Inter-American Development Bank
Banco Interamericano de Desarrollo
Latin American Research Network
Red de Centros de Investigación
Research Network Working Paper #R-491

Effects of Land Titling on Child Health

by

Sebastián Galiani*
Ernesto Schargrodsky**

*Universidad de San Andrés
**Universidad Torcuato Di Tella

July 2004

Cataloging-in-Publication data provided by the
Inter-American Development Bank
Felipe Herrera Library

Galiani, Sebastián.
Effects of land titling on child health / by Sebastián Galiani, Ernesto Schargrodsky.

p. cm. (Research Network Working papers ; R-491)
Includes bibliographical references.

1. Land titles--Argentina. 2. Children—Health and hygiene--Argentina. 3. Children—Effects of Land titles on. I.. Schargrodsky, Ernesto. II. Inter-American Development Bank. Research Dept. III. Latin American Research Network. IV. Title. V. Series.

346.0438 G665--dc21

Inter-American Development Bank
1300 New York Avenue, N.W.
Washington, DC 20577

The Research Department (RES) produces a quarterly newsletter, *IDEA (Ideas for Development in the Americas)*, as well as working papers and books on diverse economic issues. To obtain a complete list of RES publications and read or download them, please visit our web site at: http://www.iadb.org/res

Abstract

This paper analyzes the impact of land titling on child health. The empirical evaluation of the effect of property rights typically suffers from selection problems. The paper addresses the selection issue by exploiting a natural experiment in the allocation of land titles. Twenty years ago, a group of squatters occupied a piece of privately owned land in a suburban area of Buenos Aires, Argentina. When the provincial Congress passed an expropriation law transferring the land from the former owners to the squatters, some of the former owners surrendered the land (and received compensation), while others decided to sue in the slow Argentine courts. These different decisions by the former owners generated an allocation of property rights that is exogenous to the characteristics of the squatters. This paper takes advantage of this natural experiment to evaluate the effect of the allocation of urban land property rights on child health. The results show that children in the titled parcels enjoy better nutrition and lower teenage pregnancy rates than those in the untitled parcels.

1. Introduction

The lack of well-defined land property rights imposes considerable costs on poor families. First, individuals may underinvest if the fruits of their investments could be seized by others (Besley, 1995). Thus, inadequate property rights may affect incentives to invest in housing quality. Second, the non-entitled may not be able to gain from trade (Besley, 1995). The lack of property rights restricts the possibility of exchanging houses when their size is inadequate for family needs, potentially leading to overcrowding. Third, in the absence of formal property rights the poor may need to spend extra time and resources to protect their properties. For example, Field (2002) shows that the need to have an adult permanently at home to protect the house from being occupied by new squatters reduces adult labor supply and increases child labor supply; such self-protection of houses may constitute a large burden for poor families.[1] Fourth, houses without proper titles cannot be used as collateral, therefore preventing access to credit markets (De Soto, 2000). Thus, the fragility of property rights may impede the use of the small amounts of capital that the poor have, reducing their consumption and entrepreneurial opportunities.

De Soto (2000) argues that the allocation of formal land property rights to the poor is crucial for development. After his influential work, many governments in Latin America have launched land-titling programs as part of their poverty alleviation policies. The Peruvian government, for example, issued property titles to 1.2 million urban households during the 1990s. On a smaller scale, Ecuador and Paraguay have also developed titling programs. During his first week in office, Brazilian President Lula da Silva announced a massive plan to award property titles to millions of people living in the *favelas* of the major cities of Brazil.[2]

The effects of land titling have been documented by various studies: Jimenez (1984) for the Philippines; Besley (1995) for Ghana; Alston, Libecap, and Schneider (1996) for Brazil; Carter and Olinto (2000) for Paraguay; Miceli, Sirmans, and Kieyah (2001) for Kenya; Lanjouw and Levy (2002) for Ecuador; Do and Iyer (2002) for Vietnam; and Field (2002) for

[1] The inability to invest may also imply underprotection of untitled families against crime. Di Tella, Galiani, and Schargrodsky (2003) show how the inability of the poor to invest in housing security devices translates into large crime victimization rates suffered by the poor.

[2] See *El Pais*, Madrid, Spain, January 7, 2003, www.elpais.es.

Peru, inter alia. However, none of these studies has analyzed the impact of titling on health. This project exploits a natural experiment in order to evaluate the effects on child health of the allocation of land titles to very poor families in a suburban area of Buenos Aires, Argentina.

It is hypothesized that land titling may translate into positive effects on health outcomes through a variety of mechanisms. First, the allocation of property rights can have direct effects on the housing environment. In particular, under the security provided by proper land titles, households may invest in improvements in water distribution within their homes,[3] treatment of fecal evacuation,[4] treatment of garbage disposal, safety and quality of heating systems, air ventilation conditions, and soil quality.[5] Moreover, the possibility of exchanging houses when their size is inadequate for family needs (a possibility provided by proper land titles) may help to prevent overcrowding.

All of these housing factors have been shown to induce significant health effects. Numerous studies demonstrate that access to clean water is critical to containing the incidence of infectious and parasitic diseases (Merrick, 1985; Behrman and Wolfe, 1987; the Cebu Team, 1991; Lavy et al., 1996; Lee et al., 1997; and Jalan and Ravallion, 2003, inter alia). Galiani, Gertler, and Schargrodsky (2003) have recently shown the large impact on child mortality of the water network expansions associated with the privatization of water companies in Argentina. The provision of sanitation also has significant health effects (Esrey et al., 1991; Campos et al., 1995).

Inadequate garbage disposal is associated with high morbidity and mortality rates due to diarrhea (Campos et al., 1995), with helminthes infection (Curtale et al., 1999), and with air pollution, water pollution, and proliferation of vectors and reservoirs (Daniel et al., 1989). Garbage burning increases the concentrations of pollutants in soil and air (Marth, 1995). Improper heating systems, for which the majority of households in developing countries rely on solid fuels, affect health status through increased levels of carbon dioxide and burn injuries (Ferng and Lee, 2002; Smith, 2002). The presence of particles in the air generated by domestic combustion processes is associated with the prevalence of respiratory diseases and allergies

[3] The areas considered by this study are supplied by the public water network. Households, however, are privately responsible for investing in the water connection from the front of their parcels to and within their houses.

[4] In the areas considered by this study there is no public sewage network. Thus, households are individually responsible for investment in private disposal facilities such as septic tanks or cesspools.

[5] This does not refer only to the quality of the soil outside the house. In the poor neighborhoods under study, many houses have rooms with earthen floors.

(EPA, 1997; Ronco et al., 1998; Herbarth et al., 1999). Upper respiratory tract infections are associated with overcrowding, poor housing structure, less humidity inside the house than outside, cooking inside the rooms, and airborne pollutants (D'Souza, 1997; Cappelletty, 1998). Higher injury rates are also associated with poor housing characteristics such as overcrowding (Reading et al., 1999; O'Campo et al., 2000; Delgado et al., 2002).

Second, the possession of property rights may improve child health through enhancements in labor opportunities and wealth. The lack of property rights not only implies the risk of eviction by the government or by the legal owners, but also the potential danger that new squatters may seize inhabitants' houses. To protect their houses, untitled households may need to reduce adult labor supply, thus reducing labor income and potentially increasing the child labor supply (Field, 2002). Furthermore, the possession of property rights may increase wealth and employment opportunities by allowing home-owners to use their houses as collateral to access credit markets (De Soto, 2000). Possessing property rights may also allow homeowners to access employment in the formal economy. These potential differences in employment opportunities may translate into differences in labor income and wealth, in access to health care systems, and in children's education and health status.

However, it is also possible to argue that the provision of appropriate property rights increases the expected return to physical versus human capital investment (through lessening the risk of losing the capital in which the physical capital investment is made). Under the protection of housing investments that appropriate property rights provide, households may prefer to allocate their scarce resources to investments in physical capital rather than to investments in human capital, sacrificing the health and education of their children to improve housing conditions. Thus, the sign of the potential effects of land titling on child welfare needs to be empirically analyzed.

The rest of the paper is organized as follows. The next section describes the natural experiment. Section 3 describes the data, and Section 4 presents survey statistics. Section 5 discusses the empirical strategy, and Section 6 presents the results on anthropometrics and on teenage pregnancy. Section 7 presents the conclusions.

2. A Natural Experiment

The empirical evaluation of the effects of land titling programs typically poses a major methodological challenge. In most historical experiences, the allocation of property rights across families is not random but based on wealth, family characteristics, political clientelism, or other selective mechanisms. Thus, in previous studies of land titling experiences, the characteristics that determine the likelihood of receiving land titles are likely to be correlated with the outcomes under study. This correlation creates a selection bias that impedes the proper evaluation of the effects of property right acquisition. The distinctive feature of this study is the use of a natural experiment in the allocation of property rights to address the selection problem.

Between 1981 and 1982, about 2,000 families occupied more than two square kilometers of vacant land in the locality of San Francisco Solano, County of Quilmes, Province of Buenos Aires, Argentina. The occupants were groups of landless citizens organized through the Catholic Church, who explicitly wanted to avoid creating a shanty-town and therefore partitioned the occupied land into small urban-shaped parcels. At the beginning of the occupation, the squatters thought that the land belonged to the State, but they later found out that it was private property.[6]

The squatters resisted several attempted evictions during the military government. After Argentina's return to democracy, the Congress of the Province of Buenos Aires passed Law Nº 10.239 in October of 1984, expropriating these lands from the former owners to allocate it to the new occupants. Under the terms of the expropriation, the former owners would receive monetary compensation from the government and, then, the government would allocate those lands to the squatters. The government offered to each former owner compensation equivalent to the fiscal valuation of the piece of land, indexed by inflation. This fiscal valuation, which is utilized to calculate property taxes, had been set before the land occupation.

The expropriation process turned out to be asynchronous and incomplete, as the occupied area was composed of thirteen large pieces of land belonging to different owners. Each former owner or group of owners (as some pieces of land had more than one owner) had to decide whether to surrender the land (accepting the expropriation compensation) or to start a legal dispute. In 1986, eight former owners accepted the compensation offered by the

[6] For the details of the land occupation process, see the documentary film *Por una Tierra Nuestra* by Marcelo Céspedes (1984), and also Castells, Cuenya et al. (1985), Izaguirre and Aristizabal (1988), and Fara (1989).

government. Their lands were then gradually transferred to the occupants together with formal land titles that secured the ownership of the parcels. However, five former groups of owners did not accept the compensation offered by the government and decided to dispute the expropriation in the very slow Argentine courts. One of these former owners finally surrendered the land, which was recently allocated to the occupants, while the other four lawsuits are still pending.[7]

Importantly, the people who occupied parcels of land belonging to the former owners that accepted the expropriation compensation were similar on average, and arrived at the same time as the people who settled on the parcels of the former owners who did not surrender the land. There was simply no way for the occupants to know ex-ante, at the time of the occupation, which parcels of land had owners who would accept the compensation and which parcels had owners who would dispute the expropriation. In fact, at the time of the occupation the squatters thought that all the land was state-owned. Furthermore, they had no way of knowing that an expropriation law was going to be passed, or how owners of the specific parcels they occupied would respond.

Although the allocation of land titles depended on a decision by each former owner rather than any particular characteristic of the squatters, one group of families now has formal property rights, while another is still living on the parcels without having titles. Thus, by comparing the groups that received and did not receive land titles, it is possible to simulate a one-stage randomized experiment. Because randomization resolves the selection problem, this natural experiment makes it possible to identify the effects of land titling using cross-sectional information.

Galiani and Schargrodsky (2004) show, using observable variables, that the hypothesis that this natural experiment assigned land titles randomly among the squatters is not statistically rejected. That paper first compares average parcel characteristics for the group that was offered property rights and the group that was not. The three available variables are parcel surface (in squared meters), distance to a nearby creek (in blocks), and a dummy for whether the parcel is on a corner of a block. The null hypothesis of absence of differences in these parcel characteristics is not rejected at conventional levels of statistical significance. Second,

---

Additional details on the description of this natural experiment are available in Galiani and Schargrodsky (2004).

[7] In two of these cases, the expropriation lawsuit was further delayed by the death of one of the former owners, which required an inheritance process.

the paper tests the null hypotheses of absence of differences between these two groups for a large set of pre-treatment household characteristics. The tested variables are age of the household head, gender of the household head, nationality of the household head, years of education of the household head, nationality of the father of the household head, years of education of the father of the household head, nationality of the mother of the household head, and years of education of the mother of the household head. The null hypotheses of equal means are not rejected for any of these variables at conventional levels of statistical significance.

3. Description and Collection of the Data

In the geographical area under consideration for this study, there are a total of 1,839 parcels. The occupation occurred in 1981-82 and the legal expropriation process started in 1984 with the passing of Law Nº 10.239 by the Congress of the Province of Buenos Aires. The evolution of the expropriation process was followed by examination of the records of the Land Undersecretary of the Province of Buenos Aires, the Quilmes County Government, the courts, the land registry, and the tax authority. It was consequently possible to obtain detailed knowledge of the current legal status of each parcel.

Land titles were awarded in two phases. Property titles were awarded to the occupants of 1,105 parcels in 1989-91, and to the occupants of 199 parcels in 1997-98. Property rights have not been offered to the families living in 427 parcels that were occupied under the same conditions and during the very same days of 1981-82. Finally, land titles were available for 108 other parcels, but the occupants did not receive them because they had moved or died by the time of the title offers, or had not fulfilled some of the required registration steps. For these potentially endogenous reasons, the inhabitants of these 108 parcels (out of the 1,412 parcels offered for titling) missed the opportunity to receive a title, i.e., missed the opportunity to receive the treatment. Borrowing the terminology from clinical trials, this subgroup constitutes the "non-compliers" in this study, since they were "offered" the treatment (land title) but they did not "receive" it.

In a previous survey performed by the authors on this population between January and March of 2003, the inhabitants of 590 parcels (out of the total of 1,839) were interviewed. These parcels were randomly selected using the following criteria: 200 parcels from the group

that was offered for titling in 1989-91, 200 parcels that were offered for titling in 1997-98, and 190 parcels that were not offered for titling. It was found that 617 households live in the 590 parcels of the final sample (27 parcels host more than one family).

From the sample of 617 households, two exclusion criteria were applied for the purposes of this child health study. First, families were excluded whose first member arrived after 1985. This step was necessary because the first survey discovered that, although the occupation of the land occurred in 1981-82, some families arrived at the parcel they currently occupy after the former owners made the decision of surrender or sue (a set of memory-aid questions was explicitly included in the survey to identify the exact time of arrival of the household to the current parcel).[8] It is thus plausible to argue that the families that arrived after that time could have known the different expropriation statuses (i.e., the different probabilities of receiving the land) associated with each parcel; this would break the exogeneity in title allocation. Second, excluded were all families that had no members younger than 17 years old at the time of the first survey (i.e., only considered are families with members of age 16 or less as of January-March 2003), as this study focuses on child and youth outcomes. After the exclusion criteria were applied, 339 households encompassing 945 children and youth below 17 years of age (as of January-March 2003) satisfy these two criteria, and constitute the sample for this health study.

During the months of July and August of 2003, the authors again visited these 339 households, performing a specific survey including questions on morbidity, family mortality, access to health care facilities, availability of medicines, parent characteristics and nutrition.[9] A total of 290 households, encompassing 808 children, agreed to answer the health questionnaire. The remaining 49 households did not respond because they had moved (26 cases), could not be found at home after three attempts (3 cases), declined to answer (7 cases), or for unknown reasons (13 cases). This resulted in a response rate of 85.5 percent of the households (85.5 percent of the people).[10] It is worth noting that non-response is uncorrelated with titling status.

[8] See Galiani and Schargrodsky (2004) for a detailed discussion of this issue.

[9] Both the January-March and the July-August surveys were carried out by Gestión Urbana, an NGO that works in this area and has a relationship of trust with the inhabitants.

[10] In the official Argentine household survey (EPH, Encuesta Permanente de Hogares), which collects information using a rotating panel sampling scheme every six months (approximately the same time that elapsed between our two surveys), the attrition rate (i.e., the percentage of households that answered one wave but are missing in the next one, although it was not yet the time of its replacement) for the Buenos Aires metropolitan area is about 25 percent for the whole population, and 14 percent for the first two deciles. This is similar to the 14.5 percent rate of

A Probit model is estimated to predict response as a function of the Property Right Offer dummy variable, and the null-hypothesis of absence of effect (z-value = 0.1) is not rejected.[11]

Tables 1 and 2 summarize the status and time of titling for the 290 surveyed households and the 808 surveyed children, respectively. The intention-to-treat variable Property Right Offer equals 1 for the families occupying parcels that were offered for titling, and 0 otherwise; the treatment variable Property Right equals 1 for the families occupying titled parcels, and 0 otherwise.

At the end of the health survey, families were invited to bring their children to a nearby facility for height and weight measurement by a physician.[12] The anthropometric analysis was restricted to children under 12 years of age (i.e., born after 1991) for two reasons. First, children older than eleven years old were born before the end of the first wave of land titles (the first titles were awarded in 1989-91 and the anthropometric measurement was performed in 2003). Second, in the anthropometric tables internationally available, the calculation of the Weight-for-Height Z-scores is only possible for men younger than 12 years old and for women younger than 10 years old,[13] and it is preferable to have comparable results across different Z-scores.

Out of the 485 surveyed children below 12 years of age, a total of 445 children were measured. Thus, the anthropometric measures of 91.8 percent of the children who answered the survey were collected. The reasons for not attending the anthropometric measurement (after having been surveyed) were not being at home (4 cases) and being ill (1 case), while no reason was provided in 35 cases. Table 3 details the total number of surveyed and measured children by age and gender.[14] Again, a Probit model is estimated model to predict whether the child was

---

attrition found in this study.

[11] The standard errors are clustered at the household level.

[12] The team of health professionals was composed of three medical doctors specialized in Nutrition and Pediatrics and one nutritionist, working with two scales (pediatric and standing) and a wooden pediameter. The health professionals and the interviewers were not informed of the hypotheses of our study and were blind to the treatment status of each household. The authors provided a food stamp of $5 (approximately 1.7 US dollars) for each answered survey and for each measured person as a gratuity to the families willing to participate in the study.

[13] More precisely, Weight-for-Height Z-scores are available for male children through 138 months of age and less than 145 cm. of height, and for female children through 120 months of age and less than 137 cm. of height.

[14] It should be noted that the small number of children in the 0 year-old group is generated by the sampling procedure, as the July-August 2003 survey included only children who had been born by the time of the January-March 2003 survey. Thus, the sample only includes children from about 6 months of age.

measured as a function of the Property Right Offer dummy variable, and the null-hypothesis of absence of effect (z-value = 0.24) is not rejected.[15]

The anthropometric measures (Weight-for-Height and Height-for-Age Z-scores) of the children constitute the main health indicators for this study. Weight-for-Height is considered to provide a short-run measure of health status, whereas Height-for-Age reveals the accumulation of past outcomes (Falkner and Tanner, 1986). For the calculation of the Z-scores, the weight, height, date of birth, date of measurement and sex of each child were used. In addition, four different software programs were considered: the EPI-INFO and the ANTHRO programs provided by the US Centers for Disease Control (www.cdc.gov), and the NUTRI (international tables) and the NUTRI (Argentine tables) programs provided by the Argentine Pediatric Society (www.sap.org.ar). The correlation among the Z-scores obtained using those different programs is extremely high, and always above 0.97. Given this high correlation, the study focuses on the Z-scores calculated using the EPI-INFO software.[16] Table 4 presents the means and standard deviations of the Weight-for-Height and Height-for-Age Z-scores of the population in the study by age and sex.

4. Descriptive Statistics

Table 5 indicates the relationship of each child to the household head. In 82.2 percent of the cases, the child is a son or daughter of the household head, while in 14.4 percent of the cases, she/he is a grandson or granddaughter. Tables 6 through 8 show characteristics of the mother of each child. In agreement with Table 5, Table 6 shows that in 81.8 percent of the cases, the mother is currently the household head or the household head's partner, while in 13.6 percent of cases the mother is the daughter or daughter-in-law of the household head. Most mothers have only achieved a complete or incomplete primary school level (Table 7), and a significant percentage of mothers (about one quarter) are single (Table 8). In the case of the fathers, Table 9 shows that in 76.2 percent of the cases, the father is the household head or partner of the household head, while in 12.5 percent of cases the father of the child is the son or son-in-law of

[15] The standard errors are clustered at the household level.
[16] Excluded from the calculation of Z-scores were three measured children with conditions affecting their weight or height. These three included one child with brain paralysis, and two children who had undergone a nephrectomy (kidney removal).

the household head. Again, most fathers have only achieved a complete or incomplete primary school level (Table 10).

Regarding access to health facilities, the survey shows that only 19 percent of the household heads are affiliated with the social security system (*obras sociales*), while only 4.5 percent have private health insurance. The rest of the households have the public health system as their only option. According to the survey, 92.3 percent of children had a health control during the first year of life, and more than 99 percent have received the DPT, Sabin, and BCG vaccines. According to the interviews, 10.9 percent of the children suffer a chronic disease, while 2.2 percent have some form of disability. The average weight at birth was 3.200 kg. The mortality rate of the offspring of the household head is 21.5 per 1,000 live births.[17] Table 11 shows a high pregnancy rate for girls of age 14 to 17: 10.4 percent of the girls of that age group are or have been pregnant.[18]

5. Estimation Strategy

The goal of this paper is to evaluate the effect of the allocation of property rights on child health, and it is hypothesized that land titling may translate into positive effects on child health through improvements in housing conditions and enhancements in labor opportunities and wealth. However, land titling could also be harmful if it leads families to prioritize investments in physical capital over human capital. In order to analyze these effects, let H_{1i} denote the outcome of interest of child *i* if she/he lives in a titled parcel, and let H_{0i} denote the outcome of interest otherwise. Let D_i be a binary indicator of land titling. The average effect of land titling on outcome H for child *i* is

$$E[H_{1i}| D_i = 1] - E[H_{0i}| D_i = 1] \qquad (1)$$

Note that the first term of (1) is observed, but the second term is an unobserved counterfactual. Simple comparisons of outcomes by D_i generally fail to identify causal effects unless land titling is determined independently of the child's potential outcomes, as is the case in the natural experiment exploited in this paper. Exogeneity in the allocation of treatment makes it possible to use $E[H_{0i}| D_i = 0]$, the outcome of interest in the control units *i*, as the

[17] This figure does not correspond only to child mortality after the land occupation; it also includes deaths of offspring of any age and deaths that occurred before the occupation.

counterfactual realization of what would have happened to the treated group without treatment, that is, to estimate $E[H_{0i}| D_i = 1]$. Thus, the simple comparison of sample means consistently estimates the average causal effect of land titling on outcome H.[19]

In addition to differences in sample means, the effect of land titling on health outcomes is estimated by the following regression model:

$$H_{ih} = \alpha_0 + \alpha_1 X_i + \alpha_2 X_h + \beta \text{ Property Right}_h + \eta_i , \qquad (2)$$

where H_{ih} measures health outcomes of child *i* in household *h*, X_i is a vector of child characteristics (age, gender, relationship to household head, etc.), X_h is a vector of household characteristics (mother education, father education, household income, etc.), and η_i is the error term.[20] The parameter of interest is β, which captures the effect of *Property Right*$_h$, a dummy variable indicating the possession of land title by household *h*.

A potential concern with regression (2) is that a number of families that were offered the possibility of obtaining land titles did not receive them for reasons that could originate in unobservable factors that may also affect the variables under study. This non-compliance nuisance is addressed by instrumenting *Property Right*$_h$ with the fully exogenous intention-to-treat variable *Property Right Offer*$_h$, a dummy variable indicating the availability of a land title offer for the parcel occupied by household *h*. Thus, instead of estimating equation (2) by Ordinary Least Squares, estimates are reported of the effect of land titling on the health outcomes under study by Two-Stages Least Squares (see Angrist et al., 1996).

One of the outcomes under study (teenage pregnancy) is a Binary Dependent Variable. However, as noted in Angrist (2001), the problem of causal inference with Limited Dependent Variables is not fundamentally different from causal inference with continuous outcomes. If there are no covariates or the covariates are sparse and discrete, linear models and associated estimation techniques like 2SLS are no less appropriate for LDVs than for other types of dependent variables. Certainly, this is the case in a natural experiment where controls are only

[18] We did not ask this question for girls younger than 14 years of age.
[19] More precisely, the parameter identified is the average treatment effect on the treated.
[20] Community characteristics are not included as controls in this regression because all the households in the sample live in the same neighborhood and their community characteristics are exactly similar.

included to improve the efficiency of the estimates, although their omission would not bias the estimate of the parameter of interest.

6. Results

The analysis begins with the potential impact of land titling on the Weight-for-Height Z-scores. As explained, this variable, which provides a measure of short-run health status, can only be calculated for males younger than 12 years old and for girls younger than 10 years old. The first row of Table 12 tests the absence of differences in the means of this variable between children living in parcels not offered for title (the exogenous variable Property Right Offer = 0) and offered for title (Property Right Offer = 1). The null-hypothesis is rejected at the 5-percent level of significance. Children living in parcels where titles were available show better short-run health status than those in the control group. The difference is also present when the sample is divided into 0-4 and 5-11 age groups, although it is statistically significant at conventional levels only for the latter group.

The same conclusion of better short-run health status for children in the titled parcels is provided by the regression results reported in Table 13. The impact of land titling on Weight-for-Height Z-scores is estimated by addressing the potential concern of non-compliance by instrumenting the Property Right dummy variable with the intention-to-treat Property Right Offer variable. Standard errors are clustered at the household level to address the potential presence of within-household correlation. Without including control variables, the first column of Table 13 shows a positive and significant effect of land titling on Weight-for-Height. The estimated coefficient does not change at all if observations are excluded from the sample that correspond to over-weighted children that have Weight-for-Height Z-scores higher than 2 (5 percent of the sample).[21]

The second column presents an estimate of the intention-to-treat effect controlling non-parametrically for the age of the children. This is estimated by means of a kernel regression using the age of children in months as the matching variable. The purpose is to control for possible effects arising from differences in the age distribution of children in the treatment and control groups. It may be the case that age matters per se, or that different aggregate shocks

[21] The estimated coefficient of interest is 0.235 and its clustered standard error is 0.109.

affected groups differently because their children were at different stages of their physical development. The results show that the coefficient remains unaltered.

Column (3) adds three available parcel attributes as control variables: parcel surface (in square meters), distance to a nearby (polluted and floodable) creek (in blocks), and a dummy for whether the parcel is on a corner of a block. These variables could control for potential differences across households at the time of the occupation, or they may themselves affect investment in the house and hence health outcomes. Since there are no differences in the averages of these variables among treatment and control groups, the estimated effects of land titles do not change at all.

The fourth column incorporates controls for child and household characteristics. The controls are child gender, child age, relationship of the child to the household head, and educational levels of the mother and the father of the child.[22] The results remain mainly unaltered. The results also remained unaffected if income is included as a control.[23]

Finally, the last two columns explore whether the effect is different for children in the 0 to 4 age group than for children in the 5 to 11 age group. The effect appears to be larger for the older group, although the hypothesis of equality of treatment effects cannot be rejected.[24] In summary, the analysis suggests the prevalence of better short-run health status for children raised in titled parcels.

Tables 14 and 15 perform a similar analysis for the Height-for-Age Z-scores. This measure reflects the accumulation of past health outcomes (see Martorell, 1999). Table 14 presents tests of differences of means for the age groups 0 to 11, 0 to 4, and 5 to 11. There are

[22] To control for the relationship of the child to the household head, we use a Son/Daughter dummy and a Grandson/Granddaughter dummy. The baseline is any other type of relationship (see Table 5). To control for the educational level of the mother, we include a set of dummies for no schooling, incomplete primary school, complete primary school, incomplete secondary school, and complete secondary school. The baseline case is not-provided educational level (see Table 7). A similar set of dummies is included for the father's educational level (see Table 10). In this case, we treat complete and incomplete vocational schooling as complete secondary schooling.

[23] The Income variable results non-significant. We prefer not to include it in the tables presented here because income could be potentially endogenous to land title status. All results reported but not presented are available upon request.

[24] Moreover, when we explore whether the effect of titling is different for the children living in the parcels titled in 1989-91 than for the children living in the parcels titled in 1997-98, we cannot reject the equality of both coefficients.

no significant differences in Height-for-Age between children living in titled and untitled parcels.[25]

Table 15 then performs the same set of regressions presented in Table 13. Using 2SLS or matching techniques, including no-controls, controls for parcels characteristics or controls for household characteristics, and considering separately children of different age groups, these regressions fail to find significant differences in Height-for-Age Z-scores between the children in the control and treatment groups.[26] Thus, differences are found in short-run health status in favor of the children raised in titled parcels, but no similar differences exist for the variable that measures the accumulation of past health outcomes.[27]

Thus, it is found that the children in the treatment group enjoy better anthropometric outcomes, but only of the short-run nature. Moreover, the effect seems larger for the older cohort than for the younger cohort. Are there plausible explanations for these results? First, it is worth noting that the average values of both anthropometric variables are very close to zero for this population. Thus, undernourishment does not seem to be a very severe problem in this area, and, in particular, it was probably very unusual before the acute macroeconomic crisis of 2002. If titling programs only make a difference in bad times, a longer period has to elapse before long-run effects can be observed. Second, it may be the case that at the time long-run anthropometric variables were determined for most of these children, the treated households had just received the titles and were prioritizing investments in physical capital over human capital. Now that the houses have already been improved, short-run differences are observed. Third, both treatment and control young cohorts may receive enough nutrients from the alimentary programs administered by different public offices, or from within-household allocations that privilege the alimentation of young relative to older children. This could explain both the absence of long-run titling effects, and the presence of larger short-run health effects for the older than for the younger cohorts. Nevertheless, since short-run effects on

[25] The Height-for-Age results consider all 424 children between 0 and 11 years old. As explained above, the Weight-for-Height Z-scores are only available for 371 children (boys between 0 and 11 years old and less than 145 cm. of height, and girls between 0 and 9 years old and less than 137 cm. of height). None of the results in Tables 14 and 15 change if we restrict to exactly the same sample of 371 children.

[26] The relationship of the child to the household head seems related to Height-to-Age Z-scores in the regressions of Table 15. In particular, children and grandchildren of the household head show better performance than other household members. The effect of titling is similarly non-significant for the children living in the parcels titled in 1989-91 than for the children living in the parcels titled in 1997-98.

[27] No differences are found across the two groups for the measures of self-reported morbidity obtained through the questionnaire, such as hospitalization during the last 12 months or diseases during the last 30 days.

anthropometrics are much more sensitive to exogenous events than long-run effects, the positive results of this study should be taken cautiously.

Finally, Tables 16 and 17 analyze another indicator of child health and welfare. In the sample under study, teenage pregnancy is an important problem. In fact, 11.5 percent of the 14 to 17-year-old girls who answered the survey question on teenage pregnancy indicated that they were or had been pregnant at least once. Table 16 shows that the pregnancy rate is substantially higher in the untitled parcels (20.8 percent) than in the titled parcels (7.9 percent).

The regressions in Table 17 complete this analysis. The difference in teenage pregnancy between treatment and control groups remains large, and it is statistically significant at conventional levels when all the controls are included in the third column of Table 17.[28]

7. Conclusions

A natural experiment in the allocation of land titles across squatters in a poor suburban area of Buenos Aires, Argentina is exploited in order to evaluate the impact of property rights. In our previous work (Galiani and Schargrodsky, 2004), we found large effects of land titling on investment in housing improvements. A potential concern raised by those results is that the provision of appropriate property rights may increase the expected return to investment in physical vis-à-vis human capital, leading poor families to sacrifice the health of their children to improve housing conditions. Instead, our results show that children living in titled parcels enjoy better Weight-for-Height scores and lower teenage pregnancy rates than children living in untitled parcels. Thus, our findings suggest that access to land titles helps families to improve their investments in human capital.

However, the positive results of this study should be taken cautiously. We find that the children in the titled parcels enjoy better anthropometric outcomes, but only of the short-run nature. We do not find significant differences in Height-for-Age, the variable that reflects the accumulation of past health outcomes. Thus, the impact of land titling seems moderate, and certainly not strong enough as a child health policy to recommend its implementation in replacement of more direct health or nutritional interventions.

[28] No differences are found in the reduction in pregnancy rates for girls in early and late titled parcels.

References

Alston, L., G. Libecap and R. Schneider. 1996. "The Determinants and Impact of Property Rights: Land Titles on the Brazilian Frontier." *Journal of Law, Economics and Organization* 12: 25-61.

Angrist, J.D. 2001. "Estimations of Limited Dependent Variable Models with Dummy Endogenous Regressors: Simple Strategies for Empirical Practice." *Journal of Business and Economic Statistics* 19(1): 2-16.

Angrist, J.D., G.W. Imbens and D.B. Rubin. 1996. "Identification of Causal Effects Using Instrumental Variables (with discussion)." *Journal of the American Statistical Association* 91: 444-455.

Behrman, J., and B. Wolfe. 1987. "How Does Mother's Schooling Affect Family Health, Nutrition, Medical Care Usage and Household Sanitation?" *Journal of Econometrics* 36: 185-204.

Besley, T. 1995. "Property Rights and Investments Incentives: Theory and Evidence from Ghana." *Journal of Political Economy* 103: 903-937.

Campos, G. de J., S.A. dos Reis Filho, A.A. da Silva et al. 1995. "Infant Morbimortality Due to Acute Diarrhea in a Metropolitan Area of Northeastern Brazil, 1986-1989." *Revista de Saude Pública* 29: 132-139.

Cappelletty, D. 1998. "Microbiology of Bacterial Respiratory Infections." *Pediatric Infectious Disease Journal* 17 (8 Supplement): S55-61.

Carter, M., and P. Olinto. 2000. "Getting Institutions 'Right' for Whom: Credit Constraints and the Impact of Property Rights on the Quantity and Composition of Investment." Agricultural and Applied Economics Papers, Staff Paper Series 433. Madison, United States: University of Wisconsin-Madison.

J. Castells, B. Cuenya et al. 1985. "Condiciones de Habitat y Salud de los Sectores Populares: Un Estudio Piloto en el Asentamiento San Martín de Quilmes." Buenos Aires, Argentina: Centro de Estudios Urbanos y Regionales (CEUR).

Cebu Study Team. 1991. "Underlying and Proximate Determinants of Child Health: The Cebu Longitudinal Health and Nutrition Survey." *American Journal of Epidemiology* 133: 185-201.

Céspedes, M. 1984. *Por una tierra nuestra*. Buenos Aires, Argentina: Cine Testimonio. Film.

Curtale, F., P. Pezzotti, Y.S. Saad and A. Aloi. 1999. "An Analysis of Individual, Household, and Environmental Risk Factors for Intestinal Helminth Infection among Children in Qena Governorate, Upper Egypt." *Journal of Tropical Pediatrics* 45: 14-17.

Daniel, M., W. Sixl and M. Kock. 1989. "Problems of Housing and Health of People Utilizing the Garbage in Cairo from the Viewpoint of Medical Entomology." *Journal of Hygiene, Epidemiology, Microbiology and Immunology* 33 (4 Suppl): 568-576.

De Soto, H. 2000. *The Mystery of Capital: Why Capitalism Triumphs in the West and Fails Everywhere Else*. New York, United States: Basic Books.

Delgado, J., M.E. Ramírez-Cardich, R.H. Gilman et al. 2002. "Risk Factors for Burns in Children: Crowding, Poverty, and Poor Maternal Education." *Injury Prevention* 8: 38-41.

Di Tella, R., S. Galiani and E. Schargrodsky. 2003. "Crime Inequality when Victims Adapt." Buenos Aires, Argentina: Universidad Torcuato Di Tella. Mimeographed document.

Do, Q.T., and L. Iyer. 2002. "Land Rights and Economic Development: Evidence from Viet Nam." Cambridge, United States: Massachusetts Institute of Technology." Mimeographed document.

D'Souza, R.M. 1997. "Housing and Environmental Factors and their Effects on the Health of Children in the Slums of Karachi, Pakistan." *Journal of Biosocial Science* 29: 271-281.

Environmental Protection Agency (EPA). 1997. "Health and Environmental Effects of Particulate Matter." Washington, DC, United States: Environmental Protection Agency, Office of Air and Radiation, Office of Air Quality Planning and Standards.

Esrey, S., J. Potash, L. Roberts et al. 1991. "Effects of Improved Water Supply and Sanitation on Ascariasis, Diarrhea, Dracunculiasis, Hookworm Infection, Schistosomiasis and Trachoma." *Bulletin of the World Health Organization* 69: 609-621.

Falkner, F., and J. Tanner. 1986. *Human Growth: A Comprehensive Treatise*. Volume 3. Second edition. New York, United States: Plenum Press.

Fara, L. 1989. "Luchas Reivindicativas Urbanas en un Contexto Autoritario: Los Asentamientos de San Francisco Solano." In: E. Jelin, editor. *Los Nuevos Movimientos Sociales*. Buenos Aires, Argentina: Centro Editor de América Latina.

Ferng, S.F., and L.W. Lee. 2002. "Indoor Air Quality Assessment of Daycare Facilities with Carbon Dioxide, Temperature, and Humidity as Indicators." *Journal of Environmental Health* 65: 14-18.

Field, E. 2002. "Entitled to Work: Urban Property Rights and Labor Supply in Peru." Princeton, United States: Princeton University. Mimeographed document.

Galiani, S., P. Gertler and E. Schargrodsky. 2002. "Water for Life: The Impact of the Privatization of Water Services on Child Mortality." CREDPR Working Paper 154. Stanford, United States: Stanford University.

Galiani, S., and E. Schargrodsky. 2004. "Effects of Land Titling." Buenos Aires, Argentina: Universidad Torcuato Di Tella. Mimeographed document.

Herbarth, O., G.J. Fritz, J.C. Behler et al. 1999. "Epidemiologic Risk Analysis of Environmentally Attributed Exposure on Airway Diseases and Allergies in Children." *Central European Journal of Public Health* 7: 72-76.

Izaguirre, I., and Z. Aristizabal. 1988. *Las Tomas de Tierras en la Zona Sur del Gran Buenos Aires: Un Ejercicio de Formación de Poder en el Campo Popular*. Buenos Aires, Argentina: Centro Editor de América Latina.

Jalan, J., and M. Ravaillon. 2003. "Does Piped Water Reduce Diarrhea for Children in Rural India?" *Journal of Econometrics* 112: 153-173.

Jimenez, E. 1984. "Tenure Security and Urban Squatting." *Review of Economics and Statistics* 66(4): 556-67.

Lanjouw, J., and P. Levy. 2002. "Untitled: A Study of Formal and Informal Property Rights in Urban Ecuador." *Economic Journal* 112: 986-1019.

Lavy, V., J. Strauss, D. Thomas et al. 1996. "Quality of Health Care, Survival and Health Outcomes in Ghana." *Journal of Health Economics* 15: 333-57.

Lee, L., M. Rosenzweig and M. Pitt. 1997. "The Effects of Improved Nutrition, Sanitation, and Water Quality on Child Health in High-Mortality Populations." *Journal of Econometrics* 77: 209-235.

Marth, E., W. Six, V. Bencko et al. 1995. "People on the Garbage Dumps of Cairo: A Toxicological *In Vivo* Model?" *Central European Journal of Public Health* 3: 54-57.

Martorell, R. 1999. "The Nature of Child Malnutrition and its Long-Term Implications." *Food and Nutrition Bulletin* 20: 288-292.

Merrick, T. 1985. "The Effect of Piped Water on Early Childhood Mortality in Urban Brazil, 1970 to 1976." *Demography* 22: 1-24.

Miceli, T., C. Sirmans and J. Kieyah. 2001. "The Demand for Land Title Registration: Theory with Evidence from Kenya." *American Law and Economics Review* 3: 275-287.

O'Campo, P., R.P. Rao, A.C. Gielen et al. 2000. "Injury-Producing Events among Children in Low-Income Communities: The Role of Community Characteristics." *Journal of Urban Health* 77: 34-49.

Reading, R., I.H. Langford, R. Haynes et al. 1999. "Accidents to Preschool Children: Comparing Family and Neighbourhood Risk Factors." *Social Science and Medicine* 48: 321-330.

Ronco, A., M. Rehwagen and O. Herbarth. 1998. "Compuestos Orgánicos Volátiles en el Aire del Gran La Plata." *Gerencia Ambiental* 5: 106-111.

Smith, K.R. 2002. "Indoor Air Pollution in Developing Countries: Recommendations for Research." *Indoor Air* 12: 198-207.

Table 1. Allocation of Land Titles for Surveyed Households

Intention to treat (Property Right Offer = 1)				Control (Property Right Offer = 0)	Total
Year	Total	Treated (Property Right = 1)	Non-compliers (Property Right = 0)		
1989-91	104	103	1		
1997-98	101	88	13		
Total	205	191	14	85	290

Table 2. Allocation of Land Titles for Surveyed Children

Intention to treat (Property Right Offer = 1)				Control (Property Right Offer = 0)	Total
Year	Total	Treated (Property Right = 1)	Non-compliers (Property Right = 0)		
1989-91	262	260	2		
1997-98	273	234	39		
Total	535	494	41	273	808

Table 3. Number of Children Surveyed and Measured

Age (in years)	Men			Women			Total		
	Surveyed	Measured	%	Surveyed	Measured	%	Surveyed	Measured	%
0	12	12	100.0	14	12	85.7	26	24	92.3
1	20	19	95.0	19	18	94.7	39	37	94.9
2	18	18	100.0	28	26	92.9	46	44	95.7
3	14	12	85.7	19	18	94.7	33	30	90.9
4	20	17	85.0	28	26	92.9	48	43	89.6
5	13	12	92.3	15	14	93.3	28	26	92.9
6	19	17	89.5	26	23	88.5	45	40	88.9
7	24	21	87.5	23	21	91.3	47	42	89.4
8	15	14	93.3	20	18	90.0	35	32	91.4
9	24	22	91.7	20	18	90.0	44	40	90.9
10	31	25	80.6	26	25	96.2	57	50	87.7
11	16	16	100.0	21	21	100.0	37	37	100.0
Total	226	205	90.7	259	240	92.7	485	445	91.8

Table 4. Z-Scores (Software: EPI-INFO)

Age (in years)	Men			Women		
	Mean	Std Desv	Total obs.	Mean	Std Desv	Total obs.
0	0,55	1,40	12	0,21	0,87	12
1	-0,19	1,16	19	0,61	1,41	18
2	-0,15	0,65	18	-0,23	0,92	25
3	-0,33	1,21	12	0,11	0,57	18
4	-0,09	0,88	17	0,28	0,88	23
5	0,25	1,10	12	0,44	1,08	14
6	0,29	0,84	17	0,56	0,71	23
7	0,37	0,97	21	0,49	1,29	21
8	0,68	1,38	14	0,25	1,02	16
9	0,26	0,64	21	-0,22	1,02	14
10	0,22	0,92	22			
11	-0,53	0,04	2			
TOTAL	0,16	1,02	187	0,25	1,02	184

Notes: 1. Weight-for-Height Z-scores calculated for male children through 138 months of age and less than 145 cm. of height, and female children through 120 months of age and less than 137 cm. of height. 2. Decimal points are represented by commas.

Age (in years)	Men			Women		
	Mean	Std Desv	Total obs.	Mean	Std Desv	Total obs.
0	-0,47	0,80	12	-0,09	1,13	12
1	-0,68	1,19	19	-0,28	0,93	18
2	0,26	1,30	18	-0,42	0,83	25
3	-0,22	1,44	12	-0,19	1,01	18
4	-0,37	0,68	17	-0,18	0,96	24
5	-0,74	1,20	12	-0,71	0,56	14
6	-0,31	1,03	17	-0,21	0,76	23
7	-0,71	1,00	21	-0,29	0,87	21
8	-0,12	0,67	14	-0,38	1,12	18
9	-0,44	0,91	22	-0,14	0,70	18
10	-0,51	1,09	25	-0,64	1,06	25
11	-0,94	1,18	5	-0,40	1,20	14
TOTAL	-0,42	1,06	194	-0,33	0,93	230

Notes: 1. Calculated for children born after 1991. 2. Decimal points are represented by commas.

Table 5. Relationship of Child to Household Head

Relationship	#	%
Son or Daughter	664	82.2%
Grandson or Granddaughter	116	14.4%
Brother or Sister	6	0.7%
Niece or Nephew	16	2.0%
Brother-in-law / Sister-in-law	2	0.2%
Son-in-law / Daughter-in-law	2	0.2%
Other	2	0.2%
TOTAL	808	

Table 6. Relationship of Child's Mother to Household Head

Relationship	#	%
Household head	305	37.7%
Wife or Partner	356	44.1%
Former wife or partner	4	0.5%
Daughter	72	8.9%
Daughter-in-law	38	4.7%
Sister	4	0.5%
Granddaughter	2	0.2%
Sister-in-law	13	1.6%
Mother	4	0.5%
Mother-in-law	1	0.1%
Other relatives	2	0.2%
Other non-relatives	7	0.9%
TOTAL	808	

Table 7. Highest School Attainment by Child's Mother

School Attainment	#	%
No Instruction	19	2.4%
Primary School Incomplete	188	23.3%
Primary School Complete	411	50.9%
Secondary School Incomplete	88	10.9%
Secondary School Complete	55	6.8%
Not provided	47	5.8%
	808	

Table 8. Civil Status of Child's Mother

Civil Status				
	#	%	#	%
Single	234	29.0%	201	24.9%
Married	358	44.3%	339	42.0%
Unmarried partner	198	24.5%	180	22.3%
Divorced or Separated	4	0.5%	34	4.2%
Widow	6	0.7%	25	3.1%
Dead	0	0.0%	19	2.4%
Not provided	8	1.0%	10	1.2%
TOTAL	808		808	

Table 9. Relationship of Child's Father to Household Head

Relationship	#	%
Household head	377	46.7%
Husband or partner	238	29.5%
Former husband or partner	36	4.5%
Son	39	4.8%
Son-in-law	62	7.7%
Brother	4	0.5%
Grandson	0	0.0%
Brother-in-law	12	1.5%
Father	3	0.4%
Father-in-law	3	0.4%
Other relatives	1	0.1%
Other non-relatives	19	2.4%
Father unknown	5	0.6%
Not provided	9	1.1%
TOTAL	808	

Table 10. Highest School Attainment by Child's Father

School Attainment	#	%
No Instruction	17	2.1%
Primary School Incomplete	182	22.5%
Primary School Complete	474	58.7%
Secondary School Incomplete	55	6.8%
Secondary School Complete	36	4.5%
Vocational School Incomplete	2	0.2%
Vocational School Complete	1	0.1%
Not provided	41	5.1%
TOTAL	808	

Table 11. Pregnancy of Female Teenagers (14-17 years old)

Yes, it is currently the first time	3	3.1%
Yes, currently and in the past	1	1.0%
Yes, in the past	6	6.3%
No	77	80.2%
Not answered	9	9.4%
TOTAL	96	

Table 12. Weight-for-Height Z-score

Age Group	Property Right Offer = 0	Property Right Offer = 1	Diff
0-11 years old	0.065 (0.087) [132]	0.279 (0.065) [239]	-0.214** (0.109)
0-4 years old	-0.028 (0.119) [60]	0.110 (0.100) [114]	-0.139 (0.156)
5-11 years old	0.143(0.126) [72]	0.434(0.083) [125]	-0.291**(0.151)

Notes: Standard errors in parentheses. Number of observations in brackets. ** Significant at 5-percent level.

Table 13. Dependent Variable: Weight-for-Height Z-Score

	(1)	(2)	(3)	(4)	(5)	(6)
Property Right	0.231*	0.218*	0.248*	0.301**	0.227	0.454**
	(0.124)	(0.117)	(0.126)	(0.141)	(0.199)	(0.217)
Parcel Surface			-0.000	-0.000	-0.001	0.000
			(0.001)	(0.001)	(0.001)	(0.001)
Distance to Creek			-0.067	-0.059	-0.071	-0.039
			(0.043)	(0.042)	(0.061)	(0.056)
Block Corner			-0.041	-0.062	-0.425	0.095
			(0.168)	(0.164)	(0.259)	(0.238)
Male				-0.092	-0.229	0.070
				(0.105)	(0.153)	(0.149)
Age				0.022	-0.076	-0.069*
				(0.020)	(0.067)	(0.041)
Son / Daughter				0.156	0.056	0.364
				(0.323)	(0.495)	(0.510)
Grandson / Granddaughter				0.159	0.095	0.465
				(0.351)	(0.525)	(0.533)
Mother's Education—				-0.226	-0.093	-0.215
No instruction				(0.292)	(0.465)	(0.465)
Mother's Education—				-0.023	-0.081	0.158
Primary school incomplete				(0.252)	(0.429)	(0.427)
Mother's Education—				-0.161	-0.231	-0.069
Primary school complete				(0.221)	(0.344)	(0.403)
Mother's Education—				-0.395	-0.573	-0.173
Secondary school incomplete				(0.268)	(0.391)	(0.486)
Mother's Education—				0.002	0.015	-0.013
Secondary school complete				(0.318)	(0.429)	(0.529)
Father's Education—				0.614	0.159	1.320**
No instruction				(0.401)	(0.624)	(0.601)
Father's Education—				-0.004	-0.438	0.516
Primary school incomplete				(0.316)	(0.388)	(0.494)
Father's Education—				0.022	-0.559	0.647
Primary school complete				(0.309)	(0.349)	(0.499)
Father's Education—				-0.254	-0.810*	0.278
Secondary school incomplete				(0.337)	(0.457)	(0.525)
Father's Education—				-0.339	-0.964*	0.101
Secondary school complete				(0.358)	(0.490)	(0.504)
Constant	0.065		0.324	0.237	1.554**	-0.329
	(0.085)		(0.216)	(0.464)	(0.680)	(0.733)
Method	2SLS	Matching	2SLS	2SLS	2SLS	2SLS
Observations	371	371	371	371	174	197

Notes: The dependent variable is the Weight-for-Height Z-score calculated through the EPI-INFO program. The sample is restricted to boys between 0 and 11 years of age and girls between 0 and 9 years of age. The 2SLS regressions use Property Right Offer to instrument for Property Right. Standard errors are in parentheses. Kernel estimates use a bandwidth equal to 0.8. The standard errors for the Kernel Weighted Matching Estimate in column (2) are bootstrapped standard errors using 100 replications. The standard errors of the 2SLS estimates are clustered at the household level. * Significant at 10-percent level. ** Significant at 5-percent level.

Table 14. Height-for-Age Z-score

Age Group	Property Right Offer = 0	Property Right Offer = 1	Difference
0-11 years old	-0.314 (0.079) [147]	-0.398 (0.060) [277]	0.084 (0.099)
0-4 years old	-0.229 (0.118) [60]	-0.290 (0.101) [115]	0.061 (0.156)
5-11 years old	-0.372 (0.106) [87]	-0.475 (0.073) [162]	0.102 (0.129)

Notes: Standard errors in parentheses. Number of observations in brackets.

Table 15. Dependent Variable: Height-for-Age Z-score

	(1)	(2)	(3)	(4)	(5)	(6)
Property Right	-0.091	-0.086	-0.077	-0.077	0.074	-0.048
	(0.133)	(0.106)	(0.136)	(0.146)	(0.209)	(0.172)
Parcel Surface			-0.001	-0.001	-0.002***	0.000
			(0.001)	(0.001)	(0.001)	(0.001)
Distance to Creek			0.003	-0.030	-0.103*	0.031
			(0.049)	(0.047)	(0.058)	(0.061)
Block Corner			0.026	-0.043	-0.662***	0.212
			(0.179)	(0.175)	(0.254)	(0.211)
Male				-0.088	-0.142	-0.083
				(0.093)	(0.156)	(0.119)
Age				-0.004	0.028	0.011
				(0.016)	(0.060)	(0.032)
Son / Daughter				0.483*	0.463	0.517**
				(0.245)	(0.354)	(0.255)
Grandson / Granddaughter				0.470*	0.515	0.438
				(0.275)	(0.369)	(0.370)
Mother's Education—				-0.302	1.386***	-0.418
No instruction				(0.391)	(0.437)	(0.596)
Mother's Education—				-0.108	0.262	-0.028
Primary school incomplete				(0.320)	(0.367)	(0.450)
Mother's Education—				0.015	0.267	0.074
Primary school complete				(0.280)	(0.292)	(0.402)
Mother's Education—				0.495	0.452	0.892*
Secondary school incomplete				(0.319)	(0.356)	(0.476)
Mother's Education—				0.256	0.302	0.947*
Secondary school complete				(0.371)	(0.381)	(0.536)

Table 15., continued

	(1)	(2)	(3)	(4)	(5)	(6)
Father's Education—				0.559	1.514***	0.382
No instruction				(0.358)	(0.375)	(0.499)
Father's Education—				0.012	0.072	-0.106
Primary school incomplete				(0.287)	(0.362)	(0.440)
Father's Education —				0.301	0.182	0.199
Primary school complete				(0.237)	(0.258)	(0.421)
Father's Education—				-0.143	-0.339	-0.248
Secondary school incomplete				(0.370)	(0.549)	(0.504)
Father's Education —				0.344	0.377	0.004
				(0.304)	(0.376)	(0.451)
Constant	-0.314***		-0.171	-0.693*	-0.210	-1.335**
	(0.096)		(0.202)	(0.409)	(0.592)	(0.555)
Method	2SLS	Matching	2SLS	2SLS	2SLS	2SLS
Observations	424	424	424	424	175	249

Notes: The dependent variable is the Height-for-Age Z-score calculated through the EPI-INFO program. The sample is restricted to children between 0 and 11 years of age. The 2SLS regressions use Property Right Offer to instrument for Property Right. Standard errors are in parentheses. Kernel estimates use a bandwidth equal to 0.8. The standard errors for the Kernel Weighted Matching Estimate in column (2) are bootstrapped standard errors using 100 replications. The standard errors of the 2SLS estimates are clustered at the household level. * Significant at 10-percent level. ** Significant at 5-percent level. *** Significant at 1-percent level.

Table 16. Teenage Pregnancy

Group	Property Right Offer = 0	Property Right Offer = 1	Difference
Girls 14-17 years old	0.208 (0.084) [24]	0.079 (0.034) [63]	0.128 (0.091)

Notes: Standard errors in parentheses. Number of observations in brackets.

Table 17. Dependent Variable: Teenage Pregnancy

	(1)	(2)	(3)
Property Right	-0.138	-0.127	-0.199**
	(0.092)	(0.086)	(0.086)
Parcel Surface		-0.000	0.000
		(0.000)	(0.000)
Distance to Creek		-0.007	-0.001
		(0.026)	(0.021)
Block Corner		0.035	0.047
		(0.120)	(0.126)
Age			0.029
			(0.035)
Daughter			-0.164
			(0.213)
Granddaughter			-0.231
			(0.233)
Mother's Education—			-0.325
No instruction			(0.277)
Mother's Education—			-0.406
Primary school incomplete			(0.245)
Mother's Education—			-0.307
Primary school complete			(0.248)
Mother's Education—			-0.375
Secondary school incomplete			(0.241)
Mother's Education—			-0.100
Secondary school complete			(0.353)
Father's Education—			0.206
No instruction			(0.376)
Father's Education—			-0.344
Primary school incomplete			(0.274)
Father's Education—			-0.399
Primary school complete			(0.263)
Father's Education—			-0.517*
Secondary school incomplete			(0.262)
Father's Education—			-0.397
Secondary school complete			(0.275)
Constant	0.208***	0.226*	0.591
	(0.078)	(0.118)	(0.669)
Method	2SLS	2SLS	2SLS
Observations	87	87	87

Notes: The dependent variable is a dummy that equals 1 if the girl is or has ever been pregnant, and 0 otherwise. The sample is restricted to girls between 14 and 17 years of age. The 2SLS regressions use Property Right Offer to instrument for Property Right. Standard errors clustered at the household level are in parentheses. * Significant at 10-percent level. ** Significant at 5-percent level. *** Significant at 1-percent level.