University of Sussex
Business, Management & Economics

Economics Department Working Paper Series

No. 22-2010

How Does Land Title Affect Access to Credit? Empirical Evidence from an Emerging Economy

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Abstract: This paper studies the effects property rights on credit access using a unique data set based on a Brazilian land-titling program affecting 85,000 families. The causal role of land title is isolated by comparing two communities in Osasco, where some residential units are allocated titles and others not. Survey data is collected from residents before and after the title granting. In order to estimate land title impact, we have undertaken the Difference-in-Differences methodology. Some of our results suggest that land title increases the access to credit for about 60%. Additionally, land title impact by gender and credit type is presented and also positive.

JEL: D23, O43, J22.

Keywords: Property Rights, Land Title, Credit, Difference-in-Difference, and Difference-in-Differences Matching Estimator

Introduction

The role played by private rights in the economic development of the Western world has been powerfully documented by economic historians such as North & Thomas (1973). The fragility of property rights is considered a crucial obstacle for economic development (NORTH, 1990). The main argument is that individuals under-invest if others can seize the fruits of their investment (DEMSETZ, 1967). The empirical that has supported such relationship can be found, for example, in Torstensson (1994) and Goldsmith (1995) which found a significantly positive association between secure property rights and economic growth.

In such context, strengthening economic institutions is widely argued to foster investment in physical and human capital, bolster growth performance, reduce macroeconomic volatility and encourage an equitable and efficient distribution of economic opportunity (ACEMOGLU *et al*., 2002). In the current developing world scenario, a pervasive sign of feeble property rights is the 930 million people living in urban dwellings without possessing formal titles to the plots of land they occupy (United Nations, Habitat Report, 2005). The lack of formal property rights constitutes a severe limitation for the poor. The absence of formal titles creates constraints for the poor on using land as collateral to access credit markets (BESLEY, 1995), an issue that could be crucial as would allow them to escape poverty.

De Soto (2000) emphasizes that the lack of property rights limits the transformation of the wealth owned by the poor into capital. Proper titling could allow the poor to collateralize the land. Field & Torero (2002) mentioned that this credit could be invested as capital in productive projects, promptly increasing labor productivity and income. Furthermore, Besley & Ghatak (2008) defend significant economy enhancement under property rights given the partial elimination of missing markets issues, basically credit and insurance, faced by the poor. Among policy-makers as well, property titling is also increasingly considered as one of the most effective forms for targeting the poor and encouraging economic growth (BAHAROGLU, 2002; BINSWANGER *et al*., 1995).

The most famous example in Latin America is Peru. The Peruvian government issued property titles to 1.2 million urban households during the 1990's. In Asia, millions of titles are being issued in Vietnam and Cambodia[1].

In Brazil, the Federal Government announced, in 2003, a massive plan to title 750,000 families from all over the country. This program called "*Papel Passado*", and since launched has spent US$ 15 million per year from the federal budget, and provided titles to over 85,000 families and reaching 49 cities in 17 different Brazilian states. Its official goal is "*to develop*

[1] As shown in the The Economist magazine in the March 15, 2007 edition. The same edition has on the front page: "*Property Rights: China's Next Revolution*". The survey shows that China intends to put into place the most ambitious land-titling program in the world's history and includes this initiative as one of the main points of the Chinese economic development model.

land titles in Brazil and promote an increase in the quality of life for the Brazilian population" (see ASSOCIAÇÃO DOS NOTÁRIOS E REGISTRADORES DO BRASIL - ANOREG, 2007). However, the country still faces a very difficult scenario regarding land property rights. The Brazilian government estimates that approximately 7 million people live under illegal urban conditions (IB`GE, 2008) see Table 1 below.

Table 1 – Land Title Figure – Brazil 2007

	Untitled	***Titled***	**Total**
Rural	2,014,497	19,989,515	22,004,012
Column %	27.43	16.05	16.69
Urban	5,328,763	104,540,315	109,869,078
Column %	72.57	83.95	83.31
Total	7,343,260	124,529,830	131,873,090
%	100.00	100.00	100.00

Source: PNAD, 2008, Brazil.

The main objective of paper is to measure the impact of property rights on credit access on an emerging economy such as Brazil. It analyzes credit access response to exogenous changes in formal ownership status.

Positive effects of land titling have been documented by several studies. A partial listing includes Jimenez (1985), Alston *et al*. (1996), Lanjouw and Levy (2002) on real estate values. Besley (1995), Jacoby *et al*. (2002), Brasselle *et al*. (2002), Do and Iyer (2003) on agricultural investment. Place and Migot-Adholla (1998), Carter and Olinto (2003), Field and Torero (2002) on credit access, labor supply, housing investment and income. Deininger and Ali (2008), Goldstein and Udry (2008) on land title and productivity. Additionally, Deininger and Feder (2009) provide the evidence towards the importance of public interventions to extend land title to vulnerable households.

The findings in Besley (1995) are ambiguous - land rights appear to have a positive effect on agricultural investment in the Ghananian region of Angola but less noticeable impact on the region of Wassa. Using a similar approach, Jacoby *et al*. (2002) find positive effects in China, where as Brasselle *et al*. (2002) find no effects for Burkina Faso. Allendorf (2007) finds a positive correlation between land title and women empowerment in Nepal. Field and Torero (2002), in Peru, exploit timing variability in the regional implementation of the Peruvian titling program using cross-sectional data on past and future title recipients midway through the project, and also find positive effects, particularly in labor supply, credit access and housing investments. In Brazil, Andrade (2006) using cross-section data from a sample of 200 families of the *Comunidade do Caju*, an urban poor community in Rio de Janeiro, demonstrates an increase effect on the income of those that had received the land title.

Furthermore, specific about land title and credit access, for Besley and Ghatak (2009, pp. 2), *De Soto effect* is *"the idea that better access to collateral increases credit availability"*. Feder and Nishio (1999) also state that property rights allow households to use land as collateral to access credit. On the same subject, Besley (1995) considers that land titling would be expected to play a critical role in terms of reducing imperfections in credit markets. Additionally, *Gosh et al.* (1999) argues that land title allows vulnerable households to use credit markets to smooth consumption when affected by negative shocks. On the other hand, Feder and Nishio (1999) have demonstrated that well-functioning financial markets are required, which can extend long-term credits when land is used as collateral (most likely through institutional channels), in order to maximize land title effects. If various regulations restrict or disallow the enforcements of collateral or if the legal and enforcement administration for collateral contracts is too cumbersome to be effective, land registration systems will not provide benefits which are linked to the credit market.

This paper presents mainly three contributions. First, although land title would eliminate the lack of collateral - key aspect to credit analysis (Morduch, 1998), lack of evidence to support such assumption from experimental or quasi-experimental estimates is available globally and none particularly in Brazil. Secondly, a common obstacle, faced by all studies mentioned above, is how to measure the influence of land title considering the potential endogeneity of ownership rights as pointed by Demsetz (1967), Alchian and Demsetz (1973)[2]. In order to isolate the causal role of land title, this study uses a quasi-natural experiment, basically a comparison between two neighboring and very similar communities in the City of Osasco (a town with around 654,000 people located in metropolitan area of São Paulo - Brazil metropolitan area)[3]. One of them, *Jardim Canaã*, received titles in 2007 (all households received the land title) and the other, *Jardim DR*, will be part of the program scheduled in 2012, and for that reason it is a natural control group. Officially, the decision to have *Canaã* as the starter was random. Similar situation has also occurred, as Skoufias (2001) pointed, in Mexico for the evaluation of an income transfer initiative called PROGRESA, where some localities were randomly selected for participation (treatment localities) while the rest were introduced into the program at later phases (control localities). One of the benefits of having assignment at the locality level is to minimize the chances spillover effects between treated and untreated individuals at the same locality. Different from the previous studies, our analysis is based on quasi-natural two-stage survey, from *Jardim Canaã* and *Jardim DR*, both with focus on the property right issue. The first stage of the survey was collected in March 2007, before titles had been issued to *Jardim Canaã*, and the second collected in August 2008, almost one year and half after the titles. As Ravallion *et al.* (2005) argue, the best ex-post evaluations are designed and implemented *ex-ante* – often side-by-side with the program itself.

Third, as Field and Torero (2002) describes, presumably because of historic interests in agricultural investment and related politics of land reform, the majority of both academic and policy attention to property rights has centered on rural households property rights. Field (2007) applies the variable of interest related to credit access and investments on household's own land. Nevertheless, in most of the developing world, the population - and in particular the impoverished population – is increasingly urban. The empirical evidence for rural areas has not supported the thesis that the poor (or vulnerable) households become less credit constrained after acquiring land titles (see e.g. Field and Torero, 2004). The main focus of this paper main focus is urban.

The paper is organized as follow: Section 1 describes the theoretical framework, Section 2 presents the research methodology, Section 3 presents the empirical strategy that

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[2] Direct evidence of this is provided by Miceli *et al*. (2001), who analyze the extent of endogeneity of formal agricultural property rights in Kenya.

[3] Osasco is part of the *Papel Passado*'s map and has 6,000 families informally living on urban property.

identifies the average effect of the program, Section 4 that brings a discussion of the empirical results and Section 5 concludes.

1. Land Title and Credit: Theoretical Framework

Various studies provide basic framework to understand the dynamics between land title and credit access and its consequences (see Feder and Feeny (1991), Carter and Olinto (2003), Boucher, Carter and Guirkinger (2008), Besley and Ghatak (2009)).

Feder and Feeny (1991) argue that rural titling will improve the borrower's well-being (measured in terms of utility) by reducing information asymmetry under credit contracts. The model assumes a two-period horizon in which decisions regarding investment, land acquisition and consumption are made in the first period. Production, therefore, will be determined in the second period. Utility is a function of consumption in the first period; the production per unit of land, *y*, is a function of the capital-to-land ratio, where capital is assumed to be a *numeraire* good with infinite supply elasticity. The model also assumes constant returns to scale in production and that the credit is rationed for all farmers. The credit ration, *S*, is a positive function of the ownership security of the land, i.e.: $S = s(\phi)PT$, where ϕ is an exogenous risk of land expropriation, and *PT* is the nominal value of the land. By assumption, $s^{'} < 0$ and $0 < s < 1$. Thus, the amount of credit a farmer can borrow from a lender depends positively on the market price of the land and negatively on the risk of expropriation. The farmer borrower issue is given by:

$$\max_{Co,T,k} U(C_0) + \underbrace{\left[\underbrace{Ty(k)}_{Y(K)} + PT - \phi(Ty(k) + PT) - \underbrace{(1+r)S}_{Opportunity\,\cos t} \right]}_{U(C_1)} \tag{1}$$

$$s.t: W_0 + \underbrace{S}_{s(\phi)PT} = \underbrace{kT}_{K} + PT + C_0$$

Replacing the budget constraint in the objective function, the optimization problem becomes unconstraint and can be solved for two unknowns, *k* and *T*. The risk of expropriation interferes directly with the budget constraint and with the utility function. Thus the property right to land is expected to reduce the risk of expropriation and therefore increase the budget constraint and the borrower's well-being. According to Feder and Feeny (1991, pp. 150), *"The price of land includes a premium reflecting the additional income due to the credit which can be acquired by pledging the land, and which in turn increases, at the margin, the farmer's utility"*.

The model proposed by Besley and Ghatak (2009) is based on the assumption that the borrower has an illiquid wealth, *w*, that can be pledged as collateral in credit contracts since the property rights of this wealth are well defined and enforced. Letting τ be a parameter capturing how well defined is a property right, with $\tau = 0$ meaning perfect property right and $\tau = 1$ total absence of property right. Hence, the fraction of illiquid wealth which can be pledged as collateral is given by $(1-\tau)w$, the *effective wealth* owned by the borrowers. An issue emerges given that the lender (principal) cannot observe the effort level chosen by the borrower (agent). At this stage, the critical role played by property rights becomes evident. Even if the lender cannot observe the agent's level of effort, he still can extend the contract to the borrower if she decides to pledge her effective wealth as collateral. However, the value of the parameter τ should be at a certain level to make the effective wealth an asset to be accepted by the lender. Besley and Ghatak (2009, pp.7) points that:

[the fact that the effort level is not contractible] would not be a problem if the borrower had sufficient wealth to act as a bond against non-repayment… Even if the borrower's liquid wealth is sufficient for this purpose, poorly defined property rights, as argued by De Soto (2001) may place a further limit.

Hence, an improvement in property rights level would increase the value of effective wealth and thus would give incentive to the borrower to exert more effort. In this case, Besley and Ghatak (2009, pp.14) argue that the lender would be allowed *to "offer a more efficient loan by reducing the spread between the repayment demanded from a successful project and the collateral offered"*.

Similar to Galiani and Schargrodsky (2005), this papers intend to provide additional input regarding land title and access to credit and not investigate household credit behavior as Field (2007) or any specific credit contract.

2. The Research Methodology

The Federal Government has chosen Osasco, as one of the cities to be part of the "*Papel Passado*" - a program that intends to provide land titles to families living under illegal conditions.

The city of Osasco has 30,000 people (about 6,000 families) living under informal conditions, which represents almost 4.5% of its total population (ASSOCIAÇÃO DOS NOTÁRIOS E REGISTRADORES DO BRASIL - ANOREG, 2007). The program timetable for Osasco establishes that all the communities living in illegal condition will be part of the "*Papel Passado*" during the period between 2007 and 2014[4]. Officially, as released by the Osasco City Hall, the localities priority follows random criteria. However, unofficial sources from local communities in Osasco express the feelings that maybe a "political" agenda was present in the decision.

As stated Behrman and Todd (1999), randomization avoids the issue of selection bias that arises in non-experimental evaluations. Using this sample to evaluate policy ensures that the group that receives treatment is similar both in terms of observable and unobservable characteristics to the group that does not receive treatment.

The first locality to receive the land title was *Jardim Canaã*, in 2007, which has 500 families. The closest neighbor of *Jardim Canaã* is a community called *DR*, with 450 families. The *DR*'s households will be part of the "*Papel Passado*" program schedule in 2012. Hence, the data of this particular paper consist of 326 households distributed across *Jardim Canaã* and *DR* (185 from *Jardim Canaã* and 141 from *DR*).

[4] Given that fiscal resources are limited, all communities are not receiving the land title at the same time.

The master list used to sample the families from those localities was provided by the Osasco municipality and 2nd *Cartório de Osasco* (2nd Osasco's Office of Registration). Both entities have worked together to map and register all families from that particular area.

2.1 Minimizing Selection Bias Concerns

Jardim Canaã and *DR* seem to be very similar economic and social characteristics at a first look. *Canaã* and *DR* are not only official neighbors but there is no physical "borderline" among them, both are geographically united.

Regarding selection bias, Behrman and Todd (1999) also mentioned that randomization can avoid the problem of self selection. However, some other types of biases may occur in randomized evaluations. The authors listed some potential and types source of bias such as randomization bias, contamination and attrition. Given the nature of the research conducted in the city of Osasco, it is relevant to discuss the aspects that may minimize the various types of bias related to the data collected. Based on the first survey, 95.0% of the survey participants (from *Canaã* and *DR*) did not expect to receive any land title, i.e., they were not aware of "*Papel Passado*" and the meaning of it - it is important to emphasize an aspect that helps minimize the *randomization bias*[5]. It tends to avoid a potential behavior deviation from households included in the program.

Secondly, *contamination bias*[6] is avoided in the case of Osasco, the control group residents could not benefit from the program outside the treatment locality. And, there is no alternative form of formal land title program as well. Third, the land title program also does not provide a drop out option. After receiving the land title, the household can sell the property and move out of the locality. However, he was already affected by the program, reducing the probability of *attrition bias*[7].

Another aspect to be mentioned about the data collected is that it produced unique match within the same geographic area which helped to ensure that comparison units come from the same economic environment. For example, both are not only neighbors but also located 2.5 miles from downtown Osasco and that provides the exactly same access to the

[5] *Randomization bias* occurs when the introduction of randomization changes the way the social program operates, so that results obtained from the experimental evaluations may not be generalizable to a non-experimental context. For example, a common problem with implementing randomized trials for social program evaluations is that the need to recruit a greater number of applicants induces program administrators to change program admissions standards. A similar problem occurs if individuals are aware of the randomized evaluation and choose not to apply to the program given the lower chance of receiving benefits. In both of these cases, results obtained from the randomized evaluation may not be generalizable to a context where the program is not being implemented as a randomized trial.

[6] *Contamination bias* occurs if members of the randomized-out control group seek out and receive alternative forms of treatment. This is usually a problem only when there are close substitutes to the program. If contamination occurs, then the impact of the program that is estimated actually corresponds to the effect of the social program relative to other alternatives.

[7] A*ttrition bias* occurs if some members of the treatment group drop out of the program. If the purpose of the evaluation is to estimate the effect of receiving some treatment (for example, the effect of taking some drug over a length of time), then attrition bias can pose a major problem. It is usually nonrandom and can compromise the benefits of randomization.

Osasco's mainly economic center. Rubin and Thomas (2000) indicate that impact estimates based on full (unmatched) samples are generally more biased, and less robust to miss-specification of the regression function, than those based on matched samples.

The data were produced from a two-stage survey focused on the property right issue. However, to further minimize bias, the researcher have prepared and conducted does not provide any direct information for the households on what exactly the research is about. Officially for the people interviewed, the study was about general living conditions in the City of Osasco.

The survey is based on a 39 questions questionnaire applied to the 326 families randomly sampled[8]. The survey instrument, in many of its questions and methodologies, closely mirrors the IBGE (PNAD - *Pesquisa Nacional de Amostra de Domicílios do Instituto Brasileiro de Geografia e Estatística*) in content, and therefore contains a variety of information on household and individual characteristics. In addition, there are six questions designed to provide information on a range of economic, social and personal benefits associated with property formalization.

The first stage of the survey was conducted in March 2007, before titles had been issued to *Jardim Canaã*, and the second collected in August 2008, almost a year and a half after the first titles had been issued (with exactly the same households and with 98.0% recall). The reason for the time gap was to give the an opportunity to all the households interviewed during the first survey stage to have at least 1 year with the land title. The exactly dates that each household interviewed received the title were provided by the 2nd *Cartório de Osasco* (2nd Osasco's Office of Registration) along with the formal authorization from the Osasco's City Hall to conduct the research.

The study also tracks the households that moved outside both communities to check if the land title effect stands. From the original sample only 8.0% of the households that received the land title have moved away from *Canaã*[9]. From the control group, only 1 household (out of 140) has moved during the same period. On the other hand, such environment of circumstances should potentially increase the chance of *spillover effects* (an economic activity or process upon those who are not directly involved in any event) considering that the control group is so close to treated group, it is likely that they were affected as well. However, only one year may not full capture that effect. Furthermore, even if that was clearly the case, the estimates can be considered as constituting the lower bound impact of the program.

A technique from Bolfarine and Bussab (2005) was used to choose randomly 326 sample households inside the localities. The approach consists of choosing the first 150 households (from the *Canaã* and *DR*) that have the closest birth dates (day and month) in comparison with the three field researchers that conducted the survey interviews[10]. Each

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[8] Questionnaire available upon request.

[9] One of the main concerns from local authorities in Osasco was that most citizens would receive the land title, sell the property right away and return to an informal living conditions and that not has been materialized.

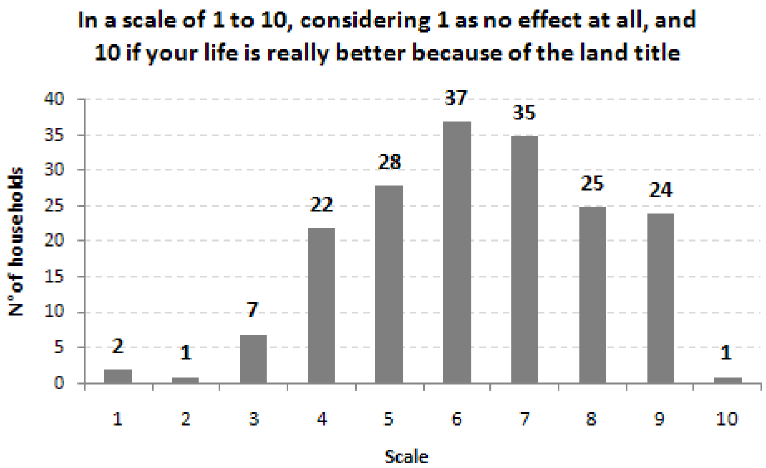
[10] The field researchers are, by the way, not from Osasco.

researcher got 50 names initially as first base. Additionally, after reaching each of those households, they could go and pick the third and the fifth neighbor on the right hand side.

2.2 The Data

Based on the first survey, 95.0% of its participants were not aware about receiving land titles and the meaning of it which re-enforce those arguments presented above and helps to minimize potential self selection issues. From the second stage of the survey, most of households that received the land title felt that it improved their lives (see Figure I) even if they had not previously expected the land title.

Figure I: How land title affected household's life?



Source: Research from the Osasco Land Title Survey – 2008

Descriptive statistics for data in Osasco for both groups combined (treated and control), is presented on Table 2 below.

Table 2 Descriptive Statistics – Both Groups (2007)

Variables	No. *Obs*	Mean	Standard Deviation	Minimum	Maximum
Gender (=1 if Female)	305	0.33		0	1
Ethinicity (=1 if African Brazilian)	305	0.66		0	1
Marital status (=1 if married)	305	0.63		0	1
Mean Age	305	40.8	14.68	17	85
Weekly hours of adult work	305	10.27	12.28	0	56
Child labor Weekly hours	305	5.5	11.15	0	56
Years of Education (Family Head)	305	7.25	4.34	0	15
Monthly In(income) (currency BRL*) per capita	305	389.35	796.84	0	8740
TV (=1 if have)	305	0.73		0	1
DVD (=1 if have)	305	0.56		0	1
Radio (=1 if have)	305	0.53		0	1
Car (=1 if have)	305	0.36		0	1
Wash machine (=1 if have)	305	0.76		0	1

Refrigerator (= 1 if have)	305	0.79		0	1
Freezer (=1 if have)	305	0.56		0	1
Informal (= 1 if Informal at work)	305	0.78		0	1
Credit (=1 if have access)	305	0.44		0	1
Child (=1 for at least one child in the family)	305	0.81		0	1

Source: Research from the Osasco Land Title Survey; *Currency 12/31/2008, 1 USD = 1.75 BRL, Central Bank of Brazil.
Note: The poverty line applied to estimate the poverty measures corresponds to the half of the Brazilian minimum wage in 2007, i.e., BRL 180. Such is common practice locally given that there is no official poverty line for the Brazilian economy as shown by Rocha (2003)

Galiani and Schargrodsky (2005) found a positive and significant impact of land title on housing investment in a suburban area of Buenos Aires (Argentina's capital city), the authors found a marginal effect on access to credit. However, variables "*access to credit*" was applied as the following: (i) credit card and banking account; (ii) non-mortgage loan received; (iii) informal credit (cooperatives and labor unions); (iv) department store credit; and (v) mortgage loan received. The estimates were statistically significant only for the last case mentioned (the effect magnitude was 4%). The survey questionnaire applied in this paper contained a set of questions that can be related either directly or indirectly to access to credit and applies some of Galiani and Schargrodsky (2005) variables as illustrated in Table 3.

Table 3 Variables - *Access to Credit (2007)*

Variables	***No. Obs***	**Mean**	**Standard Deviation**	**Minimum**	**Maximum**
Credit Type (=1 if have access to credit)*	305	0.338	0.474	0	1
Personal Loan (=1 if used)	305	0.085	0.280	0	1
Pre-Check (*Cheque Pré Datado*) (=1 if used)	305	0.066	0.248	0	1
Credit card (=1 if used)	305	0.243	0.429	0	1
Invoice (*Boleto)* (=1 if used) **	305	0.082	0.275	0	1
Credit from Department Stores (=1 if used)	305	0.354	0.479	0	1

Source: Research from the Osasco Land Title Survey.
*means if the household used one of the following, at least, to purchase during the previous year: a) credit card, b) Pre-check, checked to be deposited 30 days ahead, known as *Cheque Pré-Datado* in Brazil c) credit from department stores, (d) bank personal loans and payroll credit (which credit installments are automatically deducted from the payroll – in Brazil denominated *Crédito Consignado*). ** if households utilize future installments to purchase instead of advanced cash.

Table 4 provides an overview regarding each household borrowing behavior during the previously 30 days before the survey application. Furthermore, it shows the correlation among the variables applied and demonstrates that those are significant at 5% for most of the cases.

Table 4 Spearman Correlation - *Access to Credit (2007)*

	Credit Type	***Borrow***	***Pre-check***	***Credit card***	***Boleto***	***Credit DS****
Credit Type	1					
Borrow (=1 if borrowed)**	0.0799	1				
Pre-Check (=1 if used)	0.2590*	0.3460*	1			
Credit card(=1 if used)	0.4853*	0.2107*	0.4680*	1		
Invoice (*Boleto)* (=1 if used)	0.0899	0.5936*	0.5486*	0.2491*	1	

Credit Department Stores (=1 if used)	0.9644*	0.0686	0.3024*	0.5245*	0.1786*	1

Source: Research from the Osasco Land Title Survey: *Significant at 5%. ** (=1 if the household has done any loan from a bank and/or friends/relatives during the previous 30 days from the survey application),
*** Department Store

Table 5 summarize the *access to credit* variables distribution, which equals 1 if the household reported having used at least one of those credit sources (and 0 otherwise).

Table 5 Variable Distribution - *Access to Credit (2007)*

	Frequency	Percent (%)
No Access to Credit	170	55.74
Access to Credit	135	44.26
#Observations	305	100.00

Source: Research from the Osasco Land Title Survey.

The Table 6 compares the experimental dataset with the comparison group drawn from PNAD (2008) - Brazilian annual household survey. As an example, some variables are similar, such as informality proportion, age and household income *per capita*.

Table 6 Test and Z-score for the difference of means for covariates in 2007

	Mean Control (A)	Mean Treatment (B)	Test: A – B ≠0 *p-value*
Gender (=1 if female)	0.31	0.34	0.48
Ethnicity (=1 if African Brazilian)	0.69	0.64	0.43
Marital status (=1 if married)	0.61	0.65	0.52
Age of the head	42.60	39.40	0.06*
Weekly hours of adult work	10.10	10.40	0.81
Weekly hours of child labor (> 16 years old)	8.35	3.30	0.00***
Years of education (family head)	5.00	9.00	0.00***
Monthly income (currency BRL[a]) per capita[b]	553.10	255.80	0.00***
Wealth index	1.12	-0.94	0.00***
Informal worker (=1 if informal)	0.94	0.65	0.00***
Access to credit (=1 if have)	0.44	0.45	0.88
Number of children (> 16 years old)	0.78	0.81	0.46
Observations (households)	168	137	

Source: Research from the Osasco Land Title Survey and Central Bank of Brazil
Notes: *, **, *** rejection of the null hypothesis of equal mean at 10, 5, and 1 percent respectively.
[a] Currency exchange rate in 12/31/2008, 1 USD = 1.75 BRL (Brazilian Reais).

[b] Monthly income per capita is calculated diving monthly income by the number of residents.

Furthermore, Table 6 mainly relates to the T test for the difference of means for covariates in 2007, comparing the control and treatment groups before the program. The initial basic results have demonstrated that treatment and control groups are not completely comparable. Some examples of such variables are: years of education (family head), monthly income, monthly income per capita and informality at work. These characteristics are similar from the PROGRESA case as found by Skoufias (2001) and Behrman and Todd (1999). The authors basically demonstrate that even being similar in terms of observables at the community level, when the comparison is applied at the family level it is not fully possible to address that both groups are completely comparable. Such explains the reason to include control variables instead of simply estimate the program impact through mean test, an approach that should be applied in case of perfect randomization.

The local reason for such difference can be explained basically by the fact that households with higher level of education, in Osasco, tend to have more access to formal jobs – see Zylberstajn and Neto (1999). In Brazil, formal employers, on top of cash salaries, tend to provide other perks that are not reflected in the cash payroll. For example, a formal employee usually has health care plan for the whole family, subsidize transportation support and meal plans. On the other hand, informal workers do not have those benefits and essentially relies only on cash income to compensate lack of perks. Basically, such particularity explains the reason that more educated households have lower income, i.e, lower cash income. On the other hand, informal workers are a relevant sub-sample from the research with a total of 233 households – 92% of the control group and 64% of the treated.

The wealth index was computed with the principal component analysis and it summarizes the stock of durable goods owned by the households. In the present case, the vector of durable goods includes TV, DVD, radio, car, wash machine, refrigerator and freezer. The descriptive stats suggest that the control group is slightly better off than the treatment group. The figure A.1 in annex illustrates the distribution of this variable. Hence, in order to support such explanation, Table 7 presents a correlation among the variables years of education (family head), monthly per capita income and informality. The outcome is clearly in line with the informal and formal reality of the households.

Table 7 Spearman Correlation

	Years of Education	Informality	Monthly Income per capita
Years of Education	1		
Informality	-0.1509*	1	
Monthly Income per capita	-0.2382*	0.2243*	1

Source: Research from the Osasco Land Title Survey
Note: *Statistically significant at 5%.

3. Empirical Strategy

3.1 The Difference-in-Difference Methodology (DD)

The econometric method applied was Difference-in-Difference Estimate, known as DIFF-in-DIFF or (DD), which consists of identifying a specific intervention or treatment (often a passage of a law), see Bertrand *et al.* (2004). Imbens and Wooldrige (2008) adds that

DD compares the difference in outcome after and before the intervention for groups affected by intervention to the same for unaffected groups.

Meyer (1995) implies that DD simplicity and its potential to circumvent many of endogeneity problems that typically arise when making comparison between individuals, helps to remove the bias that could be permanent differences between the two groups or an additive structure for potential outcomes in the no-treatment effect.

The DD estimates the following regression model that can be applied to identify the treatment effect on the outcome of interest.

$$Y_{ist} = \beta_0 + \beta_1 T_{st} + \beta_2 Time + \alpha_{DD}\left(T_{st} * Time\right) + X_{ist}^{'}\gamma + u_{ist} \qquad (2)$$

where Y_{ist} is the outcome variable of interest of *i-th* individual in the community *s* at time *t*, X_{ist} is the vector of observable characteristics of *i-th* individual in the community *s* which change through time, T_{st} is a dummy variable equal to 1 if the individual resides in the treated community (s=1) and 0 otherwise, *Time* is a *dummy* variable equal to 0 in 2007, baseline period, and equal 1 in 2008, and u_{ist} denotes the error term which is assumed to be independent of *X* and *T* (see Imbens and Wooldridge, 2008 and Meyer, 1995)[11].

The parameter of interest is the coefficient of the interaction term, $T_{st} * Time$, α_{DD}, which identifies the effect of the treatment on the treated. The causal effect identification on the outcomes variables relies on three assumptions:

(i) Selection for the treatment does not depend on unobservable individual and community characteristics which change overtime;

(ii) Difference between the treated and comparison groups would be the same in the absence of the program; i. e, there is a time invariant common effect; and

(iii) Treatment does not affect access to credit of households living in the neighbor areas. Hence, no *spillover effects* are present. The assumptions (i), (ii) and (iii) imply (3) and (4), i.e.:

$$E\left(u_{ist} \mid T, Time, X\right) = E\left(u_{ist}\right) = 0 \qquad (3)$$

and

$$\begin{aligned}&[E\left(Y_{ist} \mid T = 1, Time = 2008, X\right) - E\left(Y_{ist} \mid T = 1, Time = 2007, X\right)] - \\ &[E\left(Y_{ist} \mid T = 0, Time = 2008, X\right) - E\left(Y_{ist} \mid T = 0, Time = 2007, X\right)] = \\ &\left(\beta_2 + \alpha_{DD}\right) - \left(\beta_2\right) = \alpha_{DD}\end{aligned} \qquad (4)$$

The main objection regarding (3) is the self-selection (also known as *anticipation problem*). Such certainly would be an issue if households decided to demand credit given the expectation of receiving land title in the future. However, such does not apply for this particular case given that most lenders would not provide credit without a verifiable

[11] Once all households of the treated area received the title, *S* and *T* will be the same. Thus, from now on the subscript *s* will be omitted for the sake of simplicity.

guarantee, as stated by Feder and Nishio (1999) and also because, as mentioned previously, potential borrowers were not aware about the land title program.

Furthermore, regarding the second assumption (ii), in this research, control variables are used in order to account for differences between the two groups in the baseline (2007). On top of that, fixed effect estimator is applied to check results robustness given that the unobservable could be potentially different across groups but invariant through time.

The empirical analysis in this paper also deploys the method employed by Angelucci and Attanasio (2009). The method consists of making use of *propensity scoring* to focus on the analysis of households which are part of common support, i.e., households that are comparable in observable characteristics. Such approach is applied if evidence that the groups are not completely balanced in observables is present and, hence, would likely not display parallel trends in the absence of the program (see Abadie, 2005).

3.2 Identifying the Difference-in-Differences Estimator in a Balanced Sample

As stated above, the problem of interest involves estimating the average treatment effect on the treated (ATT). Hence, let Y_{1i} be the potential result of a household who received the treatment and Y_{0i} the potential result of a household who was not treated. The effect of the treatment on the treated can be consistently estimated since it is possible to understand what would have occurred to the treated if without treatment. Such is denominated the *fundamental problem of causal inference* (Holland, 1986), both results for the same household can be observed (see also Meyer, 1995 and Imbens and Wooldridge, 2008).

However, under a random treatment, ATT could be estimated through the difference of means between the potential outcome of treated and the potential outcome of the control group. For example, let T_i be a dummy which indicates the status of treatment, where $T_i = 1$ if the household is treated. Given that household treatment effect can be computed ($Y_{1i} - Y_{0i}$), the ATT is illustrated in terms of averages, i.e.

$$E[Y_{1i} - Y_{0i} \mid T = 1] = E[Y_{1i} \mid T = 1] - E[Y_{0i} \mid T = 1] = E[Y_{1i} \mid T = 1] - E[Y_{0i} \mid T = 0] \qquad (5)$$

so,

$$E[Y_{1i} - Y_{0i} \mid T = 1] = E[Y_{1i} - Y_{01}] = E[Y_{1i}] - E[Y_{0i}], \text{ for } Y_{1i,}Y_{0i} \perp T \qquad (6)$$

When the treatment and control groups are not very similar in observables characteristics, two conditions must be satisfied to the ATT be identified. These are the following:

(1) The *ignorability* of treatment (or *unconfoundedness*): $Y_{1i}, Y_{0i} \perp T \mid X$; and
(2) The common support condition (or overlap): $0 < \Pr(T_i = 1 \mid X = x) < 1$, for all *x*.

The first assumption states that there is a conditional independency between the treatment and the potential outcomes. In other words, Y and T are independent once X variables are controlled for. This is also known as conditional independence assumption.

The second assumption states that the conditional distribution (support) of the probability of participating into a program must contain treated and control units.

Rosenbaum & Rubin (1983) showed that the property (1) can be replaced by (3) and that (2) can be empirically verified with (4):

(3) $Y_{1i}, Y_{0i} \perp T \mid p(X)$; and

(4) $X \perp T \mid p(X)$;

These assumptions guarantee that conditioning on a function of *X* (the propensity score) is equivalent to conditioning on X. The assumption (4) states that conditioning on the propensity score the *X* variables and the treatment indicator should not present correlation. Under these assumptions it is possible to estimate the ATT via DD estimator replacing the *X* vector by the predicted *p(X)* function.

The ATT is identifiable given the treated and comparison groups are statistically similar in average of observables (the mean values of the *X* variables for both groups should not be statistically different), which is the case when the model is run in the common support of the propensity score.

4. Empirical Results

Table 8 presents the estimation of the propensity score to select common support. First, the probability of an individual receiving the treatment given the set of observable characteristics, the vector X_i, is estimated through a logit model.

Table 8 Logit Estimates - Treatment Group Selection (2007)

Variables	*Dummy* = 1 if a household lives in the treated area (Canaã) (Unmatched Sample)	*Dummy* = 1 household lives in the treated area (Canaã) (Matched Sample)
Gender(=1 if have)	0.32	0.11
	(0.48)	(0.51)
Ethinicity (non-white)	0.04	0.02
	(0.45)	(0.45)
Marital status(=1 if have)	0.58	0.35
	(0.47)	(0.49)
Age	-0.03*	-0.01
	(0.01)	(0.01)
Weekly hours of adult work	0.02	0.01
	(0.01)	(0.01)
Weekly hours of child labor	-0.03	-0.01
	(0.02)	(0.02)
Years of education (head)	0.14***	0.05
	(0.05)	(0.08)
Monthly income per capita	-0.01**	-0.01
	(0.00)	(0.00)
TV(=1 if have)	-1.48**	-0.68
	(0.69)	(0.85)
DVD(=1 if have)	-0.64	-0.29
	(0.53)	(0.58)
Radio(=1 if have)	-1.68***	-0.60
	(0.50)	(0.84)

Car (=1 if have)	-0.28	-0.09
	(0.45)	(0.48)
Washing machine (=1 if have)	2.19***	1.06
	(0.65)	(0.92)
Refrigerator(=1 if have)	-6.07***	-2.76
	(1.07)	(2.15)
Informal worker	-1.73***	-0.75
	(0.62)	(0.85)
Credit	-0.17	-0.03
	(0.43)	(0.45)
Constant	8.18***	1.87
	(1.62)	(4.09)
Pseudo-R2	0.62	0.63
Prob>Chi2(16)	0.00	1.00
Observations	305	288

Source: Research from the Osasco Land Title Survey

Note: ***, **,* Statistically significant at 1%, 5% and 10%, respectively.

Table 8 has demonstrated that the common support disregarded 17 households. Hence, the number of common support households is 288 rather than 305. Table 9 shows the unconditional DD estimate for the impact of land title on access to credit. If the experiment was successfully conducted, a conditional estimate should not be deviated from the results presented below.

Table 9 Basic Difference-in-Difference Estimate - Land Title Impact on Access to Credit

	Treatment group	**Control group**	**DD (percentage points)**
2008			
Frequency	128	69	
%	76.19	50.36	
2007			
Frequency	75	60	
%	44.64	43.8	
Difference	**31.55**	**6.56**	**24.99**
#Observations	168	137	

Source: Research from the Osasco Land Title Survey

According to the results above, the causal impact of the land title on access to credit should be about 25 percentage points or about 55%. Such estimate is given by two differences:

$$\left(credit_{2008}^{Treated} - credit_{2007}^{Treated}\right) - \left(credit_{2008}^{Control} - credit_{2007}^{Control}\right) = 31.55 - 6.56 = 24.99$$

However, the DD estimates have to be computed controlling for a set of covariates in order to account for any remaining source of bias due to the differences in observable characteristics between the groups. The next set of estimates is based on the following model:

$$Credit_{ist} = \beta_0 + \beta_1 LandTitle_{st} + \beta_2 Time + \alpha_{DD}\left(LandTitle_{st} * Time\right) + X_{ist}^{'}\gamma + u_{ist} \quad (8)$$

This model pools the data and estimates the impact of the program through the OLS estimator. Thus the estimates will be based on the linear probability model. Despite the caveats underlying this approach, such as (i) the predicted probability can be negative or more than a unit, and (ii) assumption of linearity in the relationship between *Credit* and covariates, this approach renders direct estimates on the impact of the program. In the present case, the estimates range from 0 to 1.

Note that the pre-existing differences between the treated and control groups are captured by β_1 given that

$$E[Credit_{ist} \mid LandTitle = 1, Time = 2007, X_{ist}] - E[Credit_{ist} \mid LandTitle = 0, Time = 2007, X_{ist}] = \beta_1 \quad (9)$$

The first column reports the *naïve* DD estimate and can be taken as benchmark under well performed randomization. The DD estimate suggests that the land title increased the access to credit in 25 percentage points.

Table 10 Difference in Difference Estimates – Land Title Impact on *Access to Credit*

Variables	***Credit*** *(Naïve)*	***Credit*** *(Unmatched)*	***Credit*** *(Matched)*
Land title	0.00847	0.0291	0.0220
	(0.0574)	(0.0685)	(0.0699)
Land title*Year (*DD*)	0.250***	0.210***	0.216***
	(0.0418)	(0.0794)	(0.0647)
Year	0.0657***	0.0702	0.0655
	(0.0213)	(0.0571)	(0.0598)
		0.0387	0.00325**
Gender(=1 if female)		(0.0349)	(0.00141)
		-0.00964	-0.0120**
Ethnicity(=1 if African-Brazilian)			
		(0.0375)	(0.00483)
		0.00286	0.000124***
Marital Status(=1 if married)			
		(0.0412)	
Age		-0.000142	
		(0.00151)	
Weekly hours worked of adult work		0.00339**	0.00325**
		(0.00162)	(0.00141)
Child labor Weekly Hours		-0.00120	
		(0.00178)	
Years of Education (Family Head)		-0.0143***	-0.0120**
		(0.00487)	(0.00483)
Monthly Income per capita (currency BRL**)		7.49e-05***	0.000124***
		(1.44e-05)	(4.50e-05)
Wealth Index		-0.0308*	-0.0236
		(0.0174)	(0.0183)
Informal worker		0.0559	

		(0.0520)	
Constant	0.438***	0.426***	0.445***
	(0.0426)	(0.105)	(0.0556)
R2	0.075	0.113	0.096
Observations	610	610	576

Source: Research from the Osasco Land Title Survey
Note: ***, * Statistically significant at 1% and 10%, respectively. The standard errors in parentheses are computed as bootstrap with 100 repositions.

Due to the influences of some covariates in the selection of the area, the second column reports the *OLS* DD estimate controlling for a set of covariates. This time, the DD estimate point to an impact of 21 percentage points or about 47%. The third column reports the DD estimate for the matched sample. The DD estimate for the matched sample is fairly similar to the benchmark, suggesting that the experiment was well implemented.

The evidence in Table 10 constitutes this paper main result and provides additional evidence to land title impact on access to credit in urban areas.

4.1 Empirical Results by Gender

The program applied in Osasco registered titles on behalf of women independently of their position in the household. Furthermore, there is evidence suggesting that women are more credit constrained than men (Khandker, 1998; Aghion and Morduch, 2005; Mel et al. 2009) and that women tend to oppose home sale more strongly than men (Datta, 2006). Given such literature background, the impact of land title on credit access should be greater for females. This section starts with some descriptive statistics in order to compare the access to credit by males and females at the baseline.

Table 11 Descriptive Statistics *Access to Credit* – by Gender

	Credit	Credit Type	Credit DS***	Borrow
Male – Treated	44	30.9	32.7	8.2
Male – Control	44.2	34.7	35.8	3.16
P-value for the difference of means	*0.54*	*0.56*	*0.65*	*0.13*
Female – Treated	53.4	39.67	41.4	10.3
Female – Control	42.9	31	33.3	19
P-value for the difference of means	*0.3*	*0.38*	*0.42*	*0.22*

Source: Research from the Osasco Land Title Survey*** Department Store

Table 12 DD Estimates Land Title Impact Access to Credit (Decomposed by Gender)

	***Model 1* (Naïve)**		***Model 2* (Unmatched)**		***Model 3* (Matched)**	
	Male	***Female***	***Male***	***Female***	***Male***	***Female***
Land title	-0.0421	0.106	0.0139	-0.0167	0.0395	0.0809
	(0.0695)	*(0.102)*	*(0.101)*	*(0.149)*	*(0.0756)*	*(0.113)*
Title*Year	0.284***	0.181**	0.216***	0.186**	0.251***	0.154*
	(0.0508)	*(0.0751)*	*(0.0581)*	*(0.0836)*	*(0.0560)*	*(0.0822)*
Year	0.0526**	0.0952**	0.0605**	0.0906*	0.0457*	0.108**
	(0.0231)	*(0.0459)*	*(0.0260)*	*(0.0500)*	*(0.0247)*	*(0.0494)*
Constant	0.442***	0.429***	0.576***	0.540*	0.505***	0.301***
	(0.0513)	*(0.0773)*	*(0.218)*	*(0.300)*	*(0.0731)*	*(0.114)*

Controls?	*No*	*No*	*Yes*	*Yes*	*Yes*	*Yes*
R^2	0.072	0.088	0.132	0.182	0.112	0.111
Observations	410	200	410	200	382	194

Source: Research from the Osasco Land Title Survey
Note: ***, **, * Statistically significant at 1%, 5% and 10%, respectively.

Table 12 above shows that women in the treated area are less credit constrained than the men whereas in the control area the opposite is observed. Table 13 illustrates the DD estimates for the whole sample and for the sub- sample of households from common support. The first two models suggest that the impact among male is higher. The naïve model and the estimate for the matched sample point to a difference of 10 percentage points in favor of male. However, in both cases the difference between the estimates is not statistically significant. The Wald test rejects the null that the difference between the coefficients is not statistically significant. The Wald statistic is 0.26 with a p-value of 0.61 for the first model whereas for the matched sample the statistic of the test is 1.4 with a p-value of 0.24.

4.2 Empirical Results – Different Credit Types by Gender

The aim of this section is to explore the impact of land title on each credit type. These estimates are also decomposed by gender since the impact on males and females can be different depending on the type of credit used by the household. Table 13 below shows the rough DD estimates for each variable representing *access to credit*.

Table 13 Basic DD Estimates: Land Title Impact on *Access to Credit* (By Modality of Credit)

	Type of credit	Borrow	Credit DS***	Credit card	Check pre	Boleto
			2007			
Control	0.336	0.080	0.350	0.248	0.066	0.080
Treated	0.339	0.089	0.357	0.238	0.065	0.083
			2008			
Control	0.358	0.102	0.372	0.328	0.102	0.080
Treated	0.524	0.423	0.560	0.321	0.107	0.083
DD	0.163	0.312	18.1	0.003	0.006	0
%	48	350	50.7	1.3	9	0

Source: Research from the Osasco Land Title Survey; *** Department Store

According to these simple double differences of means, the impact is greater than 15 percentage points. The impact of the category *borrow* is the highest, with more than 30 percentage points' increase in access, representing 350% given the baseline low level. This is the result expected given this credit type is supposed to be more responsive to the collateral presence. Dower & Potamites (2005), for example, looking at data from Indonesia, reported an average effect of title on the probability of having had a formal bank loan of 21% and an impact of 72% on the average amount of working capital.

The estimates below add control variables to the model in order to take the differences in some covariates between the groups into account.

Table 14 DD Estimates for Different Source of Credit

Variables	Credit Type	Credit DS***	Borrow
Land Title	0.0380	0.0431	0.0342

	(0.0816)	*(0.0817)*	*(0.0511)*
Title*Year	0.133***	0.154***	0.312***
	(0.0384)	*(0.0400)*	*(0.0412)*
Year	0.0286*	0.0320**	0.0187
	(0.0151)	*(0.0150)*	*(0.0133)*
Constant	0.421**	0.507***	0.121
	(0.186)	*(0.184)*	*(0.121)*
Controls?	*Yes*	*Yes*	*Yes*
R2	0.044	0.056	0.185
Observations	610	610	610

Source: Research from the Osasco Land Title Survey

Note: ***, **, * Significant at 1%, 5% and 10%, respectively. *** Department Store

The table reports only the estimates for the categories that are expected to respond to the presence of land title. Even after controlling for the covariates the impact is 31.2 percentage points (the third column). These results not only suggest that this category plays a main role in the aggregate measure *access to credit* used in the former estimations, but also that the impact of land title is not homogenous. In fact, it depends on the category of credit, although the main effect was already seen as predicted by the theory.

Table 15 DD Estimates – Credit Type (Effect Decomposed by Gender)

	Credit Type		**Credit DS********		**Borrow**	
	Men	Women	Men	Women	Men	Women
Land title	0.0659	-0.0630	0.0771	-0.0524	0.0843	-0.0782
	(0.0985)	(0.156)	(0.0993)	(0.158)	(0.0573)	(0.116)
Title*Year	0.151***	0.108*	0.167***	0.138**	0.327***	0.292***
	(0.0503)	(0.0566)	(0.0506)	(0.0623)	(0.0516)	(0.0678)
Year	0.0412*	0.00581	0.0423*	0.0113	0.0292	0.000100
	(0.0215)	(0.0115)	(0.0215)	(0.0113)	(0.0190)	(0.0110)
Constant	0.459**	0.450	0.532**	0.555	0.0951	0.197
	(0.233)	(0.339)	(0.229)	(0.343)	(0.127)	(0.271)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	410	200	410	200	410	200
R2	0.077	0.113	0.093	0.113	0.231	0.157

Source: Research from the Osasco Land Title Survey

Note: ***, **, * Significant at 1%, 5% and 10%, respectively. **** Department Store

The title impact appears to be greater for males. The estimates for male and female are statistically different at 1% for all cases and again, the impact is greater in the third category for both male and female. The estimates point to an impact greater than 30 percentage points for both cases, despite being statistically different.

4.3 Robustness Check

This section presents the random and fixed effects estimates for the impact of the program on access to credit[12]. In theory, no statistical difference between should exist among the coefficients of random and fixed effects if the randomization was done successfully. The model takes the following specification:

Random Effect:

(10)

[12] The annex brings additional robustness check, such as propensity score matching estimates using a Kernel estimator with different bandwidths.

$$Credit_{ist} = \beta_0 + \beta_1 LandTitle_{st} + \beta_2 Time + \alpha_{DD}\left(LandTitle_{st} * Time\right) + X_{ist}^{'}\gamma + c_i + \varepsilon_{ist}$$

where the unobserved individual-specific effect, c_i, is supposed to be not correlated with covariates; i.e., $E\left(c_{it}, X_{ist}\right) = 0$ for t=0,1 and all elements of the vector X. In this case, the coefficients are computed with the generalized least square estimator (GLS).

Fixed Effect:

$$Credit_{ist} = \beta_0 + \beta_2 Time + \alpha_{DD}\left(LandTitle_{st} * Time\right) + X_{ist}^{'}\gamma + c_i + \varepsilon_{ist} \quad (11)$$

where $E\left(c_i, X_{ist}\right) \neq 0$, and the coefficient of interest is computed with the OLS *within* estimator (see Baltagi, 2005). Table 16 summarizes the estimates for both models.

Table 16 Panel Estimates – Random and Fixed Effects

Variables	**Credit** *Random Effects*	**Credit** *Fixed Effects*
Land title	0.00838	-
	(0.0793)	-
Land title*year (*DD*)	0.212***	0.214***
	(0.0434)	(0.0523)
Year	0.0649***	0.0512**
	(0.0232)	(0.0248)
Constant	0.482***	-0.579
	(0.152)	(0.619)
Controls?	*Yes*	*Yes*
Prob>Chi2(18)	0.000	-
Prob>F(11,304)	-	0.000
R2	0.28	0.30
Observations	610	610

Source: Research from the Osasco Land Title Survey

Note: Clustered standard errors in parentheses. The control variables are the same used in the second column of the DD estimate (see Table 12). ***, ** Significant at 1% and 5%, respectively.

The estimates are similar, finding which indicates that the experiment was conducted successfully. Hence, according to the random effects model, the ATT is about 21 percentage points and is in line with the DD estimates[13].

Table 17 illustrates the RE and FE estimates decomposed by gender. The ATT is greater for male, but the difference is not statistically significant at 5%.

Table 17 Random and Fixed Effects Estimates (Decomposed by Gender)

	Model 1 (Random Effects)		**Model 2 (Fixed Effects)**	
	Male	***Female***	***Male***	***Female***
Land title	-0.00729	-0.00962	-	-
	(0.0298)	*(0.0763)*	-	-
Title*Year	0.223***	0.190***	0.211**	0.203*
	(0.0346)	*(0.0424)*	*(0.00447)*	*(0.0187)*
Year	0.0547***	0.0917***	0.0294	0.0862

[13] According to Wooldridge (2002), the *pooled* OLS and the random effects estimators should very close in the presence of strict exogeneity. Therefore, statistically speaking, with a random experiment the coefficients should be the same. This explains why the RE and the DD are so close to each other.

	(0.00529)	*(0.0100)*	*(0.00837)*	*(0.0155)*
Constant	0.500***	0	-1.447**	0.910*
	(0.105)	*(0)*	*(0.0740)*	*(0.120)*
Controls?	*Yes*	*Yes*	*Yes*	*Yes*
Prob>Chi2(18)	0.000	0.000	-	-
Prob>F(10, 99)	-	-	-	0.004
Prob>F(11, 204)			0.000	-
R2	0.124	0.176	0.004	0.04
Observations	410	200	410	200

Source: Research from the Osasco Land Title Survey
Note: ***, **, * Statistically significant at 1%, 5% and 10%, respectively. The control variables are the same used in the second column of the DD estimate.

5. Conclusion

This paper has presented new evidence on the value of formal property rights in urban squatter community in a developing country. By studying the relationship between the exogenous acquisition of land title and credit access, the study has provided additional empirical support for the evidence that property title appear to increase credit access. Additionally, lack of evidence to support such assumption from experimental or quasi-experimental estimates is available globally and none particularly in Brazil.

Although existing studies indicate significant effect on access to credit, Field (2007), this particular study aims helping to fill an important gap in the literature on property rights and credit access. Furthermore, the results indicate that government property titling programs appear to have a different effect through the labor supply distribution.

Empirically, the Difference-in-Differences estimator was applied in order to obtain the effect of land title on access to credit. The main results pointed out the different impacts on the various credit types and gender. For example, the average treatment effect on the treated pointed to an impact of about 22 percentage points, or approximately 49%. Such a policy evaluation for a program implemented in a developing country (Brazil) also extends the literature on the topic, which relies heavily on the study of United States cases. Understanding the multiple channels through which land titles influence economic outcomes is particularly important for developing countries governments considering titling programs to address urban informality. In addition, the results have potential implications for understanding credit market frictions in developing countries. In places characterized by high levels of residential informality such as most of developing and poor countries, informal property protection may constitute an important tool in order to improve access to credit. That would improve the assessment of such programs in the lives of the millions of households living in urban squatter communities in developing countries across the world.

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Annex A Robustness Check-Different Credit Types by Gender

Table A1 Random and Fixed Effects Estimates (Decomposed by Credit Modalities)

	Model 1	**Effects)**	**(Random**		**Model 2 (Fixed Effects)**	
	Credit DS****	**Credit Type**	**Borrow**	**Credit DS******	**Credit Type**	**Borrow**
Land title	0.040	0.0506	0.045	-	-	-
	(0.08)	*(0.08)*	*(0.05)*	-	-	-
Title*Year	0.154***	0.135***	0.31***	0.15***	0.134***	0.315***
	(0.038)	*(0.036)*	*(0.04)*	*(0.043)*	*(0.04)*	*(0.05)*
Year	0.0281*	0.0243	0.022*	0.023	0.016	0.026
	(0.015)	*(0.015)*	*(0.013)*	*(0.017)*	*(0.016)*	*(0.017)*
Constant	0.552***	0.386**	0.116	0.502	0.15	-1.347**
	(0.185)	*(0.187)*	*(0.123)*	*(0.52)*	*(0.51)*	*(0.58)*
Controls?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Prob>Chi2(18)	0.000	0.000	0.000	-	-	-
Prob>F(11, 304)	-	-		0.000	0.000	0.000
R2	0.05	0.04	0.17	0.01	0.002	0.013
Observations	610	610	610	610	610	610

Source: Research from the Osasco Land Title Survey

Note: ***, **, * Statistically significant at 1%, 5% and 10%, respectively. The control variables are the same used in the second column of the DD estimate**** Department Store

Table A2 PSM Estimates (Decomposed by Credit Modalities)

	Bandwith (0.1)	**Bandwith (0.05)**	**Bandwith (0.01)**
		Credit Type	
Land Title	0.166***	0.166***	0.166***
	(0.05)	*(0.058)*	*(0.061)*
#Treated	168	168	168
#Control	137	137	137
		Credit DS***	
Land Title	0.187***	0.187***	0.187***
	(0.06)	*(0.061)*	*(0.051)*
#Treated	168	168	168
#Control	137	137	137
		Borrow	
Land Title	0.32***	0.32***	0.32***
	(0.045)	*(0.047)*	*(0.046)*
#Treated	168	168	168
#Control	137	137	137

Source: Research from the Osasco Land Title Survey; Note: *** Significant at 1%. The standard errors in parentheses were computed with bootstrap with 100 repetitions. *** Department Store

Table A3 Random Effects Estimates (Decomposed by Credit Types and by Gender)

	Credit DS****		**Borrow**		**Credit Type**	
	Male	**Female**	**Male**	**Female**	**Male**	**Female**
Land title	0.082	-0.062	0.09	-0.08	0.067	-0.021
	(0.097)	*(0.152)*	*(0.06)*	*(0.11)*	*(0.097)*	*(0.15)*
Title*Year	0.166***	0.146**	0.33***	0.3***	0.15***	0.115**
	(0.048)	*(0.059)*	*(0.05)*	*(0.07)*	*(0.05)*	*(0.05)*

Year	0.038*	0.008	0.0315*	0.006	0.038*	-0.004
	(0.021)	*(0.0100)*	*(0.02)*	*(0.01)*	*(0.02)*	*(0.01)*
Constant	0.484**	0.717**	0.062	0.22	0.41*	0.487
	(0.23)	*(0.33)*	*(0.13)*	*(0.26)*	*(0.23)*	*(0.34)*
Controls?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
R2	0.09	0.09	0.23	0.15	0.074	0.08
Prob>Chi2(18)	0.000	0.000	0.000	0.000	0.000	0.000
Observations	410	200	410	200	410	200

Source: Research from the Osasco Land Title Survey

Note: ***, **, * Significant at 1%, 5% and 10% respectively. The control variables are the same used in the second column of the DD estimate. **** Department Store

Table A4 PSM Estimates (Decomposed by Credit Type and by Gender)

	Credit DS* **		**Borrow**		**Credit Type**	
	Men	**Women**	**Men**	**Women**	**Men**	**Women**
Land Title	0.165**	0.236*	0.364**	0.464*	0.139*	0.225*
	(0.063)	(0.092)	(0.055)	(0.057)	(0.063)	(0.11)
#Treated	110	58	110	58	10	58
#Control	95	42	95	42	95	42

Source: Research from the Osasco Land Title Survey

Note: **,* Significant at 1% and 5% respectively. The estimates were computed with a *bandwith* of 0.05 and the standard errors in parentheses were computed with bootstrap with 100 repetitions. *** Department Store

Figure 1A: Distribution of Wealth Index