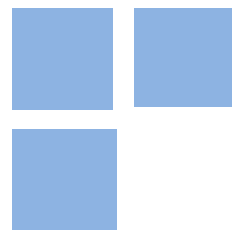


The Effect of Land Title on Child Labor Supply: Empirical Evidence from Brazil

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Abstract:

This paper assesses the effect of property-titling on child labor. Our main contribution is to investigate the potential impact of property rights on child labor supply by analyzing household response regarding the child labor force to exogenous changes in property ownership status. The causal role of legal ownership is isolated by comparing the effect of land titling using data from a unique study in two geographically close and demographically similar communities in Osasco, a town of 654,000 people in the Sao Paulo metropolitan area. Survey data were collected from households in both communities before and after the granting of land titles, with neither type knowing ex-ante whether it would receive land titles. The econometric estimates, applying the Difference-in-Difference (DD) methodology and propensity score matching, suggest that land-titling decreases child labor.

Keywords: Property Rights, Land Titling, Child Labor Force.

JEL Codes: P14; Q15; J22; O18; O54.

I. INTRODUCTION

Many researchers have studied how property rights generate economic development (e.g., North and Thomas, 1973), but few have focused on the impact of this type of policy on child labor¹. An exception related to property rights and child labor is Field (2007), who documents the effect of transferring property protection from local communities and households to the state. She concludes that there is a reduction in the number of hours worked by children from these households. Moura and De-Losso (2010) investigate the impact of exogenous changes in formal property ownership status on child labor supply. However, because they did not address observable differences in some controls between the intervention (land titled) and comparison (non-land titled) groups, their results are very likely overestimated. To overcome such a flaw, we apply the propensity score method and make a range of important improvements, allowing us to verify if and how the results change and to make them much more reliable.

The lack of a formal property rights system is more problematic for the poor, because they are constrained from using land as collateral to access credit markets (Besley, 1995; North, 1990). If such credit were available it could be invested as capital in productive projects and used to increase labor productivity and income (Demsetz, 1967; De Soto, 2000). Torstensson (1994) and Goldsmith (1995) have shown a positive relationship between economic growth and property tenure rights. The authors also recommend strengthening economic institutions to increase growth performance, encourage investment in physical and human capital, decrease macroeconomic volatility, and promote more efficient and fair distribution of economic opportunity.

Property titling increasingly is considered one of the most effective public policies to benefit poor populations and encourage economic growth around the world (Baharoglu, 2002; Binswanger *et al.*, 1995). In Asia, for example, millions of land titles have been issued in Vietnam and Cambodia, while several governments are investing in social housing in Africa (Galiani & Schargrodsky, 2010). In Latin America, Peru is the most famous example of a property-titling program; in the 1990s the government issued titles to 1.2 million urban households (source?). In 2003, the Brazilian federal government announced a massive national plan to title 750,000 families. Since its launch, this program, Papel Passado, has received US \$15 million annually from the federal budget, providing titles to over 85,000 families in 49 cities in 17 Brazilian states. Its official goal is "...to increase land titles in Brazil and to promote an increase in the quality of life for the Brazilian population" by issuing land titles to families living under illegal conditions (i.e., residents illegally squatting in urban dwellings).² This paper measures the impact of the Brazilian land-titling program, Papel Passado, on child labor supply, mostly among children between 10 and 17 years old.

¹ Defined in this paper as child work in paid activities outside of the child's home (see Edmonds and Schady, 2009).

² See *Associação dos Notários e Registradores do Brasil—ANOREG* [15]. The quotation is freely translated from Portuguese by the authors

In contrast to other research on this subject, we benefit from being able to analyze a unique dataset in a context that helps us not only to isolate the causal role of land-titling, but also to minimize the endogeneity problems characteristic of most studies in this field. We compare two similar neighboring communities in the Brazilian city of Osasco. The town — with around 654,000 inhabitants where almost 6,000 families live informally on urban property — is located in the metropolitan area of São Paulo and is part of the Papel Passado program map. In one of its communities, Jardim Canaã, all households received land titles in 2007. In another, Jardim DR, households were scheduled to receive land titles in 2012, making it a natural comparison group.

Our analysis is based on a two-stage survey conducted in Jardim Canaã and Jardim DR focusing on the property rights issue. The sample consists of 326 households distributed across both neighborhoods (185 from Jardim Canaã and 141 from DR). The first stage of the survey was conducted in March 2007, before titles had been issued to Jardim Canaã, and the second stage in August 2008, almost one-and-one-half years after the titles had been received. Those communities were municipal public land illegally invaded³. The main consequence is that Brazilian law came to protect these new property owners. The Government relinquished post rights over the land/property after the titling execution. Other land dispute cases in Brazil are much more complex if invasion occurs on private property, since that opens a window for endless legal battles.

This paper's main finding indicates that land titling has a socially positive impact on the child labor supply, *i.e.*, children from households that received a land title decreased their weekly hours of work. The available land title impact literature lacks a precise transmission channel explaining this effect. Therefore, we discuss some transmission channels without taking a firm position.

The rest of the paper is organized as follows. Section II reviews the land title literature. Section III explores the economic context of child labor. Section IV provides a basic overview of the potential mechanisms to explain child labor. Section V describes the empirical strategy to measure the effects of land titling on child work, discussing the research methodology, including the Difference-in-Difference (DD) and propensity score techniques, and providing an overview of the data collected. The empirical results are further discussed in section VI, and section VII concludes.

II. LITERATURE REVIEW ON LAND TITLING

The positive effects of land titling have been reported in several types of studies, including those on real estate values by Jimenez (1985), Alston *et al.* (1996), and Lanjouw and Levy (2002); studies on agricultural investment by Besley (1995), Jacoby *et al.* (2002), Brasselle *et al.* (2002), and Do and Iyer (2003); research on credit access, labor supply, housing investment, and income by Place and Migot-Adholla (1998) and Carter and Olinto (2003). Most of the literature and the majority of policy attention to property rights focus on tenure security of rural households. According to Field and Torero (2002), this focus is due to historical interest in agricultural investment and related policies of land reform. The impact of land rights on

³ This particular land title program operates only under public invaded areas.

agricultural investment is dependent on the location under analysis; Besley (1995) shows no impact in the region of Wassa but a positive effect in Angola. Similarly, Jacoby *et al.* (2002) estimate positive effects in China, but Brasselle *et al.* (2002) do not find any in Burkina Faso.

The Peruvian titling program was implemented regionally in different stages in the 1990s. Field and Torero (2002) take advantage of this variability in timing and use cross-sectional data to study past and future title-holders midway through the project. They also find positive effects for the Peruvian titling program, particularly on labor supply, credit access, and housing investment. In Brazil, Andrade (2006) estimates a 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. Furthermore, in Brazil Moura and De-Losso (2013) provide empirical support to show that land title can influence happiness.

III. OVERVIEW OF CHILD LABOR FORCE PARTICIPATION

The United Nations Habitat Report (2005) estimates that in 2002, 246 million children and teenagers around the world engaged in some form of work, mostly working in their own families' businesses. While Africa has the highest ratio of child labor with respect to the work force, Asia has the highest absolute number of children working.

In Brazil, the number of children between 5 and 17 years old who work is decreasing annually, as Figure 1 shows. However, the number is still high, at 9.8 per cent of this population or about 4.3 million people. The worst case is the Northeast region, with 11.7 per cent of this population working, whereas the Southeast is the best case with 7.6 per cent.

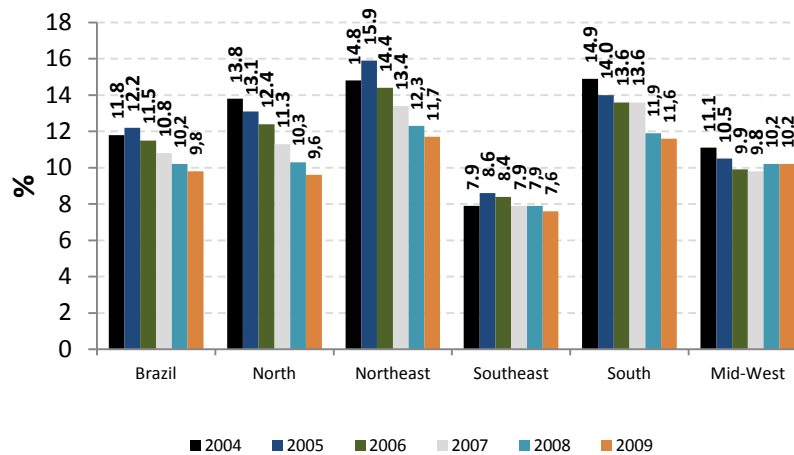


Figure 1: Occupation level among the 5-17 year-old population (% of total 5-17 population)

Source: IBGE, PNAD 2009.

Although international and local statistics are available, Edmonds (2008) claims that there is no universally agreed definition of child labor. Furthermore, many theories attempt to explain the reasons for the existence of child labor. Becker and Lewis (1973) argue that families make a cost-benefit analysis between sending their children to work or to school. Children's labor increases the household income, but reduces study and leisure time. In terms of time allocation, there is some degree of substitution between adult and child work, which is determined by the production capacity of children and parents (see Rosenzweig, 1981).

Basu and Van (1998) claim that poverty induces parents to send their children to work: since low-income households cannot afford luxury goods such as schooling and leisure, they need their children to generate income. Alternatively, Ray (1999) asserts that credit market imperfections in emerging economies cause child labor, arguing that if poor families had access to credit, then they would send their children to school rather than work given the high returns on education.

In Brazil, Kassouf (2002) concludes that the probability of reducing child labor and increasing school enrolment increases with a household's income and the parent's level of education. She finds an inverse relationship between the mother's level of education and a child's labor participation, as do Bhalotra and Heady (2003) in Ghana. Further, the latter authors find that a larger family increases the probability of child labor in Pakistan, similar to the conclusion of Patrinos and Psacharopoulos (1994) for Paraguay.

In Egypt the phenomenon of "*dynastic poverty traps*" results in child labor increasing by one-tenth in those families whose parents worked during their childhood (see Wahba, 2002). The same conclusion holds in the work of Emerson and Souza (2003), who attribute such a result to "social norms" explaining that parents are likely to view child labor as natural if they had to work during their childhood.

Some papers consider the effects of child labor outcomes. Boyden *et al* (1998) focus on the real impacts of work on children's lives and claim that some work can be positive, or at least neutral, and an important vehicle for a child's integration into society. Emerson and Souza (2011), applying data from Brazil, show that child labor has a significant negative impact on adult earnings for male children. On the other hand, Beegle *et al* (2009), using panel data from Vietnam, demonstrate that the consequences of child labor can be ambiguous; the authors found significant negative impacts on education and a higher earnings gain for young adults previously engaged in child labor. Furthermore, Dumas (2012), assessing data from Senegal, finds that children's participation activities are associated with lower adolescent cognitive achievement.

IV. LAND TITLE AND CHILD LABOR: POTENTIAL MECHANISMS

This is an empirical paper, thus it is not in its scope to develop theoretical mechanisms explaining how land title affects child labor. We use a reduced form to highlight that land titles have an important effect on child labour and provide an indication of the magnitude of said impact. However, we find unavoidable to discuss possible channels of transmission, but do not advocate for any specific mechanism. As such, we outline possible channels, which are not necessarily mutually exclusive, through which a land registration policy could positively affect child labor supply.

Field and Torero (2002), for example, argue that untitled households need to provide informal policing, both to deter prospective intruders from invading private properties and to participate actively in community enforcement efforts to protect neighborhood boundaries. For them, this is an important mechanism by which the lack of land titling removes adults from the labor force and creates incentives for child labor, since it leads to adults guarding their property instead of working. Consequently, they end up sending their children to work to supplement the family income.

In searching the literature, we found conclusions that those without formal property rights often state that their spouses look after the home while they go to work, usually taking their children with them. That means that the shift in property protection away from informal

communities and households toward formal property ownership seems to be one of the main benefits of consolidated property institutions (see more details, for example, in Cockburn, 1998).

Other articles (Carter and Zegarra, 2000; World Bank, 2000; Field, 2007; among others) conclude, in general, that the lack of formal institutions exacerbates the absence of property protection provision by the state. Thus, informal or absent property rights makes households and communities invest their effort, time, skills, and resources into providing tenure security. By contrast, titling allows them to move away from these activities, releasing time to work and increasing their income. Andrade (2006) observes that land property is highly respected and preserved by local courts, thus changing the dynamics between residents and local police as mentioned above.

De Soto (2000) argues that land titles open up formal credit markets to people otherwise unbanked. Besley (1995) points out that better-defined rights to land affect the share of wealth that can be pledged as collateral in a credit contract. Under certain conditions, we can expect a positive effect from a land title program on access to credit due to the increment in wealth. The author claims that credit can be used to afford durable goods (a proxy for investment) or for non-durable consumption. Furthermore, Dower and Potamites (2007) also show that titles play an important *ex-ante* role in providing information about the applicant to the potential lender. The basic concept is that the bank would rather lend to titled households, not only because the title mitigates the bank's risk in the case of a default, but also because the title provides information about the likelihood of default or improves the borrower's credit score. Hence, easier credit access potentially decreases child labor by stimulating investments that increase the household income.

In addition, land title increases home equity value given that a land title program is followed by a better supply of public goods such as security, electricity, garbage collection, and provision of a sewage connection (in the case of Osasco, the implementation of those public services were not immediately part of the land title program. However, several months after the titling those services — each under a different timetable and dynamic — started to be part of the community's routine). The supply of public goods has positive externalities, such as increasing the housing value of a neighbourhood and increasing household access to credit that enables them to establish a more stable consumption pattern overtime, avoiding the need for short-term income generated by child labor

V. METHODOLOGICAL ISSUES, DATA, AND DESCRIPTIVE STATISTICS

A. Minimizing Selection Bias

Osasco is part of the federal program "*Papel Passado*" and home to about 30,000 people (or 6,000 families) living under informal conditions. That figure amounts to almost 4.5% of the city's total population (ANOREG, 2007). Given the fiscal resources available, the program to award land titles to all communities is scheduled to last from 2007 to 2014. *Jardim Canaã*, with 500 resident families, was the first locality to receive land titles in 2007 (all the households from *Canaã* – without exception – were eligible and received the title). Its closest neighbor, *DR*, a community of 450 households, received land titles by the end of 2012.⁴

⁴ Skoufias (2001), for example, uses a similar experiment to evaluate the income transfer initiative PROGRESA in Mexico. In that program some localities were randomly selected for participation (intervention localities) while the

As Behrman and Todd (1999) argue, randomization avoids selection bias in program evaluation – a bias that is generally present in non-experimental evaluations. They also note that randomization can prevent the problem of self-selection. However, other types of bias may occur in randomized designs, such as *contamination* and *attrition*. In our sample, 95% of the first survey participants — both from *Jardim Canaã* and *DR* — did not expect to be awarded land title. A question was directly asked: “Do you expect to have a **deed** during the next twelve months?” 95% responded no. Indeed, respondents were unaware of *Papel Passado* and its meaning, thus curbing potential behavioral deviation by households included in the program

*Contamination bias*⁵ is also avoided here, because the comparison group residents living outside the intervention locality could not benefit from the program. Besides, no alternative formal land title program exists, and the program does not provide a drop-out option (no resident of *Canaã* could choose to be excluded from receiving a land title). After receiving the title, the household may sell the property and move away from the locality. However, by then the household would have already been affected by the program, reducing the probability of *attrition bias*.⁶

Moreover, *Jardim Canaã* and *DR* share similar economic and social characteristics. They are both official neighborhoods without a clear border separating them. No one walking in the area would be able to identify which location is *Canaã* or *DR* without previous local knowledge. For example, both neighborhoods are located exactly 2.5 miles from downtown Osasco, having precisely the same access to Osasco’s main economic center. This helps to ensure that the intervention group is similar in many observable and unobservable characteristics to the group that did not receive land titles.⁷ Additionally, this paper uses a quasi-experimental design which, as defined by Shadish *et al.* (2002), is characteristic of a particular type of study in which researchers lack control over the allocation of interventions or other factors studied.

B. The Data

The door-to-door survey focusing on property rights (and answered by the family head – 56% female and 44% male respondents) was carried out in two stages: before and after titling. To further minimize bias, neither the questionnaire nor the interviewer provided direct information to the households about the objectives of the research. Officially, respondents were told that the study was examining general living conditions in the city of Osasco. Specific questions about child labor included: (domestic work was not considered part of the answers):

rest received land titles later (control localities). Such an assignment at the local level has the benefit of minimizing the chances of spillover effects between intervention and control study participants (groups/units) in the same area.

⁵ *Contamination bias* occurs if 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 for the intervention.

⁶ *Attrition bias* occurs if some members of the intervention group drop out of the program. If the purpose of the evaluation is to estimate the effect of receiving an intervention (for example, the effect of taking a drug over a period of time), then attrition bias can pose a major problem. It is not usually random and can compromise the benefits of randomization.

⁷ Rubin and Thomas (2000) indicate that estimates based on full (unmatched) samples are generally more biased and less robust to mis-specification of the regression function than those based on matched samples.

- a) *Do any children/teenagers contribute to the family income? How many? (= more than 5 and less than 18 years old);*
- b) *How many hours do they work daily? (interviewers were asked to check exactly how many hours each minor was working); and*
- c) *How many days per week do the minors work?*

In 2008, an additional question was asked: *Regarding hours of child work, is the number of hours greater, equal, or lower than one year ago?* The questionnaire did not include a specific question about parents taking their children to work with them.

The questionnaire was administered to 326 randomly-selected households and includes 39 questions.⁸ The format of questions and methodology closely mirrors the national statistical survey (*Pesquisa Nacional de Amostra de Domicílios*, PNAD) from the Brazilian Statistical Bureau (*Instituto Brasileiro de Geografia e Estatística*, IBGE). It also requests information on household and individual characteristics including the social, personal, and economic benefits associated with property ownership.

The researchers who conducted the door-to-door survey were not from Osasco. They first administered the questionnaires in March 2007, before titles had been issued to households of *Jardim Canaã*. The second stage was carried out with the same households in August 2008 (with 2% missing interviews) about 17 months later. The time gap between stages was designed so that all households interviewed during the first stage would have possessed the land title for at least one year by the time of the second survey.⁹

The study also tracked the households that moved away from both communities. In contrast to the 8% of households that moved away from Canaã, only 0.7% of households (1 out of 141) moved away from DR during the same period.

C. Descriptive Statistics

Tables 1 and 2 summarize the answers of the family heads (326) in 2007 and 2008 regarding the weekly number of hours of child work. Table 1 shows that for both groups combined (intervention and comparison), the weekly hours of child work decreased between 2007 and 2008.

⁸ The questionnaire is available upon request.

⁹ The 2nd Osasco Office of Registration (2.º *Cartório de Osasco*) provided us with the exact date that each household received its property title after being formally authorized by the Osasco's City Hall.

Table 1. Descriptive Statistics: Selected Variables, 2007–2008, Both Communities Combined

Variable	Pre-Intervention 2007		Post-Intervention 2008	
	Mean	Std. Dev.	Mean	Std. Dev.
Weekly hours of adult work	10.19	12.22	16.18	14.33
Ethnicity ^a	2.75	1.40	2.75	1.40
Gender ^b	0.43	0.47	0.33	0.47
Mean age	40.89	14.68	41.89	14.68
Marital status ^c	1.98	0.80	1.98	0.78
Net Monthly income (currency BRL ^d)	1,126.25	1,491.92	1,138.76	1,473.35
Number of residents	3.89	1.61	3.96	1.62
Number of children (between 10 and 18)	1.12	1.04	1.13	1.07
Weekly hours of child work	6.80	1.23	6.06	1.20
Years of education (family head)	7.25	4.34	7.31	4.33
Observations (respondents in both surveys)	326	326	326 ^e	326

Sources: the Osasco Land Title Survey and the Central Bank of Brazil.

^a 0 = White-Caucasian (35%), 1 = Afro-Brazilian (50%), 2 = Asian (8%), 3 = African Indian (5%), 4 = Indian (2%)

^b 0 = male, 1 = female

^c 0 = single, 1 = married, 2 = widowed, 3 = divorced

^d Currency exchange rate on 12/31/2008: 1 USD = 1.75 BRL (Brazilian Reais) and excludes child work income.

^e It includes residents that moved away.

Table 2 reports the *t*-test results regarding differences in means between the comparison and intervention groups before the program for the covariates in 2007. It is evident that the number of observations (households) in the intervention and comparison groups is roughly comparable. However child labor, monthly income per capita, and informal labor are significantly lower, whereas education of the family head is significantly higher in the intervention than in the comparison group.

Table 2. *t*-tests and *z*-scores for the Differences in Means for Covariates, 2007 (N = 326)

	Mean Comparison (A)	Mean Treatment (B)	Test: A – B = 0 <i>p</i> -value
Gender (= 1 if female, = 0 if not)	0.31	0.34	0.48
Ethnicity (= 1 if African-Brazilian, = 0 if not)	0.69	0.64	0.43
Marital status (= 1 if married, = 0 if not)	0.61	0.65	0.52
Mean age (family head)	42.60	39.40	0.06*
Weekly hours of adult work	10.10	10.40	0.81
Weekly hours of child labor (between 10 and 18 years old)	7.55	4.60	0.00***
Children working (= 1 if yes, = 0 if not)	0.34	0.14	0.00***
Years of education (family head)	5.00	9.00	0.00***
Net Monthly income (currency BRL ^a) per capita ^b	553.10	255.80	0.00***
Wealth index	1.12	-0.94	0.00***
Informal sector worker ^c (= 1 if informal, = 0 if not)	0.94	0.65	0.00***
Access to credit (= 1 if yes, = 0 if not)	0.44	0.45	0.88
Number of residents	3.88	3.91	0.58
Number of children (between 5 and 17)	1.41	1.48	0.49
Observations (households)	185	141	

Source: The Osasco Land Title Survey and the Central Bank of Brazil

^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 – without child work income – by the number of residents

^c Defined as a individual without an active work card or a small business license

*, **, *** rejection of the null hypothesis at 10%, 5%, and 1% respectively.

While the intervention group is more educated and engages less in informal work, it has a lower income than the comparison group. This is corroborated by Spearman correlations in Table 3 and by the wealth index displayed in Table 2. The wealth index – computed using principal component analysis – summarizes the stock of durable goods owned by the households including TVs, radios, cars, washing machines, refrigerators, and freezers.

Table 3. Spearman Correlation, 2007 (N=326)

	Years of Education	Informality	Net Monthly Income per capita	Child Weekly Hours Worked
Years of Education	1			
Informality	-0.14**	1		
Net Monthly Income per capita	-0.21***	0.21***	1	
Child Weekly Hours Worked (between 10 and 18 years old)	0.11**	0.08	0.11*	1

Source: the Osasco Land Title Survey

*, **, *** rejection of the null hypothesis at 10%, 5%, and 1% respectively.

In order to explain the results, recall that households with higher levels of education tend to have more access to formal jobs in Osasco (Zylberstajn and Neto, 1999) and naturally have disposable income mostly based on wages. Therefore, they tend to receive additional savings (FGTS and public pensions) and significant perquisites that are not reflected in their direct cash

salaries, as is customary for formal employment in Brazil.¹⁰ On the other hand, informal workers do not receive these benefits, relying instead on cash income to compensate for the lack of perquisites, and in most cases they do not pay income tax and/or contribute to a public pension.¹¹

Furthermore, Table 3 shows a positive correlation between monthly income per capita and children’s hours worked per week (0.11). This is consistent with our underlying hypothesis that families financially rely on the number of hours worked by children to supplement monthly income.

VI. EMPIRICAL STRATEGY

The previous section discussed statistically significant differences between the intervention and comparison groups of our sample. These differences can be attributed to the randomization at the community level rather than at the household level. Along the same lines, Skoufias (2001) and Behrman and Todd (1999) observe that even if the comparison and intervention groups have similar characteristics at the community level, they are not fully comparable at the household level. These authors suggest the use of control variables rather than estimating the program impact only through mean tests. We follow this strategy and adopt two econometric procedures to address the differences in observable characteristics. The first is to include control variables and use the OLS and DD techniques. The second procedure is to use these same methods combined with a propensity score technique to select a more balanced sample. The following subsections explain in detail each of the empirical methods used.

A. OLS REGRESSION DEFINITION

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

$$H_i = \alpha + \delta title_i + \beta X_i + \varepsilon_i, \quad (1)$$

where H_i represents the number of hours worked weekly by children (between 10 and 17 years old) and X_i is a control vector of socio-economic variables that includes gender; ethnicity; marital status; years of schooling; number of members in the household; age; age²; a dummy for informal work; household assets; and, monthly income per capita.

These variables are common covariates for land title and are usually used to evaluate this type of program. The variable $title_i$ is a dummy equals 1 if the household participates in the

¹⁰ For example, a formal Brazilian employee usually receives a health care plan for the whole family, subsidized transportation, and a meal plan. Furthermore, formal employees have FGTS — a compulsory savings account under Brazilian Labor Law. FGTS is the Fundo de Garantia por Tempo de Serviço — Guarantee Fund for Time Service. Under the FGTS, employers deposit 1/12 of the worker’s pay in a restricted bank account whose balance is withdrawn and given to the worker if and when (s)he is fired without a just reason other than to decrease costs.

¹¹ In our data sample, 233 households had informal sector workers. They represented 92% of the workers in the control group and 64% in the intervention group (see Table 2).

titling program, and 0 otherwise. Since all households in *Canaã* received titles in 2007, the parameter of interest is the average treatment effect on the intervention (treated) group (ATT).

B. The Difference-in-Difference Methodology: Definition

The main econometric method applied is the difference-in-difference estimator, which involves comparing the difference in outcomes before and after the intervention in the affected group (treated) and in the unaffected group (comparison) (see Bertrand *et al.*, 2004; and Imbens and Wooldridge, 2008).

This method is estimated with the following regression model:

(1)

where:

$H_{i,s,t}$ is the number of hours worked weekly by child i in community s at time t ;

$treat_{s,t}$ is a dummy variable equal to 1 if the household resides in the treated community ($s = 1$), and 0 otherwise;

$year_t$ is a dummy variable equal 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 community s , changing through time; and,

$u_{i,s,t}$ denotes the error term, assumed to be independent of $X_{i,s,t}$ and $year_t$ (see Meyer, 1995; and Imbens and Wooldridge 2008).¹²

Our parameter of interest is coefficient α_{DD} , which identifies the intervention's effect on the group. (See Appendix I).

VII. OLS, DIFFERENCE-IN-DIFFERENCE, AND DDM: EMPIRICAL RESULTS

This section presents the results of the empirical analysis proposed previously. We start by discussing the findings (applying a sub-sample of the total 182 respondents out of 326). Table 4 shows that the average weekly hours of child work decreased in titled households and increased in non-titled households.

¹² Once all households of the intervention group receive titles, s and $year_t$ will be the same. Thus, from now on, the subscript s is omitted for the sake of simplicity.

**Table 4: Average Weekly Child Hours Worked
(households with children between 10 and 17 years old) (N = 182)**

	Titled (Canaā)	Non-Titled (DR)	Difference
1. Child work hours (before)	4.60	7.55	2.95**
2. Child work hours (after)	1.80	8.30	6.50**
3. Change in mean weekly child work hours	-2.80	0.75	-3.55

** significant at 5%

DD calculates the difference between “after” and “before” values of the mean outcomes for each intervention and comparison group. The difference between mean differences is the impact estimate. In Table 4, the impact estimate for children’s weekly work hours is -3.55 hours.

The mean effects of the program are analyzed in Table 5, which presents OLS estimates for four regression models using data from 2008. The results support the claim that land titles decrease child labor as measured by the number of hours worked.

Model OLS-Naïve excludes the control variables and estimates a negative and significant coefficient (-4.28) for the dummy, land title. The control variables are included in all other models and a significant coefficient is also obtained for land title. The estimates for the control variables also have the expected results. Applying a propensity score matching, which plays an important role in balancing study groups, based on Rosenbaum and Rubin (1984), a balanced model was also tested with a significant coefficient of (-3.41), in line with the analysis in Table 4.

A higher educational level (years of education) of the household head along with easier access to credit leads to a decrease in the number of weekly hours worked by children. Furthermore, the main results remain the same with the inclusion of the variable hours of adult work.

Table 5. OLS Estimates for Land Titling Impact on Child Labor (Post-Program), 2008
OLS Equation (1)

Variables	Model 1	Model 2	Model 3
	OLS Naïve	OLS	OLS Balanced Sample
Land title	-4.28*** (0.95)	-3.62*** (1.02)	-3.41*** (0.94)
Gender (= 1 if female)		1.29*** (1.03)	4.10*** (1.71)
Ethnicity (= 1 if African-Brazilian)		-1.86 (1.54)	-1.27 (1.65)
Marital status (= 1 if married)		0.18 (0.52)	0.31 (0.44)
Age		0.82 (0.56)	0.03 (0.02)
Age ²		-1.30* (0.05)	-0.07 (0.06)
Years of schooling (family head)		-1.78** (0.21)	-2.02** (0.16)
Net Monthly income (currency BRL ^a) per capita ^b		-0.00 (0.00)	-0.00 (0.00)
Access to credit (= 1 if have)		-2.34** (1.71)	-2.89** (1.38)
Wealth index ^c		0.23 (0.66)	0.36 (0.72)
Informal worker		0.58 (1.82)	1.48 (1.86)
Number of children (10 and 18 years old)		0.39 (1.28)	0.47 (1.66)
Constant	3.89*** (1.12)	3.99** (5.18)	5.57** (4.08)
Sigma			
Log-likelihood			
Pseudo- R ² /R ²	0.09	0.16	0.11
Observations	182	182	136

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

^a Currency exchange rate on 12/31/2008: 1 USD = 1.75 BRL (Brazilian Reais).

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

^c Wealth index summarizes the total value of durable goods and the principal component analysis (PCA) was applied. Imbens and Wooldridge (2008) state that the PCA is a useful technique when explanatory variables are closely related.

Notice that gender increases labor work of children. This happens most likely because women must be at home for more hours of the day. Therefore, the need for child labor is more important.

Table 6 presents the DD estimations for the years 2007-2008 (pre- and post-titling). The results corroborate the previous findings that land titling decreases the average number of hours worked by children. All columns display a negative 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. Again, the inclusion of the variable hours of adult work has not changed the overall outcome. Moura and De-Losso (2010), using the same data set, show that land title has a positive effect on labor supply and income.

**Table 6. Difference-in-Difference: Land Title Impact on Child Labor (2007–2008)
DD Equation (2)**

Variables	Weekly Hours Worked (DD Naïve)	Weekly Hours Worked (DD Unbalanced)	Weekly Hours Worked (DD Balanced)
Land title	-0.22 (1.21)	-0.16 (1.82)	-0.21 (1.86)
Land x year (<i>DD</i>)	-4.06*** (0.94)	-3.07*** (1.11)	-3.12*** (1.13)
Year	0.90*** (0.28)	0.85*** (0.37)	0.28*** (0.29)
Gender (= 1 if female)		1.95*** (1.45)	1.71*** (1.42)
Ethnicity (= 1 if African-Brazilian)		-0.72 (1.11)	-0.00 (1.12)
Marital status (= 1 if married)		-0.29 (1.32)	-0.42 (1.34)
Age		-0.03 (0.02)	-0.08 (0.03)
Age ²		-0.38** (0.04)	-0.77* (0.04)
Years of schooling (head of household)		-0.28** (0.18)	-0.29** (0.19)
Monthly income (currency BRL ^a) per capita ^b		-0.83 (0.00)	0.00 (0.00)
Access to credit		-1.33* (1.16)	-1.25 (1.14)
Wealth index ^c		0.49 (0.43)	0.75 (0.59)
Worker type (= 1 if informal)		0.007*** (1.21)	0.70 (1.33)
Number of children (10 and 17 years old)		0.003*** (1.11)	0.22 (1.29)
Constant	3.05*** (1.06)	2.21*** (1.71)	2.48*** (1.09)
R ²	0.09	0.12	0.14
Observations	364	364	272

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

^a Currency exchange rate on 12/31/2008: 1 USD = 1.75 BRL (Brazilian Reais).

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

^c Wealth index summarizes the total value of durable goods, and the PCA was applied. Imbens and Wooldridge (2008) state that the PCA is a useful technique when explanatory variables are closely related.

The DDM estimates are shown 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 comparison group to identify the parameter of interest (see Heckman *et al.*, 1997; and Smith and Todd, 2005, for a

detailed discussion of 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 represents the bias while the higher bandwidth captures the variance.

Table 7. Difference-in-Difference Matching Estimates, 2007–2008 Estimator Equation (5)

Variables	Bandwidths of Kernel Estimator		
	(0.01)	(0.05)	(0.10)
Land title	-4.03*** (2.62)	-3.85*** (2.25)	-3.56*** (2.24)
Observations	364	364	364

Notes: Standard errors (in parenthesis) are computed using a bootstrap with 100 replications.

*, **, *** statistically significant at 10%, 5%, and 1%, respectively.

The first column of Table 7 shows that for the smallest bandwidth the point estimate is very similar to the DD naïve (see Table 6). This gives additional support for the quality of the data. As the bandwidth is enlarged, the point estimates become smaller in absolute value, as does the variance. Therefore, despite the relatively high variance in the first column, we believe that the average treatment effect on the intervention group must be around 4 hours/week (or 4 fewer hours of weekly child work).

The overall magnitude interpretation allows different perspectives. First of all, assuming an average of 4 hours per week without engagement on child labor represents a 60% work hour reduction compared to pre-title context. As Field (2007) has shown child labor reduction has the potential to increase adult work hours, and as the magnitude increases (less hours of child work), greater potential to occur such transmission. Third, from a more simplistic perspective, the particular magnitude found could be marginal considering that 4 hours per week represents basically 48 minutes a day (assuming children work only on business days), or 24 minutes in the morning and 24 minutes in the afternoon. Last but not least, further research should focus on work balance (domestic x outside work) to check if those 4 hours were not, in practise, re-allocated through domestic activities.

VIII. CONCLUSION

Based on Field’s model (2007), which rationalizes the effect of land title on child labor force participation, we present evidence regarding the effect of property rights on child labor using data from a developing country.

Several existing studies, such as Field (2007) and Andrade (2006), suggest that property rights affect income, credit, and labor supply. However, no current studies compare data from before and after land titling, which is the major advantage of our paper. Our empirical results support the finding that property titling decreases child labor force participation by about 6 hours per week.

Moreover, we conclude that titling programs appear to have a different effect in terms of employment. While most welfare programs remove adult households from the labor force due to

a positive income effect, this type of program induces adults to work more and children to work less. It is important to understand the process through which economic outcomes respond to land titles, mostly because governments from developing countries may be able to use these findings to reduce urban informality regarding ownership.

These results may also help to explain labor market frictions, particularly in developing countries – a subject studied by Goldsmith (1995). High levels of residential informality coupled with informal property protection act as an obstacle to adjustments in the labor market. Therefore, public policies involving land titling could potentially play an important role in economic growth.

This analysis offers various possibilities for further research. For example, the analysis of the increase in labor force participation due to land titling and its effects on the income and utility of households could be investigated. At this stage, the increase in income gained by households through increasing their labor force participation is unclear. The distributional impact of land titling could also be investigated, not only regarding labor supply and child labor but also other economic variables, such as access to credit and fertility. This would improve the assessment of the impacts of such programs on the lives of millions of households living in urban squatter communities in developing countries around the world.

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APPENDIX I

The causal effect identification on the outcomes' variables relies on the following assumptions:

- (i) Selection for the intervention does not depend on unobservable individual and community characteristics changing over time;
- (ii) Differences between the intervention 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 intervention does not affect child labor in households living in the neighboring areas; therefore, no spillover effects are present.

These assumptions imply that

$$E(u_{i,s,t} | treat, year, X) = E(u_{i,s,t}) = 0, \quad (2)$$

and

$$\left\{ \begin{array}{l} \left[\begin{array}{l} E(H_{i,s,t} | treat = 1, year = 2008, X) \\ E(H_{i,s,t} | treat = 1, year = 2007, X) \end{array} \right] \\ \left[\begin{array}{l} E(H_{i,s,t} | treat = 0, year = 2008, X) \\ E(H_{i,s,t} | treat = 0, year = 2007, X) \end{array} \right] \end{array} \right\} = (\beta_2 + \alpha_{DD}) - (\beta_2) = \alpha_{DD}. \quad (3)$$

The main problem regarding this subject is a possible self-selection, also known as an anticipation problem. This certainly would be an issue if households decided to make their children work fewer hours given the expectation of receiving a land title. Regarding assumption (ii), we use control variables to account for differences between the two groups in the baseline year (2007). In addition, the fixed effect estimator is applied to check for the robustness of the results given that the dependent variable may differ across groups but remain invariant through time.

We also estimate the difference-in-differences-matching (DDM) estimator suggested by Heckman *et al.* (1997) and employed by, among others, Smith and Todd (2005) and Angelucci and Attanasio (2009). The main advantage of such an 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 comparison group. The aim of the weighting function is to construct a counterfactual mean effect based on the distance between the propensity score of each comparison group observation and the intervention group observation.¹³ The identification of the ATT requires that

¹³ Smith and Todd (2005), for instance, define the weighting function as $G((P_C - P_T)/b_n) / \sum_{k \in C} G((P_k - P_T)/b_n)$, where $G(\bullet)$ is the kernel function and b_n is the bandwidth, P stands for propensity score, C for control group, and T for treatment group.

$$E(\Delta H_c | P, Land = 1) = E(\Delta H_c | P, Land = 0),$$

where: P refers to propensity score and c stands for comparison group.

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

$$\alpha_{DDM} = \frac{1}{n_1} \sum_{i \in T \cap S_p} \left[H_{i,1,1} - H_{i,1,0} - \sum_{j \in C \cap S_p} w(i, j) (H_{i,0,1} - H_{i,0,0}) \right], \quad (5)$$

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 & Todd 2005) (P stands for propensity score, C for comparison group, and T for treatment group).

Appendix III

Table 8 – Land Titling Brazil 2007

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

Source: PNAD, 2008, Brazil.

Table 9. Propensity Score: Logit Estimates for the Selection of the Treatment Group, 2007

Variables	<i>Dummy</i> = 1 if a household lives in the intervention area (<i>Canaã</i>)	<i>Dummy</i> = 1 if a household lives in the intervention area (<i>Canaã</i>)
	(Unmatched Sample)	(Matched Sample)
Gender (= 1 if female)	0.42 (0.48)	0.11 (0.51)
Ethnicity (=1 if African- Brazilian)	0.04 (0.45)	0.02 (0.45)
Marital status (= 1 if married)	0.58 (0.47)	0.35 (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 work (5-17 years old)	-0.03 (0.02)	-0.01 (0.02)
Years of education (head)	0.14*** (0.05)	0.05 (0.08)
Net Monthly income per capita	-0.01** (0.00)	-0.01 (0.00)
TV (= 1 if have)	-1.48** (0.69)	-0.68 (0.85)
DVD (= 1 if have)	-0.64 (0.53)	-0.29 (0.58)
Radio (= 1 if have)	-1.68*** (0.50)	-0.60 (0.84)
Car (= 1 if have)	-0.28 (0.45)	-0.09 (0.48)
Washing machine (= 1 if have)	2.19*** (0.65)	1.06 (0.92)
Refrigerator (= 1 if have)	-6.07*** (1.07)	-2.76 (2.15)
Informal worker	-1.73*** (0.62)	-0.75 (0.85)
Credit	-0.17 (0.43)	-0.03 (0.45)
Constant	8.18*** (1.62)	1.87 (4.09)
Pseudo-R2	0.62	0.63
Prob > Chi2(16)	0.00	1.00
Observations	305	288

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