**Inter-Generational Effects of Titling Programs:**

**Physical vs. Human Capital**

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*Abstract*

Human capital investment may be affected by programs aimed at giving legal ownership titles to the occupants of land; these are called "land titling programs". Titling is associated with an income (or wealth) effect as it induces higher expenditure on normal goods like home consumption, education and health services. But there is also a substitution effect: the elimination or reduction of expropriation risk makes investment in the home more attractive and therefore increases the "opportunity cost" of other forms of spending. The net effect on human capital is ambiguous. We present a simple model to illustrate this point and test it using a natural experiment in Uruguay were human capital investment is proxied by education and health investment. Our results confirm that titling favors home investment to the detriment of some dimensions of human capital investment for children of 16 and under.

JEL Codes: K11, P14, Q15

Keywords: titling, income and substitution effects, intergenerational effects

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## 1. Introduction

In the volatile economies of Latin America, the main saving mechanism for many families is housing. Achieving home-ownership is a milestone in a family's history and is probably the most secure way of transmitting family wealth to the next generation.

Titling programs that grant formal property rights have been advocated as a powerful anti-poverty instrument. De Soto (2000) argues that a lack of formal property rights stops the poor from transforming their wealth into capital. Granting formal property rights could allow the poor to collateralize their land and gain access to the credit market. According to de Soto this will start a virtuous circle from titling to the credit market, from access to credit to investment in productive activities and from there to greater labor productivity and higher income. International organizations have taken this suggestion very seriously and have fostered land-titling programs in many developing countries. However, there is still a lack of empirical evidence to substantiate the income improvement effects of titling programs. Up to now, empirical studies have found little support to the credit channel hypothesis (Galiani and Schargrodsky 2006 and Field and Torero 2003). As regards a different transmission mechanism, Field (2007) finds that titling is associated with an increase in adult labor supply, and the author attributes this to less of a need to protect the home from illegal occupants.

The inter-generational effects of titling programs are even less clear. On the one hand, following de Soto's argument, titling will allow families to attain higher income levels. Since education and health are normal goods, we should expect more investment in both these areas. Better education and health improve the competence and ability of individuals in the labor market and as such are natural proxies for human capital investment. On the other hand, titling makes home investment relatively more profitable either because the house (in an improved condition) can now be sold[1] or because families can safely benefit from the house forever. This may prompt families, especially poor ones, to allocate more of their scarce resources to housing investment and away from human capital investment. In theory, titling programs have an income effect that favors investment in children's human capital and a substitution effect that operates in the opposite direction. The net effect remains ambiguous and this should be resolved empirically.

In the study of titling, the main empirical methodological problem is to isolate the true effects of titling and separate them from other variables that are normally jointly determined. For instance, wealthier people are more likely to own their home and to have better socioeconomic indicators in general. In this paper, we use data from a natural experiment in Uruguay involving nine neighborhoods called "comunidades" (communities).[2] The basic situation is that these communities were formed by ex ante homogeneous households, and it was possible to assign formal property rights to the members of only three communities for reasons that are independent of any characteristics of the families living in the communities or other geographical or spatial reasons. We use original data from the 1970s when families were assigned to the communities, and conducted a survey to gather detailed current information.

We find that titling is associated with a higher probability of making home improvement investments. This is in line with the empirical literature (see for instance Jimenez 1984, Besley 1995, Alston et al. 1996, Jacoby et al. 2002, and Brasselle et al. 2002). Field (2005) finds that the

[1] Before titling, houses could not be legally sold since the occupants were not the owners. However, in shanty towns sometimes houses are "sold" on informal markets.

[2] Di Tella, Galiani and Schargrodsky (2007) also use a natural experiment with a similar number of observations.

strengthening of property rights in urban slums has a significant effect on residential investment and that most of this is financed without having recourse to credit. The author concludes that this is indicative of an increase in investment incentives related to less threat of eviction. Following our main argument, the increased incentive to invest in housing in relative terms means a reduction in the incentive to invest in other things. Indeed, we find that for *some* human capital dimensions (proxied by education and health), titling is associated with worse results for children of up to 16 years of age. As yet, the effects of titling on education have not received much attention. Galiani and Schargrodsky (2004) and Vogl (2007) report positive effects of titling on child nutrition, and Gandelman (2010) reports positive effects on chronic diseases. It seems reasonable that, as regards nutrition, the income effect will dominate the substitution effect and therefore the results of Galiani and Schargrodsky (2004) and Vogl (2007) are reasonable within the framework of our research. The results of Gandelman (2010) refer to the adult population.

This paper is structured as follows. In section 2 we present a simple model of the income and substitution effect that takes places in these communities. In section 3 we present the data and our estimation strategy, and in section 4 the results. Section 5 is a conclusion.

## 2. A simple model of titling inter-generational effects

Due to income and substitution effects, the inter-generational impact of titling programs is not evident. According to De Soto's (2000) argument, titling will allow families to attain higher income levels and will therefore make for higher demand for all normal goods. Families that have benefited from formal property rights are, at least through this income effect, more likely to invest more in their children's education and health. The substitution effect operates in exactly the opposite direction. Families without formal property rights may find it less profitable to invest in their home than those with formal property rights. The titling program therefore increases the opportunity costs of education and health investment. Which of these two effects is dominant is an empirical matter.

In order to focus on how the income and substitution effects operate, we present the simplest model we could think of to illustrate this trade off. A parent needs to allocate his lifetime income between investment in his children's human capital (*H*) and investment in the home (*K*). Following De Soto's argument, income, *y*, is a function of the parents' productivity $\bar{y}$ (a draw from nature) and tenure security (*p*).

The benefits from home investment are uncertain because property rights are insecure. There are two states of nature: with probability *p* the individual does not suffer expropriation and enjoys utility level $u(K)$; with probability *(1-p)* the individual suffers expropriation, therefore her utility is $u(0)$. The utility derived from the child's human capital is $v(H)$. The parent maximizes her expected utility from home investment and investment in her child education.

Therefore the problem to be solved is:

$$\begin{aligned} &\underset{H,K}{Max} && E[u(K)] + v(H) = \\ &= \underset{H,K}{Max} && pu(K) + (1-p)u(0) + v(H) \qquad (1) \\ &\text{subject to:} && H + K \le y(\bar{y}, p) \end{aligned}$$

From the first order conditions we get:

$$pu'(K) = v'(H) \qquad (2)$$

Equation (2) and budget constraint determine the two unknowns. These equations clearly present the income and substitution effect. Without De Soto's channel (titling does not produce income-enhancing effects), $H + K \leq y(\bar{y})$. In this case the higher *p*, the higher the left hand side of (2) and the lower investment in the child's human capital, H. The substitution effect simply changes the weights of the terms of the objective function.

Suppose that the parent mistakenly perceives that his house is at no risk of expropriation and maximizes $u(K)+v(H)$ subject to budget constraint. In this case (2) becomes

$$u'(K) = v'(H) \qquad (2')$$

An increase in income due to higher *p* translates into higher home investment *K* and higher investment in the child's human capital, *H*. This is the pure income effect.

Assuming the two parts of the utility function have an isoelastic functional form: $u(x) = v(x) = \frac{x^{\frac{\sigma-1}{\sigma}} - 1}{\frac{\sigma-1}{\sigma}}$ and solving from (2) and the budget constraint we get that at the optimum:

$$H = \frac{1}{1+p^{\sigma}} y(p)$$
$$K = \frac{p^{\sigma}}{1+p^{\sigma}} y(p) \qquad (3)$$

The first term in both equations reflects the substitution effects. These effects are decreasing in *p* for human capital investment and increasing in *p* for home investment. The second terms are the income effects, which are increasing in *p* in both.

Equation (3) provides a simple way (maybe too simple) to address the size of the expected changes. There is a wide literature that can facilitate its calibration. $\frac{1}{\sigma}$ is the Arrow-Pratt measure of relative risk aversion and is also the elasticity of marginal utility with respect to income. In a seminal paper, Friend and Blume (1975) studying the demand for risky assets, estimate that the coefficient of relative risk aversion generally exceeds 1.[3] Layard et al (2008) estimates that elasticity of the marginal utility with respect to income is 1.24.

To have an idea of the sizes of the impact that could be expected in the empirical section of this paper we assume that $\frac{1}{\sigma}$ can take the value of 1, 2 or 3. A parent that is granted formal

[3] There is a wide range of estimates that go from 0 to 10.

property rights and face no risk of expropriation $(p = 1)$, will invest half of its income in their children human capital and half in the physical asset. A parent facing a risk of expropriation of 10% will invest 52.6%, 51.3% or 50.95% of its income in their children human capital respectively for $\frac{1}{\sigma} = 1, 2 \text{ or } 3$. Therefore, in the absence of income effects ( $y(p) = \bar{y}$ ), the decrease in children's human capital investment is of 5.0%, 2.6% or 1.75% respectively.

## 3. Data and estimation strategy

### 3.1. The natural experiment

The Instituto Nacional de la Vivienda Económica (INVE, National Institute of Economic Housing) was a public institution whose purpose was to provide affordable housing solutions for low income families in Uruguay. It was created in 1937. Several decades later, in 1974, the Ministry of Housing was created as the central public institution in charge of housing policies. As such the INVE was under its jurisdiction.

In the mid-1970s the INVE built nine small neighborhoods to meet the housing needs of their target population. They were built in one of the poorest areas of the city, to the sides of an important avenue that runs from the north-west to the south-east of Montevideo (the capital city). These neighborhoods were called "comunidades" (communities). In all cases purchase agreements were signed and implicit mortgage contracts were in place. There were no down payments required and families only had to pay small monthly installments.

Soon after the communities were built, in June 1977, the Ministry of Housing and the INVE were abolished by law. INVE'S functions and property were assigned to the state-owned mortgage bank, the Banco Hipotecario del Uruguay (BHU). On November 1977 the BHU and the Ministry of Education sign an agreement in which the Ministry of Education would take care of the management of the communities. Later, on July 1984, the Ministry of Education transferred the management of the communities to the Municipality of Montevideo. Less than three years later, on March 1987 the Municipality rejected this last agreement with the Ministry of Education and informed the BHU that it should take care of the management of the communities.

The management of the communities implied receiving the payments according to the purchase agreements, transferring the formal property rights to those that finished paying and initiating the legal actions to force the sale of the property of those that did not comply with the required payments. Typically this last action should end up with the auction sale of the property and the title deeds by the new landowner. But given the small amount of the payments the public institutions involved in this task were not interested in the active management of the communities. As time passed by, the entire population of the communities stopped paying the required installments but no action was taken at all.

In December 1987, a working group set up to study the situation of the communities concluded that it would be excessively expensive for the BHU to assume the management of the communities, so it was in the best interest of the BHU to sell the houses to the current occupants at whatever price they were able to pay. By the end of 1988 the board of the BHU set a nominal price of approximately $100 (one hundred United States dollars) for those occupants that could not prove they had made any previous payment. Those that had made at least one previous payment just had to pay the housing titling expenses (approximately $20).

Although, the price was just a nominal figure, selling the houses and assigning the formal property rights to the occupants could only be done in three communities because for the other communities there were no registered plans (area maps with the land division among houses) at the City Council of Montevideo. The architecture of the houses in all the communities was basically the same. They were all rectangles of about 50 square meters with no internal divisions and an external bathroom. Also, there is no correlation between area characteristics and communities with and without maps that could explain why in some cases maps were registered and in others were not.

The only reasonable conjecture reported by some authorities is that the liquidation of INVE produced this asymmetry between communities. When the INVE was eliminated they had already registered some maps but not all of them. The BHU assumed its goals but did not assign resources to the management of the communities and as a result some ended up without their maps registered at the municipality. Obviously, when the families were assigned to the communities they did not know if maps were, or at some time in the future would be, registered at City Hall.

Especially relevant to this paper is that despite the mentioned problems in the management of the communities there were no systematic differences in the provision of public goods. In particular there were not differences in public schools programs and educational and health policies between communities. For instance, currently all communities have a public school and a public health center within eight blocks from the community (no more than a 10 to 15 minutes walk).

Thus, the reason why the inhabitants of three communities were able to acquire formal property rights in the early 1990s while the inhabitants of the other communities were not able to acquire formal ownership was not related to any characteristics of the families themselves and also there were/are no community wide differences in the land or public goods disposable for different communities.

### 3.2. Definition of the treatment

The Intention to Treat Group is made up of the individuals in the three title-eligible communities.[4] Not all the individuals in the title-eligible communities completed the procedures to acquire property rights. The Treatment Group is made up of a total of 71 households (314 individuals) from the title-eligible communities that were awarded formal property rights. The control group is defined as any group who was not offered property rights. For the non title-eligible communities[5] we extracted a straight random sample 165 houses (one third of total houses) using the original door-number of houses within each community.

### 3.3. Pre-treatment characteristics

In order to be sure that the estimations are not biased we need to show that the pre-intervention characteristics of the Intention to Treat and control groups are reasonably similar. We were able

[4] 18 de Julio, 25 de Agosto and Lavalleja.

[5] The non title-eligible communities are 19 de Abril, Independencia, Sarandí, Rincón, Grito de Asencio and Guayabos.

to locate the original files from the 1970s. In these files there was information on the following socioeconomic indicators of the original occupants: family composition, presence of children, age, income level and work status.[6]

Table 1 shows that, *ex ante* the populations of the Intention to Treat and control groups were indeed very similar.

<TABLE 1 ABOUT HERE>

### 3.4. Field work

The survey was conducted in February and March 2007 by a team of four welfare workers and one sociologist specially trained to deal with populations in difficult socioeconomic environments. The survey questionnaire was very similar to typical household survey gathering information on demographic, housing, health and education. Besides that there was a section on entrepreneurship and credit access. The no-response rate was 3%.

### 3.5. Estimation strategy

Once the exogeneity of the housing titling is established, the identification of the causal effects of land titling follows from simple econometric techniques. If we had a perfect natural experiment there would be no need to control for any other variable to address the effect of titling. It is enough to simply perform t-test for equality of means between the intention to treat and control groups. This is the first set of results reported in the next section.

Naturally, there may be concerns that non-random differences between communities in pre-treatment characteristics, demographic characteristics or public goods may affect the results. In Table 1 we showed that there are no significant differences in observable pre-treatment characteristics but there may be other unobservable shocks. E.g. a new firm might open up in the surrounding area of a community providing new job opportunities. Such shocks create correlation between observations from the same community and have to be taken into account. To address these issues the empirical model proposed is

$$y_{ij} = x_{ij} + \gamma T_j + v_j + \varepsilon_{ij} \qquad (4)$$

where *j* indexes the community and *i* the household (or individual) within the community. $y_{ij}$ is the outcome of interest, $x_{ij}$ are observable characteristics that might influence the outcome variable, $T_j$ is a dummy variable indicating the treatment group, $v_j$ is an error term that is constant within communities (i.e. community wide unobservable shocks) and $\varepsilon_{ij}$ is and individual specific error term.

As long as $T_j$ is truly randomly assigned $T_{ij}$ and $x_{ij}$ are uncorrelated and the regression

[6] It was not possible to locate all the files. Overall we located 82% of the communities' files. The full breakdown is as follows: 19 de Abril 98%, Independencia 96%, 18 de Julio 85%, Sarandí 71%, Rincón 94%, Grito de Asencio 89%, 25 de Agosto 90%, Lavalleja 69% and Guayabos 70%.

remains unbiased even without the controls. But given that this is not a pure randomized experiment we control for observables to test whether this influence outcomes.

Although each family in the three Intention to Treat communities could have benefited from property rights only about half of them completed the procedures necessary to finally obtain formal rights. This non-compliance may be connected to personal or family characteristics (laziness, lack of knowledge of opportunities, family disputes, etc.) that may also impact on the variables under study. In order to lessen the consequences of this possible situation, in all cases we run two sets of regressions. First, in the OLS regressions we use a dummy variable that takes the value 1 for the Intention to Treat communities. Second, we run instrumental variable regressions in which we instrument the treatment (take up of property rights) with the completely exogenous intention to treat (all houses in the title-eligible communities).

In the early nineties the implementation of the transfer of property rights began but there was still uncertainty of which communities would be benefitted. This uncertainty was resolved by 1995 when it was known which communities were entitled to benefit from transfers of property rights and which not. Those that moved into the treatment or control communities after that date may have different characteristics to those that moved there before. There may also be differences between those moving to the treatment and the control communities. Therefore to minimize the attrition problem, in our estimation we only use those families that arrived before 1995.

Since the intention to treat and control group where not sampled in the same proportions (all the intention to treat were surveyed but only a third of the control households were surveyed) in all estimations we weight the control observation to account for the fact that they are being used to represent the behavior of the entire control group. The sampling weight is the inverse of the probability that the observation is included because of the sampling design (1 for intention to treat households and 3 for control households).

The most difficult estimation issue is to deal with community wide shocks $v_j$. Conceptually is would be correct to cluster standard errors on the community level but in this case this is not feasible due to the small number of clusters. Donald and Lang (2007) showed how clustered standard errors are biased in the case of small number of clusters. Angrist and Pischke (2010) provide 42 as a rule of thumb for how many clusters are necessary for clustering to work. Since, there are 9 communities in this case, clustering does not work. Donald and Lang (2007) provide a two stage alternative. In the first stage we regress the outcome on individual level characteristics and a vector of community fixed effects. Then we predict the community fixed effect and perform a t-test of the equality of means between the intention to treat and control communities. In this case we have only one observation per community therefore the risk of not having enough power to detect any statistically significant difference is important.

## 4. Results

### 4.1. Mean differences

Table 2 presents summary statistics and t-test of mean difference between households in the intention to treat and control communities. We report three measures of home investment. Human capital investment is proxied by three measures of investment in children's education and two measures of investment in children's health. For education we take children between 6 and 16 years old and for health we take children of less than 16 years old.

In the intention to treat communities 95% of the households made some improvements and renovation in their house while in the control communities 87% of the households made improvements. The most common improvements were in bedrooms (42% vs. 29%) and in bathrooms (52% vs. 50%).

The overall enrollment rate in primary and secondary schooling in the communities is somewhat below the average city enrollment rate but in line with the enrollment rate among the population of similar socioeconomic strata. The average enrollment rate in the control communities is 91% and in the intention to treat communities is 84%.

We considered two additional measures of education that involve larger direct expenses for the families. Overall, 94% of the children of the communities are in the free-of-charge public school system. We found that 8% of the children in the control community and 3% in the intention to treat communities go to private schools.

We also gathered information on whether the families sent their children to "extra lessons". The most common of these extra lessons are support for homework and for languages (especially English). About one in four of the children in the communities went to some extra lessons, 36% in the control and 15% in the intention to treat.

The two variables to capture investment in children's health are whether the children were taken for a routine visit to a medical doctor or to a dentist. By routine we mean that they were taken without feeling ill or having a toothache. A greater percentage of children in the control communities went for a medical checkup (44% vs. 36%) and for a dentist check up in the last 6 months (41% vs. 30%).

The t-test of mean differences shows that in two out of the three home investment variables the differences between the intention to treat and control communities is statistically significant. The five education and health variables have statistically different means between intention to treat and control variables.

<TABLE 2 ABOUT HERE>

### 4.2. Controlling for public goods, demographic and pre-treatment characteristics

In Tables 3 and 4 we estimate equation 4 including additional controls. For each dependent variable, $y_{ij}$, we report three rows. In the first row we control for demographic variables and if appropriate for public goods. The second row controls for pre-treatment characteristics. We do not have this information for every household and that is why we miss some observations between the first and second row. In the third row we include all controls.

The pre-treatment controls are those of Table 1: a dummy for female household headship, family size, percentage of minors, school enrollment, employment rate and income. The demographic controls are the age and education (in years) of the household head and a dummy for female household headship. In the education and health regressions we also include in the demographic controls the age and sex of the children. The public good control is in the education regressions the distance (in meters) to the nearest public school and in the health regressions the distance to the nearest public health care institution.

The results of Tables 3 and 4 are consistent. The estimated coefficient have all the expected sign implying more physical investment in the intention to treat communities and more human capital investment in the control communities. Unfortunately some coefficients loose statistical significance.

After including all controls, we have significant results for two physical capital variables (improvements and bathroom improvements). When controlling for pre treatment characteristics the significance of bedroom improvements disappears. The linear probability model of Table 3 implies that the probability that a household in the intention to treat community made an improvement in general is 8-9% larger in the control communities. Given the unconditional probability of having performed a reform this implies an elasticity of about 0.1. The instrumental variable estimation imply a somewhat larger elasticity of about 0.25.

We also have significant results after including all controls for two of the education regressions (school enrollment and extra-classes). Private school is not significant after controlling for demographic characteristics. Children in the intention to treat communities have a 10-11% lower probability of being school enrolled. Given the unconditional probability of being enrolled, this implies an elasticity of enrollment to titling of about 0.12. The instrumental variable estimations imply a somewhat larger elasticity of about 0.3-0.4. The probability of receiving extra lessons for children in the control community is much larger than in the intention to treat with implied elasticity of about 1 according to the OLS regression and even larger according to the instrumental variable estimations.

One of the regressions for health investment (medical checkup) is significant for all set of controls but the other (dentist visit) is only significant after including demographic and public goods controls but no pre treatment controls. According to Table 3 the probability of having made a routine visit to a medical doctor for a children in the intention to treat communities is about 10% lower than for a children in the control communities. This implies an elasticity of about 0.25. The instrumental variable estimations reported in Table 4 imply a larger elasticity of about 0.6.

<TABLE 3 ABOUT HERE>

<TABLE 4 ABOUT HERE>

### 4.3. Controlling for community wide shocks

In this section we implement Donald and Lang (2007) two stage methodology. We included as controls al individual or household level variables as in the previous sections. We estimated the community fixed effect and in Table 5 we report t-test for differences in mean between the intention to treat and control communities. Since we have only 9 observations corresponding to the 9 communities the statistical power of the test is not high so it is not surprising that the significance of many variables is lower in this Table than in previous tables.

We find that even after controlling for community wide shocks the probability that households in the intention to treat communities have performed improvement in their bathroom or in their home in general is statistically significantly larger than for the control communities. Also we found that it is statistically significant that school enrollment and going to extra lessons is smaller in the intention to treat than in the control communities. We fail to find statistically

significant differences in the health variables.

<TABLE 5 ABOUT HERE>

## 5. Conclusions

In this paper we claim that the inter-generational effects of titling programs are ambiguous. The implicit argument put forward by de Soto (2000) is that that the greater income generated by the transfer of formal property rights should at least in part translate into greater investment in children's human capital. Therefore, besides the potential higher inheritance value that the children may receive, they should have more education and better health care. This typical income effect may be upset by a substitution effect since titling alters the family resource allocation in favor of physical assets and against human capital investment. As the literature in the area suggests, greater land or property security is likely to induce more home investment. This is due to income and substitution effect that run in the same direction. On the contrary the income and substitution effect on children human capital investment run in opposite directions. Therefore the net inter-generational effect of titling programs is a matter of empirical research.

Using data from a natural experiment in Uruguay we found that titling is effectively associated to higher probabilities of investing in home improvements. On the other hand, several dimensions of education investment in children could be worsened due to titling (school enrollment and having lessons outside regular school). We also found worse indicators for some health outcomes in children (routine medical checkup and dental attention) but these latter results were not robust to controls for eventual community wide shocks.

Overall our empirical results seem to confirm that the inter-generational effects of titling programs are not necessarily positive. According to our estimates, investment in children's human capital may be negatively affected by a preference for home investment induced by the transfer of property rights. This does not mean that titling programs are necessarily bad or should be discontinued. On the contrary, it suggests that titling programs should monitor collateral effects and eventually take action to prevent undesirable externalities.

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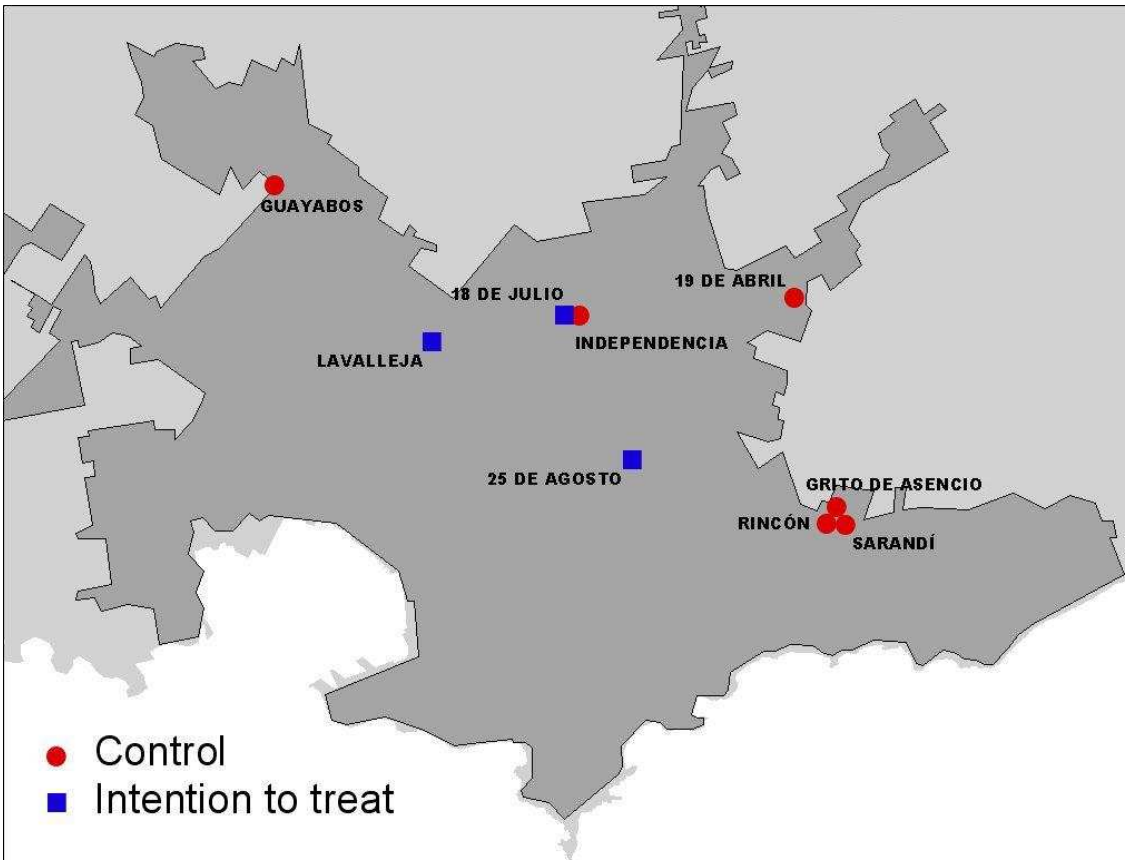
**Appendix**
**Figure: Map of the treatment and control communities**



| Table 1. Pre-treatment characteristics | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Intention to treat | | | Control | | | Mean | t-test |
| | mean | sd | Cases | mean | sd | Cases | difference | |
| % female household heads | 46% | 50% | 134 | 44% | 50% | 494 | 2% | -0.4 |
| Family size | 4.6 | 2.5 | 133 | 5 | 2.7 | 484 | -0.4 | 1.4 |
| % minors | 42% | 28% | 133 | 47% | 27% | 484 | -5% | 1.7 |
| School enrollment | 85% | 29% | 73 | 81% | 32% | 301 | 4% | -0.8 |
| Employment rate | 58% | 31% | 128 | 59% | 31% | 439 | -1% | 0.1 |
| Income | 449 | 306 | 116 | 471 | 284 | 372 | -22 | 0.7 |

* significant at 10%; ** significant at 5%; *** significant at 1%

| Table 2. Summary statistics | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Intention to treat | | | Control | | | Mean difference | t-test | |
| | mean | sd | Cases | mean | sd | Cases | | | |
| *Investment in physical capital: home investment* | | | | | | | | | |
| Improvement | 95% | 21% | 106 | 87% | 34% | 140 | -8% | -2.2 | ** |
| Bedroom improvements | 42% | 50% | 106 | 29% | 46% | 140 | -12% | -2.0 | ** |
| Bathroom improvements | 52% | 50% | 106 | 45% | 50% | 140 | -7% | -1.1 | |
| *Investment in human capital: children´s education* | | | | | | | | | |
| School enrollment | 84% | 36% | 154 | 91% | 29% | 154 | 6% | 1.7 | ** |
| Private school | 3% | 18% | 144 | 8% | 27% | 160 | 5% | 1.7 | ** |
| Extra lessons | 15% | 36% | 112 | 36% | 48% | 128 | 21% | 3.7 | *** |
| *Investment in human capital: children's health* | | | | | | | | | |
| Medical check up | 36% | 48% | 174 | 44% | 50% | 187 | 8% | 1.6 | * |
| Visit dentist last 6 months | 30% | 46% | 174 | 41% | 49% | 187 | 10% | 2.0 | ** |

* significant at 10%; ** significant at 5%; *** significant at 1%

**Table 3. Impact of titling on physical and human capital investment**
OLS regressions. Independent variable: dummy for intention to treat communities

| | coeff. | s.e. | Observations | R-squared | Public good controls | Demographic controls | Pre-treatment controls |
|---|---|---|---|---|---|---|---|
| *Investment in physical capital: home investment* | | | | | | | |
| Improvement | 0.084 | (0.037)** | 240 | 0.02 | Yes | Yes | No |
| | 0.082 | (0.048)* | 163 | 0.06 | No | No | Yes |
| | 0.091 | (0.049)* | 161 | 0.12 | Yes | Yes | Yes |
| Bedroom improvements | 0.127 | (0.064)** | 240 | 0.01 | Yes | Yes | No |
| | 0.103 | (0.080) | 163 | 0.03 | No | No | Yes |
| | 0.109 | (0.081) | 161 | 0.03 | Yes | Yes | Yes |
| Bathroom improvements | 0.043 | (0.065) | 240 | 0.03 | Yes | Yes | No |
| | 0.195 | (0.083)** | 163 | 0.07 | No | No | Yes |
| | 0.199 | (0.081)** | 161 | 0.10 | Yes | Yes | Yes |
| *Investment in human capital: children´s education* | | | | | | | |
| School enrollment | -0.104 | (0.044)** | 306 | 0.10 | Yes | Yes | No |
| | -0.098 | (0.050)* | 208 | 0.07 | No | No | Yes |
| | -0.110 | (0.052)** | 206 | 0.15 | Yes | Yes | Yes |
| Private school | -0.011 | (0.030) | 302 | 0.09 | Yes | Yes | No |
| | -0.083 | (0.043)* | 206 | 0.04 | No | No | Yes |
| | -0.045 | (0.038) | 204 | 0.13 | Yes | Yes | Yes |
| Extra lessons | -0.346 | (0.072)*** | 239 | 0.11 | Yes | Yes | No |
| | -0.299 | (0.073)*** | 168 | 0.11 | No | No | Yes |
| | -0.430 | (0.087)*** | 167 | 0.19 | Yes | Yes | Yes |
| *Investment in human capital: children's health* | | | | | | | |
| Medical check up | -0.096 | (0.052)* | 359 | 0.21 | Yes | Yes | No |
| | -0.110 | (0.066)* | 250 | 0.06 | No | No | Yes |
| | -0.109 | (0.067)* | 248 | 0.30 | Yes | Yes | Yes |
| Visit dentist last 6 months | -0.103 | (0.055)* | 359 | 0.07 | Yes | Yes | No |
| | -0.060 | (0.067) | 250 | 0.02 | No | No | Yes |
| | -0.084 | (0.074) | 248 | 0.12 | Yes | Yes | Yes |

* significant at 10%; ** significant at 5%; *** significant at 1%

**Table 4. Impact of titling on physical and human capital investment**

IV regressions. Take up of property rights instrumented with dummy for intention to treat communities

| | coeff. | s.e. | Observations | R-squared | Public good controls | Demographic controls | Pre-treatment controls |
|---|---|---|---|---|---|---|---|
| *Investment in physical capital: home investment* | | | | | | | |
| Improvement | 0.197 | (0.090)** | 240 | | Yes | Yes | No |
| | 0.217 | (0.131)* | 163 | 0.04 | No | No | Yes |
| | 0.241 | (0.135)* | 161 | 0.09 | Yes | Yes | Yes |
| Bedroom improvements | 0.299 | (0.149)** | 240 | 0.01 | Yes | Yes | No |
| | 0.273 | (0.209) | 163 | 0.03 | No | No | Yes |
| | 0.288 | (0.213) | 161 | 0.04 | Yes | Yes | Yes |
| Bathroom improvements | 0.100 | (0.154) | 240 | 0.03 | Yes | Yes | No |
| | 0.517 | (0.223)** | 163 | 0.06 | No | No | Yes |
| | 0.527 | (0.220)** | 161 | 0.08 | Yes | Yes | Yes |
| *Investment in human capital: children´s education* | | | | | | | |
| School enrollment | -0.276 | (0.123)** | 306 | 0.02 | Yes | Yes | No |
| | -0.252 | (0.138)* | 208 | 0.01 | No | No | Yes |
| | -0.324 | (0.166)* | 206 | 0.07 | Yes | Yes | Yes |
| Private school | -0.027 | (0.075) | 302 | 0.09 | Yes | Yes | No |
| | -0.207 | (0.109)* | 206 | 0.02 | No | No | Yes |
| | -0.126 | (0.109) | 204 | 0.13 | Yes | Yes | Yes |
| Extra lessons | -0.910 | (0.213)*** | 239 | | Yes | Yes | No |
| | -0.754 | (0.211)*** | 168 | 0.03 | No | No | Yes |
| | -1.250 | (0.290)*** | 167 | 0.02 | Yes | Yes | Yes |
| *Investment in human capital: children's health* | | | | | | | |
| Medical check up | -0.215 | (0.118)* | 359 | 0.19 | Yes | Yes | No |
| | -0.305 | (0.187)* | 250 | 0.05 | No | No | Yes |
| | -0.281 | (0.176)* | 248 | 0.28 | Yes | Yes | Yes |
| Visit dentist last 6 months | -0.231 | (0.126)* | 359 | 0.05 | Yes | Yes | No |
| | -0.167 | (0.188) | 250 | 0.01 | No | No | Yes |
| | -0.216 | (0.192) | 248 | 0.11 | Yes | Yes | Yes |

* significant at 10%; ** significant at 5%; *** significant at 1%

**Table 5. Impact of titling on physical and human capital investment controlling for community wide shocks**

Donald and Lang (2007) Two stage methodology

| | Intention to treat | | | Control | | | Mean | t-test | |
|---|---|---|---|---|---|---|---|---|---|
| | mean | sd | Cases | mean | sd | Cases | difference | | |
| *Investment in physical capital: home investment* | | | | | | | | | |
| Improvement | 66% | 5% | 3 | 58% | 8% | 6 | -8% | -1.6 | * |
| Bedroom improvements | 23% | 11% | 3 | 13% | 24% | 6 | -10% | -0.6 | |
| Bathroom improvements | 2% | 23% | 3 | -15% | 7% | 6 | -17% | -1.8 | * |
| *Investment in human capital: children´s education* | | | | | | | | | |
| School enrollment | 106% | 10% | 3 | 120% | 8% | 6 | 14% | 2.2 | ** |
| Private school | -5% | 7% | 3 | -3% | 5% | 6 | 3% | 0.6 | |
| Extra lessons | -6% | 5% | 3 | 22% | 32% | 6 | 27% | 1.4 | * |
| *Investment in human capital: children's health* | | | | | | | | | |
| Medical check up | 70% | 24% | 3 | 77% | 22% | 6 | 6% | 0.4 | |
| Visit dentist last 6 months | -28% | 9% | 3 | -16% | 25% | 6 | 12% | 0.8 | |

* significant at 10%; ** significant at 5%; *** significant at 1%