}$$

Type i is indifferent between the full-insurance contingent contract and the best non-contingent contract when

$$V(p_i Q_H + (1-p_i)Q_L - L_o) - k = p_i V(Q_H - L_o) + (1-p_i)V(Q_L - L_o) \tag{4}$$

Let $k(p)$ be the k that solves the above equality for a type with probability of a high realization of p and note that $k(0) = 0$ and $k(1) = 0$. Differentiating $k(p)$ we get

$$k'(p) = (Q_H - Q_L)V'(pQ_H + (1-p)Q_L - L_o) - V(Q_H - L_o) + V(Q_L - L_o) \tag{5}$$

which is negative if p is large and positive if p is small. First notice that $k(p)$ has a unique maximum since $k''(p) < 0$ implying that k is positive over the interval [0,1]. Then as long as $p_b > argmax k(p)$, then $k_g = k(p_g) < k(p_b) = k_b$.

Therefore we can conclude that if k is small ($k < k_g$), then both types choose contingent contracts and if k is too large ($k > k_b$) then both choose non-contingent contracts. For the intermediate range ($k \in [k_g, k_b]$), if p_b and p_g are sufficiently large, then the good type offers a non-contingent contract and the bad type offers a contingent contract.

Asymmetric information

Suppose now that the bank does not observe the household's type. The full-information contract may no longer be sustainable. Under zero transaction costs both types prefer the contingent contract. The bad type strictly prefers the contingent contract the good type would offer under full information. Since the (IR) constraint binds in the full information case, it would be violated if the bad and good type pool on the good type's optimal full information contract. Consequently, the good type solves for the optimal contract under asymmetric information with an additional constraint.

$$\max_{R_H, R_L} E_Q V(R_L, R_H; p, k)$$

subject to

$$pR_H + (1-p)R_L \geq L_o \tag{6}$$

and

$$E_Q V(R_L, R_H; p_b, k) \leq V_{FI}^*(p_b, k) \tag{7}$$

where $V_{FI}^*(p_b, k)$ is the optimal contract under full information.

Similar to the full information case, the best non-contingent contract the household can choose is $\{L_o, L_o\}$. If k is small, then both types choose contingent contracts and if k is too large $(k > k_b)$ then both choose non-contingent contracts. But now the intermediate range expands to $[\tilde{k}, k_b]$ where $\tilde{k} \in [0, k_g)$ and given p_b and p_g are sufficiently large, then the good type offers a non-contingent contract and the bad type offers a contingent contract over this range. To see why this is the case, notice that if the incentive compatibility (IC), equation 7, binds, then the best contingent contract is dominated by the full insurance contract under full information. The IC binds when $k \leq k_g$ by arguments given above. Then the good type will strictly prefer the non-contingent contract at $k = k_g$. Finally, notice that the payoff of the contingent contract is decreasing in k and when $k = 0$ the good type prefers the contingent contract. Thus there exists a $\tilde{k} \in [0, k_g)$. The intuition behind the signaling outcome is good types are able to self-insure better than bad types because they have a higher expected return. Good types choose to distinguish themselves by showing a non-reliance on insurance.

One attractive extension of the model for this paper would allow loan size to vary by project. A simple modification treats projects as exogenous. Each project requires a fixed cost to start. We could think of expensive investment projects, E, and cheap ones, C, requiring loans from a bank. Now the bank has an opportunity cost that varies with the size of the loan, $L_o^E > L_o^C$. Raising the bank's outside option tightens the (IR) constraint. Holding Q_H and Q_L fixed, a tighter (IR) constraint, equation 6, lowers the household's value of the loan contract. Since the (IR) constraint always binds in the full information case, higher investment makes it more difficult for the good type to signal over

the intermediate range of transaction costs in the asymmetric case. Thus this model has a convenient interpretation if land title has an effect on loan amounts. The collateral effect of land title should dominate the informational value of land title as loan size increases holding all else constant.

5 Data Description

We will be utilizing data from two different sources in this paper. Our main source is the BRI MASS 2002 household survey. 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.

MASS data

This survey consists of over 1400 households spread across 70 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 [2003]).

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 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] 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%).

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[15]Potensi Desa Statistik or Village Potential Statistics which was actually collected in 2002.

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

[17]Alternatively it could also be 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 all their plots titled or all untitled.

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 of reported loans are formal.[18] The most common formal loan use reported was working capital for an existing venture (37%) followed by home improvement (24%). While working capital is a very general term, we can note that loan uses such as diversification or starting a new business were listed less than 2% of the time. We can classify roughly 40% of our loans as being used for production purposes and the remaining 60% as being for consumption or other.

Because our loan amounts are from all different years, with the majority of loans in 1998-2002 (only 9% of the formal loans are from the earlier 1990's), we normalized the loan amounts by converting to U.S. dollars. Table 3 breaks down the formal loan amounts by the type of security offered.

Table 3: Formal Loan Amounts in U.S. dollars by type of Security Used

Security Used	Mean Loan Amount	Std	N
Land title certificate	1124	1340	93
Other land certificate	629	444	60
Fixed Income	903	767	177
Vehicle ownership	1046	1368	16
Savings/time deposit	105	.	1
Home/appliance/furniture	426	496	2
No security	446	475	18
Total	893	959	367

Source: BRI MASS 2002

Table 4 gives the summary statistics for both the covariates that we will be including in our estimations and other descriptive statistics. FormalBank equals one if the household ever reports having a formal loan.

PODES 2003

Table 5 gives the summary statistics for village level covariates from the PODES data set that we will be including 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 (65% 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%.

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[18]74% are formal bank loans, 14% are microbank loans, 12% are from other informal sources.

Table 4: BRI MASS 2002 Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Formalbank	0.287	0.453	0	1	1310
Distance to bank (km)	6.778	9.907	0	56	1297
FormalLoanAmtDollars	890	955	5.67	11687	371
MicroLoanAmtDollars	353	537	6.63	2922	76
InformalLoanAmtDollars	355	803	6.94	4926	58
Total Value of Fixed Assets (approx.US$)	5920	10800	2.5	157000	1310
Log total fixed asset value	17.229	1.138	10.127	21.174	1310
Income per cap/poverty line	3.309	4.737	0.011	69.819	1308
Household size	4.437	1.729	1	13	1310
Female household head (HH)	0.093	0.291	0	1	1310
HH's education (years)	7.99	3.943	0	16	1310
HH's age	46.332	11.87	20	87	1309
Years in village	27.759	16.102	1	99	1303
Has salary	0.411	0.492	0	1	1310
Mean income/povline in village	3.38	2.113	0.409	13.328	1310
Mean years land is held in village	17.623	5.773	3	40.034	1310
Rural	0.604	0.489	0	1	1310

Table 5: PODES 2003 Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Village has bank	0.202	0.401	0	1	1310
Village has registered council	0.646	0.478	0	1	1292
Population density (adultpop/hectare)	28.17	52.95	0.06	270	1310
RicePaddy	0.428	0.495	0	1	1310

6 Empirical Strategy and Results

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 as we saw above loans securitized by formal land titles tend to be larger on average. We will first consider a straightforward test that looks at how possessing title affects credit access while controlling for other observables that might also influence credit access.

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

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 as well as subdistrict fixed effects.

We have to make a further assumption on the error structure when the outcome of interest is binary and we are using a Probit model. Letting y^* be the latent variable, the

unobserved value to the bank of giving a loan to household i, we have:

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

We also report on the following specification and compare γ_{titl} to γ_{othr}.

$$y_i = X_i\beta + \gamma_{titl}\text{lnValTitledLand}_i + \gamma_{othr}\text{lnValOtherLand}_i + \gamma_{ass}\text{lnValOtherAsset}_i + \epsilon_i \quad (8)$$

One of the main problems with either of these approaches is that as discussed in the introduction possessing a title is hardly an exogenous variable in the MASS data set so therefore we do not expect $E(\epsilon|\text{title}, X) = 0$ to hold and our estimates will be biased. Initially we expected γ to be biased upwards because we thought that the main source of endogeneity woud be unobservables such as entrepeneurship that would be positively correlated with both high loan amounts and having title. To account for this problem we will make use of instrumental variables.

To be precise, in order for $z \in Z$ to be an instrumental variable for title the following two conditions must hold: (1) $Cov(\epsilon, z) = 0$ and (2) $Corr(\text{title}, z|X) \neq 0$. In the following section, we will argue that household head's age and whether or not rice paddies are the village's primary source of income are valid instruments. To instrument in the binary variable case we will use the bivariate probit model; that is we also estimate the probability of a household being titled (including our instruments) and allow the error terms in the two equations to be correlated.

Next we will address the fact that the reduced form approach does not let us separate supply from demand. One way to think what we are doing above is that we are assuming excess demand for credit so that any changes in observed credit can be attributed to changes in supply. This is not very satisfactory so in section 6.3 we will discuss how we can make use of some hypothetical questions that were included in BRI MASS survey in order to separate the demand effects from the supply effects. While obviously the use of hypothetical questions is not without its problems, they have been used in a wide sample of work with relative success (Barham et al. [1996]).

A third issue is the sample selection problem. We would like to know the effect of land title on loan size for the general population. We need to correct for any selectivity bias due to the correlation between those who received a formal bank loan and the loan size. Below

is the selection model we will estimate.

$$y = X\beta + \gamma title + \upsilon$$
$$y_s = I[Z\delta + \mu title + \nu_s > 0] \text{ where } \nu_s \sim N(0,1)$$
$$E(\upsilon|\nu_s) = \eta\nu_s$$

In this model, Z and y_s are always observed while y is only observed only when $y_s = 1$. We assume that υ and ν_s are independent of Z with zero mean but later relax this assumption to consider the possible endogeneity of title. In order to ensure sufficient variation in the selection equation, we let Z equal the X controls plus household distance to the nearest bank and whether a village has a bank. Both these variables influence whether a household has or wants a loan but are unlikely to influence the loan size after including other controls.

6.1 Instrumenting for Title

In this section, we provide justification for our two instruments for possession of a formal land title.

Whether rice paddies are a village's primary source of income provides a measure of how likely a household is to be titled. Rice paddies demand irrigation which is an investment-intensive activity that can establish ownership. Considering the high cost of obtaining land titles, we would expect to see less titles in areas where there are other means of establishing ownership. Indeed, in rural areas the probability of being titled given that rice paddies are the primary source of income in your village is 18% contrasted with 48% if it is not. In urban areas there is a smaller difference (59% as opposed to 66%), but in our sample only 3 urban villages have rice paddies as their primary source of income.

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. This instrument might be subject to the criticism that it is a measure of the overall development level of the village. In order to attempt to mitigate this problem, we include the mean income level in the village, the village population density and the village mean number of years that land assets are held (to control for the degree of activity in the land markets). We also include a measure of village infrastructure - whether the village has a registered council.

Since obtaining title is a time consuming, costly process, the age of the household head describes to some extent the number of potential opportunities a given household has had to obtain title. Controlling for wealth and income and other important variables, being older should lead to a higher probability that the household has had enough time to save the money necessary for title and to go through the process of obtaining title. In our data, this age variable is positively correlated with whether the household has a land title (see

figure 1). Age should not be correlated with any innate personal characteristics important to the loan decision. Of course, age could be associated with village status or responsibility that would affect the loan decision. To control for this we include the years a household has been in a village, whether or not the household head is female, the education of the household head and household size.
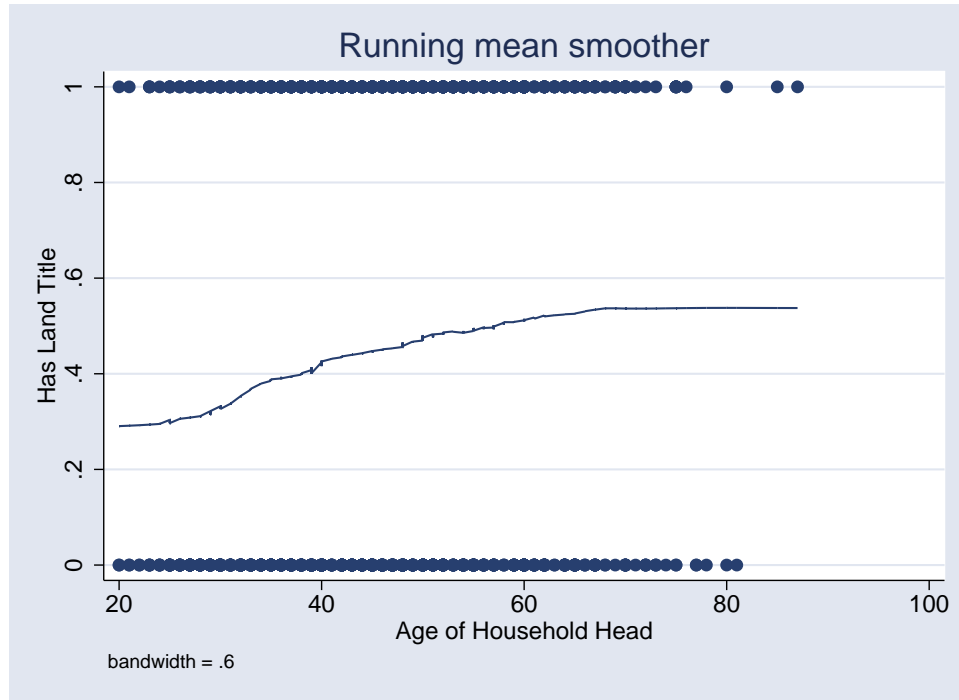
Figure 1:

After running the first stage of 2SLS, we are able to check if our instruments do explain title after including other controls. The two instruments discussed here all always have coefficients that are significantly different from zero both individually and jointly. And in the specifications we present below they pass the typical overidentification test which suggests that they do not influence credit access except via land title.

In the following sections we present both the naive results if one simply includes whether or not a household has a formal land title as an exogenous right-hand side variable and the instrumented results for the following dependent variables - whether or not the household has ever had a formal bank loan, whether or not they were judged feasible for a formal bank loan (see section 6.3), the log of the feasible loan amounts, the log of formal loans amounts. We chose to put in fixed effects at the sub-district level since BRI Units lending areas are usually a sub-district. In general we find that including the geographic dummies increases the precision with which we are able to measure the effect of title but our results are robust to their exculsion.

6.2 Formalbank

After controlling for household and village-level characteristics and including subdistrict level dummies to control for any unobserved geographic differences, we find a positive correlation between having had a formal bank loan and possessing a land title (see columns (1) and (2) in table 8 for a linear probability model and table 9 for the probit specification). We find that using the probit model, at the mean, switching to having a land title from being completely untitled increases your probability of having a formal loan by 11% (with a robust standard error of 4%).[19] This suggests to us that land titles may function as a signal of credit-worthiness since other land documents are also used as collateral but nevertheless titles have an effect on the probability of a household having formal credit.

In table 10 we present the specification described by equation 10. γ_{titl} is positive and significantly greater than zero. In magnitude it is almost three times as large as γ_{othr} and γ_{othr} fails to be significantly different than zero in the specification including subdistrict dummies. Note that titled land also has more of an effect than other assets. An increase in one standard deviation of $lnValTitledLand$ would increase the probability of having had a formal bank loan by almost 11%. This result could be interpreted as support for either the collateral or signaling story. It simply suggests that titled land has a differernt effect on the bank's decision compared to other documented land.

After instrumenting for the possession of title (columns (3) and (4) of table 9) we see that the effect of title has actually increased to 21% (with a robust standard error of 11%). In the bivariate probit, ρ, (the correlation of the error terms across the two equations) is $-.353$ and is significantly different from zero indicating an endogeneity problem.[20] The unobservables affecting the titling decision correlate negatively with the unobservables determining formal bank usage after controlling for other characteristics. Given that our model is correctly specified, our instruments are valid and are distributed somewhat randomly across the sample, then naive estimate is actually biased downwards from the coefficient on title in the structural model.

6.3 Using Hypotheticals

Credit Supply

The BRI MASS survey was conducted by BRI loan officers that usually work in different areas. 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 for how much and under what terms. These questions give us a

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[19]The linear probability model gives similar results. The probability decreases to 9% (with a robust standard error of 3%).

[20]In the instrumented linear probability model (column (4) in table 8) we also strongly reject the null hypothesis of exogeneity. The Wu-Hausman $F(1,1218) = 7.20$ with a p-value of .007 and the Durbin-Wu-Hausman $\chi^2(1) = 7.46$ with a p-value of .006

measure of hypothetical supply. That is, without considering the effects of title on credit demand, we can evaluate whether title systematically affects how much credit a household is at least hypothetically eligible for. Given that these questions were answered by actual BRI loan officers adds to their validity as a measure of credit supply. We run the same specifications described above (naive probit and bivariate probit, see table 11) on whether a household was judged feasible for a formal bank loan. We find that after instrumenting and including subdistrict dummies, possession of a title increases the probability at the median of being judged feasible by 31%. This suggests to us that the reduced form estimates we discussed in the previous section are driven by the supply side of the credit market.

In general the 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 $870. We choose to run a Tobit model to account for the possible corner solution when the loan officer decided the household would not be eligible for a formal bank loan. Accepting the distributional assumptions on the error term in the Tobit model is preferred to OLS. After instrumenting title actually passed the Wu-Hausman F test and Durbin-Wu-Hausman χ^2 test for exogeneity.[21] Therefore we present only the OLS and Tobit results in table 12. We report the marginal effects at the mean conditional on being uncensored for the Tobit. We note that the results are similar to the simple OLS model shown in column (1). In neither case does the possession of a land title significantly effect the loan amount judged feasible for the household.

Credit Demand

The MASS survey also asked households how much credit they hypothetically would be interested in obtaining. Of the 901 households that were judged feasible, 565 have never had a formal bank loan. Their answers as to why they have never had a formal bank loan can give us some insight into the question of demand for formal credit by those who are hypothetically not credit-rationed. Over half of those who were judged feasible and have never had a formal bank loan claim that it is because they do not want to be in debt. They were then asked a series of hypothetical questions about their possible desired loan amounts and conditions. We turn to the Heckman selection model to correct for possible selection bias since we want to say something about how title affects demand for credit for the entire population. We find that title almost has zero effect on desired loan amount even after correcting for the very plausible non-random assignment into this group who has never had a formal loan (see table 13). The correlation between the error terms is negative as expected: those who are less likely to have had a formal loan desire smaller loans.

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

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[21]Wu-Hausman F(1,1119)=.0048 with a p-value of .945. Durbin-Wu-Hausman $\chi^2(1)$ = .0050 with a p-value of .944

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.

6.4 Loan Size

The next question would be whether having a formal title influences the size of observed formal loans. The results presented in table 14 compare least squares estimates with estimates constructed using the Heckman procedure to correct for selection bias. Overall we find positive but weakly significant effects except in the case where we instrument over just the sample of those who had formal loans (column (2)). However our instruments are relatively poor predictors of title over this sample. Comparing column (1), OLS, to column (3) which accounts for the selection we see that there are no huge changes and ρ is not significantly different from zero downplaying the importance of the selection. However in the IV case, there are large differences when one considers the selection appropriately.[22] In column (4) we see that the impact of title on loan size is not huge; on average having a title increases loan size by 66% of one standard deviation of LogFormalLoanAmt.

6.5 Security Offered

In this section we look at whether the effect of offering different types of security. We note that among formal banks, almost all loans have some security.[23] What is important for us is whether the type of security offered makes a difference among those who get formal bank loans. Table 3 shows that the average loan size of those securitized by formal land titles is significantly larger than those securitized by other land documents. However when we include dummy variables for the type of security offered (missing category is offering an informal land document) we see that the coefficient on offering titled land as security is not significantly different from zero at the 10% level. We do this exercise with OLS on just the selected sample of those who had formal bank loans (this tells us the effect of title on those who get formal bank loans) and using a Heckman selection model to estimate the general effect in the population. See table 15.

Despite the fact that the coefficients on fixed income fails to be significantly different from the coefficient on land title as security, we would still like to argue that there are systematic differences between those that choose fixed income contracts and those that use land titles as security. To do this we need to consider the population that has both outside employment and a formal land title. We label a household as a good type if they have a formal fixed income loan (non-contingent) and a bad type if they have securitized their

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[22]We use the 2-step procedure outlined in Wooldridge [2002]

[23]Only 5% of formal loans are unsecuritized compared to 56% of microfinance loans and 64% of informal loans

formal loan with anything else.[24] Using this classification among households that both have outside employment and formal land title, we have 128 good types and 72 bad. The good types get significantly larger formal loans and more years of schooling (see table 6, differences in bold are significant at the 5% level or greater).

Table 6: Households with Land Titles and Salary

Types	LogFormLoan	LogAssets	Inc/povline	HHeduc	ConcernRepay
Good	**6.467958**	17.92066	5.441371	**12.03516**	.125
Bad	**5.836268**	17.71765	4.606819	**8.951389**	.2083333

Source: BRI MASS 2002 data set

Though our interest is primarily in formal land titles and their direct and indirect use, we can also carry out this same exercise (see table 7) with those who have both other land documents and salary. Again we compare the differences between those that choose fixed income loans (N=54) and those that choose other contingent contracts (N=42). We see that while there are some differences between these two groups, the only significant difference is that the bad group is more concerned about loan repayment. This suggests that having a title makes it easier for types to separate via contract choice. If title was not acting as an indirect signal in this way then we would expect to see the same separation among these other-documented types. Since despite their lack of formal land title they still have the opportunity to choose non-contingent fixed income loans and therefore reveal something about their "goodness".

Table 7: Households with Other Land Documents and Salary

Types	LogFormLoan	LogAssets	Inc/povline	HHeduc	ConcernRepay
Good	6.37571	17.37454	5.906893	10.36111	**.0185185**
Bad	6.114259	17.62744	5.112355	9.47619	**.1190476**

Source: BRI MASS 2002 data set

6.6   Does the Effect of Title Vary?

We would also like to extend the results from section 6.2 to allow for systematic variation in the coefficient on title. We are especially interested to see if the effect of title is different across areas with different concentrations of land title. For instance if the effect of title is only significant in areas with high concentrations of titled land then one might argue that it is only after there is a large scale titling movement that banks change their lending policies to require title. Therefore it is more a matter of requirements than a question of

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[24]In the sense that this contract is contingent because if the household is unable to repay, the bank will have to attempt to collect the security.

signaling. Alternatively one might expect that titles will signal the most information in areas with low concentrations of title. But here one might have a different problem. Since so few people have titles, banks use completely different methods of gathering information and enforcing repayment. Therefore a land title is irrelevant to the credit decision but for different reasons.

Since we only have a sample of 20 households for each village in the MASS data set, to construct our measure of the concentration of titled land in a village, we turn to PODES 2003 data set. We divide the number of titled hectares by the total village area as reported by the village head to calculate the percentage of titled land in each village in our MASS data set. We then divide our sample into three sections, villages where less than 20% of the land is titled (the low group), villages where between 20% and 70% of the land is titled (the medium group) and villages where more than 70% of the land is titled (the high group). We chose these cutoffs after looking at the kernel density shown in figure 2. In table 16 we
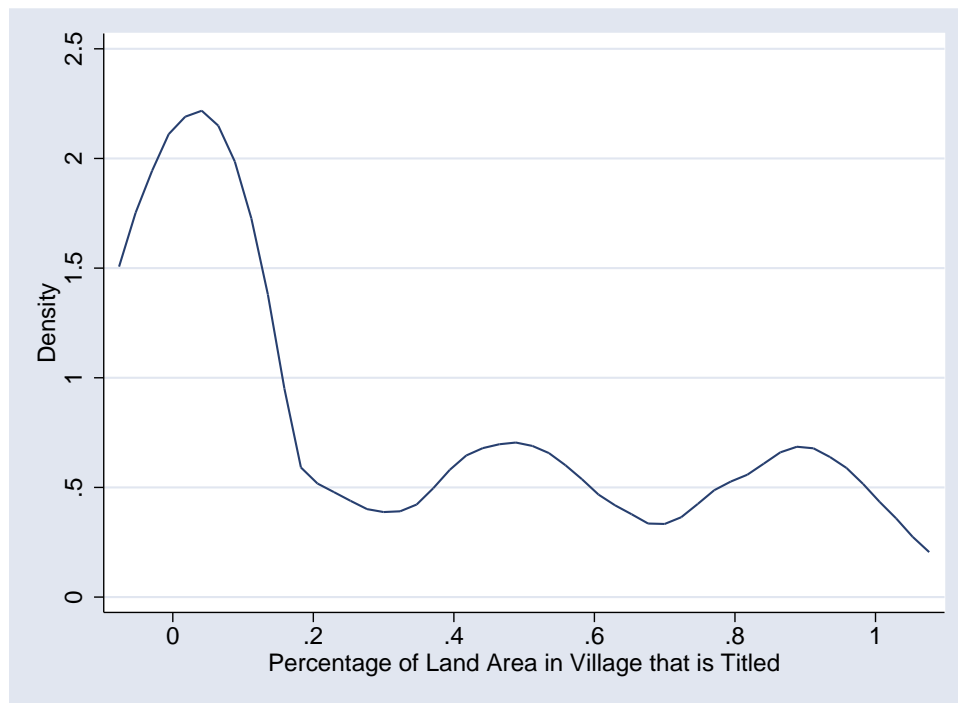
Figure 2:

present the bivariate probit model to measure the effect of title on the probability of ever having had a formal bank loan, instrumenting for title with household age and rice paddy as before. We see that the population that was driving the significant positive result reported in column (3) in table 9 is the middle group. In fact this is the only group in which the effect of title is positive and significant. For households that live in areas where 20 to 70% of the land is titled having a title increases the probability of having had a formal bank loan by 54%.

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This provides us with at least indirect evidence of our signaling story. If the effect of title was due to its value as collateral the effect would be predicted to be the same across areas with different concentrations of titled land (or perhaps we would predict a monotonically stronger effect in the areas with more title because possibly the legal system has adapted such that the process of collecting titled collateral is more efficient). But the u-shaped effect that we are observing would be harder to explain if one is not considering the informational value of title.

7   Conclusion

The approach of this paper has been to tell a story of how land titles might affect an underdeveloped credit market. Actually there are two stories at play here: the Hernando de Soto story and the BRI Public Relations story. In the first story, land titles are seen as unleashing the productivity of otherwise 'dead" assets. It is title's ability to serve as collateral that transforms fixed assets into liquidity and back again with ease. In the second story, sophisticated bankers have already unleashed these assets, including human and social capital, without the aid of formal land titles.

Our results place us somewhere between these two camps. On the one hand we show that titles are important determinants of credit access, as measured by whether a household has ever had a formal loan or been judged feasible for one, and also have some influence on loan size.[25] But on the other hand we argue that these results are due to signaling not collateral by showing that possessing a title is more important than offering it as security and that the effect of title is strongest in areas where titles are neither too rare nor too common.

It should not be overlooked that BRI and other banks in Indonesia are in favor of titling programs. This suggests that there are important dynamic effects that should be considered. How valid our short term results will be depend on the underlying legal institution in Indonesia. If high legal costs represent in some way the disparity between the formal system and what various communities across the country want, then change will likely be slow and the short-term will resemble the long term. Or it may be the case that a large-scale titling program could generate enough widespread coordination to significantly alter legal or financial institutions as Sumarto [2002] suggests.

Our paper poses some new empirical questions in the context of a well-established line of research. This research program uses institutions to identify and estimate economic effects. On one hand, institutions should be viewed as exogenous and the results of the analysis are conditional on the institutional framework (Acemoglu and Johnson [2003]). On the other hand, most development policies attempt to marginally change institutions and so one must try to incorporate the institutional framework explicitly. A policy for economic development

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[25]Though here the story is less clear; titles do not effect hypothetical loans amounts either demanded or supplied.

must not overlook the institutional constraints. Successful policy implementation requires adjusting to entrenched aspects of already in-place institutions. This may well be the case in Indonesia and the LAP II (the newest World Bank land project in Indonesia). However it has been common not to worry so much about contracting institutions assuming that as long as they do not function extremely poorly they should not affect the long run outcome. The story that this paper tells about the credit market and land titling in Indonesia leaves open the possibility that contracting institutions will interact with property institutions and influence the long-run results. If the reader believes that the state of the credit market today will shape the economy tomorrow in a fundamental way, then foreclosure law, the ability of households to contract and their relationship with land titles will impact the positive results of economic analysis in the long-run.

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Table 8: Linear Probability Model

Dependent Variable = FormalBank	OLS	OLS	IV	IV
	(1)	(2)	(3)	(4)
Titled	.070**	.090**	.158*	.989**
	(.029)	(.035)	(.091)	(.426)
Log total fixed asset value	.077***	.074***	.070***	-.009
	(.014)	(.016)	(.016)	(.043)
Income per cap/poverty line	-.0003	-.0009	-.0002	-.002
	(.004)	(.004)	(.004)	(.003)
Household size	.003	-.006	.002	-.006
	(.007)	(.008)	(.008)	(.010)
Female household head	-.035	-.028	-.041	-.054
	(.041)	(.042)	(.040)	(.050)
Household head's education	.009***	.012***	.008**	.003
	(.004)	(.004)	(.004)	(.006)
Has salary	.119***	.153***	.110***	.151***
	(.027)	(.030)	(.028)	(.038)
Years in village	-.0001	.0004	-.0002	-.0002
	(.0008)	(.0008)	(.0008)	(.001)
Log distance to bank	-.089***	-.066	-.079***	.078
	(.029)	(.044)	(.030)	(.092)
Village has Bank	.043	.048	.048	.035
	(.034)	(.052)	(.033)	(.063)
Population Density	-.0009***	-.001***	-.001***	-.002***
	(.0003)	(.0004)	(.0003)	(.0006)
Mean income/povline in village	.038***	.035***	.035***	.020
	(.007)	(.009)	(.007)	(.013)
Village has registered council	.019	.133*	.018	.019
	(.027)	(.077)	(.027)	(.111)
Mean years land is held in village	.0003	.003	.002	.003
	(.002)	(.004)	(.003)	(.005)
With Subdistrict Dummies	*No*	*Yes*	*No*	*Yes*
N	1270	1270	1269	1269
Adjusted R^2	.186	.224	.18	-.248
Joint F-test for instruments in first stage			53.68	5.43
Hansen J (overident. test for all instruments)			6.45	.014
$\chi^2(1)$ P-value			.011	.905

Note: Robust standard errors reported in parentheses

Table 9: Marginal Effects

Dependent Variable = FormalBank	Probit		Bivariate Probit	
	(1)	(2)	(3)	(4)
Titled	.075**	.107***	.221***	.212**
	(.031)	(.039)	(.082)	(.108)
Log total fixed asset value	.091***	.093***	.056***	.029*
	(.018)	(.021)	(.017)	(.016)
Income per cap/poverty line	-.002	-.003	-.001	-.001
	(.004)	(.003)	(.003)	(.001)
Household size	.002	-.005	.0003	-.002
	(.009)	(.010)	(.006)	(.004)
Female household head	-.033	-.033	-.030	-.014
	(.044)	(.047)	(.029)	(.018)
Household head's education	.009**	.013***	.005	.004
	(.004)	(.005)	(.003)	(.003)
Has salary	.135***	.186***	.099***	.104**
	(.029)	(.034)	(.030)	(.044)
Years in village	-.0004	.0002	-.0003	.0000
	(.0009)	(.001)	(.0006)	(.0004)
Log distance to bank	-.118***	-.093	-.070*	-.023
	(.042)	(.059)	(.036)	(.028)
Village has Bank	.037	.056	.036	.025
	(.035)	(.058)	(.027)	(.026)
Population Density	-.0009***	-.001***	-.0009***	-.0006**
	(.0003)	(.0005)	(.0002)	(.0003)
Mean income/povline in village	.037***	.032***	.023***	.011*
	(.007)	(.010)	(.006)	(.006)
Village has registered council	.025	.111	.016	.030
	(.031)	(.075)	(.023)	(.027)
Mean years land is held in village	.0001	.001	.002	.0005
	(.003)	(.005)	(.002)	(.002)
With Subdistrict Dummies	*No*	*Yes*	*No*	*Yes*
N	1270	1193	1269	1269
Log-Likelihood or Pseudo LL	-633	-584	-1244	-1021
Pseudo R^2	.174	.21		
ρ			-.307	-.353
Wald test of $\rho = 0 \chi^2(1)$			3.79	3.39
P-value			.052	.066

Note: Robust standard errors reported in parentheses

Table 10: Marginal Effects at Mean

Dependent Variable = FormalBank	Probit	
	(1)	(2)
Log land value w/title	.018***	.017***
	(.004)	(.004)
Log land value w/other document	.011***	.006
	(.004)	(.005)
Log other assets	.007***	.010***
	(.003)	(.003)
Income per cap/poverty line	.0009	.0002
	(.004)	(.004)
Household size	.004	-.004
	(.009)	(.010)
Female household head	-.027	-.023
	(.044)	(.048)
Household head's education	.012***	.017***
	(.004)	(.005)
Has salary	.118***	.173***
	(.029)	(.034)
Years in village	-.0002	.0007
	(.0009)	(.001)
Log distance to bank	-.118***	-.104*
	(.042)	(.058)
Village has bank	.051	.065
	(.035)	(.059)
Population density	-.0009***	-.001***
	(.0003)	(.0005)
Mean income/povline in village	.041***	.040***
	(.007)	(.010)
Village has registered council	.034	.063
	(.031)	(.082)
Mean yrs land is held in village	.002	.003
	(.003)	(.005)
With Subdistrict Dummies	No	Yes
N	1270	1193
Log-Likelihood	-647.558	-594.277
Pseudo R^2	.156	.196

Note: Robust standard errors reported in parentheses

Table 11: Marginal Effects on Hypothetical Credit Supply

Dependent Variable = Feasible	Probit		Bivariate Probit	
	(1)	(2)	(3)	(4)
Titled	.023	.061	.108	.314**
	(.033)	(.042)	(.088)	(.160)
Log total fixed asset value	.123***	.126***	.114***	.115***
	(.020)	(.025)	(.019)	(.037)
Income per cap/poverty line	.035***	.035***	.034***	.038***
	(.008)	(.008)	(.008)	(.009)
Household size	.008	.005	.007	.005
	(.009)	(.010)	(.009)	(.012)
Female household head	.031	.045	.025	.041
	(.049)	(.051)	(.048)	(.060)
Household head's education	.014***	.017***	.013***	.015**
	(.004)	(.005)	(.004)	(.006)
Has salary	-.195***	-.461***	-.157***	-.174***
	(.031)	(.037)	(.038)	(.042)
Years in village	-.001*	-.002*	-.001	-.002**
	(.0009)	(.001)	(.0009)	(.001)
Log distance to bank	-.024	-.075	-.011	-.030
	(.035)	(.051)	(.036)	(.069)
Village has Bank	.006	.054	.010	.076
	(.038)	(.057)	(.038)	(.070)
Population Density	-.0006*	-.001**	-.0008*	-.002***
	(.0003)	(.0005)	(.0004)	(.0006)
Mean income/povline in village	.009	.019	.007	.018
	(.008)	(.013)	(.008)	(.015)
Village has registered council	-.008	.249*	-.009	.257**
	(.031)	(.137)	(.031)	(.131)
Mean years land is held in village	-.001	.005	.0007	.006
	(.003)	(.005)	(.003)	(.006)
With Subdistrict Dummies	No	Yes	No	Yes
N	1252	1213	1251	1251
Log-Likelihood or Pseudo LL	-661	-570	-1256	-998
Pseudo R^2	.167	.267		
ρ			-.192	-.490
Wald test of $\rho = 0 \chi^2(1)$			1.04	1.99
P-value			.308	.158

Note: Robust standard errors reported in parentheses

Table 12: Hypothetical Credit Supply

Dependent Variable = LogFeasibleLoanAmts	OLS	Tobit MFX
	(1)	(2)
Titled	.333	.294
	(.352)	(.314)
Has had loan from formal bank	2.465***	2.173***
	(.304)	(.266)
Log total fixed asset value	1.137***	.977***
	(.143)	(.129)
Income per cap/poverty line	.103***	.08***
	(.03)	(.026)
Household size	.003	-.006
	(.081)	(.074)
Female household head	.569	.537
	(.44)	(.396)
Household head's education	.138***	.129***
	(.04)	(.036)
Has salary	-1.666***	-1.524***
	(.295)	(.268)
Years in village	-.024***	-.021***
	(.008)	(.008)
Log distance to bank	-.457	-.446
	(.472)	(.425)
Village has bank	.636	.651
	(.502)	(.448)
Population density	-.007	-.009**
	(.004)	(.004)
Mean income/povline in village	-.06	-.055
	(.092)	(.081)
Village has registered council	-.272	-.422
	(.787)	(.691)
Mean yrs land is held in village	.015	.016
	(.042)	(.038)
N	1252	1252
Censored Observations		437
Adjusted/Pseudo R^2	.496	.085

Note: Marginal effects for Tobit are conditional on being uncensored.

All with subdistrict fixed effects.

Table 13: Formal Credit Demand of the "Unbanked"

Dependent Variable = LogDesiredLoanAmts	OLS	Heckman
	(1)	(2)
Titled	-.100	.042
	(.097)	(.107)
Log total fixed asset value	.372***	.487***
	(.051)	(.056)
Income per cap/poverty line	.041***	.041***
	(.009)	(.011)
Household size	.056**	.053**
	(.024)	(.025)
Female household head	-.170	-.166
	(.154)	(.157)
Household head's education	.054***	.073***
	(.011)	(.013)
Has salary	-.234**	.005
	(.092)	(.100)
Years in village	-.003	-.002
	(.002)	(.003)
Population density	-.002	-.003**
	(.001)	(.002)
Mean income/povline in village	.027	.071**
	(.031)	(.031)
Village has registered council	.379	.599**
	(.264)	(.249)
Mean yrs land is held in village	.011	.008
	(.012)	(.013)
N	773	764
Adjusted R^2	.496	
Censored Observations		371
ρ		-.850
$\chi^2(1)$ LR test of $\rho = 0$		41.76
P-value		.000
Log-likelihood		-1495

Note: Robust standard errors reported in parentheses.

All with subdistrict fixed effects.

Table 14: Loan Size

Dependent Variable = LogFormalLoanAmts	OLS	IV	Heckman	Heck-IV
	(1)	(2)	(3)	(4)
Titled	.276*	-1.942	.355**	.659*
	(.154)	(1.265)	(.161)	(.372)
More than one past loan	.247**	.397**	.254**	.246**
	(.118)	(.165)	(.111)	(.112)
Log total fixed asset value	.026	.177	.104	.254*
	(.06)	(.111)	(.074)	(.132)
Income per cap/poverty line	.039***	.059***	.038***	.035***
	(.013)	(.019)	(.012)	(.012)
Household size	-.046	-.031	-.056	-.068*
	(.035)	(.043)	(.035)	(.035)
Female household head	.086	.283	.032	-.037
	(.206)	(.27)	(.199)	(.204)
Household head's education	.026	.035*	.033*	.056**
	(.016)	(.02)	(.017)	(.024)
Has salary	-.015	-.129	.146	.455
	(.119)	(.157)	(.146)	(.278)
Years in village	-.008**	-.005	-.008**	-.008**
	(.004)	(.005)	(.004)	(.004)
Population density	-.001	.002	-.002	-.004
	(.002)	(.003)	(.002)	(.003)
Mean income/povline in village	.005	.013	.028	.073
	(.035)	(.042)	(.036)	(.049)
Village has registered council	.288	.728*	.391	.57*
	(.304)	(.44)	(.295)	(.31)
Mean yrs land is held in village	.008	-.006	.012	.014
	(.019)	(.024)	(.018)	(.018)
Inverse Mills Ratio			.424	1.281*
			(.253)	(.067)
N	368	368	364	364
Censored Observations			899	899
Joint F-test for instruments in first stage		3.49		51.15
Sargan (overident. test for all instruments)		.014		.906
$\chi^2(1)$ P-value on Sargan		.907		.341
ρ			.450	
$\chi^2(1)$ LR test of $\rho = 0$			1.66	
P-value			.198	
Adjusted R^2	.119			

Note: All with subdistrict fixed effects

Table 15: Security Offered

Dependent Variable = LogFormalLoanAmts	OLS	Heckman
	(1)	(2)
Titled	.116	.230**
	(.114)	(.117)
Land title security	.234	.228
	(.155)	(.147)
Fixed Income	.101	.073
	(.152)	(.143)
Other security	.006	-.059
	(.203)	(.187)
No Security	-.443**	-.473***
	(.202)	(.183)
More than one past loan	.135	.144*
	(.083)	(.077)
Log total fixed asset value	.042	.075***
	(.025)	(.028)
Income per cap/poverty line	.035***	.035***
	(.008)	(.008)
Household size	.034	.015
	(.026)	(.028)
Female household head	-.092	-.132
	(.140)	(.141)
Household head's education	.024**	.049***
	(.012)	(.014)
Has salary	-.158*	.025
	(.091)	(.104)
Years in village	-.009***	-.009***
	(.003)	(.003)
Population density	-.001	-.002
	(.002)	(.002)
Mean income/povline in village	.006	.051*
	(.025)	(.027)
Village has registered council	.062	.095
	(.201)	(.206)
Mean yrs land is held in village	.028**	.025*
	(.014)	(.014)
N	365	363
Censored Observations		997
ρ		.760
$\chi^2(1)$ LR test of $\rho = 0$		2.54
P-value		.111
Adjusted R^2	.247	
Log-likelihood		-936

Note: Missing category is offering other land documents security

All with subdistrict fixed effects.

Table 16: Marginal Effects on Pr(FormalBank=1) at Median from Bivarite Probit

Dependent Variable = FormalBank	Low % Titled	Medium %	High %
	(1)	(2)	(3)
Titled	-.012	.543***	-.143
	(.109)	(.048)	(.431)
Log total fixed asset value	.101***	.004	.123**
	(.033)	(.025)	(.050)
Income per cap/poverty line	.006	-.010	-.0004
	(.005)	(.012)	(.008)
Household size	.015	-.004	-.027
	(.013)	(.020)	(.019)
Female household head	-.074	.112	-.038
	(.060)	(.075)	(.075)
Household head's education	.009*	.008	.001
	(.006)	(.009)	(.008)
Has salary	.053	.121*	.179**
	(.046)	(.063)	(.075)
Years in village	-.0004	-.001	-.00003
	(.001)	(.002)	(.002)
Log distance to bank	-.203**	.011	.028
	(.083)	(.083)	(.124)
Village has bank	-.065	-.458***	.008
	(.064)	(.151)	(.081)
Population density	-.002***	-.0002	.0004
	(.0004)	(.0009)	(.0007)
Mean income/povline in village	.025	.088***	.064
	(.016)	(.028)	(.054)
Village has registered council	.081**	.374***	-.007
	(.041)	(.139)	(.177)
Mean yrs land is held in village	.004	-.014	-.003
	(.004)	(.010)	(.015)
N	613	196	186
Log pseudo-likelihood	-560	-172	-160
ρ	.153	0	.517
Wald test of $\rho = 0 \chi^2(1)$			
P-value			
Pr(formalbank=1)	.230	.740	.160

Note: Robust standard errors reported in parentheses