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The Distributive Effects of Land Title on Labor Supply: Evidence from Brazil

Mauricio Moura, Caio Piza, and Marcos Poplawski-Ribeiro

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The Distributive Effects of Land Title on Labor Supply: Evidence from Brazil[1]

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Abstract

This paper studies the effects of property-titling on labor supply. The role of legal ownership security is isolated by comparing the effect that being part of, or excluded from, a land title program in a unique quasi-experiment in two similar communities in the Brazilian city of Osasco. Our main innovation is the estimation of the distributive impact of land title on hours worked via the quantile regression methodology and the weighting estimator of Firpo (2007). The estimates suggest that the impact of land-titling on labor supply is heterogeneous and greater for those households with fewer hours worked before the program.

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I. INTRODUCTION

Economic historians have powerfully documented the role played by property rights in economic development. North and Thomas (1973) and North (1990), for example, consider property rights' fragility a crucial obstacle for economic development. That is empirically supported by United Nations (2005), who estimates that 930 million people around the world, particularly in the developing economies, live in urban dwellings without possessing formal titles to the plots of land they occupy.

The lack of a formal property right system constitutes a severe limitation particularly for the poor. Among other effects, it creates constraints for them on using land as collateral to access credit markets (Besley 1995). Significantly, if such credit were available, it could be invested as capital in productive projects, promptly increasing labor productivity and income (Demsetz 1967, De Soto 2000, Field and Torero 2002).

The lack of property rights also tends to make the real estate market highly illiquid, while keeping market prices for real estate poorly defined in low-income countries (LICs). These factors, in turn, weaken a potentially important channel for arbitrage, and diminish the power of the asset channel in monetary policy in those countries (Mishra, Montiel, and Spilimbergo 2010). Figure 1 illustrates some of the main channels by which land-titling could affect poverty and economic growth.[5]

Property-titling is thus increasingly considered one of the most effective policies for targeting the poor and encouraging economic growth around the globe (Baharoglu 2002, Binswanger, Deninger, and Feder 1995, and Field 2007). In Asia, millions of titles are being issued in Vietnam and Cambodia, while China is also considering implementing such a policy.[6] In Africa, several governments are investing in social housing. In Latin America, the most famous example of a property-titling program is Peru's; during the 1990s, the Peruvian government issued property titles to 1.2 million urban households. Yet, urban property-titling has also been successfully implemented in Argentina (Galiani and Schargrodsky, 2011).

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

[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 reports China's intension to put in place the most ambitious land-titling program in world history, including this initiative as one of the main points of the Chinese economic development model.

Brazil and to promote an increase in the quality of life for the Brazilian population."[7] It is intended to issue land titles to families living under illegal conditions (i.e., illegal residents squatting in urban dwellings). The Brazilian government estimates that 12 million people currently live under illegal urban conditions in the country (IBGE 2007).

This paper aims to measure some of the impacts of the Brazilian land-titling program on labor supply. Such analysis could provide lessons for titling programs and pro-poor spending in other developing and emerging economies. The paper also examines the distributive impact of exogenous changes in formal property rights on the household's supply of labor (with a specific focus on non-agricultural households), and on the value to urban residents of increased ownership security. To the best of our knowledge, this is the first time that potential distributional effects of a land-titling program on labor supply are analyzed.

We use a unique quasi-experimental dataset that helps to isolate the causal role of land-titling, and eliminate the endogeneity problems featured in most of the studies in this field. We compare two neighboring, very similar communities in the Brazilian city of Osasco. This town—with around 654,000 inhabitants and nearly 6,000 families living informally on urban property—is located in the metropolitan area of São Paulo and is part of the Papel Passado's program map. In one of its communities, Jardim Canaã, all households received the land title in 2007. In another, Jardim DR, the same program is scheduled for 2012, making it a natural control group. Officially, the City Hall of Osasco claims that the decision to have Jardim Canaã as the starter community was random.

Our analysis is based on a two-stage survey run in Jardim Canaã and Jardim DR and focuses on the property right issue. The sample consists of 326 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 stage was collected in August 2008, almost one year and half after the titles had been received.

The findings suggest that land title programs could be more effective if directed toward the poorest households with fewer initial hours worked outside their home. Our results firstly confirm that land-titling has a positive impact on adult labor supply and hours worked. As the related literature argues, the main reason for the increase in hours worked is that, with formal property rights, individuals feel themselves secure enough to supply external labor rather than staying at their homes to protect them (Field and Torero, 2002).

In addition, our new empirical findings indicate that the impact of land-titling on labor supply is heterogeneous. The households that benefit most from the formal property right appear to be those that worked fewer weekly hours before the program (i.e., those most excluded from the labor force, initially).

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[7] See *Associação dos Notários e Registradores do Brasil*—ANOREG, 2007. The quotation's translation into English is ours.

The rest of the paper is organized as follows. Section II presents a literature review and theoretical background on why land-titling could increase the labor supply of squatter households. Section III discusses methodological issues, the data collected, and some descriptive statistics. Section IV describes our empirical strategy, including the different econometric methods employed, namely, OLS, difference-in-difference, quantile regression and quantile treatment effect. The empirical results are then discussed in Section V. Lastly, Section VI concludes the paper, fleshing out some policy recommendations and proposing areas for future research.

II. LITERATURE REVIEW AND THEORETICAL BACKGROUND

Effects of land-titling have already been documented by several studies. A partial listing includes: (i) Jimenez (1985), Alston, Libecap, and Schneider (1996), and Lanjouw and Levy (2002) on real estate values; (ii) Besley (1995), Brasselle, Gaspart, and Platteau (2002), Jacoby, Li, and Rozelle (2002), and Do and Iyer (2003) on agricultural investment; and (iii) Place and Migot-Adholla (1998), Carter and Olinto (2003), Field and Torero (2002), and Galiani and Schargrodsky (2011) on credit access, labor supply, housing investment, and income.

Most of this literature and the majority of the policy attention to property rights center on rural households' tenure security. According to Field and Torero (2002), this is presumably because of the historical interest in agricultural investment and related politics of land reform. In this area, Besley's (1995) findings, for instance, are ambiguous: land rights appear to have a positive effect on agricultural investment in the Ghananian region of Angola, but a less noticeable impact on the region of Wassa. Along the same lines, Jacoby, Li, and Rozelle (2002) find positive effects of land title in China, whereas Brasselle, Gaspart, and Platteau (2002) find no effects for Burkina Faso.

The effects of property title have been analyzed less often in urban settings than in rural ones, although most of the developing world (particularly its impoverished population) is increasingly urban. For urban locations, empirical work has focused on real estate prices. Jimenez (1984), for example, uses a general equilibrium model of urban squatting to show 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. His empirical application to the Philippines finds equilibrium price differentials between formal and informal sector unit dwelling prices in the range of 58.0%, which is greater for lower income groups and larger households.

In turn, Field and Torero (2002) cite the positive effects of the Peruvian titling program, particularly in labor supply, credit access, and housing investments. They exploit timing variability in the regional implementation of the program using cross-sectional data on past and future title recipients midway through the project. In Argentina, Galiani and Schargrodsky (2011) find that entitled families substantially increased housing investment, reduced household size, and enhanced the education of their children. For them, land-titling can be an important tool for poverty reduction, although not through credit access and entrepreneurial income, but through the slow channel of increased physical and human capital investment. In Brazil, Andrade (2006) demonstrates the positive effect of land title on income, using cross-sectional data from a sample of 200 families of the Comunidade do Caju, a poor, urban community in the city of Rio de Janeiro.

A common obstacle faced by all of the studies mentioned above is how to measure the influence of land title, considering the potential endogeneity of ownership rights (Demsetz 1967, and Alchian and Demsetz 1973).[8] Field and Torero (2002), for example, show that untitled households are constrained by the need to provide informal policing, both to deter potential burglars from invading private property, and to participate actively 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.

Hence, 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. An exogenous increase in the formal property protection should lower the opportunity cost of outside labor, increasing the hours worked outside the house and making the likelihood of a rise in the current income of those households higher (Field and Torero 2002).

Previous literature has demonstrated this point with a theoretical model for a representative household. Yet, here, we are also interested in the heterogeneous response of hours worked outside the house owing to stronger property rights. In particular, we aim to investigate whether or not land-titling affects the increase in labor supply differently for households diverging in terms of initial hours worked.

To illustrate this point in a setting of incomplete property rights, we assume that the standard labor-leisure choice is influenced by different household members' wages and the demand for property security. Specifically, we follow a model extension of Field's (2003) with adult and child labor, adapting it for the case of two different adults in the household. The two adults (A and B) diverge in terms of their wage level depending on their respective set of characteristics (e.g., education and skills). We assume that individual A has a higher wage than individual B $\left(w_A > w_B\right)$, and therefore, that individual A works initially more hours than individual B .

The model is a simple variation of the basic agricultural household static set-up used in Field (2003, 2007).[9] The utility function has a term representing a tenure security function, $s(\bullet)$, such that leisure enters the household utility through two separate channels: (i) through its

[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] For a survey on different and more advanced models of labor supply, see Blundell and MaCurdy (1999).

own effect on utility, and (ii) through its effect on home security.[10] The security value of time spent at home is modeled as a household public good, such that individual utility depends on the leisure of the other household members via $s(\bullet)$.

Utility is an increasing function of per capita leisure, consumption, and home security, and is determined by the total hours of leisure $L = l_A + l_B$. In turn, total hours of leisure, as opposed to Graham and Green's (1984), is here assumed to be a perfect substitute for: (i) the hours an individual spends on property protection;[11] (ii) the exogenous household level of formal property rights, θ; and (iii) the degree of informal or de facto rights the household has acquired, τ.

For tractability, the other assumptions are, again, the same as in Field (2003, 2007). First, the household is assumed to maximize per capita leisure, and not the leisure of individual members. Second, there is no outside labor market for the provision of home security. Third, while the model does not explicitly include hired security, there is room to incorporate the existence of a black market for property protection into τ.

Hence, letting x_A and x_B be the consumption of each household individual, h_A and h_B the time endowment of each household member, and

$$H = h_A + h_B,\ X = x_A + x_B,\ \bar{x} = \frac{X}{2},\ \bar{l} = \frac{L}{2}, \tag{1}$$

the value of work outside the home is given by the market wages w_A and w_B.

In turn, household utility is given by:

$$U\left(\bar{x}, l_A, l_B, s; \psi, E_A, E_B\right), \qquad s = s\left(L, \theta, \tau\right). \tag{2}$$

[10] As opposed to models of joint production in the same vein of Gronau (1977), we make the assumption of incomplete substitution between market goods and home security owing to the absence of an outside market for home security protection.

[11] As Field (2003) argues, while this assumption might seem unreasonable in light of the fact that the leisure time contributing to home security is constrained relative to leisure itself, which can be spent inside or outside of the home, incorporating a jointness function that measures the personal value of a home relative to market production does not change the comparative statics of the model.

Here, $U(\bullet)$ and $s(\bullet)$ are twice continuously differentiable, concave, and increasing in each argument.[12] The parameter θ can be thought as of either a binary indicator of a legally registered property title, or a continuous parameter reflecting the level of formal legal recognition of a household's tenure status (level of efficiency of court systems, levels of police cooperation, etc.).

The household faces the following constraints:

$$
\begin{gathered}
s = s(L,\theta,\tau), \\
w_A * (h_A - l_A) + w_B * (h_B - l_B) = X, \\
l_A, l_B, h_A, h_B, X \geq 0.
\end{gathered}
\tag{3}
$$

Assuming that the utility function (2) is additive and fully separable in each one of its terms, we can then rewrite the household maximization problem as

$$
\max_{l_A, l_B, \bar{x}} U\left(\frac{w_A}{2}(h_A - l_A) + \frac{w_B}{2}(h_B - l_B), \frac{l_A + l_B}{2}, s(L,\theta,\tau)\right). \tag{4}
$$

The first-order conditions yield:

$$
\frac{\partial U}{\partial l_A} = -\frac{w_A}{2} U_{\bar{x}} + \frac{1}{2} U_{l_A} + s_L U_s = 0, \tag{5}
$$

$$
\frac{\partial U}{\partial l_B} = -\frac{w_B}{2} U_{\bar{x}} + \frac{1}{2} U_{l_B} + s_L U_s = 0, \tag{6}
$$

where $U_p = \dfrac{\partial U}{\partial p}$ is the marginal utility with a particular parameter p, and s_L represents $\dfrac{\partial s(L,\theta,\tau)}{\partial L}$. By taking the total derivatives of the first-order conditions (5) and (6), and solving for $\dfrac{\partial l_A}{\partial \theta}$ and $\dfrac{\partial l_B}{\partial \theta}$, we can find the effect of an increase in the level of formal property rights θ on the leisure-labor choice of each member of the household.

Thus, assuming that all first derivatives of $U(\bullet)$ and $s(\bullet)$ are weakly positive, and that their second derivatives are weakly negative, the sign of $\dfrac{\partial l_A}{\partial \theta}$ is ambiguous. More formal property rights can either decrease the number of leisure hours for individual A, or actually reduce

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[12] Again, as in Field (2003), we assume that the security inputs (L,θ,τ) are substitutes in production and we also make corresponding assumptions on the cross-partial derivatives of $s(\bullet)$.

them.[13] Yet, along the same lines as in Field (2003), an increase in formal property rights enhances the incentives for the individual initially working fewer hours (adult B) to supply more labor, unambiguously reducing his leisure time, namely, $\frac{\partial l_B}{\partial \theta} < 0$.

The intuition for these results is that the land title increases the property security for the entire household. Hence, the title provides incentives for the individual *B* to supply more labor outside the house, which, in turn, reduces the pressure on the individual *A* to work excessive hours in order to obtain the same level of consumption per capita for the entire household.

The results also yield the following proposition, relevant for our empirical analysis:[14]

Proposition 1. *If the wage differential between the household members A and B is sufficiently high* $\left(w_A >> w_B\right)$*, the effect of higher property rights on the number of labor hours supplied is larger for the individual initially working fewer hours (adult B) than for the individual initially working longer hours (adult A), namely,*

$$\frac{\partial l_A}{\partial \theta} > \frac{\partial l_B}{\partial \theta}. \tag{7}$$

Proposition 1 and Equation (7) suggest, then, that land-titling programs would increase more the number of hours worked for individuals belonging to the lower quantiles of the initial distribution of hours worked, than for those individuals belonging to the higher quantiles. The following sections attempt to test this proposition.

III. Methodological Issues, Data, and Descriptive Statistics

This section discusses some of the issues with the sample used to evaluate the titling program, as well as describes the dataset, presenting some its initial descriptive statistics.

A. Minimizing Selection Bias Concerns

The Brazilian federal government chose Osasco as one of the cities to make part of the Papel Passado titling program. 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 (ANOREG

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[13] That second possibility can happen owing to substitution effects in case individual's *A* wage is sufficiently larger than that of individual *B*, $w_A >>> w_B$. The second-order effect of the utility of consumption is higher than the that of the utility of leisure $\left(U_{\bar{x}\bar{x}} > U_{l_B l_B}\right)$. See the theoretical appendix (available upon request).

[14] See the theoretical appendix (available upon request) for the proof of Proposition 1.

2007). The program timetable for Osasco establishes that all the communities living in illegal condition would be part of the Papel Passado during the period 2007–2014.

Yet, given that fiscal resources are limited, not all of the communities are receiving the land title at the same time. The first locality to receive the land title in 2007 was Jardim Canaã, where 500 families live.[15] The closest neighborhood to Jardim Canaã is one called DR, containing 450 families. The DR's households are scheduled to be part of the Papel Passado program in 2012.

The Osasco City Hall officially claims that the priority of localities to receive the titles follows a random criterion.[16] This randomization minimizes the issue of selection bias and self-selection in the program evaluation, common in non-experimental evaluations (Behrman and Todd 1999).[17] In addition, 95 percent of the first survey participants—both from Jardim Canaã and DR—did not expect to receive any land titles. They were not aware of Papel Passado nor the meaning of it, reducing a potential behavioral deviation from households included in the program (*randomization bias*).[18]

In turn, *contamination bias* is avoided here, given that the control group of residents cannot benefit from the program outside the treatment locality. Besides that, there are no other land title programs in the region.[19] The program also does not provide a dropout option. If, even after receiving the title, the householder sells the property and moves out of the locality, he already will have been affected by the program, lowering the probability of *attrition bias*.[20]

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[15] The medium size of the titled land was 38 square meters. In Osasco, property tax is charged only for properties above 50 square meters. Thus, only 10 families began to pay property tax owing to the received title.

[16] Yet, unofficial sources from local communities in Osasco allege that a "political" agenda may have influenced the decision.

[17] Skoufias (2001) uses a similar natural experiment to evaluate the income transfer initiative of Mexico, called PROGRESA. 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). This assignment at the locality level minimizes the chances of spillover effects between treated and untreated individuals at the same locality.

[18] Randomization bias occurs, for example, when 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 cases, results obtained from the evaluation may not be generalized to a context where the program is not being implemented as a randomized trial.

[19] Contamination bias happens when members of the randomized-out control group seek and receive alternative forms of treatment. This is usually a problem only when there are close substitutes to the program.

[20] Attrition 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 period of time), then the attrition bias can pose a major problem. It is usually nonrandom and can compromise the benefits of randomization.

Further, Jardim Canaã and DR have very similar economic and social characteristics. They are not only official neighborhoods but also, there is no physical border between them, and are, therefore, geographically united. They are located 2.5 miles from downtown Osasco, having precisely the same access to the Osasco's main economic center. This ensures that the treatment group is similar, both in terms of observable and unobservable characteristics to the group that did not receive the land titles.[21]

B. The Data

A two-stage survey focusing on property rights originates the dataset. Minimizing potential other biases, the survey questionnaire and its applicant did not provide the households with any direct information 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 (available upon request) contained 39 questions and was applied to the 326 randomly sampled families. The survey instrument, in many of its questions and methodologies, closely mirrored the content of 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.

In March 2007, the first stage of the survey was conducted before the titles were issued to the households of Jardim Canaã, by researchers not originally from Osasco. The second stage was carried out with exactly the same households in August 2008 (with 98 percent recall), almost a year and a half after the first titles were issued. This time gap between stages was implemented purposely, so that all the households interviewed during the first stage would have had the land title for at least 1 year.[22]

The study also tracked the households that moved away from both communities. The attrition is reasonably low; only 8 percent of the households that received the land title moved away from Canaã.[23] Yet, from the control group, only one household (out of 140) moved out during the same period.

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[21] 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.

[22] This is different from Galiani and Schargrodsky (2011), who ran their two used surveys after the titles were provided. Further, the exact dates on which each interviewed household received the title in our sample were obtained from the 2nd Osasco Office of Registration (*2º Cartório de Osasco*), along with the formal authorization from Osasco City Hall, to conduct the research. Both entities have worked together to map and register all families from that particular area.

[23] Initially, one of the main concerns of local authorities in Osasco was that most citizens would receive the land title, sell the property right away, and return to informal living conditions.

Given the information in the surveys, a technique from Bolfarine and Bussab (2005) was used to randomly select 326 sample households from the two localities: 185 from Jardim Canaã, and 141 from DR. The approach consisted in choosing the first 150 households from Canaã and DR that have the closest birth dates (day and month) to the three field researchers that conducted the survey interviews. Each researcher received 50 names initially as the first base. After reaching each of those households, they could then select the third and the fifth households on the right hand side.

C. Descriptive Statistics

Figure 2 shows that most of the households who received the land title felt that their lives had improved (at least up to the period corresponding with the second stage of the survey). This happened even when they had not previously expected the land title.[24]

In turn, Tables 1 and 2 and Figures 3 and 4 summarize the households' answers (2007 and 2008) regarding weekly hours of work. They show that for both sample groups combined (treated and control), weekly hours of adult work went up between 2007 and 2008. Figure 3 also shows that the treatment group increased its number of hours worked after the program. Yet, for the control group, that value remains practically constant overtime (Figure 4).

Table 2 reports the T-test for the difference of means for covariates in 2007, comparing the control and treatment groups before the program. There, one can readily notice that the numbers of the observations (households) in the treated and control groups are comparable. In turn, child labor, monthly income per capita, informal labor, and the number of residents are significantly lower, whereas education of the family head is significantly higher in the treated group than in the control group.

Thus, while the treated sample is more educated and less informal, it has a lower income than the control group. This is also corroborated Spearman correlations in Table 3 and by the wealth index displayed in Table 2. The wealth index—computed using a principal component analysis—summarizes the stock of durable goods owned by the households, including TVs, radios, cars, washing machines, refrigerators, and freezers. Figure A in the Appendix shows the distribution of this index.

One of the reasons for these results is the fact that households with higher levels of education tend to have more access to formal jobs in Osasco (see Zylberstajn and Neto 1999). So, they tend to receive additional perks that are not reflected in the cash payroll, as is customary for

[24] Whether the provision of land titles to squatters could have encouraged new squatting (and, therefore, violation of landowners' property rights) in other areas is beyond the scope of our study, but that should not be ignored in the evaluation of the impact of this type of interventions (see also Galiani and Schargrodsky, 2011).

formal employees in Brazil.[25] Informal workers, on the other hand, do not have those benefits, relying, essentially, on cash income to compensate for the lack of perks.[26]

Table 3 also shows a positive correlation among monthly income per capita and hours worked weekly (0.11). This goes in line with our underlying assumption in the theoretical section that more initial income leads to more weekly hours worked initially.[27]

IV. Empirical Strategy

The previous section discussed some statistically significant differences between the treated and control groups for our sample. These differences can be attributed to the randomization performed at the community level rather than at the household level. Along the same lines, Skoufias (2001), and Behrman and Todd (1999), demonstrate for the PROGRESSA program that even if the control and treated groups are similar in terms of observables at the community level, they are not fully comparable at the household level. They suggest the use of control variables instead of only estimating the program impact through mean tests.

Here we follow this same empirical strategy and adopt two econometric procedures in order to address the differences in observables. The first is to include control variables and use the methods of ordinary least squares (OLS) and difference-in-difference (DD). The second procedure is to use those same methods, but applying a propensity score technique to select a more balanced sample.

In addition, as a main innovation, we test potential distributional effects of the titling program as illustrated by Proposition 1. These effects are analyzed via quantile regression (QR) and quantile treatment effects (QTE). The following subsections explain in detail each one of the empirical methods used.

A. The OLS Regression Analysis

A standard OLS procedure is used at first to investigate the mean effect of land-titling through the following equation:

$$H_i = \alpha + \delta title_i + \beta X_i^{'} + \varepsilon_i, \qquad (8)$$

[25] For example, a formal Brazilian employee usually receives a health care plan for the whole family, subsidized transportation, and meal plans.

[26] In our data sample, 233 households are informal workers. They represent 92 percent of the workers of the control group and 64 percent of the treated group (see Table 2).

[27] Unfortunately the sample does not contain information on wages, which is the actual variable that our assumption is based upon.

where H_i, as in (1), represents the number or hours worked weekly by the household, and $X_i^{'}$ is a control vector of social-economic variables, including: (i) gender; (ii) ethnicity; (iii) marital status; (iv) age; (v) age squared, in order to test non-linear effects of age; (vi) number of members of the household; (vii) weekly hours of child labor; (viii) years of education; (ix) monthly income per capita; (x) a dummy for access to credit; (xi) household wealth, measured by the wealth index; and (xii) a dummy for informal work. These variables are common covariates for land title and are usually employed in the evaluation of this type of programs.

In turn, the variable $title_i$ is a dummy equaling 1 if the household participated in the titling program, and 0 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).

We also use a variant of the difference-in-differences matching method proposed by Heckman, Ichimura, and Todd (1997), and employed by Smith and Todd (2005) and Angelucci and Attanasio (2009), to estimate the ATT semi-parametrically.[28] We also make estimates (8) based solely on the households who pertain to the common support (or balanced sample), namely, households that are comparable in observable characteristics. As in our case, such an approach is recommended if the control and treated groups are not completely balanced in observables (see e.g., Blundell and Dias 2002, and Abadie 2005).

B. The Difference-in-Difference Methodology

The second econometric method applied is the difference-in-difference estimator. It consists in comparing the difference in outcome before and after the intervention to the group affected (treated), as well as to the group unaffected (control), by the intervention (see Bertrand, Duflo, and Mulainathan 2004, and Imbens and Wooldrige 2008).

This method is estimated by the following regression model:

$$H_{i,s,t} = \beta_0 + \beta_1 treat_{s,t} + \beta_2 year_t + \alpha_{DD}\left(treat_{s,t} * year_t\right) + X_{i,s,t}^{'}\gamma + u_{i,s,t}, \quad (9)$$

where $H_{i,s,t}$ is, again, the number of hours worked weekly by the household i in the community s at time t; $treat_{s,t}$ is a dummy variable equaling to 1 if the individual resides in the treated community ($s = 1$), and 0 otherwise; $year_t$ is a dummy variable equaling to 0 in 2007 (baseline period), and to 1 in 2008; $X_{i,s,t}$ is a vector of observable characteristics of household i in the community s, changing through time; and $u_{i,s,t}$ denotes the error term,

[28] We refer to our estimate as semi-parametric because the propensity score is here estimated via a logit model. Nevertheless, the propensity score could be estimated non-parametrically via, for instance, a local logit. In this case, we would consider the difference-in-differences matching estimator non-parametric.

assumed to be independent of $X_{i,s,t}$ and $year_t$ (see Meyer 1995; and Imbens and Wooldridge 2008).[29]

Our parameter of interest is the coefficient α_{DD}, which identifies the effect of the treatment on the treated group. The causal effect identification on the outcomes' variables relies on the assumptions that (i) the selection for the treatment does not depend on unobservable individual and community characteristics changing overtime; (ii) the 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) the treatment does not affect access to credit of households living in the neighboring areas, and therefore, no spillover effects are present.

These assumptions imply that

$$E\left(u_{i,s,t} \mid treat, year, X\right) = E\left(u_{i,s,t}\right) = 0, \tag{10}$$

and

$$\left\{\begin{array}{l}\left[\begin{array}{l}E\left(H_{i,s,t} \mid treat=1, year=2008, X\right)- \\ E\left(H_{i,s,t} \mid treat=1, year=2007, X\right)\end{array}\right]- \\ \left[\begin{array}{l}E\left(H_{i,s,t} \mid treat=0, year=2008, X\right)- \\ E\left(H_{i,s,t} \mid treat=0, year=2007, X\right)\end{array}\right]\end{array}\right\} = \left(\beta_2 + \alpha_{DD}\right) - \left(\beta_2\right) = \alpha_{DD}. \tag{11}$$

The main issue regarding (10) is the self-selection problem (also known as the anticipation problem). That certainly would be an issue if households decided to work more hours given the expectation of receiving land title in the future. Regarding assumption (ii), we use control variables in order to account for differences between the two groups in the baseline year (2007). In addition, the fixed effect estimator is applied to check the robustness of the results given that the dependent variable could potentially be different across groups, but invariant through time.

We also estimate the difference-in-differences-matching (DDM) estimator suggested by Heckman, Ichimura, and Todd (1997) and employed, for instance, by Smith and Todd (2005), and Angelucci and Attanasio (2009). The main advantage of this estimator is that it does not impose any functional form on the regression model and uses a kernel function to weight the subsample of the control group. The aim of the weighting function is to construct a counterfactual mean effect based on the distance between the propensity score of each

[29] Once all households of the treated area receive the title, s and $year_t$ will be the same. Thus, from now on, the subscript s is omitted for the sake of simplicity.

control group observation and the treatment group observation.[30] The identification of the ATT requires that

$$E(\Delta H_C \mid P, Land = 1) = E(\Delta H_C \mid P, Land = 0)$$

Under this condition, the DDM estimator for the ATT is given by:

$$\alpha_{DDM} = \frac{1}{n_1} \sum_{i \in T \cap S_p} \left[H_{i,1,1} - H_{i,1,0} - \sum_{j \in C \cap S_p} W(i,j)(H_{i,0,1} - H_{i,0,0}) \right],$$

where n_1 is the subsample of treated units, $W(\bullet)$ is the weighting function, and the subsets below the summations indicate that the estimate is computed in the common support, S_p (see Smith and Todd 2005).

C. The Quantile Treatment Effects

This section describes the quantile treatment effect methods applied in the paper to test Proposition 1 empirically for the Brazilian titling program. Frölich and Melly (2008) show that the distribution of a dependent variable may change in many ways that are not completely revealed by an examination of averages. Accordingly, Heckman and Hotz (1989) find empirical evidences of distributional effects on the labor market using experimental data from the *National Job Training Partnership Act Study*.

The advantage of QTE methods 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. For any fixed percentile, QTE corresponds to the horizontal distance between two cumulative distribution functions. It is a powerful and intuitive tool that allows researchers to discover the effects on the entire distribution.[31]

Our investigation starts by comparing both groups' cumulative distributions of hours worked for the years before and after the land title program (2007 and 2008, respectively). The implementation of this first order stochastic dominance analysis can be described as follows (see Abadie 2002; and Duflo, Hanna, and Ryan 2007). Given two cumulative distribution functions (CDFs), $F^1(h)$ and $F^2(h)$, [32] and a variable $h \in [0,1]$, the CDF $F^1(h)$ first

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[30] Smith and Todd (2005), for instance, define the weighting function as $G((P_C - P_T)/b_n) \big/ \sum_{k \in C} G((P_k - P_T)/b_n)$, where $G(\bullet)$ is the kernel function and b_n is the bandwidth, P holds for propensity score, C for control group, and T for treatment group.

[31] Most of the existing literature using QTE methods is based on social experiments in employment, training, and welfare programs in the United States (Dammert 2009).

[32] $F^k(h) = \int_0^{+\infty} f^k(h)\,dh.$

degree stochastic dominates $F^2(h)$ if, and only if, $F^1(h) \leq F^2(h)$ for all $h \in [0,1]$, and with $F^1(h) < F^2(h)$ for some $h \in [0,1]$ (Gravelle and Rees 2004).[33]

For the next step, we use quantile regressions to estimate the distributive effects of the program. The main advantages of this method are:[34] (i) the technique allows us 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 QR can be applied in cases in which the distribution is not Gaussian, (iv) the QR 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 intervals can be estimated directly from the demanded conditional quantiles, and (vii) the QR can be represented as a linear programming model.[35]

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

$$Q_T(H_i \mid X_i) = F_y^{-1}(\tau \mid X_i), \tag{12}$$

where H_i is our dependent variable, X_i is again our vector of control variables, and τ denotes the quantile of interest. $F_y^{-1}(\tau \mid X_i)$ is the distribution function for H_i at h, conditional on X_i. Given that there is no assumption for the distribution of errors, the QR is considered a semi-parametric regression technique. The QR estimators can be written as

$$H_\tau = \alpha + \delta_{1\tau} title + X_i^{'} \alpha_\tau + \varepsilon_i. \tag{13}$$

It is important to bear in mind, though, that the quantile regressions (conditional or unconditional) do not tell us anything about the effect of a policy/treatment on individuals (or households). In fact, they just inform us whether the effect of the treatment is different in distinct points of the outcome variable distribution. In other words, if the coefficient of the first decile is higher than the coefficient of the top decile, this does not mean that individuals in the first decile were more affected by the treatment. That coefficient just tells us that the effect is higher in the lower tail of the outcome variable distribution (see Angrist and Pischke 2009 for this point). However, by assuming that the intervention does not change the households' ordering in terms of the outcome variable distribution, then one is able to argue that the impact is higher amongst the individuals in the lower tail of the outcome variable distribution. This assumption is known as rank-invariance or rank-preserving.

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[33] The theorem also applies for variables not limited to the unitary interval.

[34] See also Silva and Porto Junior (2004), Koenker (2005), and Rivera and Currais (2005).

[35] In case of land title, such a technique also allows for the investigation of whether or not the program has differential effects for any subpopulation (Bitler, Gelbach, and Hoynes 2006).

The estimation of the QTE can then be described as follows. Let the $H_i(1), H_i(0)$ be the potential outcome for a household *i*. $H_i(1)$ would be realized if household *i* were to receive the treatment 1 (land title), and $H_i(0)$ would be realized otherwise. The observed outcome, H_i, is equal to $H_i = H_i(1)treat_i + H_i(0)(1-treat_i)$. Hence, the main objective of our quantile regression is to estimate the entire distribution function of $H(1)$ and $H(0)$.

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

$$\Delta^{\tau} = Q^{\tau}_{H(1)} - Q^{\tau}_{H(0)}. \tag{14}$$

According to Frölich and Melly (2008), this estimator has at least three main advantages over the conditional estimator. First, the definition of the unconditional QTE does not alter when a set of covariates changes. In other words, the point estimate does not depend on the *X* vector. Second, unconditional effects can be estimated consistently at the rate $\sqrt{n}$ without any parametric restrictions, something impossible to do with the conditional quantile estimator. The third advantage is that for policy makers, the effects on the entire population are often more interesting than a large number of effects for different covariate combinations.

In summary, the quantile regression is both conditional on X and parametric, whereas the QTE, as explained above, is unconditional and less dependent on functional form assumptions since it can be estimated semi-parametrically.[36]

The particular case of the land title program in Osasco can be qualified as an unconditional QTE with exogenous treatment. Melly (2006), Firpo (2007), and Frölich (2007) provide different econometric methods to deal with such a case. Here, we follow Firpo's (2007) method and estimate the QTE in a two-step procedure. In this method, under (i) ignorability of treatment, (ii) common support, and (iii) quantile monotonicity existence, estimating the QTE is possible by using the weighting estimator of the *check-functions*. When the sample is not well-balanced in observables' characteristics, the weighting function depends on a *propensity score* estimated in the first step.

The following QTE estimator is then proposed:

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

[36] Thus, the QR estimates can be interpreted as conditional quantile treatment effect (see Frölich and Melly 2008).

where $\hat{\omega}_i\left(treat_i, X_i\right)=\frac{treat_i}{\hat{p}\left(treat=1 \mid X_i\right)}+\frac{1-treat_i}{1-\hat{p}\left(treat=1 \mid X_i\right)}$ is the weight function; $treat=treat_i+\left(1-treat_i\right)$ is a dummy that represents the treatment: $treat_i=1$ if the household 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 $treat$, given X and a constant.

Firpo (2007) suggests the estimation of the first step non-parametrically. However, given the limited size of our sample, in this paper we estimate the propensity score parametrically applying a logit model. Thus, the weight function, $\omega(\bullet)$, used to identify the unconditional quantile treatment effect, is here computed with the propensity score estimated parametrically. Following Firpo (2007), the first set of QTE estimates is not restricted to the common support. However, we also provide a second set in which we condition the estimates to the common support.[37]

V. Empirical Results

This section presents the results of the empirical analysis proposed in the previous section. We start by discussing the findings of the analysis for the mean. Then, we consider the distributive effects estimated via the QTE methods.

A. Mean Effects of Land Title Programs on Hours Worked

Table 4 presents the estimation results of the empirical model (8) for the baseline year of 2007. In that year, the program had not yet been implemented and no household had received the land title (i.e. pre-program period) from Papel Passado. The estimations, including (and excluding) the control variables, find no significant impact of land title on the number of hours worked weekly. As expected, the fact of whether or not a householder owned property before the program began did not lead, on average, to a statistically significant difference in the number of hours worked. Prior to the program, the incentives to increase labor supply due to a land title were weak, reducing the possibility of self-selection bias (or anticipation bias) among the titling program participants.

The mean effects of the program are then analyzed in Table 5. That table presents the OLS estimates for four regression models using the data for 2008. Its results provide an initial support for the claim that land title increases labor supply and the number of hours worked. Model OLS-Naïve, excluding the control variables, estimates a positive and significant coefficient (9.37) for the dummy land title. For all other columns, including the control variables, a significant coefficient is also obtained for land title. The estimates for the control variables also have the expected results. A higher educational level (years of education) of

[37] This procedure is analogous, at least to some extent, to "trimming" the propensity score distribution before computing the weights used to identify the QTE.

the households' heads, along with easier access to credit, leads to increases in the number of hours worked weekly. Age, instead, marginally reduces the number of hours worked.

The last column of Table 5 estimates a Tobit model appropriate to deal with the households that reported zero hours worked weekly in 2007. The high significance of the sigma coefficient suggests that the selection to join the labor market might not be random and that its omission could result in an omitted bias.[38] However, the difference between the coefficients in Models 3 and 4 is not statistically significant as indicated by a Wald test.[39] The following set of estimates supposes the data is uncensored, and therefore, might be seen as conservative estimates for the impact of the land title.

Table 6, in turn, presents the estimations in DD for the years 2007–2008 (pre- and post-program period). Its results corroborate the previous finding that land title increases the number of hours worked, on average. All columns display a positive and statistically significant coefficient for the interaction term between the dummy for land title and the year of the program (2008). Likewise, most of the previous significant control variables in the OLS estimation remain significant when we use the DD method. The second and third columns show a similar impact, suggesting that conditioning the sample on the common support does not influence the point estimate of the interaction term.

The DDM estimates are presented in Table 7. The main advantage of this estimator is that it does not impose any functional form a priori and uses all observations of the control group to identify the parameter of interest (see Heckman, Ichimura, and Todd 1997; and Smith and Todd 2005, for a detailed discussion on this estimator). Since there is a trade-off between bias and variance, we provide estimates for three different bandwidths. The lower bandwidth in the kernel estimator is the bias, and the higher one captures the variance.

The first column of Table 7 shows that for a rather small bandwidth the point estimate is very similar to the DD naïve and Tobit estimates (see Table 5). This gives additional support for the quality of the (quasi-) experiment. As the bandwidth is enlarged, the point estimates become smaller, as does the variance. Therefore, despite the relatively high variance in the first column, we believe that the average treatment effect on the treated group must be around 9 hours/week.

This section, therefore, has shown that, on average, the land title program appears to have a positive effect on labor supply and on the number of hours worked weekly. The distributive effects are analyzed in the following section.

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[38] In the Tobit model, we are interested in fitting the following regression: $E(y \mid X, y > 0) = X\beta + \sigma\left[\frac{\phi(x_i\beta/\sigma)}{\Phi(x_i\beta/\sigma)}\right]$. Hence, the sigma coefficient will provide an estimate for the inverse of the Mills ratio. This term results from the probit component in the log-likelihood function and captures the magnitude of self-selection in the sample. See Wooldridge (2002).

[39] Prob > F(1, 275) = 0.34.

B. Distributive Effects of Land Title Programs (Quantile Regressions)

This subsection presents the findings of the distributive empirical analysis. Before discussing the results of the QTE analysis, we start by presenting the results of an analysis of first-degree stochastic dominance.

Cumulative Distribution and First Degree Stochastic Dominance

Figure 5 shows the result of the first order stochastic dominance analysis for the pre-treatment year (2007). It conveys that the distribution of adults' weekly hours worked for the treatment group level dominates, in the first degree, the distribution of adults under the control group during that year. Yet, Figure 6 shows that such dominance increases significantly in the post-treatment period. This already indicates that the land title program positively affects the whole cumulative distribution curve of the hours worked weekly, and not only its average.

Quantile Regression

The estimated results of the quantile regression are presented in Table 8 and Figure 7.[40] Table 8 shows that the effects in the first two quartiles are totally convergent with the double difference estimate for the ATT analysis. Thus, the land title has a clear positive impact on labor supply.

Corroborating Proposition 1, Table 8 also indicates that the increase in property rights (or land-titling) leads to a higher supply of labor by those households with initially lower numbers of hours worked. The coefficient of land title significantly differs among percentiles, depending on the initial amount of hours worked by the household.[41]

The same coefficients are illustrated graphically in Figure 7. There, one can observe the difference between the average treatment and quantile treatment analyses of Table 8. It shows that the effects of the titling program are concentrated in the middle of the outcome variable distribution. The highest effect of the program is estimated for the first quartile (0.25) and the second quartile (0.50), 7.8 hours/week, and 9.7 hours/week, respectively. In addition, the similarity between the coefficients for the median and average of the distribution (previous section), suggests that outliers are not driving the estimate of the ATT estimates.

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[40] Table A in the Appendix, in turn, reports the logit regression for the unbalanced and balanced samples (using propensity score matching). For the balanced sample (column 2), none of the control variables are statistically significant. That is one of the caveats of using the propensity score in our analysis. The sample size is reduced, given that only households in the common support are considered for the estimation. In that column, 17 observations are lost, and the analysis is computed for 288 households only.

[41] With a simple T-test, it can be shown easily that the difference between the coefficients of the first and second quartiles (median) is not statistically significant.

Table 9 compares the coefficients of land title estimated for two different quantiles. It conveys whether the differences between two point estimates are significantly different from zero. For instance, its first column compares the coefficient of land title for the median of the sample distribution with the same coefficient for the first quartile of the sample. Hence, the value 1.89 in that first column corresponds to the difference between the coefficients 9.66 and 7.77 reported in Table 8. Column 1 in Table 9 suggests thus that the interquantile difference is not statistically significant. Yet, Columns 4 e 5 show, instead, that the land title effect for the individuals belonging to the sample median of hours worked is not only higher, but statistically distinct from those observed in the third quartile and in the top decile of the distribution.

This finding of distributive effects of land title on both hours worked and labor supply is new in the literature. Field and Torero (2002), for example, find that households with land titles work, on average, 12.2 hours more per week compared to those without land titles. Yet, they do not discuss the differences of the effects of land title on the distribution of hours worked.

Quantile Treatment Effects

Turning to the QTE estimation, Table 10 and Figure 8 show the results of using Firpo's (2007) procedure. Although the point estimates differ from the QR estimates (which is expected since the QR estimates provide the conditional version of QTE); both estimators provide a similar qualitative result. The impact of land-titling seems to be higher in the first half of the outcome variable distribution than in the second. According to the unconditional QTE, when considering the whole sample, the impact of land title is significant in the first, second and third quartiles. The large impact on the median (25 hours/week) suggests that some outlier may be driving the results. Thus, when restricting the analysis to the common support, the main difference that appears is that relating to the effect on the median. The coefficient declines from 25 to 15 hours per week, and the estimate is significant at 1 percent.

Given that, in the common support, we are comparing only individuals with similar pre-treatment observable characteristics, we believe that the second set of estimates provides more reliable evidence about the distributive effects of the program. Yet, unlike the results of using the quantile regression, this time, the upper tail of the outcome distribution presents the positive effect of land title on labor supply, even though the coefficient is not statistically significant. Thus, both the conditional and unconditional QTE estimates indicate that the effect of land title is heterogeneous and more concentrated around the median.[42]

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The main implication of our empirical analysis is that, indeed, the Brazilian land title program had a positive effect on hours worked and labor supply. While the OLS and DD

[42] One caveat of our analysis using the QTE procedure is the limited sample size. In addition, if we exclude the workers who did not work before those years in the sample size (0 initial hours worked), the difference between coefficients in the higher quartiles becomes, once again, significant.

estimates point to the positive effects of land-titling on the average of hours worked, the QR method suggests the heterogeneity of this result. The impact of Papel Passado may be concentrated in the lower tail of the distribution of hours worked.

As Section II suggests, one explanation for this finding is that the land title provides property security, particularly for those households mostly excluded from the labor market (Proposition 1). That security enables the householders to search for, and find, occupations, instead of staying at home protecting their property. This way, our findings also indicate the relevance of such titling programs for the poorest urban households of developing countries.

Two additional implications can be drawn from the quantile (distributive) analysis. First, the heterogeneous effect should be taken into account by authorities when designing public policies. The quantile analysis suggests that the impact of the policy (in this case, land title) can be restricted only to a range within the variable distribution. Thus, assessing the distributive impacts of the policy would help authorities to better target their policies, improving their efficiency.

Second, the results, in particular those with the QTE methodology, show the importance of using the correct sampling when assessing programs. Program evaluation should be performed using as many households as possible with similar observable characteristics. By focusing on this common support, unconditional quantile treatment effects could also be estimated.[43]

VI. Conclusion

This paper presents new evidence on the value of formal property rights in urban squatter communities. First, it conveys a simple model, based on Field (2003, 2007), that rationalizes the effect of land title on labor supply. With land title, individuals feel themselves secure enough to supply external labor rather than staying at their homes to protect their possession. The model then introduces heterogeneous households and indicates that receiving a land title could lead to a higher or lower impact on the household's labor supply, depending on the wage level and initial number of hours worked.

Empirically, the study provides additional support for the finding that property title gives incentive to labor supply, increasing hours worked weekly. Moreover, quantile regression techniques are applied, suggesting that the effect of land-titling may differ among the quantiles of weekly hours worked in the sample. The main results indicate that the poorest households appear to be the ones who benefit most from formal property rights. Such findings could not have been investigated using a simple mean estimation analysis.

The weighting estimator of Firpo (2007) is also used to minimize the bias caused by selection on observables. This quantile treatment effects (QTE) procedure indicates the limitations of

[43] Our results show the differences of applying the quantile regression approach with balanced and unbalanced samples, even when there is evidence that the unobservables are not driving the results.

randomization and the benefits of using propensity score matching techniques to select the sample.

In addition, such policy evaluation for a program implemented in an emerging market (Brazil) also contributes to 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 the governments of all developing countries who are considering the implementation of titling programs to address urban informality.

The results have potential implications for understanding labor market participation and frictions (Goldsmith 1995), and designing better pro-poor spending in emerging and developing countries. As the theoretical model suggests, in places characterized by high levels of residential informality, informal property protection may constitute an important obstacle to labor market adjustment. It may even affect monetary policy. Hence, land title could potentially be applied as a tool to improve public policy actions that directly impact poverty, economic growth, and macroeconomic stability.

The current analysis offers various possibilities for further research. For example, the effects on the income and utility of the households owing to the increase in the number of hours worked via the land-titling program, could be better investigated. At this stage, the income gain for the households of increasing their labor force participation is not clear. Further, the distributional impact of land-titling in other economic variables, such as access to credit and fertility, could also be analyzed. That would improve the assessment of such programs in the lives of the millions of households living in urban squatter communities in developing and emerging economies 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 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 on 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 (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
Mean age	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***
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 income (currency BRL[a]) per capita[b]	553.10	255.80	0.00***
Wealth index[c]	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 residents	1.97	1.70	0.00***
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 on 12/31/2008: 1 USD = 1.75 BRL (Brazilian reais).
[b] Monthly income per capita is calculated dividing monthly income by the number of residents.
[c] Wealth index summarizes the total value of durable goods. It is computed via a principal component analysis (PCA). Imbens & Wooldridge (2008) state that the PCA is a technique that is useful where explanatory variables are closely related.

Table 3. Spearman Correlation, 2007

	Years of Education	Informality	Monthly Income per capita	Weekly Hours Worked
Years of education	1			
Informality	-0.15**	1		
Monthly income per capita	-0.23***	0.23***	1	
Weekly Hours Worked	0.14**	0.09	0.11*	1

Notes: *, **, *** Statistically 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	0.36	0.69
	(1.42)	(1.99)
Gender (= 1 if female)		7.26***
		(1.56)
Ethnicity (= 1 if African-Brazilian)		-0.80
		(1.39)
Marital status (= 1 if married)		-1.95
		(1.56)
Age		-0.23
		(0.25)
Age^2		-0.01
		(0.01)
Number of residents		-0.26
		(0.43)
Weekly hours of child labor (< 16 years old)		-0.07
		(0.05)
Years of education (family head)		0.37**
		(0.18)
Monthly income (currency BRL[a]) per capita[b]		-0.01
		(0.01)
Access to credit (= 1 if have)		1.05
		(1.30)
Wealth index[c]		1.11*
		(0.69)
Informal worker		2.94
		(1.63)
Constant	10.07***	4.09***
	(1.06)	(5.76)
Observations	305	305
R-squared	0.00	0.17

Notes: *, **, *** Statistically significant at 10, 5, and 1 percent, respectively.
[a] Currency exchange rate on 12/31/2008: 1 USD = 1.75 BRL (Brazilian reais).
[b] Monthly income per capita is calculated dividing monthly income by the number of residents.
[c] Wealth index summarizes the total value of durable goods. It is computed via a PCA analysis. Imbens & Wooldridge (2008) state that the PCA is a technique that is useful where explanatory variables are closely related.

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

	Model 1	Model 2	Model 3	Model 4
Variables	OLS Naïve	OLS	OLS Balanced Sample	Tobit-Balanced Sample
Land title	9.37***	6.01**	6.25**	9.66***
	(1.55)	(2.50)	(2.50)	(3.55)
Gender (= 1 if female)		5.18***	5.14***	7.39***
		(1.68)	(1.72)	(2.41)
Ethnicity (= 1 if African-Brazilian)		-1.74	-2.02	-3.04
		(1.56)	(1.66)	(2.39)
Marital status (= 1 if married)		0.98	1.23	1.47
		(1.63)	(1.70)	(2.51)
Age		-0.57*	-0.06**	-0.82*
		(0.31)	(0.32)	(0.48)
Age^2		0.01	0.007*	0.01
		(0.01)	(0.003)	(0.01)
Number of residents		-0.23	-0.37	-0.39
		(0.52)	(0.57)	(0.78)
Weekly hours of child labor (<16 years old)		-0.05	-0.01	-0.00
		(0.06)	(0.08)	(0.13)
Years of education (family head)		0.49**	0.50**	0.68**
		(0.22)	(0.23)	(0.33)
Monthly income (currency BRL[a]) per capita[b]		-0.00	-0.00	-0.00
		(0.00)	(0.00)	(0.00)
Access to credit (= 1 if have)		4.07**	3.68**	5.44**
		(1.63)	(1.72)	(2.59)
Wealth index[c]		0.36	0.52	0.72
		(0.76)	(0.79)	(1.08)
Informal worker		1.39	1.46	2.50
		(1.89)	(1.95)	(2.80)
Constant	11.08***	19.76*	22.16***	16.09
	(1.12)	(7.44)	(7.73)	(11.08)
Sigma				18.45***
				(1.01)
Log-likelihood				-909.36
Pseudo-R2/R2	0.10	0.20	0.17	0.03
Observations	305	305	288	288

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

[a] Currency exchange rate on 12/31/2008: 1 USD = 1.75 BRL (Brazilian reais).

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

[c] Wealth index summarizes the total value of durable goods. It is computed via a PCA analysis. Imbens & Wooldridge (2008) state that the PCA is a technique that is useful where explanatory variables are closely related.

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	-0.39	-0.45
	(1.42)	(2.00)	(2.02)
Land*year (*DD*)	9.01***	8.02***	8.13***
	(1.16)	(1.24)	(1.26)
Year	1.00***	1.20***	1.25***
	(0.28)	(0.35)	(0.38)
Gender (= 1 if female)		6.23***	6.24***
		(1.46)	(1.49)
Ethnicity (= 1 if African-Brazilian)		-1.26	-1.50
		(1.29)	(1.35)
Marital status(= 1 if married)		-0.47	-0.28
		(1.40)	(1.45)
Age		-0.17	-0.25
		(0.24)	(0.25)
Age^2		0.00	0.01
		(0.01)	(0.01)
Number of residents		-0.26	-0.25
		(0.40)	(0.43)
Weekly hours of child labor		-0.06	-0.03
		(0.05)	(0.06)
Years of education (head)		0.42**	0.43**
		(0.19)	(0.20)
Monthly income (currency BRL[a]) per capita[b]		-0.00	0.00
		(0.00)	(0.00)
Access to credit		2.36*	1.91
		(1.25)	(1.29)
Wealth index[c]		0.74	0.83
		(0.61)	(0.63)
Informal worker (= 1 if have)		1.76	1.73
		(1.46)	(1.48)
Constant	10.07***	11.49**	12.66**
	(1.06)	(5.61)	(5.83)
R2	0.10	0.21	0.19
Observations	610	610	576

Notes: Robust standard errors are in parentheses. *, **, *** statistically significant 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 dividing monthly income by the number of residents.

[c] Wealth index summarizes the total value of durable goods. It is computed via a PCA analysis. Imbens and Wooldridge (2008) state that the PCA is a technique that is useful where explanatory variables are closely related.

Table 7. Difference-in-Difference Matching Estimates, 2007–2008

	Bandwidths of Kernel Estimator		
Variables	(0.01)	(0.05)	(0.10)
Land title	9.00*** (3.50)	7.98 (2.70)	7.8*** (2.50)
Observations	610	610	610

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

Table 8. The distributive effect of land title on adult labor supply
Conditional QTE: Quantile Regression, 2008

Variables	0.25	0.50	0.75	0.90
Land title	7.77*	9.66***	1.41	0.43
	(4.18)	(3.27)	(3.26)	(4.77)
Gender (= 1 if female)	6.42*	4.86**	1.73	4.31
	(3.30)	(2.07)	(2.48)	(3.40)
Ethnicity (=1 if African-Brazilian)	-2.62	-1.02	-1.58	0.43
	(2.81)	(2.26)	(2.56)	(2.38)
Marital status (= 1 if married)	-0.24	0.77	-0.13	3.98
	(2.28)	(2.46)	(2.72)	(2.95)
Age	-0.58	-0.67	-0.67	-0.57
	(0.55)	(0.49)	(0.58)	(0.54)
Age^2	0.01	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Number of residents	-0.28	0.02	-0.03	0.29
	(0.86)	(0.75)	(0.85)	(0.39)
Access to credit	2.44	4.62*	3.76	4.79*
	(2.66)	(2.31)	(2.74)	(3.20)
Weekly hours of child labor (<16 years-old)	-0.03	-0.09	0.05	-0.13
	(0.07)	(0.13)	(0.13)	(0.12)
Years of education (head)	0.51	0.64**	0.69**	0.44
	(0.33)	(0.25)	(0.35)	(0.44)
Monthly income (currency BRL[a]) per capita[b]	0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Wealth index[c]	1.08	0.59	-0.07	0.03
	(1.10)	(0.73)	(1.13)	(1.23)
Informal worker (= 1 if have)	3.33	0.55	-0.84	1.88
	(3.81)	(2.56)	(2.38)	(3.50)
Constant	7.38	17.37	32.32**	34.39***
	(10.60)	(11.27)	(14.52)	(11.26)
Observations (balanced sample)	288	288	288	288

Notes: Standard errors (in parenthesis) are computed using a bootstrap with 100 replications. *, **, *** significant at 10, 5, and 1 percent, respectively. The estimates shown here are computed in the common support. The estimates for the whole sample (available upon request) are almost identical.

[a] Currency exchange rate on 12/31/2008: 1 USD = 1.75 BRL (Brazilian reais).

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

[c] Wealth index summarizes the total value of durable goods. It is computed via a PCA analysis. Imbens and Wooldridge (2008) state that the PCA is a technique that is useful where explanatory variables are closely related.

Table 9. Gaps between Interquantile Estimates of the Quantile Regression, 2008

Variables	(1) 0.5-0.25	(2) 0.25-0.75	(3) 0.25-0.9	(4) 0.5-0.75	(5) 0.5-0.9	(6) 0.75-0.9
Land title	1.89	3.36	7.34	8.25**	9.23*	0.98
	(4.95)	(5.57)	(5.95)	(3.62)	(5.02)	(4.39)
Observations	288	288	288	288	288	288

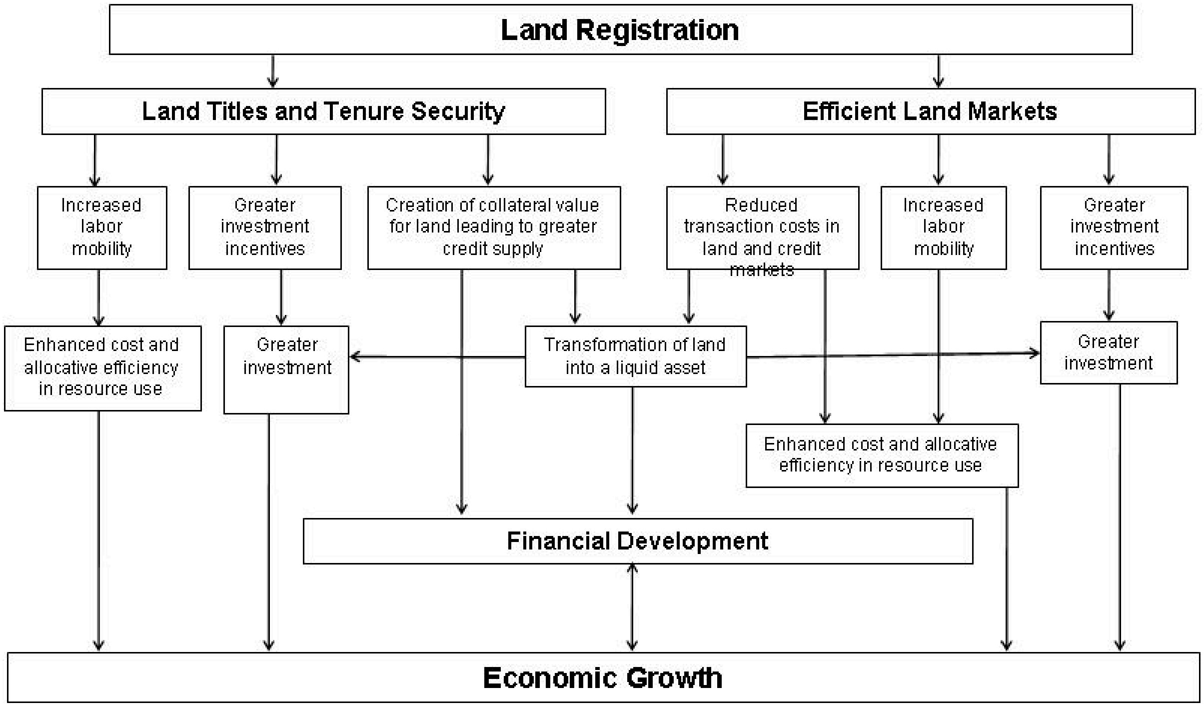
Notes: Standard errors (in parenthesis) are computed using a bootstrap with 100 replications. *, **, *** statistically significant at 10, 5, and 1 percent, respectively.

Table 10. Quantile Treatment Effect Estimates
Unconditional QTE, 2008

Variables	(0.10)	(0.25)	(0.50)	(0.75)	(0.90)
Whole Sample:					
Land Title	-	12.00**	25.00***	7.50***	7.60
	-	(5.30)	(1.90)	(2.80)	(4.80)
Common Support:					
Land Title	-	12.20**	14.96***	4.70	6.24
	-	(5.30)	(2.84)	(3.16)	(4.88)
Observations					
Whole Sample	305	305	305	305	305
Common Support	288	288	288	288	288

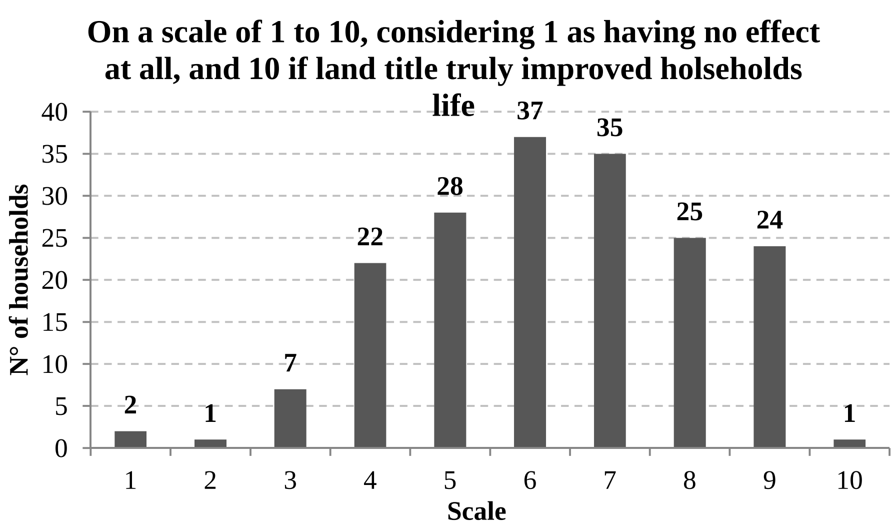
Notes: Standard errors (in parenthesis) are computed with the algorithm proposed by Frölich and Melly (2009). *, **, *** Statistically significant at 10, 5, and 1 percent, respectively. Using a T-test, it can be checked that the interquartile estimates are not statistically different from 0. For instance, the T-test for the difference between the coefficients in the third and second quartiles in the first row can be computed as $t = \frac{25 - 12}{\sqrt{(5.3)^2 + (1.9)^2}} \cong 2.3$

Figure 1. Land Registration Diagram



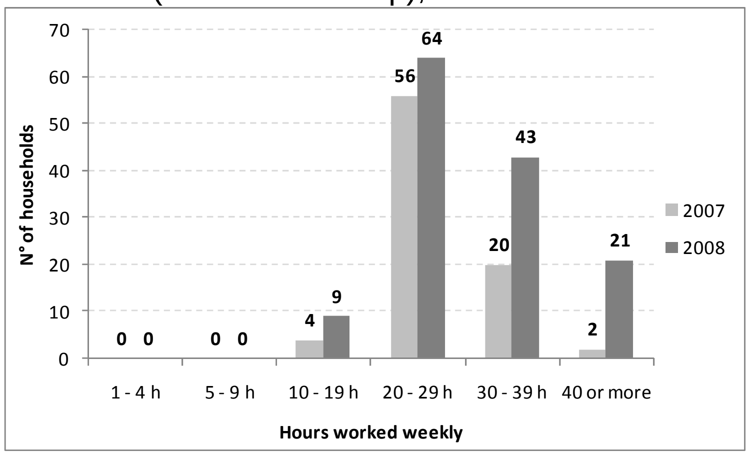
Source: World Bank, 2000

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



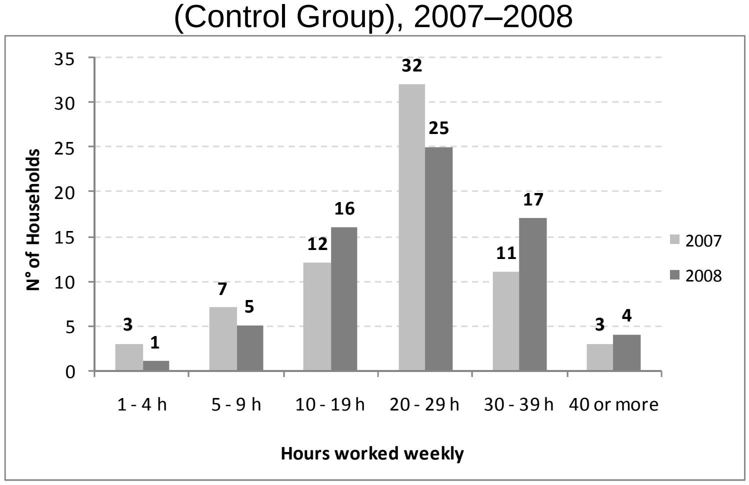
Source: Author’s calculations, research from the *Osasco Land Title Survey*, 2008.

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



Source: Author's calculations, research from the *Osasco Land Title Survey*, 2007–2008.

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



Source: Author's calculations, research from the *Osasco Land Title Survey*, 2007–2008.

Figure 5. Differences Between Cumulative Distribution Functions of the Adults' Weekly Hours Worked for the Treated and Control Groups, 2007

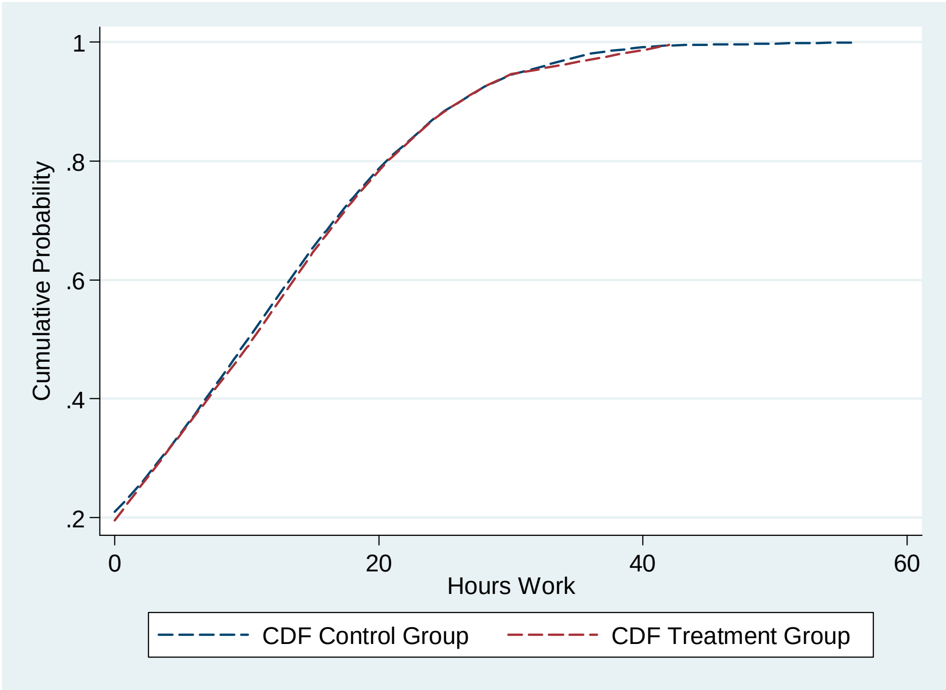


Figure 6. Differences Between Cumulative Distribution Functions of the Adults' Weekly Hours Worked for the Treated and Control Groups, 2008

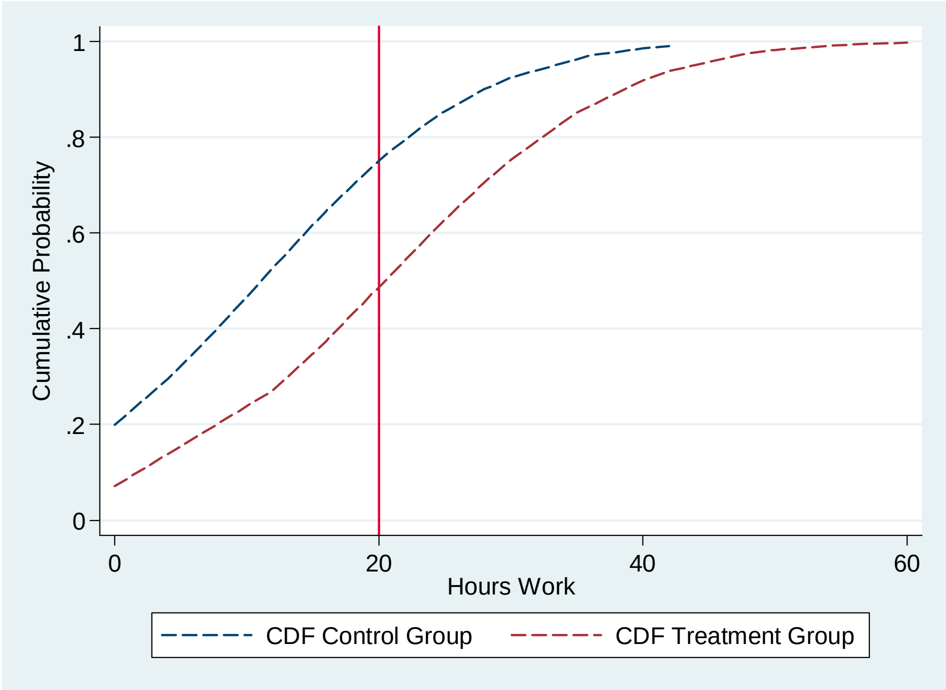
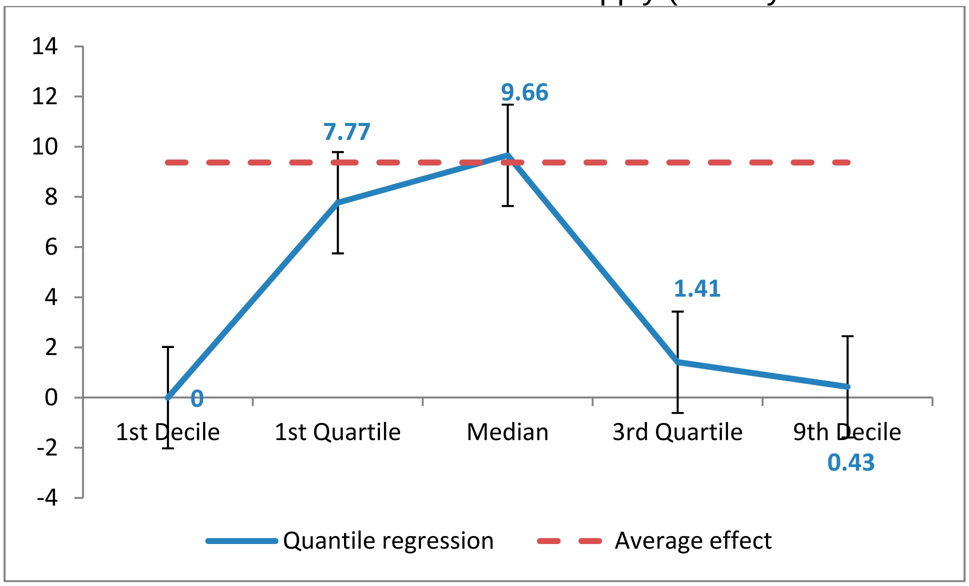
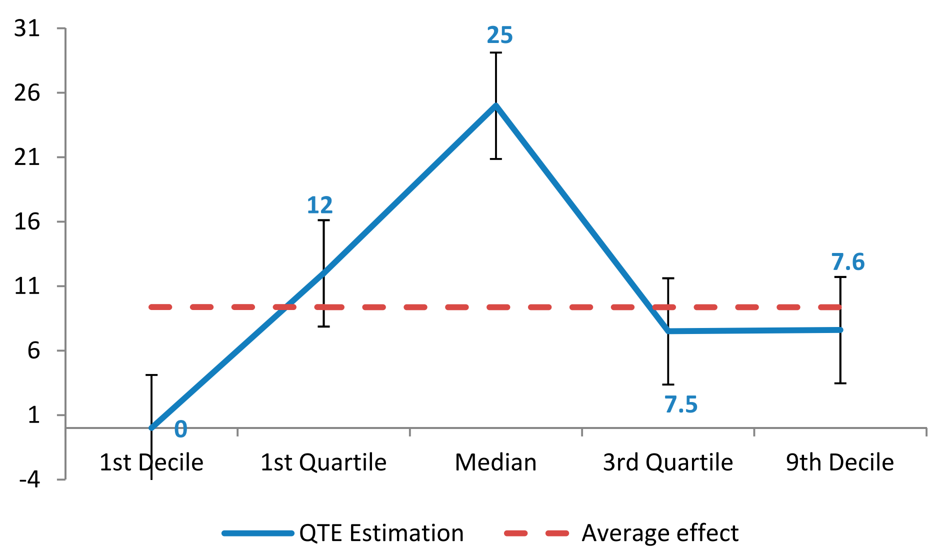


Figure 7: Quantile Regression Effects with Control Variables (Complete Sample)
The distributive effect of land title on adult labor supply (Weekly hours worked), 2008



Source: Authors' calculations from quantile regression, parametric conditional.

Figure 8: Quantile Treatment Effects with Control Variables—Common Support
The distributive effect of land title on adult labor supply (Weekly hours worked), 2008



Source: Authors' calculations from quantile treatment effects, Firpo (2007).

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APPENDIX

Table A. 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)	0.04	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

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

Figure A. Distribution of Wealth Index, 2008