The Distributive Effects of Property Rights: How Does Land Title Affect Labor Supply?

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Abstract

This paper studies the effects of granting secure property rights on labor supply of adults using a unique data set. The causal role of legal ownership security is isolated by comparing a land-titling program in two close and similar communities in the Brazilian city of Osasco. Not only OLS and Difference-in-Difference methodology is applied but also the distributive impact of land title on labor supply is estimated by the regular Regression Quantile Methodology and by the weighting estimator of Firpo (2007). All estimates reveal that the impact is different through the weekly hours of adult work. Additionally, the impact is greater on the lower quantiles.

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I. INTRODUCTION

The role played by property rights in economic development has been powerfully documented by economic historians such as North and Thomas (1973). Their fragility is considered a crucial obstacle for economic development (North, 1990). The main argument is that individuals underinvest if others can seize the fruits of their investment (Demsetz, 1967).[5] 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, Johnson, and Robinson, 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 and Torero (2002) mentioned that this credit could be invested as capital in productive projects, promptly increasing labor productivity and income. 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; and Binswanger, Deninger, and Feder, 1995) as translated in the Figure 1.

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, whereas China is also considering implementing such a policy.[6] 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*", has spent US$ 15 million per year from the federal budget since launched providing titles to over 85,000 families and reaching 49 cities in 17 different Brazilian states. Its official goal is "*to develop land titles in Brazil and to promote an increase in the quality of life for the Brazilian population*".[7] It is intended to provide land titles to families living under illegal conditions.

[5] Torstensson (1994) and Goldsmith (1995), for example, find a significantly positive association between secure property rights and economic growth.

[6] See The Economist, 2007. The same edition brings on its front page: "*Property Rights: China's Next Revolution*". It shows that China intends to put into place the most ambitious land-titling program in the world's history, including this initiative as one of the main points of the Chinese economic development model.

[7] See Associação dos Notários e Registradores do Brasil—ANOREG, 2007. The English translation of the quote is ours.

However, the country still faces a very difficult scenario regarding land property rights. The Brazilian government estimates that 12 million people live under illegal urban conditions (IBGE, 2007).

This paper aims to measure the impact of property rights on labor supply in Brazil, deriving lessons to other developing and emerging economies. It also analyzes household response regarding distributive supply of labor force to exogenous changes in formal ownership status.

An important contribution of this paper is the specific focus on non-agricultural households and the value to urban residents and their families of increased ownership security. As discussed above, large proportions of urban and rural residents alike lack tenure security, in developing economies. This research also provides a unique dataset that helps to eliminate the endogeneity appearing in most of the studies on such subject.

Field and Torero (2002) shows that untitled households are constrained by the need to provide informal policing, both to deter prospective invaders from invading private properties and to actively participate in community enforcement efforts to protect neighborhood boundaries. This is one important mechanism by which the lack of land title removes individuals from the labor force.

Given such context, titling efforts that effectively increase household tenure security should allow households and communities to reallocate time, resources and human talent away from the informal policing role. Hence, strengthening formal property rights decreases work hours inside the house and increases time spent outside. This reflects the fact that an exogenous increase in the formal property protection, lowers the opportunity cost of outside labor and makes the likelihood of an increase in the current income of those households higher (Field and Torero, 2002).

Effects of land titling have been documented by several studies. A partial listing includes Jimenez (1985), Alston, Libecap, and Schneider (1996), and Lanjouw and Levy (2002) on real estate values. Besley (1995), Jacoby, Li, and Rozelle (2002), Brasselle, Gaspart, and Platteau (2002), and Do and Iyer (2003) on agricultural investment. Place and Migot-Adholla (1998), Carter and Olinto (2003) and Field and Torero (2002) on credit access, labor supply, housing investment and income.

As Field and Torero (2002) describes, presumably because of historic interests in agricultural investment and related politics of land reform, most of this literature and the majority of both academic and policy attention to property rights has centered on rural households tenure security. Nevertheless, in most of the developing world, the population—and in particular the impoverished population—is increasingly urban. However, in urban settings, the value of property titles has been measured far less often and empirical work has focused on real estate prices. A major contribution made by Jimenez (1984), using an general equilibrium model of urban squatting, shows that the difference in unit housing prices between the non-squatting (formal) sector of a city and its squatting (informal) sector reflects the premium associated with security. The accompanying empirical analysis of real estate markets in the Philippines

finds equilibrium price differentials between formal and informal sector unit dwelling prices in the range of 58.0% and greater for lower income groups and larger households.

In Besley (1995), the findings 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, Li, and Rozelle (2002) find positive effects in China, whereas Brasselle, Gaspart, and Platteau (2002) find no effects for Burkina Faso. For Peru, Field and Torero (2002) 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. They 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 positive effect on the income of those who have received the land title.

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), and Alchian and Demsetz (1973).[8]

In order to isolate the causal role of land title, we use a natural experiment, comparing two neighboring and very similar communities in Osasco (a town with around 654,000 people located in metropolitan area of São Paulo - Brazil metropolitan area) [9]. In one of them, *Jardim Canaã*, all households received the land title in 2007. In the other, *Jardim DR*, the same program is scheduled only for 2012, making it a natural control group. Officially, the City of Osasco Mayor guaranteed that the decision to have *Canaã* as the starter was random.[10]

Different from the previous studies, our analysis is based on two-stage survey, from *Jardim Canaã* and *Jardim DR*, both with focus on the property right issue. The dataset consists of three hundred and twenty-six households distributed across both neighborhoods. 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.

This paper main result indicates that land title has positive impact on adult labour supply. Secondly, it is shown that the impact is heterogenous. The households who benefit most from

[8] Direct evidence of this is provided by Miceli, Sirmans, and Kieyah (2001), who analyze the extent of endogeneity of formal agricultural property rights in Kenya.

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

[10] Skoufias (2001), for example, uses a similar natural experiment to evaluate the income transfer initiative called PROGRESA in Mexico. In that program some localities were randomly selected for participation (treatment localities) while the rest were introduced into the program at later phases (control localities). Such assignment at the locality level has the benefit of minimizing the chances of spillover effects between treated and untreated individuals at the same locality

the formal property right are those in the first two quartiles of the labor force participation. Thus, the mostly excluded from the labor force are the ones gaining more from the titling program.

The paper is organized as follows. Section II presents a simple model on why land tiltling increases the labor supply and income of squatter households. Section III describes the empirical strategy, discussing the research methodology, including the OLS, Difference-in-Difference, Quantile Regression and Quantile Treatment Effect and providing an overview about the data collected. The empirical results are also discussed in this section. Section IV concludes the paper.

II. MODEL

Based on Field (2003)…

III. METHODOLOGICAL ISSUES AND DESCRIPTIVE STATISTICS

A. Minimizing Selection Bias Concerns

The federal government chose Osasco as one of the cities to be part of the program "*Papel Passado*". The city of Osasco has 30,000 people (about 6,000 families) living under informal conditions. This represents almost 4.5% of its total population (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.[11]The first locality to receive the land title in 2007 was *Jardim Canaã*, where 500 families live. The closest neighborhood to *Jardim Canaã* is a community called *DR*, with 450 families. The *DR*'s households will be part of the "*Papel Passado*" program scheduled in 2012.

The Osasco City Hall officially claims that the localities priority follows a random criterion.[12] As Behrman and Todd (1999) argue, that randomization avoids the issue of selection bias in the program evaluation, common in non-experimental evaluations. Those authors also discuss that randomization can prevent the problem of self selection. However, some other types of biases may occur in randomized evaluations, such as *randomization bias*, *contamination* and *attrition*.

In addition, ninety-five percent of the first survey participants—both from *Jardim Canaã* and *DR*—did not expect to receive any land title. They were not aware of "*Papel Passado*" and the meaning of it, curbing the potential behavioral deviation from households included in the

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

[12] However, unofficial sources from local communities in Osasco express the feelings that maybe a "political" agenda was present in the decision.

program (*randomization bias*).[13] In turn, *contamination bias* is avoided in the case of Osasco since the control group residents cannot not benefit from the program outside the treatment locality. Besides, no alternative form of formal land title program exists. [14] The land title program also does not provide a drop out option. After receiving the title, the household could sell the property and move out of the locality. However, he would have already been affected by the program, reducing the probability of *attrition bias.*[15]

Further, *Jardim Canaã* and *DR* are very similar economic and social characteristics. They are not only official neighborhoods but there is no physical "borderline" among them, being geographically united. These neighborhoods are both located 2.5 miles from downtown Osasco, having the exactly same access to the Osasco's mainly economic center.
This ensures that the treatment group is similar both in terms of observable and unobservable characteristics to the group that does not receive the land titles (treatment).[16]

B. The Data

The data is produced from a two-stage survey focusing on the property right issue. To further minimize biases, the survey questionnaire and its applicant did not provide any direct information for the households on the objective of the research. Officially, for the people interviewed, the study was about general living conditions in the city of Osasco.

The questionnaire contained 39 questions and was applied to the 326 families randomly sampled.[17] The survey instrument, in many of its questions and methodologies, closely mirrored in content the national statistical survey (*Pesquisa Nacional de Amostra de Domicílios*—PNAD) from the Brazilian statistical bureau *(Instituto Brasileiro de Geografia e Estatística*—IBGE). It requested a variety of information on household and individual characteristics. In addition, six questions were designed to obtain information on a range of economic, social and personal benefits associated with property formalization.

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[13] *Randomization bias* occurs, for example, when the the need to recruit a greater number of applicants induces program administrators to change program admissions standards. A similar problem happens 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 evaluation may not be generalized to a context where the program is not being implemented as a randomized trial.

[14] *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.

[15] 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.

[16] Rubin and Thomas (2000) indicate that 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.

[17] The questionnaire is available upon request.

The first stage of the survey was conducted by researchers not originally from Osasco in March 2007, before the titles were issued to the households of *Jardim Canaã*. The second stage was carried out with exactly the same households in August 2008 (with ninety-eight percent recall), almost a year and a half after the first titles were issued. This time gap between stages was designed on purpose, so that all the households interviewed during the first stage would have the land title for at least 1 year.[18]

The study also tracked the households that moved away from both communities. The attrition is reasonably low, only eight percent of the households that received the land title moved away from *Canaã*.[19] Yet, from the control group, only one household (out of hundred and forty) moved out during the same period.

Given the information of the surveys, a technique from Bolfarine and Bussab (2005) was used to randomly choose 326 sample households of the two localities: hundred and eighty-five from *Jardim Canaã*, and hundred and forty-one from *DR*. The approach consisted in 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. Each researcher got fifty names initially as first base. After reaching each of those households, they could go and pick the third and the fifth neighbor on the right hand side.

C. Descriptive Statistics

Figure 2 shows that most of households who received the land title felt that their lives improved (at least up to the period of the second stage of the survey), even if they had not previously expected the land title.

In turn, Tables 1 and 2 and Figures 3 and 4 summarize the household's answers (2007 and 2008) about weekly hours of work. They show that for both sample groups combined (treated and control) an increase of weekly hours of adult work between 2007 and 2008. From Figure 4 it is also visible that the treatment group increased its working after the program. Yet, for the control group the scenario remains practically constant overtime.

Table 2 reports the T-test for the difference of means for covariates in 2007 comparing the control and treatment groups before the program. We should not find systematic differences of observables characteristics between the two groups. However, the results demonstrate that the sample of treated and control groups are well balanced in observables in the baseline.

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[18] The exact dates that each household interviewed received the title were obtained from the 2nd Osasco's Office of Registration (*2º Cartório de Osasco*) along with the formal authorization from the Osasco's City Hall to conduct the research. Both entities have worked together to map and register all families from that particular area.

[19] 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.

We believe that these differences are due to the fact that the randomization was performed in the community level rather than household level. Therefore, even in case we had comparable communities this sort of problem could emerge. Such differences in observables characteristic between control and treated sample groups also appears in the PROGRESA experiment. As it is argued by Skoufias (2001) and Behrman and Todd (1999), for instance, it is because the public policy randomized the communities instead of the households. Thus, they demonstrate that even being similar in terms of observables at the community level, both groups are not fully comparable at the household level. Hence, this calls for the inclusion of control variables instead of simply estimating the program impact through mean tests. In fact, this paper will adopt two econometric procedures in order to approach the differences on observables. The first is to include control variables at the OLS and Difference-in-Difference methods and the secondly to apply the same approach using a balanced sample. In this later case, we use the propensity score to select the well-balanced sample (see Angelucci and Attanasio, 2009 for a similar approach).

The table 2 contains a puzzle. For example, the average of years of education (family head), monthly income, monthly income per capita and informality at work are statistically significantly different between the two sample groups in 2007.

The reason for this sample mean difference can be explained by the fact that households with higher level of education tend to have more access to formal jobs in Osasco (see Zylberstajn and Neto, 1999). Brazilian formal employers, on top of cash salaries, tend to receive other perks that are not reflected in the cash payroll. For example, a formal Brazilian employee usually receives health care plan for the whole family; subsidized transportation; and meal plans. Instead, informal workers do not have those benefits, relying essentially on cash income to compensate the lack of perks. This explains why the more educated households have lower income (*i.e.* lower cash income) in Table 2. Table 3 also 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. Yet, informal workers are a relevant sub-sample with a total of 233 households, representing 92 percent of the control and 64 percent of the treated group (see Table 2).

IV. EMPIRICAL ANALYSIS

Our empirical analysis follows three steps. The mean effect of land title on hours worked weekly is firstly estimated by a simple OLS regression and Differnce-in-Differnce. Then, the distributional effects of the titling are analyzed via Quantile Regression and Quantile Treatment Effects.

A. The OLS Regression Analysis

The mean effect of land titling is investigated by a standard OLS regression:

$$Hours\ worked\ weekly_i = \alpha + \delta Land\ title_i + \beta X_i^{'} + \varepsilon_i, \quad (1)$$

where $X_i^{'}$ is a control vector of social-economic variables, including: gender; ethnicity; marital status; years of education; number of members of the household; age; age squared; a dummy for informal work, household assets and monthly income per capita. Those are common covariates for land title (see Field and Torero, 2002). The variable $Land\ title_i$ is a dummy equaling one if the household had participated in the titling project; and zero otherwise. Since all households in Canaã received the title, i.e., all households who were eligible to participate in the program effectively received the title, the parameter of interest is the average treatment effect on the treated (ATT) instead of the intent to treat (ITT).

The empirical analysis in this paper also deploys the method employed by Angelucci & Attanasio (2009). The method consists of making use of *propensity scoring* to focus the analysis on the households which pertain to the common support (or balanced sample), 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 make the assumption of parallel trends in the absence of the program even stronger (see also Abadie, 2005).

Table 4 presents the estimation results of (1) for the baseline year of 2007, when the program had not yet been implemented and no household had received the land title (*i.e.* pre-program period).

The results demonstrate that land title does not have a significant positive impact in labor supply for the baseline case, including or not the control variables in the regression. In Model 2, the coefficient for land title becomes negative for the regression using the vector of control variables. These outcomes indicate that the households who legally owned a land before the titling program, on average, did not significantly work more hours weekly. Thus, the incentives to increase labor supply owning to a land title seem weak, reducing the possibility of self selection bias (or *anticipation bias*) among the titling-program participants.[20]

Table 5 presents the OLS estimates for four scenarios, using the data for the year 2008 (post-program period). Its results provide an initial support for the claim that land title increases labor supply. Model OLS-Naive, excluding the control variables, estimates a positive and significant coefficient (9.37) for the dummy land title. Given that the groups are not well-balanced in the baseline, the next columns include the set of control variables. As can be seen, the same result is obtained in the next two columns of Table 5. For the entire set of control variables, Model OLS, or for a subset of them, Model OLS-Balanced, the coefficient for land title remains positive and highly significant 6.06 and 6.39 respectively. We also estimated a Tobit model in order to approach the households that reported zero hours worked weekly in 2007. Although the Tobit estimate is greater than the OLS-Balanced, the difference between the coefficients is not statistically significant.

[20] *Self-selection* indicates any situation in which individuals select themselves into a treated group, causing a biased sample.

The estimates for the controls variables also display the expected sign. Heads of the households with more years of education tend marginally to work longer hours in the week. Age, instead, marginally reduces the hours worked weekly. Furthermore, other controls such as the dummy for informality, ethnicity and access to credit tend to increase labor supply.

B. The Difference-in-Difference Methodology (DD)

The econometric method applied was the Difference-in-Difference Estimator, 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 & 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 can be estimated by the following regression model that can be applied to identify the treatment effect on the outcome of interest.

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

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)[21].

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:

[21] 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.

(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.:

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

and

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

The main objection regarding (3) is the self-selection (also known as *anticipation problem*). Such certainly would be an issue if households decided to work more hours given the expectation of receiving land title in the future.

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) and make the common trend assumption weaker. 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.

Table 6 presents the DD estimates for three scenarios, using the data for the years 2007-2008 (pre and post-program period). Its results provide an additional support for the claim that land title increases labor supply. Model DD-Naive, excluding the control variables, estimates a positive and significant coefficient (9.01) for the dummy land title. The convergent result is obtained in the next two columns of Table 6. For the entire set of control variables, Model DD-Unbalanced and Model DD-Balanced; the coefficient for land title remains positive and highly significant – 8.07 and 8.19 respectively.

C. The Quantile Treatment Effects

Even though, we find a mean effect of land title in the hours worked weekly, it would be interesting to investigate the distributional effects of the titling in the hours worked. Frolich and Melly (2008) show that the distribution of a dependent variable may change in many ways which are not completely (or only incompletely) revealed by an examination of averages. For example, the wage distribution can become more compressed or the upper tail inequality may increase while the lower tail inequality decreases. Indeed, Heckman and Hotz (1989) find evidences that heterogeneity is an important feature of impact distribution using experimental data from the National Job Training Partnership Act Study.

Therefore, applied economists and policy makers are increasingly interested in distributional effects. This section, thus, investigates these distributional effects by using quantile treatment effect (QTE) methods for the Brazilian titling program example. The advantage of the QTE approach relative to the common effect model is that the impact of the program on different quantiles of the outcome distribution does not have to be constant. It is a powerful and intuitive tool that allows researchers to discover the effects on the entire distribution. In addition, Dammert (2009) states that most of the existing literature is based on social experiments in employment, training, and welfare programs in the United States.

For any fixed percentile, QTE corresponds to the horizontal distance between two cumulative distribution functions. Depending on the type of endogeneity, four different cases can occur. First, it is necessary to distinguish between conditional and unconditional effects. Secondly, whenever selection is on observables or on unobservables. Selection on observables is often referred to as a matching assumption or as an exogenous treatment choice (i.e. exogenous conditional on X). In contrast, Frolich and Melly (2008) refers to selection on unobservables as endogenous treatment choice.

The case of land title in Osasco, given the program context, can be qualified as an unconditional QTE with exogenous treatment. Firpo (2007), Frölich (2007b), and Melly (2006) provide different econometric methods to deal with such a case. This research follows the econometric procedure proposed by Firpo (2007). Before estimating the QTE, it is worth comparing the cumulative distributions of hours worked of both groups before and after in order to verify if there is some evidence of distributive effect of the program.

Cumulative Distribution and First Degree Stochastic Dominance

A first order stochastic dominance analysis is applied to compare the CDF's of treated and control groups. The idea, as it was mentioned, is to get a better grasp of the data before estimating the treatment effect.

Abadie (2002), Duflo, Hanna, and Ryan (2007) applies the first order stochastic dominance analysis to compare the treatment effect through the entire distribution for the two groups in analysis (treatment and control group). This analysis can be implemented as follow. Given two cumulative distribution functions (CDF), $F^1(y)$ and $F^2(y)$,[22] the CDF $F^1(y)$ first degree stochastic dominate $F^2(y)$ if and only if $F^1(y) \leq F^2(y)$ for all $y \in [0,1]$ and $F^1(y) < F^2(y)$ for some $y \in [0,1]$ (see Gravelle and Rees, 2004).

According to Figure 5, the distribution of adult weekly hours for the treatment group level during the pre-treatment period already dominates in first degree the distribution of adults under the control group. Nevertheless, Figure 6 shows that such dominance increases significantly in the period post-treatment. That is because $F^1(y_1) < F^2(y_0)$ for all $y \in [0,1]$,

[22] $F^k(y) = \int_0^{+\infty} f^k(y)\,dy.$

where the subscript 1 and 0 in *y* represent the distributions of adult weekly hours for the treatment and control groups respectively.

These results suggest that land titling affects positively all the cumulative distribution curve of the weekly hours worked and not only its average. Given that both groups are not perfectly balanced by the baseline level, therefore, the stochastic dominance analysis points to the relevance of a deeper investigation of the heterogeneous effects of land title in the weekly hours of work.

The Quantile Regression Model

As Koenker (2005), Rivera and Currais (2005) and Silva and Porto Junior (2004), among others, discuss, the main advantages of a quantile regression are that: (i) the technique allows to feature all conditional distribution from one response variable given a set of regressors; (ii) the point slope estimates of each quantile are obtained considering the complete set of data; (iii) the quantile regression can be applied in cases that the distribution is not Gaussian; (iv) the quantile regression provides outlier robustness; (v) the estimators from the quantile regression can be more efficient compared to the OLS estimators, if the error terms do not have a normal distribution; (vi) the parameters confidence interval can be estimated directly from the demanded conditional quantiles; and (vii) the quantile regression can be represented as a linear programming model, which makes easier to parameters estimates. Additionally, in the present context such technique allows to investigate whether the land title have differential effects for any subpopulation as it is stated by Bitler, Gelbach and Hoynes (2006).

The starting point for a quantile regression is the conditional quantile function (Imbens and Wooldridge, 2008):

$$Q_T(Y_i \mid X_i) = F_y^{-1}(\tau \mid X_i) \tag{2}$$

where Y_i is a vector of dependent variables, X_i is a vector of control variables, and τ denotes the quantile of interest. $F_y^{-1}(\tau \mid X_i)$ is the distribution function for Y_i at y, conditional on X_i. Given that there is no assumption for the distribution of errors, the quantile regression (QR) is considered a semi parametric regression technique. Below, both OLS and QR estimators are available:

$$Hours\ worked\ weekly_\tau = \alpha + \delta_{1\tau} Land\ title + X_i^{'}\alpha_\tau + \varepsilon_i. \tag{3}$$

It is relevant to emphasize the important characteristic so called *"attention point"* of quantile regressions (Angrist and Pischke, 2009). As mentioned before, the effects on distribution are not equal to effects on individuals. Those will be equal only if an intervention/treatment is a rank-preserving and the intervention does not change the individuals ordering. If that is not the case, the treated adults of a specific quantile are better (or worse) off than control adults of the same quantile. This generates a comparison between quantiles of different distributions

for the treated and control groups. Such particular deviation from the quantile regression is basically denominated QTE.

Estimation of Quantile Treatment Effects

Let the $Yi(1), Yi(0)$ be the potential outcome for an individual *i*. $Yi(1)$ would be realized if individual *i* were to receive the treatment 1 (land title) and $Yi(0)$ would be realized otherwise. The observed outcome, Yi is equal to $Yi = Yi(1)Ti + Yi(0)(1-Ti)$. Hence, the main objective of the procedure applied in this study is to estimate the entire distribution function of $Y(1)$ and $Y(0)$.

The unconditional QTE (for quantile τ) is given by:

$$\Delta^{\tau} = Q^{\tau}_{Y^1} - Q^{\tau}_{Y^0}. \tag{4}$$

First, the definition of the unconditional QTE does not change when a set of covariates changes. Second, unconditional effects can be estimated consistently at the rate $\sqrt{n}$ without any parametric restrictions. A further advantage is that for policy makers, the effects in the entire population are often more interesting than a large number of effects for different covariate combinations. The unconditional effects can be directly conveyed and summarized. In summary, the quantile regression is conditional on X and parametric, whereas the QTE, as explained above, can be semi-parametric and unconditional.

The QTE estimates can be improved by creating weighting for the treated and control samples via a *propensity score*. Firpo (2007) demonstrates that under (1) *ignorability of treatment*, (2) *common support* and (3) *quantile monotonicity existence* estimating the QTE is possible by using the weighting estimator of the *check-functions* obtained from a *propensity score* procedure. Thus, we follow the method suggested by Firpo (2007) and use the following QTE estimator:

$$\hat{\Delta q}_{\tau} = \arg\min_{\alpha,\Delta} \left\{ \frac{1}{N} \sum_{i=1}^{N} \hat{\omega}_i \left(T_i, X_i\right) \rho_{\tau} \left(Y_i - \alpha - \Delta T_i\right) \right\},$$

where $\hat{\omega}_i(T_i, X_i) = \dfrac{T_i}{\hat{p}(T=1 \mid X_i)} + \dfrac{1-T_i}{1-\hat{p}(T=1 \mid X_i)}$, $T = T_i + (1-T_i)$ is a *dummy* that represents the treatment ($T_i = 1$ if belongs to treated group and 0 if is related to the control group) and $\hat{p}(X_i)$ is the *propensity score* obtained non-parametrically by a local logit estimator from T given X and constant. In the present case, given the small sample size, we opted to estimate the propensity score parametrically using a logit model. The table 9 in appendix reports the logit regression for the unbalanced and for the balanced sample. As can be observed, none of the *X* variables are significant in the estimate that regards the balanced

sample. The caveat of using the *propensity score* is the reduction of the sample size given that only households in the common support are considered for the estimation. In our case, we lost 17 observations. Hence, the analysis is computed for 288 households only.

V. EMPIRICAL RESULTS

The empirical results of the analysis suggested above are presented in Tables 7 and 8, and Figures 7 and 8.

The results on Table 7 show that not only land title has a positive effect on labor supply but also the effects are not similar across the quartiles.[23] They provide a different picture compared to mean treatments analysis discussed before, such as Field and Torero (2002). Those authors find that households with land title work on average 12.2 work hours more per week compared to households without land titles. However, those authors do not discuss the differences of the effects of land title in the distribution of working hours.

Figures 7 and 8 illustrate the difference between the ATT and quantile effects. In the first analysis, the first quartile (0.25) and the second quartile (0.50) present a coefficient value for land title equal to 8.37 and 10.50 respectively, greater than the third quartile (0.75) and last decile (0.90) with 2.57 and 2.20. This suggests that the program may have affected positively mainly those who did not worked many hours previously, reinforcing the positive impact of land titling on property security and labor supply[24]. The common support analysis follows the same trend, i.e, with first quartile (0.25) and the second quartile (0.50) having the highest levels, 6.50 and 10.85. However, it is worth mentioning that the similarity between the impact on the median and on the average suggest that outliers are not driving the estimate of the ATT.

In turn, Table 8 shows that QTE results obtained with the Firpo (2007) method is different from the quantile regression estimates. For example, the impact of land title is higher in the first quartile of the outcome variable distribution, 20 hours per week, and it is significant. The impact on the median is equal to 11 hours per week but it is not statistically significant. As can be noted, the effect of land title seems to be concentrated among the first and third quartiles of the outcome variable distribution.

There are two main implications of this analysis: (1) the use of quantile approach points out the importance to take into consideration the heterogeneous effect of a public policy, and (2)

[23] By using Wald tests (not shown here), the coefficient of the first quartile is not statistically different from the others. This is true for the three sets of estimates. Regarding the second block, the coefficient of third quartile is not statistically different from the coefficient of ninth decile. None of coefficients are statistically different from that of the third block.

[24] Though, that the first quartiles of distribution were more affected by the program. However, if the *rank invariance assumption* is invoked then it can be argued that those adults who worked less in the baseline were the most affected by the policy.

the relevance to focus the analysis on households pertaining to the common support, i.e., households that are similar in observables characteristics.

These results show the constraints of applying quantile regression with unbalanced sample, even when there are evidences that the unobservables are not contaminating the experiment. Estimates using the *propensity score* technique to select comparable households seem to be more appropriated when the observables characteristics of the treated and control groups are fairly distinct. Nevertheless, the main results remain applying both techniques. While the OLS and DD estimates pointed to a positive effect of the land title on the average of hours worked weekly, the QR estimates and the QTE estimates showed that the effect is quite heterogeneous. The quantile methods present, in addition, that the effect of land title on hours worked weekly is greater for the first two quartiles of the distribution of hours worked weekly in our sample. As our model suggests, one explanation for that is property security that the land title provides particularly for those households out of the labor market, enabling them to find an occupation instead of staying at home protecting it. This finding evinces then the relevance of such titling programs for the poorest urban households of developing countries

VI. CONCLUSION

This paper show new evidence on the value of formal property rights in urban squatter communities of a developing country. First, it presents a simple model based on Field (2007) that rationalizes the effect of land title on labor force participation. The model introduces heterogeneous households and shows that receiving a land title may have higher or lower impact on the labor supply of the household, depending on her initial level of participation on the labor force.

On the empirical front, although existing studies indicate significant effects on access to credit, income, home investment, labor supply and fertility (see Field, 2007; and Andrade, 2006), this study fills an important gap in the literature on property rights. By studying the relationship between the exogenous acquisition of land title and hours worked weekly, the study provides additional empirical support for the finding that property title increases labor force participation.

Additionally, it applies quantile treatment effects (QTE) techniques showing that the effect of the land titling differs among the quantiles of the labor market participation in the sample. The main results indicate that the households who benefit most from the formal property right are those in the first two quartiles of the labor force participation. Thus, the mostly excluded from the labor force are the ones gaining more from the titling program. Such findings could not have been revealed using a simple mean estimation analysis.

Econometrically, the quantile treatment effect is obtained via the estimation of quantile regressions. Further, the weighting estimator of Firpo (2007) is also used to minimize the bias caused by selection on observables. The results also indicate the limitation of randomization in the estimations and the gains of using propensity score matching techniques to selected the sample. 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 governments from developing countries across the world, which considering titling programs to address urban informality. In addition, the results have potential implications for understanding labor market frictions in those developing countries (Goldsmith, 1995). As our model also suggests, in places characterized by high levels of residential informality such as most of developing and poor countries, informal property protection may constitute an important obstacle to labor market adjustment. Hence, land title could potentially be applied as an asset to improve public policy actions that directly impact economic growth.

The current analysis offers various other possibilities for further research. For example, the analysis of the increase in labor force participation owning to the land titling on the income and utility of the households could be investigated. At this stage the income gain for the households of increasing their labor force participation is not clear. Further, the distributional impact of the land titling could be investigated to other economic variables such as access to credit and fertility. That would improve the assessment of such programs in the lives of the millions of households living in urban squatter communities of developing countries across the world.

TABLES AND FIGURES

Table 1. Descriptive Statistics on Some Variables, 2007–2008

	Pre-Program 2007		Post Program 2008	
Variables	Mean	Std. Dev.	Mean	Std. Dev.
Weekly hours of adult work	10.19	12.22	16.18	14.33
Ethnicity (=1 if African Brazilian)	2.75	1.40	2.75	1.40
Gender (=1 if Female)	0.33	0.47	0.33	0.47
Mean Age	40.89	14.68	41.89	14.68
Marital status (=1 if married)	1.98	0.80	1.98	0.78
Monthly In(income) (currency BRL[a])	1,126.25	1,491.92	1,138.76	1,473.35
Number of residents	3.89	1.61	3.96	1.62
Child Labor Weekly Hours	5.50	1.11	5.13	1.20
Years of Education (Family Head)	7.25	4.34	7.31	4.33
Observations	304	304	304	304

Source: Research from the Osasco Land Title Survey and Central Bank of Brazil.
Notes: [a]Currency in exchange rate 12/31/2008, 1 USD = 1.75 BRL (Brazilian Reais).

Table 2. T test and Z-score for the Difference of Means for Covariates, 2007

	Mean Control	Mean Treatment	Treatment-Control *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
Mean Age	42.6	39.4	0.06
Weekly hours of adult work	10.1	10.4	0.81
Weekly hours of Child labor (> 16 years old)	8.35	3.3	**0.00*** **
Child Labor2 (=1 if children current work)	0.34	0.14	**0.00*** **
Years of education (Family Head)	5.00	9.00	**0.00*** **
Monthly In(income) (currency BRL[a])per capita	553.1	255.8	**0.00**
TV(=1 if have)	0.95	0.55	**0.00**
DVD (=1 if have)	0.74	0.42	**0.00**
Radio(=1 if have)	0.81	0.31	**0.00**
Car(=1 if have)	0.44	0.3	0.015
Washing machine (=1 if have)	0.62	0.87	**0.00**
Refrigerator(=1 if have)	0.98	0.62	**0.00**
Freezer(=1 if have)	0.46	0.64	**0.00**
Informal worker (=1 if informal)	0.94	0.65	**0.00**
Household income per capita*Informal	537.4	188.38	**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	168	137

Source: Research from the Osasco Land Title Survey and Central Bank of Brazil
[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
*, **, *** rejection of the null hypothesis of equal mean at 10, 5, and 1 percent respectively.

Table 3. Spearman Correlation, 2007

	Years of Education	Informality	Monthly Income per capita
Years of Education	1		
Informality	-0.15*	1	
Monthly Income per capita	-0.23*	0.23*	1

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

Table 4. Land Title Impact on Labor Supply (Pre Program), 2007

Variables	Model (1)	Model (2)
Land Title	0.72	-1.50
	(1.41)	(1.62)
Control Vector	Without	With
Observations	304	304
R2	0.01	0.14

Notes: Robust standard errors in parentheses.
*, **, *** significant at 10, 5, and 1 percent respectively.

Table 5. OLS Estimates Land Title Impact on Labor Supply (Post-Program), 2008

VARIABLES	Model OLS-Naïve	Model OLS	Model OLS Balanced	Model Tobit
Land Title	9.37***	6.06**	6.39**	9.12 **
	(1.55)	(2.51)	(2.63)	(3.80)
Gender (=1 if Female)		4.79***	4.69***	7.14***
		(1.68)	(1.73)	(2.40)
Ethnicity (=1 if African Brazilian)		-1.66	-1.96	-2.29
		(1.56)	(1.65)	(2.37)
Marital status (=1 if married)		0.22	0.30	0.48
		(1.58)	(1.70)	(2.41)
Age		-0.10*	-0.08	-0.21**
		(0.05)	(0.06)	(0.08)
Weekly hours of Child labor (> 16 years old)		-0.04	-0.01	-0.07
		(0.060)	(0.07)	(0.11)
Years of education (Family Head)		0.34	0.32	0.50
		(0.21)	(0.23)	(0.33)
Monthly In(income) (currency BRL[a])per capita		-0.00	0.00	-0.00
		(0.00)	(0.01)	(0.01)
Access to Credit (=1 if have)		3.97**	3.42**	6.11**
		(1.64)	(1.74)	(2.55)
TV(=1 if have)		1.14	1.23*	1.83*
		(0.70)	(0.71)	(0.95)
DVD (=1 if have)		0.44	0.38	0.50
		(0.84)	(0.87)	(1.22)
Radio(=1 if have)		-2.37**	-2.41**	-3.60**
		(1.10)	(1.11)	(1.76)
Car(=1 if have)		0.14	0.20	0.50
		(0.39)	(0.41)	(0.60)
Washing Machine(=1 if have)		-0.23	-0.35	-0.24
		(0.78)	(0.86)	(1.18)

Refrigerator(=1 if have)		0.33	0.38	-0.66
		(2.29)	(2.30)	(3.21)
Informal Worker		1.61	1.68	2.83
	11.08***	(1.90)	(1.95)	(2.80)
Constant	(1.12)	10.08**	9.80*	4.66
		(5.04)	(5.17)	(7.34)
Sigma				18.48***
				(1.01)
Pseudo-R2/R2	0.10	0.20	0.17	0.03
Observations	305	305	288	305

Notes: Robust standard errors in parentheses.
*, **, *** significant at 10, 5, and 1 percent respectively.

Table 6.Difference-in-Difference – land title impact of labor supply (2007, 2008)

VARIABLES	Weekly Hours Worked (DD Naïve)	Weekly Hours Worked (DD-Unbalanced)	Weekly Hours Worked (DD-Balanced)
Land Title	0.36	-0.31	-0.128
	(1.42)	(2.03)	(2.07)
Land*Year (*DD*)	9.01***	8.07***	8.2***
	(1.16)	(1.25)	(1.27)
Year	1***	1.177***	1.25***
Gender (=1 if Female)	(0.28)	(0.36)	(0.39)
		5.93***	5.82***
		(1.47)	(1.50)
Ethnicity (=1 if African Brazilian)		-1.21	-1.49
		(1.30)	(1.36)
Marital Status(=1 if married)		-0.44	-0.40
		(1.38)	(1.46)
Age		-0.118***	-0.10**
		(0.04)	(0.04)
Weekly hours of Child labor		-0.0489	-0.02
		(0.05)	(0.05)
Years of Education (Head)		0.277	0.24
		(0.19)	(0.20)
Monthly In(income) (currency BRL[a])per capita		-7.57e-05	0.00165
		(0.000723)	(0.00164)
Access to Credit		2.149*	1.692
		(1.226)	(1.264)
TV(=1 if have)		1.018*	1.122**
		(0.543)	(0.545)
DVD(=1 if have)		0.136	0.0689
		(0.644)	(0.666)
Radio(=1 if have)		-1.214	-1.401
		(0.992)	(1.001)
Car(=1 if have)		0.227	0.265
		(0.327)	(0.346)
Washing machine(=1 if have)		-0.308	-0.473
		(0.559)	(0.602)
Refrigerator(=1 if have)		1.372	1.604
		(1.623)	(1.636)
Informal worker(=1 if have)		1.885	1.854
		(1.444)	(1.460)
Constant	10.07***	8.675**	8.265**
	(1.06)	(3.786)	(3.870)
R2	0.10	0.22	0.20
Observations	609	609	575

Notes: Robust standard errors in parentheses.
*, **, *** significant at 10, 5, and 1 percent respectively.

Table 7. The distributive effect of land title on adult labor supply
Quantile Regression – Parametric Conditional

Variables	(0.10)	(0.25)	(0.50)	(0.75)	(0.90)
Land title	0.00	8.37*	10.5***	2.57***	2.20**
	(0.05)	(4.68)	(3.91)	(3.46)	(6.01)
Control vector (Complete sample)	With	With	With	With	With
Land title		6.50	10.85***	3.36	1.06
		(4.70)	(3.69)	(3.66)	(5.60)
Control vector (Common Support)	With	With	With	With	With
Observations					
Complete Sample	305	305	305	305	305
Common Support	288	288	288	288	288

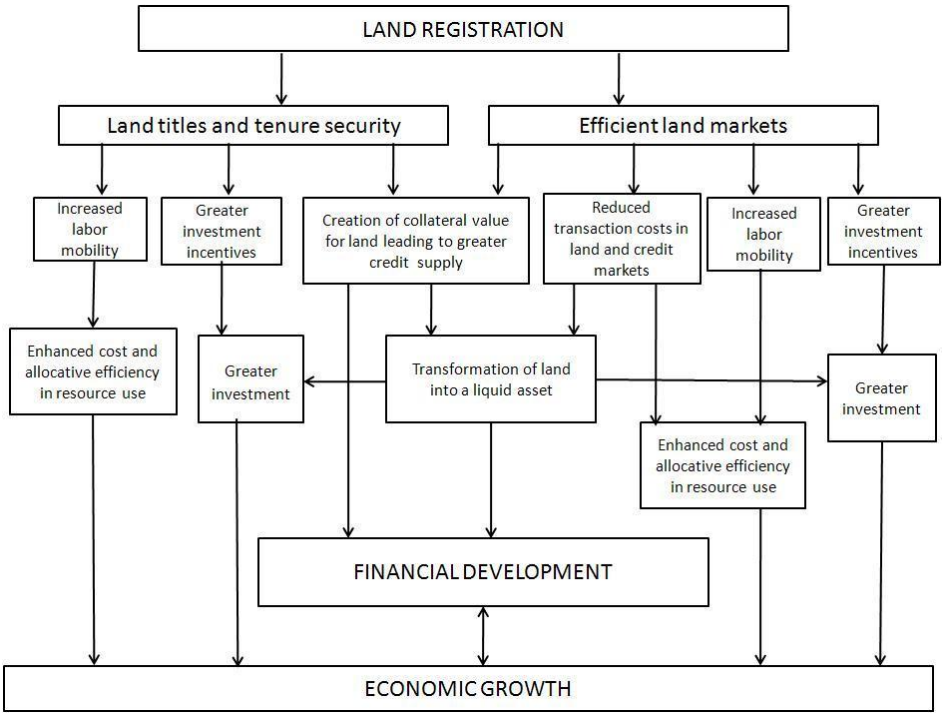
Notes: Standard errors (in parenthesis) are computed using bootstrap with 100 replications. *, **, *** significant at 10, 5, and 1 percent respectively.

Table 8. Quantile Treatment Effect Estimates
QTE – Semi Parametric Unconditional

Variables	(0.10)	(0.25)	(0.50)	(0.75)	(0.90)
Land Title	0.00	20.00***	11.00	9.00**	-6.00
	(0.13)	(3.15)	(11.00)	(4.10)	(5.24)
Control Vector (Complete sample)	With	With	With	With	With
Observations	288	288	288	288	288

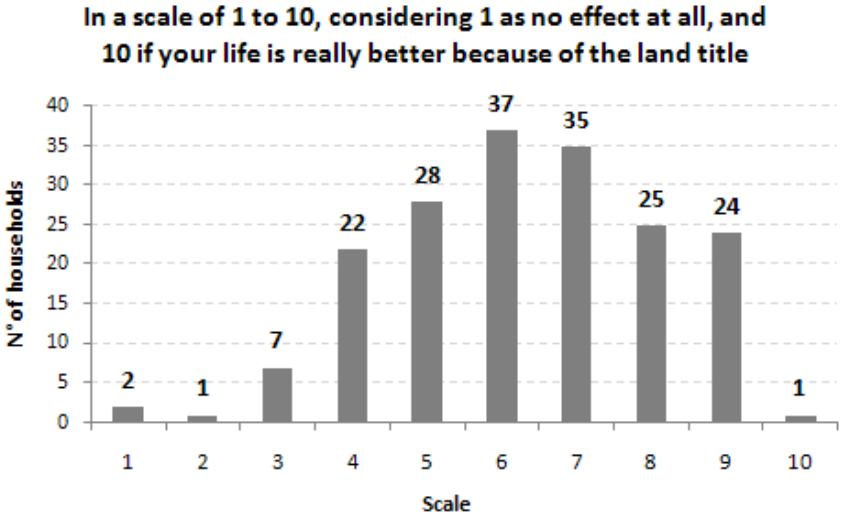
Notes: Standard errors (in parenthesis) are computed using bootstrap with 100 replications. . *, **, *** significant at 10, 5, and 1 percent respectively.

Figure 1. Land Registration Diagram



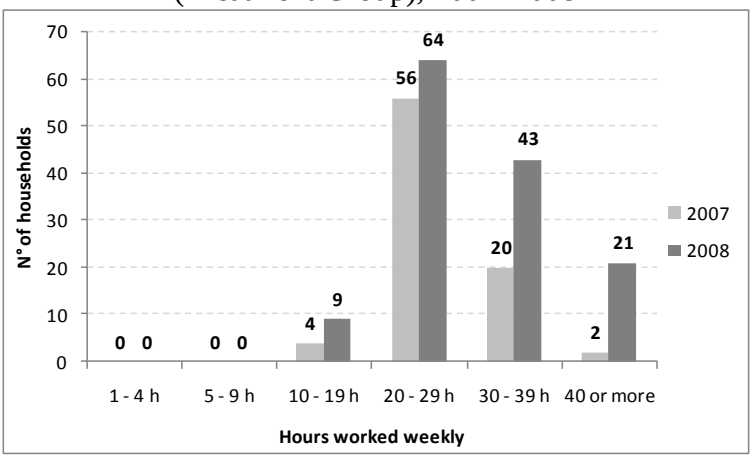
Source: World Bank, 2008

Figure 2. How Land Title Affected Household's Life?



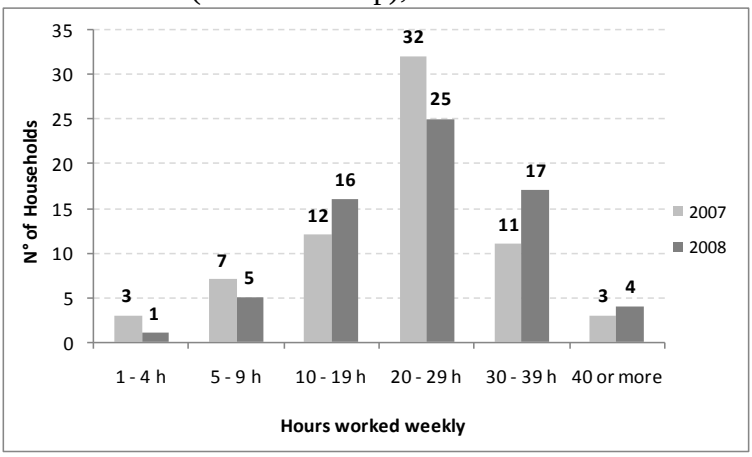
Source: Research from the Osasco Land Title Survey – 2008

Figure 3. Adult Labor Force Hours Worked Weekly x Number of Households (Treatment Group), 2007–2008



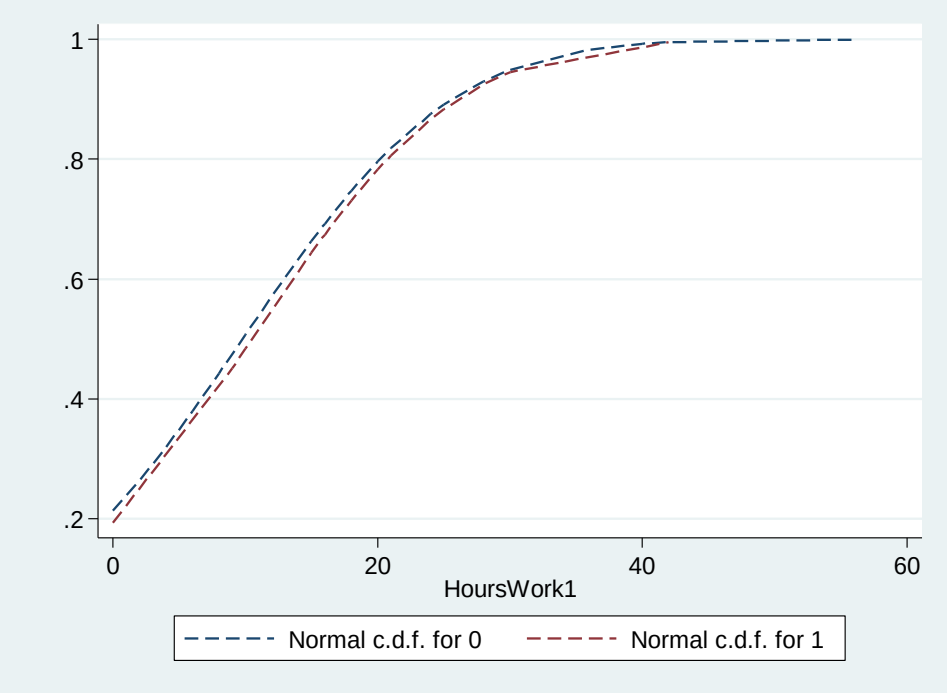
Source: Research from the Osasco Land Title survey.

Figure 4. Adult Labor Force Hours Worked Weekly x Number of Households (Control Group), 2007–2008



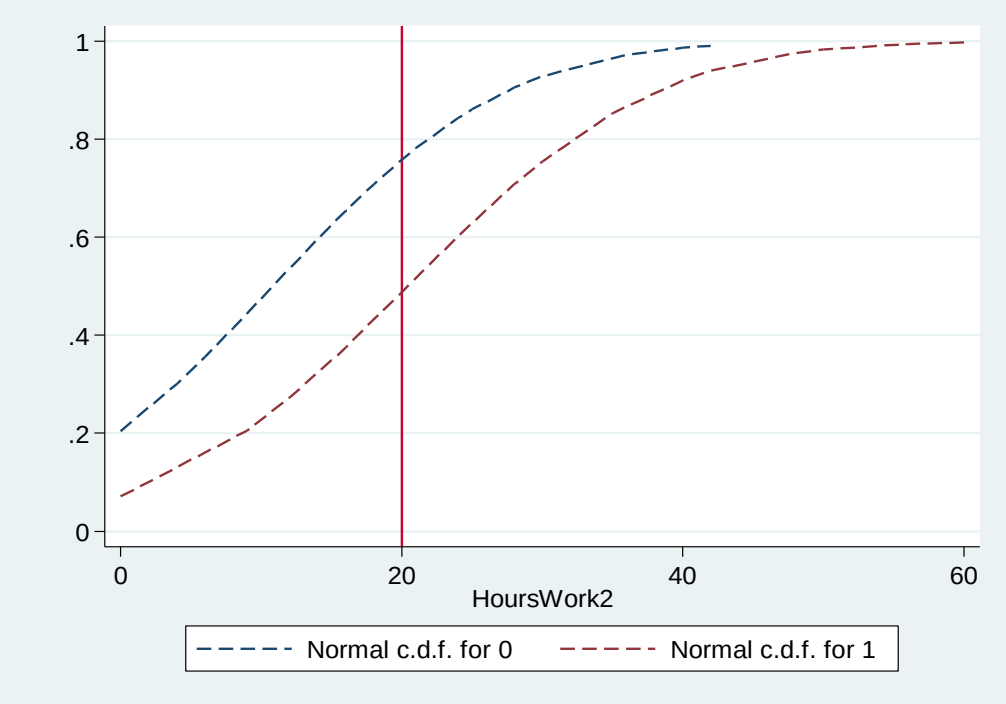
Source: Research from the Osasco Land Title survey.

Figure 5. Differences Between Cumulative Distribution Functions of the Adult Weekly Worked Hours for the Treated and Control Groups, year



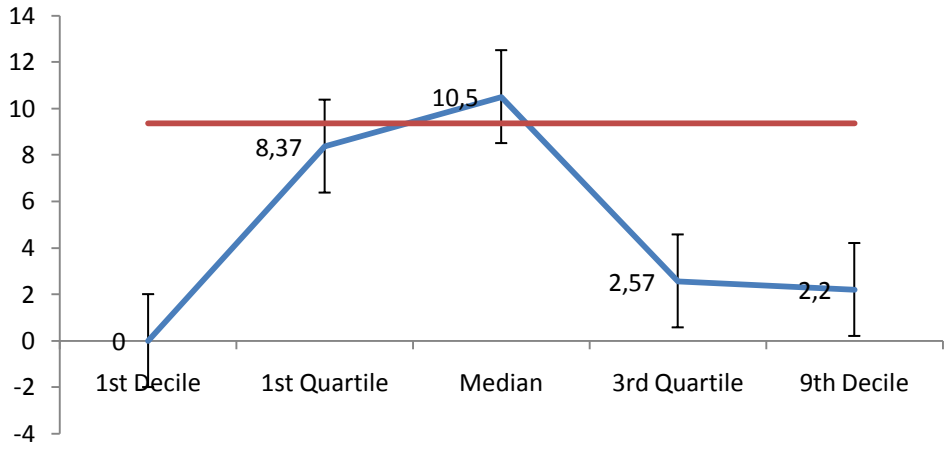
Note: Pre treatment cumulative distributions of weekly worked hours per adult in the treated (=1) and control (=0) groups.

Figure 6. First order stochastic dominance analysis for the impact of title land on weekly worked hours per adult, year



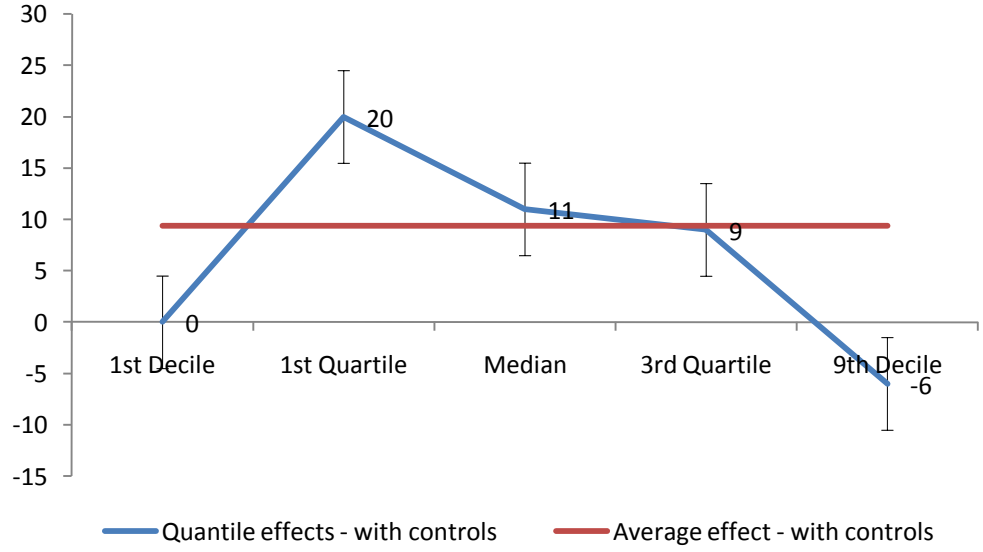
Note: Post treatment distribution of weekly hours worked per adult in the treated (= 1) and control (=0) groups. The distribution for the treated first order dominates the distribution of control group.

Figure 7: Quantile Regression Effects with Control Variables



Source: Author analysis

Figure 8. Quantile Treatment Effects with Control Variables



Source: Author analysis

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Annex A:

Table 9: Propensity Score:
Logit Estimates for the Selection of the Treatment Group (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)	0.041	0.023
	(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	0.020	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 ln(Income)per capita	-0.01**	-0.01
	(0.000638)	(0.000727)
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	-0.033
	(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

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