Property Rights for the Poor: Effects of Land Titling

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November 2, 2006

Abstract

Secure property rights are considered a key determinant of economic development. The evaluation of the causal effects of property rights, however, is a difficult task as their allocation is typically endogenous. We exploit a natural experiment in the allocation of land titles to overcome this identification problem. More than twenty years ago, a group of squatters occupied a piece of land in a poor suburban area of Buenos Aires. When the Congress passed a law expropriating the land from the former owners with the purpose of entitling it to the occupants, some of the original owners accepted the government compensation, while others are still disputing the compensation payment in the slow Argentine courts. These different decisions by the former owners generated an allocation of property rights that is exogenous in equations describing the behavior of the squatters. We find that entitled families increased housing investment, reduced household size, and improved the education of their children relative to the control group. However, effects on credit access are modest and there are no effects on labor income.

JEL: P14, Q15, O16, J13
Keywords: Property rights, land titling, natural experiment, urban poverty.

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I. Introduction

The fragility of property rights is considered a crucial obstacle for economic development (North and Thomas, 1973; North, 1981; De Long and Shleifer, 1993; Acemoglu, Johnson and Robinson, 2002; Johnson, McMillan and Woodruff, 2002; Acemoglu and Johnson, 2005; inter alia). The main argument is that individuals underinvest if others can seize the fruits of their investments (Demsetz, 1967; Alchian and Demsetz, 1973). In today's developing world, a pervasive manifestation of feeble property rights are the millions of people living in urban dwellings without possessing formal titles of the plots of land they occupy (Deininger, 2003). The absence of formal property rights constitutes a severe limitation for the poor. Besides its investment effects, the lack of formal titles impedes the use of land as collateral to access the credit markets (Feder et al., 1988). It also affects the transferability of the parcels (Besley, 1995), making investments in untitled parcels highly illiquid. In addition, the absence of formal titles deprives poor families of the possibility of having a valuable insurance and savings tool that could provide protection during bad times and retirement, forcing them instead to rely on extended family members and offspring as insurance mechanisms.

Land-titling programs have been recently advocated in policy circles as a powerful instrument for poverty reduction. De Soto (2000) emphasizes that the lack of property rights impedes the transformation of the wealth owned by the poor into capital. Proper titling could allow the poor to collateralize the land. In turn, this credit could be invested as capital in productive projects, promptly increasing labor productivity and income. Inspired by these ideas, and fostered by international development agencies (such as the World Bank and USAid) and private institutions (such as the World Economic Forum, the Clinton Foundation, and the Cato Institute), land-titling programs have been launched throughout developing and transition economies as part of poverty alleviation efforts. For example, the Peruvian government issued property titles to 1.2 million urban households during the 1990s, millions of titles are being issued in Vietnam and Cambodia, land-titling programs are currently under development in Colombia, Mexico, Honduras, Paraguay, Ghana, Egypt, South Africa, Turkey and the Philippines, while President Lula announced during his first week in office a massive plan to title the huge *favelas* of the major Brazilian cities.

In this paper, we investigate the effects of issuing land titles to a very deprived population. The identification of land-titling effects, however, is a difficult task because it typically faces the problem that formal property rights are endogenous. The allocation of property rights across households is usually not random but based on wealth, family characteristics, individual effort, previous investment levels, or other mechanisms built on differences between the groups that acquire those rights and the groups that do not. Exogenous variability in the allocation of property rights is necessary to solve this selection problem.

Effects of land titling on different variables have been documented by several studies. A partial listing includes Jimenez (1984), Alston et al. (1996) and Lanjouw and Levy (2002) on real estate values; Besley (1995), Jacoby et al. (2002), Brasselle et al. (2002), Do and Iyer (2002) and Field (2005) on investment; Field (2003) on labor supply; and Feder et al. (1988), Place and Migot-Adholla (1998), and Carter and Olinto (2002) on access to credit. Previous work has ignored the problem of endogeneity, or addressed it by exploiting standard exclusion restrictions or variability in the regional timing of policy interventions. A distinctive characteristic of our study is that we exploit a natural experiment in the allocation of property rights to identify the causal effects of land titling.

More than 20 years ago, a group of squatters occupied an area of wasteland in the outskirts of Buenos Aires, Argentina. The area was composed of different tracts of land, each with a different legal owner. An expropriation law was subsequently passed, ordering the transfer of the land from the original owners to the state in exchange for a monetary compensation, with the purpose of entitling it to the squatters. However, only some of the original legal owners surrendered the land. The parcels located on the ceded tracts were transferred to the squatters with legal titles that secured the property of the parcels. Other original owners, instead, are still disputing the government compensation in the slow Argentine courts. As a result, a group of squatters obtained formal land rights, while others are currently living in the occupied parcels without paying rent, but without legal titles. Both groups share the same household pre-treatment characteristics. Moreover, they live next to each other, and the parcels they inhabit are identical. Since the decision of the original owners of accepting or disputing the expropriation payment was orthogonal to the squatter characteristics, the allocation of property rights is exogenous in equations describing the behavior of the occupants.

Thus, this natural experiment provides a control group that estimates what would have happened to the treated group in the absence of the intervention, allowing us to identify the causal effects of land titling.

Exploiting this natural experiment, we find significant effects on housing investment, household size, and child education. Housing investments are substantially higher in the titled parcels. Moreover, households in the titled parcels have a smaller size, both through a diminished presence of extended family members and through a reduced fertility of the household heads, and they invest more in the education of their children. However, we only find modest effects on access to credit markets as a result of entitlement, and no improvement in labor market performance. Our results suggest that land titling can be an important tool for poverty reduction, albeit not through the shortcut of credit access and entrepreneurial income, but through the slow channel of increased physical and human capital investment, which should help to reduce poverty in the next generation.

The rest of the paper is organized as follows. In the next section we describe the natural experiment. In section III we present our data, and in section IV we discuss the econometric methods. Section V presents our empirical results, while section VI concludes.

II. A Natural Experiment

The empirical evaluation of the effects of land titling poses a major methodological challenge. The allocation of property rights across families is typically not random but based on wealth, family characteristics, individual effort, previous investment levels, or other selective mechanisms. Thus, the individual characteristics that determine the likelihood of receiving land titles are probably correlated with the outcomes under study. Since some of these personal characteristics are unobservable, this correlation creates a selection problem that obstructs the proper evaluation of the effects of property right acquisition.

In this paper, we address this selection problem by exploiting a natural experiment in the allocation of property rights. In 1981, about 1,800 families occupied a piece of wasteland

in San Francisco Solano, County of Quilmes, in the Province of Buenos Aires, Argentina. The occupants were groups of landless citizens organized through a Catholic chapel. As they wanted to avoid creating a shantytown, they partitioned the occupied land into small urban-shaped parcels. At the beginning of the occupation the squatters believed that the land belonged to the state, but it was actually private property.[1] The occupants resisted several attempts of eviction 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 them to the squatters.

According to the expropriation law, the government would pay a monetary compensation to the former owners and it would then allocate the land to the squatters. In order to qualify for receiving the titles, the squatters should have arrived to the parcels at least one year before the sanctioning of the law, should not possess any other property, and should use the parcel as their family home. Within each household, the titles would be awarded to both the household head identified at that time and to her/his spouse (if married or cohabitating). The law also established that the squatters could not transfer the property of the parcels for the first ten years after titling.

The process of expropriation resulted to be asynchronous and incomplete. The occupied area turned out to be composed of thirteen tracts of land belonging to different owners. In 1986, the government offered each owner (or group of co-owners, as several tracts of land had more than one owner) a payment proportional to the official valuation of each tract of land, indexed by inflation. These official valuations, assessed by the tax authority to calculate property taxes, had been set before the land occupation. After the

[1] This is explained by the squatters in the documentary movie “Por una tierra nuestra” by Cespedes (1984). On the details of the land occupation process also see Briante (1982), CEUR (1984), Izaguirre and Aristizabal (1988), and Fara (1989). Information on the land expropriation process was obtained from the Land Secretary of the Province of Buenos Aires, the office of the General Attorney of the Province of Buenos Aires, the Quilmes County Government, the Land Registry, and the judicial cases. Additional information presented in this section was gathered through a series of interviews with key informants, including the Secretary of Land of the Province of Buenos Aires (Maria de la Paz Dessy), Undersecretary of Land of the Province of Buenos Aires (Alberto Farias), Directors of Land of Quilmes County (Daniel Galizzi and Alejandro Lastra), Secretary of Public Works and Land Registry of Quilmes County (Hector Lucas), General Attorney of the Province of Buenos Aires (Ricardo Szelagowski), attorney in expropriation offers’ office (Claudio Alonso), lawyer on expropriation lawsuit (Horacio Castillo), former land owners (Hugo Spivak and Alejandro Bloise -heir-), squatters (Juan Carlos Sanchez and Jorge Valle, inter alia), and President of NGO Gestion Urbana (Estela Gutierrez).

government made the compensation offers, the owner/s of each tract had to decide whether to surrender the land (accepting the expropriation compensation) or to start a legal dispute. Eight former owners accepted the compensation offered by the government. Five former owners, instead, did not accept the government offer and filed charges with the aim of obtaining a higher compensation. In 1989-91, the tracts of land of the former owners that accepted the government compensation were transferred to the squatters occupying them, together with formal land titles that secured the property of the parcels.[2,3]

The people who occupied parcels located on the tracts of land that belonged to the former owners that accepted the expropriation compensation, were ex-ante similar, and arrived at the same time, than the people who settled on the tracts of the former owners that 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 it. In fact, at the time of the occupation the squatters believed that all the land was state-owned and they could not know that an expropriation law was going to be passed, nor what was going to be the future response of the owner of each specific parcel.

[2] The "new" urban design traced by the squatters differed from the previous land tract divisions. Thus, some "new" parcels overlapped over tracts of land that belonged to more than one former owner. This could be interpreted as further evidence of the squatters' ignorance about the previous land ownership status. Had they known the existence of different private owners, they should have followed the previous land design to avoid being exposed to the decisions of two or three landowners rather than one. For regulatory reasons, parcels could not be delimited and titled if one portion of them was still under dispute.

[3] The market value of land parcels comparable to the ones titled to the squatters amounted to approximately 7.4 times the monthly average total household income for the first quintile of the official household survey (EPH) of October 1986 for the Buenos Aires metropolitan area (market value of parcels in the neighboring non-squatted area obtained from evidence presented in "Kraayenbrink de Beurts et al. v. Province of Buenos Aires"). This figure, however, constitutes only an upper bound of the differential wealth transfer received by the entitled households for three reasons. First, the expropriation law established that each titled squatter had to pay the government the proportionally prorated share of the official valuation of the occupied tract of land. The law, however, established that the payments should be made in monthly installments that could never surpass 10% of the (observable) household income and there was no indexation for inflation. Given the hyperinflationary periods experienced by the Argentine economy during the period of analysis and the high labor informality of this population, the real values paid by the squatters were probably quite small. In practice, there are no records of the amounts and dates of the payments made by each household. Second, entitled households are supposed to regularly pay property taxes. Third, untitled squatters pay no rent.

A potential concern, however, is that the different former owners' decisions could reflect differences in land quality. In turn, these differences could be correlated with squatters' heterogeneity. For example, more powerful squatters could have settled in the best parcels. An advantage of our experiment is that the parcels of land in the treatment (titled) and control (untitled) groups are almost identical and basically next to each other. Indeed, after the data description, we show in Section IV that there are no differences in observable parcel characteristics (distance to a polluted creek, distance to the closest non-squatted area, parcel size, location in a corner of a block) between the treatment and control groups.[4] We also show that there are no differences in pre-treatment observable household characteristics (age, gender, nationality and years of education of the person who was the household head at the time of the occupation, and nationality and years of education of her/his parents). Importantly, all the evidence on the occupation process (the documentary movie, the articles, the judicial files, and the interviews with squatters, lawyers and former owners described in footnote 1) coincide that the squatters had no direct contact with the former owners to influence their decisions. Moreover, the dwellings constructed by the squatters had to be explicitly ignored in the calculation of the expropriation compensation, and the government offers were very similar (in per-square-meter terms) for the accepting and contesting owners, in accordance with the proximity and alikeness of the land tracts.[5]

Given the similarity in land quality and compensation offers, the different responses might instead reflect heterogeneity of the former owners regarding decision-making, subjective land value, or litigation costs. Although thirteen tracts of land provide a small number for a statistical analysis, a few patterns emerge. The average number of co-owners in the groups of accepting owners is 1.25, while the average number of co-owners for the contested tracts is 2.2. Moreover, when we defined a dummy equal to 1 if there is more than one co-owner sharing the same family name, and 0 otherwise, the

[4] There are also no differences in altitude. The Buenos Aires metropolitan area is totally flat and all these parcels are within the same 5-meter topographical range. Besides, as this is urban land, agricultural productivity is not an issue.

[5] In Argentine pesos (of January 1986) per square meter, the accepted offers had a mean of 0.424 and a median of 0.391. The contested offers had a mean of 0.453 and a median of 0.397. Indeed, the similitude of the offers is repeatedly used as an argument by the government attorneys in the expropriation lawsuits to demonstrate that the government offers were fair, as they were similar to the ones accepted by other owners. The same argument is utilized in a low-court verdict in "Kraayenbrink de Beurts et al. v. Province of Buenos Aires" citing jurisprudence of the Supreme Court.

average for this dummy for the accepting owners is 0.125 while the average for the challenging owners is 0.6. Thus, it appears that having many co-owners and several in the same family made it more difficult for the owners to agree on accepting the government offer.[6] Note that if, in spite of this discussion, one may still fear that the challenging owners did so because the unobservable quality of their land was higher, that would imply that the squatters that did not receive titles are standing on land of better quality.

As explained, five former owners did not accept the compensation offered by the government and went to trial. In these lawsuits, all the legal discussion hinges around the determination of the monetary compensation. The Congress constitutionally approved the law and, thus, the expropriation itself could not be challenged. The squatters had no participation in these legal processes (the lawsuits were exclusively between the former owners and the provincial government), and the value of the dwellings they constructed was explicitly excluded from the dispute over the monetary compensation ("Cordar SRL v. Province of Buenos Aires"). One of these five lawsuits ultimately ended with a final verdict, and the squatters on this tract of land received titles in 1998. The other four lawsuits are still pending in the slow Argentine courts. If one is still worried about the possibility that the former owners' decisions of surrendering or suing was correlated with land quality or squatters' characteristics, then an additional feature of this experience is that it allows us to separately compare the squatters in this lastly titled tract of land relative to the control group. Although these two groups of squatters settled in tracts of land which are homogenous regarding their respective former owners' decisions of going to trial, one group already received titles while the other is still waiting for the end of the legal processes.[7]

[6] Kaplan et al. (2006) find that cases are less likely to be settled (more likely to go to court) when they involve multiple plaintiffs. See also Fiss (1984). On family economic decisions see, for example, Burkart et al. (2003) and Bennedsen et al. (2006). Within the challenging owners, we also found one case in which an owner was a lawyer who was representing himself in the case (which may suggest lower litigation costs), while in another case, one of the original owners had passed away before the sanctioning of the law but her inheritance process was still under way at the time the family had to make a decision.

[7] We can still wonder, within this group of former owners that disputed the compensation, why some are still on trial while one concluded. Exogenous reasons lengthened the pending trials. In two cases, the expropriation lawsuit was delayed by the death of one of the former owners, which required an inheritance process. In another case (mentioned in footnote 6) one of the original owners had died just before the sanctioning of the law and her inheritance process had not finished. In the fourth case, the legal process was delayed by a mistake made in the description of the land tract in a low-court judge's verdict.

The final outcome of this expropriation process is that a group of families now has legal property rights, while another group is still living in the occupied parcels enjoying free *usufructuary* rights but without possessing formal land titles. This allocation of land titles was the result of an expropriation process that did not depend on any particular characteristic of the squatters nor of the parcels of land they occupied. Thus, by comparing the groups that received and did not receive land titles, we can act as if we have a randomized experiment.

III. Data Description

The area affected by Expropriation Law Nº 10.239 covers a total of 1,839 parcels. 1,082 of these parcels are located in a contiguous set of blocks. However, the law also included another non-contiguous (but close) piece of land currently called San Martin neighborhood, which comprises 757 parcels. As this area is physically separated from the rest, we focus on the 1,082 contiguous parcels to improve comparability, and then pool the San Martin parcels when we analyze the robustness of our findings.

We have precise knowledge of the titling status of each parcel. Land titles were awarded in two phases. Property titles were awarded to the occupants of 419 parcels in 1989, and to the occupants of 173 parcels in 1998. Land titles are not available to the families living in 410 parcels located on tracts of land that have not been surrendered to the government in the expropriation process. Finally, there are 80 parcels that were not titled because the squatters occupying them had not fulfilled some of the required registration steps, or had moved or died at the time of the title offers, although the original owners had surrendered these pieces of land to the government. Borrowing the terminology from clinical trials, this subgroup constitutes the “non-compliers” in our study, since they were offered the treatment (land title) but they did not receive it.

Table 1 summarizes the process of allocation of land titles for the main area. The variable Property Right Offer equals 1 for the families occupying parcels that were

surrendered by the original owners, and 0 otherwise; while the variable Property Right equals 1 for the squatters that received property titles, and 0 otherwise.[8]

Table 1 – **Allocation of Land Titles**

Property Right Offer = 1				Property Right Offer = 0 & Property Right = 0	Total
Year	Property Right = 1	Property Right = 0	Total		
1989	419	23	442		
1998	173	57	230		
Total	592	80	672	410	1082

A survey performed in 2003 provides the data utilized for this study. The inhabitants of 590 randomly selected parcels (out of the total of 1,839) were interviewed. 617 households living in these 590 parcels (27 parcels host more than one family) were surveyed. Excluding the non-contiguous San Martin neighborhood, we interviewed 467 households living in 448 parcels. At the same time, we sent a team of architects to perform an outside evaluation of the characteristics of the houses.[9]

IV. Econometric Methods

We seek to identify the effect of the allocation of property rights on several outcome variables exploiting a natural experiment in the allocation of land titling. In a natural experiment, like in a randomized trial, there is a control group that estimates what would have happened to the treated group in the absence of the intervention, but nature or other exogenous forces determine treatment status instead. The validity of the control group is evaluated by examining the exogeneity of treatment status with respect to the potential outcomes, and by testing that the pre-intervention characteristics of the treatment and control groups are reasonably similar. In section II we discussed at length

[8] The 757 parcels of San Martin, which belonged to an owner who accepted the expropriation compensation without suing, were offered for titling in 1991. 712 were titled, while 45 correspond to non-compliers.

[9] Gestion Urbana, an NGO that works in this area, carried out the household survey and the housing evaluation. The interviewers and the architects were not informed of the hypotheses under study and were blind to the treatment status of each parcel. We distributed a food stamp of $10 (about 3 US dollars at the time of the survey) for each answered survey as a token of gratitude to the families willing to participate in our study. In 10 percent of the cases, the survey could not be performed because there was nobody at home in three visit attempts, the parcel was not used as a house, rejection, or other reasons. These parcels were randomly replaced. Non-response rates were similar for titled and untitled parcels.

the process of allocation of land title offers and argued that this process was exogenous to the characteristics of the squatters in our experiment. We now test the similarity of pre-treatment characteristics between the treatment and control groups.

In Table 2, we compare parcel characteristics for the non-intention-to-treat and intention-to-treat groups (i.e., Property Right Offer = 0 and Property Right Offer = 1, respectively) to analyze the presence of potential differences. The variables under comparison are distance to a nearby (polluted and floodable) creek, distance to the closest non-squatted area, parcel size, and a dummy for whether the parcel is located in a corner of a block. We only reject the hypotheses of equality for parcel size (at the 8.9% level of significance). Nevertheless, the difference in average parcel sizes between these two groups is relatively small –parcels are only 3% larger in the non-intention-to-treat group– and if something, it is the control group the one that inhabits slightly larger parcels.

Table 2 – **Pre-Treatment Parcel Characteristics**

Parcel Characteristics	Property Right Offer=0	Property Right Offer=1	Difference
Distance to Creek (in blocks)	1.995 (0.061)	1.906 (0.034)	0.088 (0.070)
Distance to Non-Squatted Area (in blocks)	1.731 (0.058)	1.767 (0.033)	-0.036 (0.067)
Parcel Size (in squared meters)	287.219 (4.855)	277.662 (2.799)	9.556* (5.605)
Block Corner=1	0.190 (0.019)	0.156 (0.014)	0.033 (0.023)

Notes: Standard errors are in parentheses. * Significant at 10%.

In Table 3, we compare pre-treatment characteristics of the “original squatter” between the non-intention-to-treat and intention-to-treat groups (i.e., Property Right Offer = 0 and Property Right Offer = 1, respectively) for the families that arrived before treatment. We define the “original squatter” as the household head at the time the family arrived to the parcel they are currently occupying. We cannot reject the hypotheses of equality in age, gender, nationality and years of education of the original squatter, suggesting a strong similarity between these groups at the time of their arrival to this area. Moreover, we do not reject the hypotheses of equality in nationality and years of education of the mother and father of the original squatter across the groups, suggesting that these groups had been showing similar trends in their socio-economic development before their arrival to

this area.[10] The similarity across pre-treatment characteristics is consistent with the exogeneity in the allocation of property rights described above.

Table 3 – **Pre-Treatment Characteristics of the Original Squatter**

Characteristics of the Original Squatter	Property Right Offer=0	Property Right Offer=1	Difference
Age	48.875 (0.938)	50.406 (0.761)	-1.532 (1.208)
Female=1	0.407 (0.046)	0.353 (0.035)	0.054 (0.058)
Argentine=1	0.903 (0.028)	0.904 (0.022)	-0.001 (0.035)
Years of Education	6.071 (0.188)	5.995 (0.141)	0.076 (0.235)
Argentine Father=1	0.795 (0.038)	0.866 (0.025)	-0.072 (0.046)
Years of Education of the Father	4.655 (0.147)	4.417 (0.076)	0.237 (0.165)
Argentine Mother=1	0.804 (0.038)	0.856 (0.026)	-0.052 (0.046)
Years of Education of the Mother	4.509 (0.122)	4.548 (0.085)	-0.039 (0.149)

Notes: Standard errors are in parentheses.

Once treatment status has been shown to be ignorable, estimation of average treatment effects is straightforward. Operationally, we analyze the effect of land titling on variable Y by estimating the following regression model:

$$\mathrm{Y}_i = \alpha + \gamma \ \text{Property Right}_i + \beta \ \mathrm{X}_i + \varepsilon_i \qquad (1)$$

where Y_i is any of the outcomes under study, and γ is the parameter of interest, which captures the causal effect of Property Right$_i$ (a dummy variable indicating the possession of land title) on the outcome under consideration.[11] X_i is a vector of pre-treatment parcel and original squatter characteristics, and ε_i is the error term.[12]

[10] In 23 percent of the cases, the current household head does not coincide with the original squatter, either because she/he arrived later than the first member of the family that occupied the parcel, or because she/he arrived at the same time but was not the household head at the arrival time. This percentage is similar for the treatment and control groups. We obtain similar results when we compare the pre-treatment characteristics of the current household head between the two groups.

[11] Some of the variables under study are Limited Dependent Variables (LDV). The problem of causal inference with LDV is not fundamentally different from the problem of causal inference with

A typical concern when conducting statistical inference after estimating the parameters of equation (1) is that the errors in that equation might not be independent across households. For example, treatment might interact with parcel characteristics and hence, it might affect similarly households residing nearby. In order to control for these potential nuisances, we also compute Huber-White robust standard errors by clustering the parcels located in the same block and the parcels belonging to the same former owner.[13] There are no obvious reasons to suspect that the errors in equation (1) are more likely to be correlated among squatters residing in a track of land that belonged to the same former owner than among those living in the same block, sidewalk of a block, or rectangular block. However, the robust standard errors we compute when the errors are clustered by former owner might be less accurate than in the other cases since the number of former owners is not large. Thus, as an additional robustness check, for the clustering at the former owner level we also compute block bootstrapped standard errors and significance levels.

To this point, our model has assumed that all the squatters actually received the treatment to which they were assigned. In many experiments, however, a portion of the participants fail to follow the treatment protocol, a problem termed treatment non-compliance. In our case, this might be of potential concern since a number of families that were offered the possibility of obtaining land titles did not receive them for reasons that may also affect their outcomes. In order to address this problem of non-compliance, we also report the reduced-form estimates from regressing the outcomes of interest on the intention-to-treat Property Right Offer variable, a dummy indicating the availability of land title offers, and also the 2SLS estimates of the treatment effects from instrumenting the Property Right variable with the Property Right Offer variable.

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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 LDV than for other types of dependent variables. This is certainly the case in a natural experiment where controls are only included to improve efficiency, but their omission would not bias the estimates of the parameters of interest.

[12] Our estimates show no change if we include as controls the personal characteristics of the current household head instead of those of the original squatter, when they differ.

[13] For the former owner clustering, if a set of parcels overlaps on the borders of the previous tract divisions, occupying a piece of land that belonged to one owner and another piece that corresponded to another owner, the former owner is defined as the combination of the two former owners. For the block clustering, a block is defined as both sides of the segment of a street between two corners. These procedures define 18 former owner clusters and 83 block clusters. Similar results are obtained using other clustering units, such as each sidewalk of a block or the rectangular block delimited by consecutive streets.

Finally, in any investigation where the impact takes time to materialize (like the investment, household size and education variables considered in this paper), some participants will inevitably drop out from the analysis. For example, the most widely used longitudinal dataset in economics, the Michigan Panel Study on Income Dynamics, has experienced a 50 percent sample loss from cumulative attrition after 30 years from its initial sample (see Fitzgerald et al., 1998).[14] Participation attrition, hence, is another potential problem that might bias the estimates of causal effects in long-term studies.

In our survey, we asked each family the time of arrival to the parcel they are currently occupying, and found that some families arrived after the former owners made, during 1986, the decision of surrender the land or sue. From the sample of 467 interviewed households, we found that 313 families had arrived to the parcel before the end of 1985, while 154 families arrived after 1985.[15] As it is plausible to argue that the families that arrived after the former owners' decisions could have known the different expropriation status (i.e., the different probabilities of receiving the land) associated to each parcel, in order to guarantee exogeneity we need to exclude from the analysis the families that arrived to the parcel they are currently occupying after 1985. Once this exclusion is made, there is basically no variability (nor differences between treatment and control groups) in our sample in the year of arrival of the households to the parcels they are currently occupying.

This raises, however, a problem of attrition. If some families arrived after 1985, they could have replaced some original squatters in our treatment and control parcels that had left before we ran our survey in 2003.[16] Moreover, the availability of titles could have affected household migration decisions. Indeed, column (1) of Table 4 shows that 62.4

[14] For example, see also Alderman et al. (2003) and Behrman et al. (2003). Previous studies of land-titling effects ignore this issue.

[15] To identify with accuracy the time of arrival of each family to the parcel they are currently occupying, our survey asked where the original squatter was living when Diego Maradona scored the 'Hand of God' goal in the 1986 World Cup game against England. It is impossible for an Argentine not to remember where she/he was on that day (Amis, 2004).

[16] For the families that arrived after 1985, our questionnaire attempted to collect information on the names and destination of the previous occupants of the parcels. In both treatment and control parcels, the current occupants could provide a name and/or destination of the previous occupant only for less than 20 percent of the cases. Although the information obtained is very poor, it does not suggest that the households that left the untitled parcels moved to richer areas than the families that left the titled parcels.

percent of the parcels in the non-intention-to-treat group are inhabited by families that arrived before 1986, while the proportion is 70.0 percent for the intention-to-treat group in the second column.[17]

Table 4 – **Household Attrition**

Variables	Property Right Offer=0 (1)	Property Right Offer=1 (2)	Property Right Offer 1989=1 (3)	Property Right Offer 1998=1 (4)
Household arrived before 1986=1	0.624 (0.036)	0.700 (0.028)	0.729 (0.051)	0.689 (0.033)
Difference relative to column (1)		-0.076* (0.045)	-0.105* (0.063)	-0.064 (0.049)

Notes: Standard errors are in parentheses. * Significant at 10%.

Of course, the migration decision could be potentially correlated with the outcomes under study. We exploit two alternative strategies to address this potential nuisance. Our first strategy takes advantage of the asynchronous timing in the titling process. The third column of Table 4 shows a significant difference in attrition for the parcels titled in 1989 (early treatment) relative to the control group. Instead, the last column shows no statistically significant differences in attrition for the parcels titled in 1998 (late treatment) relative to the control group. Thus, once we incorporate the fact that the analysis must be done on the survivors of the experiment, the non-intention-to-treat group appears a priori as a better control group for the late-intention-to-treat group than for the early-intention-to-treat squatters. First, there is no significantly differential attrition between these two groups. Second, the unobservable variables that might have affected migration decisions are, a priori, more likely to be ignorable when comparing these two groups than when comparing the control and early treatment groups. This is so because the squatters titled in 1998 were under the same conditions as those in the control group for 17 out of the 22 years elapsed from the land invasion to the time of our survey, so we should expect them to have broadly similar experiences, for example, in terms of shocks that affect attrition and incentives to respond to them. Indeed, most of the out-migration for these two groups occurred during the interim period they were both untitled. The survival rates for the late titled and control groups since 1997 (i.e., just before the late treated received titles) are 0.958 (s.e. 0.014) and 0.939 (s.e. 0.017), respectively. Thus,

[17] These survival rates could be overestimating attrition by assuming that there were no vacated parcels left after the occupation.

the estimated effects of land titling for the late titled group are unlikely to be biased by attrition. Additionally, the comparison of these coefficients with those corresponding to the estimated effects of land titling for the early-treated group leads to an indirect test of whether attrition in the latter group is also ignorable.

A more standard strategy assumes the data are missing at random conditional on observable characteristics.[18] The idea is then to compare the outcomes for treated and control survivors with similar pre-treatment characteristics. This approach leads to matching methods based on the propensity score of sample selection. This is in the spirit of Ahn and Powell (1993), who propose to eliminate the selection bias by differencing observations with similar probabilities of selection.

This strategy requires that at least one of the pre-treatment characteristics predicts attrition. The only pre-treatment characteristics available for the whole set of squatters (attrited and non-attrited) are the parcel characteristics reported in Table 2. We estimate a Logit model of the likelihood of survival since 1985 on these parcel characteristics, and find that the distance to the nearby polluted and floodable creek has a positive and statistically significant effect on this likelihood. We exploit the variability in attrition induced by this pre-treatment characteristic to correct for sample selection.

We implement the matching selection correction by means of the method of stratification matching. First, we eliminate observations outside the common support of the estimated propensity score for the distributions of titled and untitled groups. Second, we divide the range of variation of the propensity score in intervals such that within each interval, treated and control units have on average the same propensity score. Third, within each interval, the difference between the average outcomes of the treated and the controls is computed. The parameter of interest is finally obtained as an average of the estimates of each interval weighted by the share of treated units in each interval on all treated units.

V. Results

[18] Another strategy to address the problem of attrition is to model the selection mechanism using latent index models (Heckman, 1979). However, in the absence of an instrumental variable predicting attrition but not affecting the outcomes under analysis, something difficult to conceive in most concrete cases, identification is hardly plausible.

In this section we investigate the causal effect on housing investment, household structure, child education, access to credit, and labor earnings, of providing squatters with formal titles of the parcels of land they occupy. This is the treatment of interest in policy analysis in the developing world, where most interventions consist of titling occupied tracts of land to the current inhabitants.[19]

Ownership of property gives its owner multiple rights. In its most complete form, they include the rights to use the asset, to exclude others from using it, to transfer the assets to others, and to persist in these rights (Barzel, 1997). In our natural experiment, the entitled households acquired full property rights (with the only restriction that the parcels cannot be legally transferred for the first ten years after titling). The untitled households, instead, are still living in the occupied parcels without paying rent and property taxes, but they are uncertain about when and if the parcels will be titled. Moreover, the untitled may feel uncertain about which member of the household would receive the title, and they may fear the occupation of their parcels by new squatters before titling. In the meantime, the untitled cannot legally transfer their *usufructuary* rights.

V.1. Effects on Housing Investment

The possession of land titles may affect the incentives to invest in housing construction through several concurrent mechanisms. The traditional view emphasizes security from seizure. Individuals underinvest if others may seize the fruits of their investments. Land titles can also encourage investment by improving the transferability of the parcels. Even if there were no risk of expropriation, investments in untitled parcels would be highly illiquid, whereas titling reduces the cost of alienation of the assets. A third mechanism is through the credit market. Transferability might allow the use of the land as collateral, diminishing the funding constraints on investment. Finally, a fourth link is that land titles provide poor households with a valuable savings tool. Poor households, especially in unstable macroeconomic environments, lack appropriate savings instruments. Land titles allow households to substitute present consumption and leisure into long-term savings in

[19] Whether the provision of land titles to squatters in this area could have encouraged new squatting (and therefore, violation of landowners' property rights) in other zones is beyond the scope of our study, but should not be ignored in the evaluation of the overall impact of this type of interventions.

real property. We now investigate empirically the impact of legal land titles on housing investment.

In Table 5 we summarize the analysis of the effect of property rights on housing investments. An important clarification is that before the occupation this was a wasteland area without any construction. Thus, the treatment and control areas had a similar (i.e., zero) baseline investment level before the occupation.[20] In each column, we present the coefficient of the treatment dummy Property Right on a different housing characteristic. All the estimates reported in Table 5 are from regressions including controls for pre-treatment characteristics of the parcel and the original squatter.

Table 5 - **Housing Investment**

	Good Walls	Good Roof	Constructed Surface	Concrete Sidewalk	Overall Housing Appearance
	(1)	(2)	(3)	(4)	(5)
Property Right	0.20***	0.15**	8.27**	0.11**	8.42***
	(3.47)	(2.49)	(2.34)	(2.18)	(3.65)
Control Group Mean	0.50	0.32	67.63	0.67	22.71
%Δ	40.00%	46.87%	12.23%	16.42%	37.08%

Notes: Good Walls, Good Roof, and Concrete Sidewalk are dummy variables that equal 1 if the house has walls of good quality, a roof of good quality, and a sidewalk made of concrete, respectively, and 0 otherwise. Constructed surface is measured in squared meters. Overall Housing Appearance measures the overall aspect of each house from 0 to 100 points. The parcel is the unit of observation. All the regressions control for parcel and original squatter pre-treatment characteristics: parcel size; distance to creek; distance to nearest non-squatted area; block corner; age, gender, nationality, and years of education of the original squatter; and nationality and years of education of father and mother of the original squatter. The robustness of the results and detailed variable definitions are presented in Table 6 and Appendix Tables A.1 through A.4. Absolute values of t statistics are in parentheses. ** Significant at 5%; *** significant at 1%.

The first two columns present large effects of land titling on the probability of having walls (first column) and roof (second column) of good quality. The proportion of houses with good quality walls rises by 40 percent under land titling, while the increase reaches 47 percent for good quality roof. The third column presents the effect of land titling on the total surface constructed in the parcel. Our results suggest a statistically significant increase of about 12 percent in constructed surface under the presence of land titles.

[20] In practice, it is not possible to identify one date of construction for these houses. Houses in this neighborhood are typically self-constructed through gradual incorporations (CEUR, 1984; Cespedes, 1984; Izaguirre and Aristizabal, 1988).

The fourth column shows a statistically significant increase of 16 percent in the proportion of houses with sidewalks made of concrete. In the last column, the variable Overall Housing Appearance summarizes the overall aspect of each house using an index from 0 to 100 points assigned by the team of architects. The coefficient shows a large and significant effect of land titling on housing quality. Relative to the baseline average sample value, the estimated effect represents an overall housing improvement of 37 percent associated to titling.

In Table 6, we show for the first investment variable (Good Walls) the robustness of the results regarding all the methodological concerns discussed in section IV. For the other investment variables, the same analysis is relegated to Appendix Tables A.1 to A.4 for the sake of space. In Table 6, column (1) repeats the model in the first column of Table 5, but displaying all the coefficients (and t-statistics) for the control variables. In column (2), we start with a simple model without including any control variables, while in column (3) we add back the control variables for the parcel characteristics. In these two alternative specifications, the point estimates on the variable of interest are very similar to those in column (1). In column (4) we add the observations for the San Martin neighborhood that were excluded from the baseline analysis in order to enhance geographical comparability between treatment and control groups (see Section III). Again, the point estimates are similar to those in the baseline model in column (1) and the difference is not statistically significant.

Columns (5), (6) and (7) address the potential presence of correlation in the errors of the models. In columns (5) and (6) we report t-statistics computed using Hubert-White robust standard errors after clustering the parcels located in the same block and the parcels from the same former owner. The significance level of the variable of interest remains unaltered. In column (7) we also report block bootstrapped t-statistics and significance levels computed by resampling with replacement blocks of households grouped by former owner. The significance level of the variable of interest shows little change. For the other four investment variables, we also find that the significance levels of the Property Right variable remain basically unaltered when standard errors robust to the presence of clusters in the data are computed. Thus, this finding suggests that there is no evidence that the errors in equation (1) lack independence.

Columns (8) and (9) deal with the potential problem of non-compliance. In column (8) we estimate the reduced-form parameter on the intention-to-treat Property Right Offer variable, while in column (9) we report the 2SLS estimates of instrumenting the Property Right variable with Property Right Offer. For the five investment variables, both estimates are very similar to those obtained from OLS in the baseline specification and the differences are not statistically significant at conventional levels, suggesting that non-compliance is not an issue of concern in our sample.[21]

In Columns (10) and (11) we address the concern that these results might be generated by attrition in the original squatter population and are not the cause of treatment. In Column (10) we separately report the effects for early and late land titling, exploiting the fact that, as shown in Table 4, the attrition rates of the late-treated and control groups are not significantly different. The results show that both the early and late treatments have positive significant effects on Good Walls and the other investment variables. For all the variables, the point estimates for the late treatment coefficient are very similar to the ones in the baseline specification in column (1). Moreover, the F-statistics show that we cannot reject the null hypotheses that the effects for the early-treated group and late-treated group are similar at conventional levels of significance.[22] Column (11) reports the matching estimates discussed in the previous section. Again, for Good Walls and the other investment variables, the point estimates are quite similar to those in the baseline specification and the differences are never statistically significant. Thus, the evidence suggests that the estimates in Table 5 identify the causal effect of land titling on investment and not a statistical artifact due to attrition.

Finally, in column (12) we consider the whole sample of 448 parcels where households were interviewed, instead of considering only the parcels occupied by households that arrived before the time the former owners decided to surrender the land or sue. This

[21] The first-stage regression of Property Right on Property Right Offer is very strong. For the households that arrived before 1986 (i.e. the non-attrited group) and live in parcels offered for titling, the non-compliance rate is 11.2% (9.3% for the early treated, and 12% for the late treated). Note that non-compliance rates are similar between early and late treatment groups and lower than those suggested by Table 1, once the analysis is restricted to the non-attrited sample.

[22] If one was still to worry about the possibility that the former owners' decisions of accepting or disputing the government offer was correlated with land or squatter characteristics, the significance of the late-treatment coefficients and their similarity with the early-treatment ones should be reassuring. In both the late-treated and control areas, the squatters settled on

analysis investigates a different parameter than the one considered so far. The estimated coefficient measures the causal effect of securing property rights on investment in a given parcel regardless of whether the selection of the family that is currently occupying it could have depended on titling status.[23] The estimated coefficients are of similar magnitude.

A final question relates to the interpretation of the identified causal effect of land titling on investment. Is this an incentive effect induced by owning formal property rights, or is it mainly a wealth effect from titled households that became richer, housing being a normal good? The evidence suggests the treatment operates by affecting the incentives to invest. First, the size of the differential wealth transfer was moderate (see footnote 3) and seems considerably smaller than the value of the constructed dwellings.[24] Second, the families could not have financed the investments with the wealth transfer. It would be impossible to sell the land and, at the same time, invest the collected money on it. Moreover, access to credit improved little with titling (see section V.4). Third, Appendix Table 5 shows no differences in the consumption of durable goods (refrigerators, freezers, washing machines, TV sets and cellular phones). This suggests that the large investment effects presented in this section are a result of a change in the economic returns to housing investment induced by the land titles, and not just a response to a wealth effect that should have also affected the consumption of these goods.

We conclude that moving a poor household from *usufructuary* rights to full property rights substantially improves housing quality. The estimated effects are large and robust, and seem to be the result of changes in the economic returns to housing investment induced by land titling. Thus, our micro evidence supports the hypothesis that securing property rights significantly increases investment levels.

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eventually contested tracts of land and are, therefore, homogenous regarding the decisions of their respective original owners (see section II).

[23] In these regressions that ignore household attrition, the estimated coefficient can be interpreted as "what grows in a parcel when it is entitled" regardless of whether the same family has been occupying it or has been replaced by another one. Instead, the estimates obtained exclusively on the non-attrited households measure "what a given family builds in a parcel when receives a land title".

[24] For areas of this level of development in the Buenos Aires outskirts, Zavalia Lagos (2005) estimates that the values of the constructed houses exceed the parcel values by five times.

V.2. Effects on Household Size

The possession of land titles may also affect the size and structure of households. There are several potential reasons for that to happen. Insurance motives seem to be the most important. The poor lack access to well-functioning insurance markets and pension systems that could protect them during bad times and retirement. With limited access to risk diversification, to savings instruments, and to the social security system, the need for insurance has to be satisfied by other means. A traditional provider of insurance among the poor is the extended family. Another possibility is to use children as future insurance. In particular, old-age security motives can induce higher fertility (see, among others, Cain, 1985, Nugent, 1985, Ray, 1997, and Portner, 2001).[25] By allowing the use of housing investment as a savings tool, by securing shelter for the old age, and by potentially improving the access to the credit market, land titling may provide some of the needed insurance, therefore reducing the demand for household members among the titled group.[26]

Moreover, the lack of land titles might reduce the ability of household heads to restrict their relatives from residing in their houses. The household heads may feel less powerful to expel or to deny access to members of their extended family when they lack formal titles. The lack of titles may also impede the division of wealth among family members, forcing claimants to live together to enjoy and retain *usufructuary* rights. For example, siblings (with their spouses and children) may end up having to live together if they cannot divide their inheritance upon the death of their untitled parents. In addition, untitled households may feel in need of increasing the number of family members in order to protect their houses from occupation by other squatters (Lanjouw and Levy,

[25] "[An] important question is whether having many children and/or a large extended household is an optimizing strategy allowing households to derive benefits otherwise lost due to poorly functioning markets" (Birdsall 1988, pp. 502).

[26] David and Sundstrom (1984) explain the fertility changes in US history using a similar argument. Suppose, they argue, that large families were designed to be old-age insurance for the parents. At the time of independence, the superabundance of arable land meant that the price of land would not rise over time sufficiently to be a nest egg for old age, and children would be needed to care for their aged parents. When, late in the nineteenth century, the best lands were growing scarce, then the rent, and therefore the price, of land already owned and settled would increase becoming a nest egg due to its capital gain. Thus, investment in land operated as a substitute for more children. The scarcer the land, the higher the economic rent and capital gain, and the fewer children needed to provide for the declining years of the parents.

2002; Field, 2003). Through these concurrent mechanisms, the lack of formal land titles may generate, on average, larger households among the untitled group.

In Table 7, we find large differences in household size between titled and untitled families. Untitled families have an average of 6.06 members, while titled households have 0.95 members less. Table 7 also shows that the difference in household size does not originate in a more frequent presence in the control group of a spouse of the household head (column 2), nor of offspring of the household head older than 13 years old, i.e. born before the first land titles were issued (column 3). This last result is important, because it suggests that there were no differences in the number of children of the household head born before treatment.[27]

The difference in household size seems to originate in two factors. First, column (4) of Table 7 shows a higher presence (0.68 members) of non-nuclear relatives in untitled households. Untitled households report a much larger number of further relatives of the household head who are not her/his spouse or offspring (i.e., siblings, parents, in-laws, grandchildren, etc.) than entitled households.[28]

Table 7 - **Household Size**

	Number of Household Members (1)	Household Head Spouse (2)	Offspring of the HH (≥14 years old) (3)	Other Relatives (no Spouse or Offspring of HH) (4)
Property Right	-0.95***	-0.01	-0.01	-0.68***
	(2.81)	(0.27)	(0.06)	(3.53)
Control Group Mean	6.06	0.74	1.69	1.25
%Δ Property Right	-15.68%	-1.35%	-0.59%	-54.40%

	Offspring of the HH (5-13 years old)		Offspring of the HH (0-4 years old)	
	(5)	(6)	(7)	(8)
Property Right	-0.17		-0.07	

[27] The regression in column (3) only considers offspring living in the house. Non-significant differences are also obtained for the total number of household head's offspring older than 13 (i.e., living and not living in the parental home).

[28] The hypothesis that extended family members are valuable to protect the house from other squatters would suggest a larger share of males among non-nuclear adult members in the control group than in the treatment group. In our dataset, however, this proportion is smaller in the control group.

	(1.18)		(1.03)	
Property Right 1989		-0.38*		-0.08
		(1.88)		(0.81)
Property Right 1998		-0.06		-0.07
		(0.37)		(0.86)
Control Group Mean	1.06	1.06	0.33	0.33
%Δ Property Right	-16.04%		-21.21%	
%Δ Property Right 1989		-35.85%		-24.24%
%Δ Property Right 1998		-5.66%		-21.21%

Notes: En each column, the dependent variable is the number of household members of each group. The household is the unit of observation. All the regressions control for parcel and original squatter pre-treatment characteristics: parcel size; distance to creek; distance to nearest non-squatted area; block corner; age, gender, nationality, and years of education of the original squatter; and nationality and years of education of father and mother of the original squatter. The robustness of the results and detailed variable definitions are presented in Appendix Tables A.6 through A.11. Absolute values of t statistics are in parentheses. * Significant at 10%; *** significant at 1%.

Second, the entitled households show a smaller number of offspring of the household head born after the title allocation. To better analyze this result, we split the household heads' offspring into those born between the first and the second title allocation (children between 5 and 13 years old), and those born after the second title allocation (children between 0 and 4 years old). For the 5-13 age group, column (6) of Table 7 shows a significant reduction of 36% in the number of household heads' children for the early-treated households. This decrease corresponds to 8.5% of the sample average of total household heads' offspring.[29] The effect, instead, is not significant for the late-treated group. This result is reassuring, since treatment could not have affected fertility for the late-treated group in the 5-13 age bracket as these children were born before titling for this group. For the household heads' children in the 0-4 age group, column (8) of Table 7 shows that the effect, however, is not significant for both the late and early treated households. Still, in both cases the estimated coefficients correspond to a reduction in the number of offspring of more than 20%.[30]

[29] This fertility effect does not depend on whether a woman or a man received the title. According to the expropriation law, the titles were awarded to both the household head and her/his spouse (if married or cohabitating). In our sample, 95.2 percent of the titled parcels include a woman as owner or co-owner.

[30] A plausible explanation for the lack of significant effects on the number of household heads' offspring of 0-4 years of age is that by 2003 our household heads were fairly old and, therefore, their fertility rate is low. Remember that three quarters of them were already the heads of their households at the time of the occupation (see footnote 10). In our sample, the average household head age is 46 years old, and the average age of the female head (the household head if female or the age of his spouse if male) is 43.7 years old.

The robustness of these results regarding the methodological concerns discussed in section IV is presented in Appendix Tables A.6 to A.11.[31] Moreover, the results are robust to controlling for whether the original squatter is the current household head, for the age of the household head, and, in the regressions for the household heads' children of 5-13 and 0-4 years of age, for the number of offspring of the household head previously born. In summary, we find that entitled households are smaller than untitled ones. The larger size of households in the untitled parcels is due to both a larger number of offspring of the household head and a more frequent presence of non-nuclear relatives.

V.3. Effects on School Performance

The seminal work of Becker and Lewis (1973) advanced the presence of parental trade-offs between the quantity and the quality of children. This trade-off appears because limited parents' time and resources are spread over more children (see Rosenzweig and Wolpin (1980) and Hanushek (1992) for empirical evidence). If land titling causes a reduction in fertility, it could also induce households to increase educational investments in their children. Moreover, land titling may have beneficial effects on the education of household heads' offspring through the reduction in the number of extended family members living in the house and the potential health consequences of improved housing (Goux and Maurin, 2005).

We explore this hypothesis by looking at differences in educational outcomes. In Table 8 we analyze the performance of children at school. We collapse differences in school dropout, grade repetition, and age of school initiation, in the School Achievement variable, which is the difference between the school grade the child is currently attending or the maximum grade attained (if she/he is not currently attending school) minus the grade corresponding to her/his age. For the offspring of the household head in the 5-13 age group in the early-treated households (the group of children for which in column (6) of Table 7 we found a reduction in the number of members), column (2) of Table 8

[31] For those results that should only be present for the early-treated group, we cannot exploit our early versus late titling strategy to rule out the possibility that the results are generated by attrition. In these cases, we only deal with the attrition concern using the matching estimators.

shows a large effect on School Achievement.[32] The children in the control group show an average delay of 1.09 years in their school achievement, whereas this delay is 0.42 years shorter for the children in the early-titled parcels. The effect is not significant for the children in the late-treated parcels, which had not shown a reduction in their number for this age group.

Table 8 – **Education**

Offspring of the Household Head (5-13 years old)

	School Achievement		School Absenteeism	
	(1)	(2)	(3)	(4)
Property Right	0.15		-0.39**	
	(1.28)		(2.43)	
Property Right 1989		0.42**		-0.55**
		(2.20)		(2.12)
Property Right 1998		0.05		-0.33*
		(0.40)		(1.86)
Control Group Mean	-1.09	-1.09	0.67	0.67

Notes: In columns (1) and (2), the dependent variable is the difference between the school grade each child is currently attending or the maximum grade attained (if not attending school) minus the grade corresponding to the child age. In columns (3) and (4), the dependent variable is the number of days the child missed school out of the last five days of classes. The child is the unit of observation. All the regressions control for child age, child gender, and parcel and original squatter pre-treatment characteristics (parcel size; distance to creek; distance to nearest non-squatted area; block corner; age, gender, nationality, and years of education of the original squatter; and nationality and years of education of father and mother of the original squatter). The robustness of the results and detailed variable definitions are presented in Appendix Tables A.12 and A.13. Absolute values of t statistics are in parentheses. * Significant at 10%; ** significant at 5%.

How large is the effect of land titling on school achievement? In order to answer this question we need to establish a benchmark. Consider the successful Mexican anti-poverty program Progresa, which provides monetary transfers to families that are contingent upon their children's regular school attendance. The estimates in Behrman et al. (2005) indicate that if children were to participate in the program between their 6 to 14 years of age, they would experience an increase of 0.6 years in average educational attainment levels, an effect comparable to the one we estimate for land titling for a similar age group.

[32] Schooling is mandatory in Argentina since pre-school (age 5). Similar results are obtained if we limit the sample to children in the 6-13 age group. The regressions in Table 8 are estimated at the child level and include controls for child age and gender. In addition to clustering the standard errors at the block and former owner levels, Appendix Tables A.12 and A.13 report standard errors clustered at the household level, together with the other robustness checks.

Finally, columns (3) and (4) of Table 8 show that, associated to titling status, there is a reduction of 0.4 days in the number of days children missed school out of the last five days of classes. In this case, the effect is present for both the early and late treated, suggesting that impacts on this variable could be more immediate.

V.4. Effects on Performance in the Credit and Labor Markets

Financial markets in developing countries are highly imperfect and these imperfections are particularly severe for the poor. The possession of formal property rights could allow the use of land as collateral, improving the access of the poor to the credit markets (Feder et al., 1988). In turn, this collateralized credit could be invested as capital, increasing labor productivity and income (De Soto, 2000). Moreover, land titling may have direct labor market effects if it relieves families from the need of leaving adults at home to protect their houses from occupation by other squatters (Field, 2003). We investigate whether land titles improve the performance of households in the credit and labor markets.

In Table 9 we find no differences across groups in the access to credit cards and banking accounts; and to non-mortgage formal credit from banks, the government, labor unions or cooperatives. Indeed, these families show very little access to these types of formal credit. The access to credit is higher for informal credit from relatives, colleagues, neighbours, and friends, and for on-trust credit that families receive from the stores in which they perform their daily purchases. However, titling status shows no effect on access to these informal sources of credit.

Table 9 - **Access To Credit**

	Credit Card & Bank Account (1)	Non-Mortgage Loan Received (2)	Informal Credit (3)	Grocery Store Credit (4)
Property Right	-0.01	0.01	-0.06	0.01
	(0.71)	(0.19)	(1.00)	(0.16)
Control Group Mean	0.05	0.09	0.41	0.27

	Mortgage Loan Received	
	(5)	(6)
Property Right	0.02	
	(1.58)	
Property Right 1989		0.04***
		(3.19)
Property Right 1998		0.00
		(0.06)
Control Group Mean	0.00	0.00

Notes: Credit Card & Bank Account is a dummy variable that equals 1 if the household head has a credit card or bank account, and 0 otherwise. Non-Mortgage Loan Received, Informal Credit, Grocery Store Credit, and Mortgage Loan Received are dummy variables that equal 1 if the household has received formal non-mortgage credit; informal credit from relatives, colleagues, neighbors or friends; on trust credit from grocery stores; and formal mortgage credit; respectively, and 0 otherwise. The household is the unit of observation. All the regressions control for parcel and original squatter pre-treatment characteristics: parcel size; distance to creek; distance to nearest non-squatted area; block corner; age, gender, nationality, and years of education of the original squatter; and nationality and years of education of father and mother of the original squatter. The complete regressions and detailed variable definitions are presented in Appendix Table A.14. Absolute values of t statistics are in parentheses. *** Significant at 1%.

In the second panel of Table 9 we analyze the impact of titling on the access to mortgage loans. For this exercise, we separate the effect for the early and late treatment households. The late treatment group was not yet in a legal situation to mortgage the land at the time of the survey, as the ten years established by the expropriation law before allowing property transfers had not elapsed since the 1998 titling (see section II). For the early titled group, although we find a statistically significant effect of land titling on the access to mortgage markets, the effect is quantitatively modest. Only 4% of the early-treated households have ever received a mortgage loan.

Finally, we investigate the effect of land titling on labor market outcomes. For this exercise, a further advantage of our experiment is that treated and control households are all in the same labor market. In Table 10, we show no differences between control and treatment households in household head income, total household income, total household income per capita, total household income per adult, and employment status of the household head. There are also no significant differences in the pension status of the household heads, in female employment, and in child labor.[33] In spite of land titling, these families are still very poor. Relative to the population of the Buenos Aires

[33] In our population, the frequency of child labor (for children 10-14 years old) is 0% in the treatment group, and 1.05% in the control group (the difference is not significant). These figures coincide with the negligible levels of child labor for the Buenos Aires metropolitan area (0.18% for

metropolitan area, the households in our sample show low income levels. Their average household income level is in the 25th centile of the income distribution in the official household survey (EPH, May 2003), while their average per capita income is in the 14th centile of the distribution. Moreover, their average household income amounts to only 38% of the official poverty line, and 94% of the households are below this line.[34]

The modest effects of titling on the credit markets should not be too surprising. Previous evidence on the credit effects of land titling is ambiguous (see, among others, Feder et al., 1988; Place and Migot-Adholla, 1998; Carter and Olinto, 2002; Field and Torero, 2003; and Calderon, 2004. Also see Woodruff, 2001, for a critical review of De Soto's book). Real estate possession does not seem to be a sufficient condition for access to formal credit, which is largely restricted in Argentina to formal workers with requirements of minimum tenure in the current job and high wages. In addition, potential lenders probably evaluate that success in the legal eviction of households in these socioeconomic groups in the event of default is unlikely (Arrunada, 2003) and, if feasible, the cost of the legal process may exceed the market value of the parcels. Moreover, the few observed mortgage loans are probably not invested in business projects. The poor may lack good productive projects, or they may consider the land too valuable to be jeopardized in an entrepreneurial activity. Thus, the modest credit effects do not further translate into labor market differences.[35]

Table 10 - **Labor Market**

	Household Head Income (1)	Total Household Income (2)	Total Household Income per Capita (3)	Total Household Income per Adult (4)	Employed Household Head (5)
Property Right	-27.35 (1.10)	-43.56 (1.27)	1.04 (0.13)	-4.45 (0.38)	0.03 (0.63)

the overall and 0.29% for the first income quintile according to the official household survey of May 2003).

[34] The results on the effects of land titling on the credit and labor markets remain unaltered when we perform all our robustness checks. For the sake of space, Appendix Tables A.14 and A.15 only include the main specification and the early-late regression. The other specifications are available from the authors upon request.

[35] The coexistence of strong investment effects and weak credit market access in our natural experiment can be interpreted as an illustration of Acemoglu and Johnson (2005)'s distinction between institutions that protect citizens against expropriation and institutions that enable private contracts.

Control Group Mean	272.54	374.59	73.72	118.73	0.73

Notes: Household Head Income is the total income earned by the household head in the previous month. Total Household Income is the total income earned by all the household members in the previous month. Total Household Income per Capita is Total Household Income divided by the number of household members. Total Household Income per Adult is Total Household Income divided by the number of household members older than 16 years old. All income variables are measured in Argentine pesos. Employed Household Head is a dummy variable that equals 1 if the household head was employed the week before the survey, and 0 otherwise. The household is the unit of observation. All the regressions control for parcel and original squatter pre-treatment characteristics: parcel size; distance to creek; distance to nearest non-squatted area; block corner; age, gender, nationality, and years of education of the original squatter; and nationality and years of education of father and mother of the original squatter. The complete regressions are presented in Appendix Table A.15. Absolute values of t statistics are in parentheses.

VI. Conclusions

Land-titling programs have been recently advocated in policy and business circles as a powerful anti-poverty instrument, and several countries in the developing world adopted or are in the process of adopting interventions to provide squatters with formal titles of the land they occupy. The main premise is that land titling could allow the poor to access the credit markets, transforming their wealth into capital and, hence, increase their labor productivity and income. Rigorous evidence supporting these hypothesized effects is, however, scarce and ambiguous. Are land-titling programs an effective tool to rapidly reduce poverty? What are the effects of land titling?

Identifying the causal effects of land titling is difficult because the allocation of property rights across households is not random, but typically endogenous in equations describing the outcomes under study. Previous work exploited standard exclusion restrictions or variability in the timing of policy interventions to deal with this selection problem. In this paper, instead, we exploit a natural experiment in the allocation of land titles across squatters in a poor suburban area of Buenos Aires, Argentina. We believe that our strategy credibly identifies the effect of land titling: untitled and entitled households were extremely similar before titling, the parcels they inhabit are identical, and the allocation of property rights did not depend on the characteristics of the squatters.

We only find a modest but positive effect of land titling on access to mortgage credit, and no impact on access to other forms of credit. Moreover, we do not find any effect on the labor income of the treated households. Should we therefore conclude that entitling the

urban poor renders them little progress? Not necessarily. We showed that moving a poor household from *usufructuary* land rights to full property rights substantially increased investment in the houses. Moreover, land titling reduced the fertility of the household heads (especially when treated being young), and the presence of extended family members. Also, these smaller families invested more in the education of their children. In sum, entitling the poor increases their investment both in the house and in the human capital of their children, which should contribute to reduce the poverty of the next generation.

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Table 6 – **Robustness of Housing Investment Results: Good Walls**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Property Right	0.20***	0.19***	0.19***	0.14***	0.20***	0.20***	0.20**		0.18***		0.21***	0.11**
	(3.47)	(3.32)	(3.37)	(2.65)	(3.18)	(4.20)	(3.06)		(2.62)		(3.34)	(2.35)
Property Right Offer								0.16***				
								(2.59)				
Property Right 1989										0.23***		
										(2.77)		
Property Right 1998										0.19***		
										(2.90)		
Parcel Surface	-0.00***		-0.00***	-0.00**	-0.00**	-0.00***	-0.00***	-0.00**	-0.00***	-0.00***		-0.00**
	(2.69)		(2.65)	(2.19)	(2.23)	(4.15)	(4.13)	(2.47)	(2.74)	(2.70)		(2.44)
Distance to Creek	0.07**		0.07**	0.02	0.07**	0.07***	0.07**	0.07**	0.07**	0.07**		0.08***
	(2.31)		(2.29)	(0.88)	(2.47)	(2.99)	(2.71)	(2.26)	(2.21)	(2.11)		(3.27)
Block Corner	-0.06		-0.09	-0.01	-0.06	-0.06	-0.06	-0.04	-0.06	-0.06		-0.09
	(0.66)		(1.15)	(0.17)	(0.68)	(0.83)	(0.70)	(0.45)	(0.71)	(0.67)		(1.28)
Distance to Non-Squatted	0.03		0.04	0.05*	0.03	0.03	0.03	0.03	0.03	0.03		0.02
Area	(0.97)		(1.49)	(1.80)	(0.84)	(1.65)	(1.00)	(0.91)	(0.96)	(0.97)		(0.89)
Age of Original Squatter<50	0.01			-0.02	0.01	0.01	0.01	0.00	0.01	0.01		
	(0.18)			(0.47)	(0.18)	(0.32)	(0.26)	(0.03)	(0.16)	(0.16)		
Female Original Squatter	0.05			-0.05	0.05	0.05	0.05	0.05	0.05	0.05		
	(0.81)			(0.99)	(0.82)	(0.80)	(0.82)	(0.83)	(0.81)	(0.81)		
Argentine Original Squatter	-0.16			-0.12	-0.16	-0.16	-0.16	-0.17	-0.17	-0.16		
	(1.12)			(0.95)	(1.23)	(1.24)	(1.10)	(1.21)	(1.16)	(1.13)		
Years of Education of the	-0.02			-0.01	-0.02	-0.02	-0.02	-0.02	-0.02	-0.02		
Original Squatter	(1.03)			(0.60)	(0.98)	(1.37)	(1.35)	(1.05)	(1.02)	(1.04)		
Argentine Father of the	-0.23**			-0.16	-0.23**	-0.23***	-0.23*	-0.23**	-0.23**	-0.23**		
Original Squatter	(2.03)			(1.52)	(2.59)	(3.53)	(2.78)	(2.02)	(1.99)	(2.01)		
Years of Education of	0.02			-0.01	0.02	0.02	0.02	0.01	0.01	0.02		
Original Squatter's Father	(0.60)			(0.35)	(0.66)	(0.78)	(0.74)	(0.54)	(0.55)	(0.61)		
Argentine Mother of the	0.27**			0.15	0.27**	0.27	0.27**	0.28**	0.27**	0.27**		
Original Squatter	(2.38)			(1.42)	(2.18)	(1.49)	(1.29)	(2.45)	(2.41)	(2.36)		
Years of Education of	0.00			0.03	0.00	0.00	0.00	0.00	0.00	0.00		
Original Squatter's Mother	(0.08)			(0.96)	(0.08)	(0.12)	(0.11)	(0.11)	(0.11)	(0.07)		
Constant	0.71***	0.50***	0.58***	0.66***	0.71**	0.71***	0.71**	0.72***	0.73***	0.72***		0.57***
	(3.09)	(11.92)	(3.81)	(3.54)	(2.49)	(4.54)	(4.15)	(3.05)	(3.18)	(3.11)		(4.78)
F-stat										0.16		
Observations	295	295	295	403	295	295	295	295	295	295	273	441

Notes: The dependent variable is a dummy that equals 1 if the house has walls of good quality (brick, stone, block or concrete with exterior siding), and 0 otherwise. The parcel is the unit of observation. Column (1) is summarized in Column 1 of Table 5. Column (2) includes no controls, and Column (3) only controls for parcel characteristics. Column (4) adds the observations for the San Martin neighborhood. Huber-White standard errors clustered at the block level are used in Column (5), and at the former owner level in Column (6). Column (7) considers block bootstrapped standard errors and significance levels by former owner. The reduced-form regression on the intention-to-treat variable Property Right Offer is displayed in Column (8). The 2SLS regression (instrumenting the treatment variable Property Right with the intention-to-treat variable Property Right Offer) is presented in Column (9). Column (10) shows separately the effect of early and late treatments. The F-stat tests the null hypothesis: Property Right 1989 = Property Right 1998. Column (11) presents the matching estimate using the propensity score of the probability of attrition (with bootstrapped standard errors). The regression in Column (12) is estimated on all the interviewed households (for any time of household arrival). The control variables are described in Appendix Table A.1. Absolute value of t statistics in parentheses. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Table A.1 - GOOD ROOF												
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Property Right	0.15**	0.14**	0.15***	0.14***	0.15**	0.15**	0.15**		0.16**		0.12*	0.12**
	(2.49)	(2.41)	(2.65)	(2.67)	(2.26)	(2.68)	(2.32)		(2.22)		(1.66)	(2.55)
Property Right Offer								0.14**				
								(2.22)				
Property Right 1989										0.22***		
										(2.63)		
Property Right 1998										0.11*		
										(1.66)		
Parcel Surface	0.00		0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00		0.00
	(0.42)		(0.46)	(1.45)	(0.36)	(0.24)	(0.24)	(0.59)	(0.43)	(0.38)		(0.98)
Distance to Creek	0.03		0.03	0.02	0.03	0.03	0.03	0.04	0.03	0.02		0.03
	(0.94)		(1.00)	(0.75)	(0.84)	(1.31)	(0.86)	(1.03)	(0.95)	(0.55)		(1.07)
Block Corner	0.08		0.08	0.12	0.08	0.08	0.08	0.10	0.08	0.08		0.02
	(0.91)		(1.02)	(1.60)	(0.94)	(0.68)	(0.72)	(1.12)	(0.91)	(0.91)		(0.26)
Distance to Non-Squatted	-0.01		-0.02	-0.02	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01		-0.01
Area	(0.31)		(0.60)	(0.56)	(0.31)	(0.54)	(0.38)	(0.36)	(0.31)	(0.31)		(0.58)
Age of Original Squatter<50	-0.01			0.01	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01		
	(0.13)			(0.11)	(0.14)	(0.23)	(0.16)	(0.25)	(0.13)	(0.20)		
Female Original Squatter	-0.04			-0.04	-0.04	-0.04	-0.04	-0.04	-0.04	-0.04		
	(0.64)			(0.73)	(0.65)	(1.20)	(0.83)	(0.64)	(0.64)	(0.65)		
Argentine Original Squatter	0.18			0.13	0.18	0.18	0.18	0.17	0.18	0.17		
	(1.21)			(0.98)	(1.13)	(1.38)	(1.07)	(1.16)	(1.21)	(1.15)		
Years of Education of the	0.00			0.01	0.00	0.00	0.00	0.00	0.00	0.00		
Original Squatter	(0.22)			(0.50)	(0.21)	(0.13)	(0.18)	(0.19)	(0.22)	(0.19)		
Argentine Father of the	-0.02			0.05	-0.02	-0.02	-0.02	-0.03	-0.02	-0.01		
Original Squatter	(0.15)			(0.47)	(0.14)	(0.21)	(0.22)	(0.21)	(0.16)	(0.11)		
Years of Education of	-0.00			-0.01	-0.00	-0.00	-0.00	-0.00	-0.00	0.00		
Original Squatter's Father	(0.03)			(0.51)	(0.03)	(0.05)	(0.04)	(0.05)	(0.02)	(0.00)		
Argentine Mother of the	-0.09			-0.11	-0.09	-0.09	-0.09	-0.08	-0.09	-0.09		
Original Squatter	(0.73)			(1.02)	(0.62)	(0.62)	(0.63)	(0.68)	(0.74)	(0.78)		
Years of Education of	0.00			0.01	0.00	0.00	0.00	0.00	0.00	0.00		
Original Squatter's Mother	(0.17)			(0.19)	(0.18)	(0.26)	(0.22)	(0.14)	(0.16)	(0.14)		
Constant	0.12	0.32***	0.22	0.12	0.12	0.12	0.12	0.11	0.12	0.16		0.22*
	(0.52)	(7.53)	(1.41)	(0.63)	(0.50)	(0.44)	(0.43)	(0.45)	(0.49)	(0.69)		(1.85)
F-stat										1.48		
Observations	297	297	297	405	297	297	297	297	297	297	276	445

Notes: The dependent variable is a dummy that equals 1 if the house has a roof of good quality (asphalt shingle, membrane, tile, slab, slate or clay roof tile), and 0 otherwise. The parcel is the unit of observation. Column (1) is summarized in Column 2 of Table 5. Column (2) includes no controls, and Column (3) only controls for parcel characteristics. Column (4) adds the observations for the San Martin neighborhood. Huber-White standard errors clustered at the block level are used in Column (5), and at the former owner level in Column (6). Column (7) considers block bootstrapped standard errors and significance levels by former owner. The reduced-form regression on the intention-to-treat variable Property Right Offer is displayed in Column (8). The 2SLS regression (instrumenting the treatment variable Property Right with the intention-to-treat variable Property Right Offer) is presented in Column (9). Column (10) shows separately the effect of early and late treatments. The F-stat tests the null hypothesis: Property Right 1989 = Property Right 1998. Column (11) presents the matching estimate using the propensity score of the probability of attrition (with bootstrapped standard errors). The regression in Column (12) is estimated on all the interviewed households (for any time of household arrival). Parcel Surface is measured in squared meters. Distance to Creek and Distance to Non-Squatted Area are measured in blocks. For deceased original squatters, the age was calculated from year of death and age at death. We use (non-reported) dummies for missing data on original squatter's age, and original squatter parents' nationality and years of education (a total of ten observations). Absolute value of t statistics in parentheses. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Table A.2 – CONSTRUCTED SURFACE

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Property Right	8.27**	7.99**	9.89***	5.30*	8.27**	8.27	8.27**		9.87**		8.55**	8.61***
	(2.34)	(2.33)	(2.87)	(1.68)	(2.15)	(1.44)	(1.22)		(2.41)		(2.18)	(3.02)
Property Right Offer								9.06**				
								(2.41)				
Property Right 1989										10.34**		
										(2.09)		
Property Right 1998										7.18*		
										(1.80)		
Parcel Surface	-0.01		-0.00	0.01	-0.01	-0.01	-0.01	-0.00	-0.01	-0.01		0.01
	(0.36)		(0.02)	(0.63)	(0.37)	(0.35)	(0.36)	(0.12)	(0.31)	(0.37)		(0.68)
Distance to Creek	5.90***		6.42***	2.63**	5.90***	5.90***	5.9**	6.25***	6.07***	5.54***		4.80***
	(3.03)		(3.42)	(2.09)	(3.11)	(3.06)	(2.85)	(3.17)	(3.10)	(2.73)		(3.15)
Block Corner	4.38		3.98	3.03	4.38	4.38	4.38	5.78	4.57	4.39		8.37**
	(0.87)		(0.82)	(0.67)	(0.77)	(1.59)	(0.99)	(1.13)	(0.91)	(0.87)		(2.10)
Distance to Non-Squatted	3.67**		4.51***	3.05*	3.67	3.67	3.67*	3.58**	3.69**	3.67**		2.02
Area	(2.06)		(2.60)	(1.93)	(1.60)	(1.43)	(1.32)	(2.01)	(2.07)	(2.06)		(1.36)
Age of Original Squatter<50	-1.68			-2.63	-1.68	-1.68	-1.68	-2.02	-1.59	-1.80		
	(0.48)			(0.88)	(0.51)	(0.51)	(0.52)	(0.58)	(0.45)	(0.51)		
Female Original Squatter	-0.70			-1.99	-0.70	-0.70	-0.7	-0.67	-0.69	-0.73		
	(0.20)			(0.66)	(0.21)	(0.23)	(0.24)	(0.19)	(0.19)	(0.20)		
Argentine Original Squatter	-7.54			-8.07	-7.54	-7.54	-7.54	-7.62	-7.18	-7.76		
	(0.87)			(1.07)	(0.85)	(1.48)	(0.97)	(0.88)	(0.83)	(0.90)		
Years of Education of the	-0.08			-0.14	-0.08	-0.08	-0.08	-0.12	-0.09	-0.09		
Original Squatter	(0.08)			(0.17)	(0.08)	(0.06)	(0.07)	(0.12)	(0.09)	(0.09)		
Argentine Father of the	-5.45			-2.43	-5.45	-5.45	-5.45	-6.05	-5.68	-5.28		
Original Squatter	(0.79)			(0.39)	(0.83)	(1.14)	(1.05)	(0.88)	(0.83)	(0.77)		
Years of Education of	-3.31**			-3.22**	-3.31**	-3.31*	-3.31**	-3.29**	-3.24**	-3.29**		
Original Squatter's Father	(2.06)			(2.38)	(2.21)	(1.89)	(1.96)	(2.05)	(2.01)	(2.04)		
Argentine Mother of the	10.73			8.19	10.73	10.73***	10.73	10.92	10.54	10.51		
Original Squatter	(1.55)			(1.28)	(1.27)	(3.04)	(2.07)	(1.58)	(1.53)	(1.52)		
Years of Education of	3.59**			3.58**	3.59*	3.59*	3.59**	3.50**	3.54**	3.57**		
Original Squatter's Mother	(2.10)			(2.28)	(1.68)	(1.78)	(1.89)	(2.04)	(2.06)	(2.08)		
Constant	54.32***	67.63***	46.28***	58.62***	54.32***	54.32***	54.32***	52.19***	52.66***	55.53***		49.97***
	(3.98)	(26.49)	(5.00)	(5.32)	(3.39)	(4.32)	(3.95)	(3.77)	(3.81)	(4.02)		(6.78)
F-stat										0.36		
Observations	299	299	299	407	299	299	299	299	299	299	277	447

Notes: The dependent variable is the constructed surface in squared meters. The parcel is the unit of observation. Column (1) is summarized in Column 3 of Table 5. Column (2) includes no controls, and Column (3) only controls for parcel characteristics. Column (4) adds the observations for the San Martin neighborhood. Huber-White standard errors clustered at the block level are used in Column (5), and at the former owner level in Column (6). Column (7) considers block bootstrapped standard errors and significance levels by former owner. The reduced-form regression on the intention-to-treat variable Property Right Offer is displayed in Column (8). The 2SLS regression (instrumenting the treatment variable Property Right with the intention-to-treat variable Property Right Offer) is presented in Column (9). Column (10) shows separately the effect of early and late treatments. The F-stat tests the null hypothesis: Property Right 1989 = Property Right 1998. Column (11) presents the matching estimate using the propensity score of the probability of attrition (with bootstrapped standard errors). The regression in Column (12) is estimated on all the interviewed households (for any time of household arrival). The control variables are described in Appendix Table A.1. Absolute value of t statistics in parentheses. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Table A.3 - CONCRETE SIDEWALK												
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Property Right	0.11**	0.08	0.12**	0.10**	0.11	0.11	0.11**		0.10		0.08	0.16***
	(2.18)	(1.43)	(2.24)	(2.41)	(1.60)	(1.55)	(1.32)		(1.63)		(1.42)	(3.85)
Property Right Offer								0.09				
								(1.62)				
Property Right 1989										0.16**		
										(2.14)		
Property Right 1998										0.09		
										(1.55)		
Parcel Surface	-0.00		-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00		-0.00
	(0.44)		(0.69)	(0.51)	(0.40)	(0.24)	(0.25)	(0.33)	(0.47)	(0.46)		(1.15)
Distance to Creek	0.09***		0.09***	0.08***	0.09**	0.09**	0.09***	0.09***	0.09***	0.09***		0.11***
	(3.29)		(3.19)	(4.64)	(2.40)	(2.25)	(1.74)	(3.25)	(3.22)	(2.90)		(4.89)
Block Corner	-0.12		-0.14**	-0.13**	-0.12	-0.12*	-0.12	-0.11	-0.12	-0.12		-0.08
	(1.57)		(1.98)	(2.17)	(1.65)	(2.01)	(1.78)	(1.38)	(1.59)	(1.57)		(1.29)
Distance to Non-Squatted	-0.07**		-0.07***	-0.05**	-0.07*	-0.07	-0.07**	-0.07**	-0.07**	-0.07**		-0.09***
Area	(2.49)		(2.66)	(2.27)	(1.68)	(1.69)	(1.54)	(2.50)	(2.49)	(2.49)		(4.14)
Age of Original Squatter<50	-0.10*			-0.07*	-0.10**	-0.10*	-0.10*	-0.11**	-0.10*	-0.10*		
	(1.92)			(1.78)	(2.03)	(2.09)	(2.55)	(2.02)	(1.94)	(1.97)		
Female Original Squatter	-0.05			-0.06	-0.05	-0.05	-0.05	-0.05	-0.05	-0.05		
	(0.95)			(1.54)	(0.90)	(1.45)	(1.04)	(0.95)	(0.96)	(0.97)		
Argentine Original Squatter	0.06			0.05	0.06	0.06	0.06	0.06	0.06	0.06		
	(0.50)			(0.47)	(0.41)	(0.74)	(0.59)	(0.45)	(0.48)	(0.47)		
Years of Education of the	-0.02			-0.02	-0.02*	-0.02**	-0.02	-0.02	-0.02	-0.02		
Original Squatter	(1.44)			(1.50)	(1.81)	(2.75)	(2.34)	(1.45)	(1.43)	(1.46)		
Argentine Father of the	-0.03			-0.02	-0.03	-0.03	-0.03	-0.04	-0.03	-0.03		
Original Squatter	(0.34)			(0.19)	(0.40)	(0.32)	(0.28)	(0.36)	(0.32)	(0.30)		
Years of Education of	0.03			0.03	0.03	0.03*	0.03	0.03	0.03	0.03		
Original Squatter's Father	(1.37)			(1.48)	(1.55)	(1.74)	(1.57)	(1.33)	(1.34)	(1.39)		
Argentine Mother of the	-0.02			-0.03	-0.02	-0.02	-0.02	-0.02	-0.02	-0.03		
Original Squatter	(0.21)			(0.34)	(0.15)	(0.23)	(0.19)	(0.16)	(0.19)	(0.25)		
Years of Education of	-0.03			-0.03	-0.03	-0.03	-0.03	-0.03	-0.03	-0.03		
Original Squatter's Mother	(1.15)			(1.35)	(1.11)	(0.99)	(1.08)	(1.17)	(1.14)	(1.17)		
Constant	0.84***	0.67***	0.71***	0.81***	0.84***	0.84**	0.84***	0.84***	0.85***	0.86***		0.68***
	(4.10)	(17.19)	(5.17)	(5.54)	(3.27)	(2.26)	(2.55)	(4.06)	(4.13)	(4.18)		(6.34)
F-stat										0.71		
Observations	300	300	300	408	300	300	300	300	300	300	278	448

Notes: The dependent variable is a dummy that equals 1 if the house has a sidewalk made of concrete, and 0 otherwise. The parcel is the unit of observation. Column (1) is summarized in Column 4 of Table 5. Column (2) includes no controls, and Column (3) only controls for parcel characteristics. Column (4) adds the observations for the San Martin neighborhood. Huber-White standard errors clustered at the block level are used in Column (5), and at the former owner level in Column (6). Column (7) considers block bootstrapped standard errors and significance levels by former owner. The reduced-form regression on the intention-to-treat variable Property Right Offer is displayed in Column (8). The 2SLS regression (instrumenting the treatment variable Property Right with the intention-to-treat variable Property Right Offer) is presented in Column (9). Column (10) shows separately the effect of early and late treatments. The F-stat tests the null hypothesis: Property Right 1989 = Property Right 1998. Column (11) presents the matching estimate using the propensity score of the probability of attrition (with bootstrapped standard errors). The regression in Column (12) is estimated on all the interviewed households (for any time of household arrival). The control variables are described in Appendix Table A.1. Absolute value of t statistics in parentheses. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Table A.4 – OVERALL HOUSING APPEARANCE

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Property Right	8.42***	7.45***	8.07***	5.39***	8.42***	8.42***	8.42***		10.17***		8.23***	6.97***
	(3.65)	(3.38)	(3.58)	(2.67)	(3.91)	(2.98)	(2.03)		(3.80)		(3.63)	(3.87)
Property Right Offer								9.34***				
								(3.81)				
Property Right 1989										6.27*		
										(1.95)		
Property Right 1998										9.54***		
										(3.68)		
Parcel Surface	-0.02		-0.02	-0.00	-0.02	-0.02*	-0.02	-0.01	-0.02	-0.02		-0.02*
	(1.28)		(1.46)	(0.47)	(1.49)	(2.00)	(1.88)	(0.89)	(1.19)	(1.25)		(1.87)
Distance to Creek	2.47*		2.32*	-0.26	2.47*	2.47**	2.47**	2.84**	2.66**	2.83**		3.01***
	(1.95)		(1.90)	(0.32)	(1.85)	(2.88)	(2.31)	(2.21)	(2.08)	(2.14)		(3.13)
Block Corner	0.13		-0.70	1.94	0.13	0.13	0.13	1.59	0.34	0.13		0.02
	(0.04)		(0.22)	(0.68)	(0.04)	(0.03)	(0.04)	(0.48)	(0.10)	(0.04)		(0.01)
Distance to Non-Squatted	-0.00		-0.14	-0.19	-0.00	-0.00	-0.00	-0.09	0.02	-0.01		-0.49
Area	(0.00)		(0.13)	(0.18)	(0.00)	(0.00)	(0.00)	(0.08)	(0.02)	(0.01)		(0.52)
Age of Original Squatter<50	0.61			0.12	0.61	0.61	0.61	0.26	0.71	0.73		
	(0.26)			(0.06)	(0.31)	(0.43)	(0.35)	(0.11)	(0.31)	(0.32)		
Female Original Squatter	-3.21			-3.98**	-3.21	-3.21	-3.21	-3.18	-3.20	-3.18		
	(1.39)			(2.08)	(1.46)	(1.71)	(1.84)	(1.38)	(1.38)	(1.37)		
Argentine Original Squatter	6.82			2.65	6.82	6.82**	6.82	6.77	7.22	7.04		
	(1.21)			(0.55)	(0.95)	(2.33)	(1.20)	(1.20)	(1.28)	(1.25)		
Years of Education of the	0.70			0.48	0.70	0.70	0.70	0.66	0.69	0.72		
Original Squatter	(1.08)			(0.93)	(0.88)	(0.73)	(0.99)	(1.02)	(1.06)	(1.11)		
Argentine Father of the	-9.82**			-5.85	-9.82**	-9.82**	-9.82**	-10.46**	-10.07**	-9.99**		
Original Squatter	(2.19)			(1.45)	(2.20)	(2.40)	(2.32)	(2.34)	(2.25)	(2.23)		
Years of Education of	-1.13			-0.65	-1.13	-1.13**	-1.13	-1.10	-1.05	-1.16		
Original Squatter's Father	(1.08)			(0.75)	(1.37)	(2.17)	(1.86)	(1.05)	(1.00)	(1.10)		
Argentine Mother of the	-1.94			-1.18	-1.94	-1.94	-1.94	-1.75	-2.14	-1.71		
Original Squatter	(0.43)			(0.29)	(0.33)	(0.45)	(0.35)	(0.39	(0.47)	(0.38)		
Years of Education of	-1.22			-0.81	-1.22	-1.22	-1.22	-1.32	-1.28	-1.19		
Original Squatter's Mother	(1.09)			(0.81)	(1.00)	(0.97)	(1.86)	(1.18)	(1.14)	(1.07)		
Constant	34.15***	22.71***	24.63***	33.20***	34.15***	34.15***	34.15***	31.85***	32.33***	32.89***		24.73***
	(3.84)	(13.82)	(4.08)	(4.72)	(3.71)	(3.89)	(3.79)	(3.53)	(3.58)	(3.66)		(5.31)
F-stat										0.91		
Observations	299	299	299	407	299	299	299	299	299	299	277	446

Notes: The dependent variable measures the overall aspect of each house from 0 to 100 points assigned by the team of architects assuming 0 for the worst dwelling in a shanty town of Solano and 100 for a middle-class house in downtown Quilmes (the main locality of the county). Similar results are obtained using an alternative index that measures the overall aspect of each house from 0 to 100 points assuming 0 for the worst and 100 for the best houses within this neighborhood. The parcel is the unit of observation. Column (1) is summarized in Column 5 of Table 5. Column (2) includes no controls, and Column (3) only controls for parcel characteristics. Column (4) adds the observations for the San Martin neighborhood. Huber-White standard errors clustered at the block level are used in Column (5), and at the former owner level in Column (6). Column (7) considers block bootstrapped standard errors and significance levels by former owner. The reduced-form regression on the intention-to-treat variable Property Right Offer is displayed in Column (8). The 2SLS regression (instrumenting the treatment variable Property Right with the intention-to-treat variable Property Right Offer) is presented in Column (9). Column (10) shows separately the effect of early and late treatments. The F-stat tests the null hypothesis: Property Right 1989 = Property Right 1998. Column (11) presents the matching estimate using the propensity score of the probability of attrition (with bootstrapped standard errors). The regression in Column (12) is estimated on all the interviewed households (for any time of household arrival). The control variables are described in Appendix Table A.1. Absolute value of t statistics in parentheses. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Table A.5 - DURABLE CONSUMPTION					
	Refrigerator with Freezer	Refrigerator without Freezer	Washing Machine	TV	Cellular Phone
	(1)	(2)	(3)	(4)	(5)
Property Right	0.05	0.04	0.04	-0.01	-0.01
	(0.92)	(0.61)	(0.67)	(0.40)	(0.32)
Parcel Surface	-0.00	0.00	0.00	-0.00	0.00
	(1.28)	(0.82)	(0.39)	(0.44)	(0.98)
Distance to Creek	0.09***	-0.03	0.06**	0.06***	0.03*
	(2.98)	(0.83)	(2.11)	(3.12)	(1.88)
Block Corner	-0.04	0.09	0.05	0.03	0.02
	(0.47)	(1.05)	(0.56)	(0.67)	(0.57)
Distance to Non-Squatted Area	-0.01	0.02	0.06**	0.02	0.00
	(0.28)	(0.80)	(2.04)	(0.95)	(0.31)
Age of Original Squatter<50	0.03	-0.01	0.02	0.01	-0.01
	(0.58)	(0.20)	(0.32)	(0.38)	(0.32)
Female Original Squatter	0.02	-0.04	-0.05	0.03	0.00
	(0.35)	(0.74)	(0.86)	(1.04)	(0.20)
Argentine Original Squatter	0.05	-0.06	-0.02	-0.22***	-0.01
	(0.37)	(0.37)	(0.14)	(2.67)	(0.22)
Years of Education of the	0.02	-0.02	0.02	0.01	-0.00
Original Squatter	(1.02)	(1.29)	(1.30)	(1.55)	(0.64)
Argentine Father of the	-0.10	0.05	-0.04	0.06	0.04
Original Squatter	(0.87)	(0.43)	(0.39)	(0.90)	(0.90)
Years of Education of	0.01	0.01	-0.01	-0.00	0.02*
Original Squatter's Father	(0.21)	(0.18)	(0.54)	(0.14)	(1.91)
Argentine Mother of the	-0.03	0.06	-0.07	0.08	-0.02
Original Squatter	(0.30)	(0.50)	(0.62)	(1.18)	(0.49)
Years of Education of	0.01	-0.01	-0.05*	-0.01	-0.02**
Original Squatter's Mother	(0.35)	(0.38)	(1.77)	(0.82)	(2.06)
Constant	0.19	0.55**	0.66***	0.85***	-0.01
	(0.86)	(2.33)	(3.02)	(6.64)	(0.12)
Observations	311	311	311	312	312

Notes: The dependent variable of each column is a dummy that equals 1 if the household possesses the good, and 0 otherwise. The household is the unit of observation. The control variables are described in Appendix Table A.1. Absolute value of t statistics in parentheses. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Table A.6 – NUMBER OF HOUSEHOLD MEMBERS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Property Right	-0.95***	-0.87***	-0.86**	-0.92***	-0.95**	-0.95**	-0.95***		-1.19***		-0.87**
	(2.81)	(2.66)	(2.55)	(3.06)	(2.55)	(2.76)	(2.34)		(3.02)		(2.33)
Property Right Offer								-1.10***			
								(3.03)			
Property Right 1989										-1.18**	
										(2.50)	
Property Right 1998										-0.82**	
										(2.16)	
Parcel Surface	0.00		0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	
	(0.58)		(0.68)	(0.86)	(0.61)	(1.12)	(0.72)	(0.22)	(0.51)	(0.60)	
Distance to Creek	0.03		-0.04	0.08	0.03	0.03	0.03	-0.02	0.01	0.07	
	(0.19)		(0.20)	(0.66)	(0.19)	(0.21)	(0.15)	(0.12)	(0.05)	(0.39)	
Block Corner	-0.05		0.05	0.03	-0.05	-0.05	-0.05	-0.25	-0.07	-0.05	
	(0.10)		(0.10)	(0.06)	(0.11)	(0.12)	(0.11)	(0.51)	(0.15)	(0.10)	
Distance to Non-Squatted	0.03		-0.02	0.10	0.03	0.03	0.03	0.04	0.03	0.03	
Area	(0.17)		(0.11)	(0.64)	(0.17)	(0.19)	(0.12)	(0.21)	(0.16)	(0.17)	
Age of Original Squatter<50	1.04***			0.77***	1.04***	1.04***	1.04***	1.08***	1.02***	1.05***	
	(3.08)			(2.70)	(3.04)	(3.86)	(3.74)	(3.20)	(3.02)	(3.11)	
Female Original Squatter	-0.09			-0.09	-0.09	-0.09	-0.09	-0.75	-0.09	-0.08	
	(0.25)			(0.32)	(0.24)	(0.18)	(0.18)	(0.22)	(0.27)	(0.25)	
Argentine Original Squatter	-0.90			-0.69	-0.90	-0.90	-0.9	-0.92	-0.96	-0.87	
	(1.07)			(0.95)	(1.15)	(1.16)	(0.83)	(1.10)	(1.14)	(1.04)	
Years of Education of the	-0.08			-0.11	-0.08	-0.08	-0.08	-0.07	-0.07	-0.07	
Original Squatter	(0.79)			(1.49)	(0.78)	(0.54)	(0.60)	(0.74)	(0.77)	(0.76)	
Argentine Father of the	1.24*			1.09*	1.24*	1.24*	1.24*	1.33**	1.27*	1.22*	
Original Squatter	(1.85)			(1.77)	(1.78)	(1.92)	(1.51)	(1.98)	(1.89)	(1.82)	
Years of Education of	-0.18			-0.17	-0.18	-0.18	-0.18	-0.19	-0.19	-0.18	
Original Squatter's Father	(1.15)			(1.35)	(1.23)	(1.30)	(1.25)	(1.21)	(1.21)	(1.16)	
Argentine Mother of the	-0.75			-0.59	-0.75	-0.75	-0.75	-0.76	-0.73	-0.73	
Original Squatter	(1.11)			(0.95)	(1.06)	(1.48)	(0.78)	(1.13)	(1.08)	(1.07)	
Years of Education of	0.07			0.04	0.07	0.07	0.07	0.09	0.08	0.07	
Original Squatter's Mother	(0.43)			(0.29)	(0.48)	(0.46)	(0.45)	(0.53)	(0.47)	(0.45)	
Constant	6.41***	6.06***	5.72***	6.52***	6.41***	6.41***	6.41***	6.77***	6.67***	6.26***	
	(4.89)	(24.97)	(6.34)	(6.17)	(5.51)	(6.74)	(5.26)	(5.07)	(5.02)	(4.71)	
F-stat										0.51	
Observations	313	313	313	425	313	313	313	313	313	313	290

Notes: The dependent variable is the total number of household members. The household is the unit of observation. Column (1) is summarized in Column 1 of Table 7. Column (2) includes no controls, and Column (3) only controls for parcel characteristics. Column (4) adds the observations for the San Martin neighborhood. Huber-White standard errors clustered at the block level are used in Column (5), and at the former owner level in Column (6). Column (7) considers block bootstrapped standard errors and significance levels by former owner. The reduced-form regression on the intention-to-treat variable Property Right Offer is displayed in Column (8). The 2SLS regression (instrumenting the treatment variable Property Right with the intention-to-treat variable Property Right Offer) is presented in Column (9). Column (10) shows separately the effect of early and late treatments. The F-stat tests the null hypothesis: Property Right 1989 = Property Right 1998. Column (11) presents the matching estimate using the propensity score of the probability of attrition (with bootstrapped standard errors). The control variables are described in Appendix Table A.1. Absolute value of t statistics in parentheses. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Table A.7 – HOUSEHOLD HEAD SPOUSE											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Property Right	-0.01	-0.02	-0.02	-0.03	-0.01	-0.01	-0.01		0.01		-0.05
	(0.27)	(0.37)	(0.37)	(0.56)	(0.26)	(0.41)	(0.30)		(0.20)		(0.70)
Property Right Offer								0.01			
								(0.20)			
Property Right 1989										-0.03	
										(0.36)	
Property Right 1998										-0.01	
										(0.13)	
Parcel Surface	-0.00		-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	
	(0.94)		(1.45)	(0.86)	(0.98)	(1.53)	(1.32)	(0.86)	(0.89)	(0.93)	
Distance to Creek	0.02		0.01	0.01	0.02	0.02	0.02	0.02	0.02	0.02	
	(0.67)		(0.47)	(0.73)	(0.70)	(0.58)	(0.44)	(0.75)	(0.75)	(0.70)	
Block Corner	0.03		0.06	0.03	0.03	0.03	0.03	0.03	0.03	0.03	
	(0.41)		(0.84)	(0.44)	(0.44)	(0.45)	(0.36)	(0.46)	(0.45)	(0.41)	
Distance to Non-Squatted	0.01		0.00	0.03	0.01	0.01	0.01	0.01	0.01	0.01	
Area	(0.37)		(0.03)	(1.22)	(0.36)	(0.39)	(0.33)	(0.37)	(0.37)	(0.37)	
Age of Original Squatter<50	0.03			0.04	0.03	0.03	0.03	0.03	0.03	0.03	
	(0.56)			(1.02)	(0.47)	(0.57)	(0.43)	(0.58)	(0.59)	(0.57)	
Female Original Squatter	-0.28***			-0.31***	-0.28***	-0.28***	-0.28***	-0.28***	-0.28***	-0.28***	
	(5.41)			(7.20)	(4.82)	(4.66)	(4.70)	(5.40)	(5.39)	(5.40)	
Argentine Original Squatter	-0.01			0.01	-0.01	-0.01	-0.01	-0.00	0.00	-0.00	
	(0.05)			(0.14)	(0.05)	(0.04)	(0.03)	(0.00)	(0.00)	(0.04)	
Years of Education of the	0.02*			0.01	0.02*	0.02**	0.02*	0.02*	0.02*	0.02*	
Original Squatter	(1.71)			(1.02)	(1.77)	(2.22)	(2.03)	(1.70)	(1.71)	(1.72)	
Argentine Father of the	-0.05			-0.02	-0.05	-0.05	-0.05	-0.05	-0.05	-0.05	
Original Squatter	(0.45)			(0.24)	(0.55)	(0.77)	(0.69)	(0.48)	(0.48)	(0.45)	
Years of Education of	0.00			0.02	0.00	0.00	0.00	0.00	0.00	0.00	
Original Squatter's Father	(0.03)			(0.85)	(0.03)	(0.03)	(0.03)	(0.07)	(0.07)	(0.02)	
Argentine Mother of the	-0.06			-0.09	-0.06	-0.06	-0.06	-0.06	-0.06	-0.06	
Original Squatter	(0.58)			(0.93)	(0.63)	(0.53)	(0.38)	(0.60)	(0.60)	(0.56)	
Years of Education of	-0.02			0.00	-0.02	-0.02	-0.02	-0.02	-0.02	-0.02	
Original Squatter's Mother	(0.81)			(0.12)	(0.64)	(0.85)	(0.79)	(0.84)	(0.83)	(0.80)	
Constant	0.89***	0.74***	0.84***	0.73***	0.89***	0.89***	0.89***	0.86***	0.86***	0.88***	
	(4.46)	(19.44)	(6.01)	(4.59)	(5.25)	(9.38)	(4.71)	(4.23)	(4.27)	(4.36)	
F-stat										0.06	
Observations	313	313	313	425	313	313	313	313	313	313	290

Notes: The dependent variable is a dummy that equals 1 if the household head lives with a spouse, and 0 otherwise. The household is the unit of observation. Column (1) is summarized in Column 2 of Table 7. Column (2) includes no controls, and Column (3) only controls for parcel characteristics. Column (4) adds the observations for the San Martin neighborhood. Huber-White standard errors clustered at the block level are used in Column (5), and at the former owner level in Column (6). Column (7) considers block bootstrapped standard errors and significance levels by former owner. The reduced-form regression on the intention-to-treat variable Property Right Offer is displayed in Column (8). The 2SLS regression (instrumenting the treatment variable Property Right with the intention-to-treat variable Property Right Offer) is presented in Column (9). Column (10) shows separately the effect of early and late treatments. The F-stat tests the null hypothesis: Property Right 1989 = Property Right 1998. Column (11) presents the matching estimate using the propensity score of the probability of attrition (with bootstrapped standard errors). The control variables are described in Appendix Table A.1. Absolute value of t statistics in parentheses. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Table A.8 – NUMBER OF OFFSPRING OF THE HOUSEHOLD HEAD ≥ 14 YEARS OLD											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Property Right	-0.01	-0.03	0.01	-0.06	-0.01	-0.01	-0.01		-0.18		0.05
	(0.06)	(0.15)	(0.06)	(0.37)	(0.06)	(0.06)	(0.05)		(0.84)		(0.26)
Property Right Offer								-0.17			
								(0.85)			
Property Right 1989										-0.34	
										(1.29)	
Property Right 1998										0.16	
										(0.77)	
Parcel Surface	0.00		0.00	0.00*	0.00	0.00	0.00	0.00	0.00	0.00	
	(1.00)		(1.12)	(1.92)	(0.96)	(1.55)	(1.17)	(0.82)	(0.91)	(1.05)	
Distance to Creek	0.10		0.08	0.03	0.10	0.10	0.1	0.07	0.08	0.15	
	(0.95)		(0.81)	(0.43)	(0.89)	(1.26)	(0.90)	(0.71)	(0.77)	(1.43)	
Block Corner	-0.06		-0.01	0.14	-0.06	-0.06	-0.06	-0.11	-0.08	-0.06	
	(0.23)		(0.02)	(0.58)	(0.25)	(0.40)	(0.36)	(0.39)	(0.30)	(0.24)	
Distance to Non-Squatted	0.08		0.06	0.04	0.08	0.08	0.08	0.08	0.08	0.08	
Area	(0.88)		(0.71)	(0.53)	(0.89)	(1.33)	(1.11)	(0.89)	(0.87)	(0.88)	
Age of Original Squatter<50	0.27			0.24	0.27	0.27	0.27	0.27	0.26	0.29	
	(1.47)			(1.53)	(1.59)	(1.61)	(1.55)	(1.46)	(1.41)	(1.57)	
Female Original Squatter	-0.02			-0.11	-0.02	-0.02	-0.02	-0.02	-0.02	-0.02	
	(0.10)			(0.73)	(0.10)	(0.12)	(0.11)	(0.11)	(0.12)	(0.09)	
Argentine Original Squatter	-0.16			-0.10	-0.16	-0.16	-0.16	-0.19	-0.20	-0.12	
	(0.34)			(0.25)	(0.38)	(0.32)	(0.33)	(0.42)	(0.43)	(0.26)	
Years of Education of the	0.00			-0.01	0.00	0.00	0.00	0.00	0.00	0.00	
Original Squatter	(0.04)			(0.30)	(0.03)	(0.03)	(0.03)	(0.06)	(0.05)	(0.09)	
Argentine Father of the	0.49			0.30	0.49*	0.49**	0.49	0.52	0.51	0.47	
Original Squatter	(1.33)			(0.89)	(1.71)	(2.22)	(2.21)	(1.41)	(1.39)	(1.27)	
Years of Education of	0.01			0.01	0.01	0.01	0.01	0.00	0.00	0.01	
Original Squatter's Father	(0.14)			(0.11)	(0.12)	(0.10)	(0.09)	(0.06)	(0.05)	(0.11)	
Argentine Mother of the	-0.12			0.07	-0.12	-0.12	-0.12	-0.11	-0.10	-0.08	
Original Squatter	(0.31)			(0.21)	(0.34)	(0.23)	(0.22)	(0.28)	(0.27)	(0.22)	
Years of Education of	0.04			0.02	0.04	0.04	0.04	0.05	0.05	0.05	
Original Squatter's Mother	(0.47)			(0.25)	(0.48)	(0.62)	(0.54)	(0.54)	(0.52)	(0.53)	
Constant	0.48	1.69***	1.03**	0.76	0.48	0.48	0.48	0.67	0.65	0.27	
	(0.66)	(12.83)	(2.11)	(1.31)	(0.74)	(0.69)	(0.76)	(0.91)	(0.89)	(0.36)	
F-stat										3.16*	
Observations	313	313	313	425	313	313	313	313	313	313	290

Notes: The dependent variable is the number of sons or daughters of the household head older than 13 years old living in the house. The household is the unit of observation. Column (1) is summarized in Column 3 of Table 7. Column (2) includes no controls, and Column (3) only controls for parcel characteristics. Column (4) adds the observations for the San Martin neighborhood. Huber-White standard errors clustered at the block level are used in Column (5), and at the former owner level in Column (6). Column (7) considers block bootstrapped standard errors and significance levels by former owner. The reduced-form regression on the intention-to-treat variable Property Right Offer is displayed in Column (8). The 2SLS regression (instrumenting the treatment variable Property Right with the intention-to-treat variable Property Right Offer) is presented in Column (9). Column (10) shows separately the effect of early and late treatments. The F-stat tests the null hypothesis: Property Right 1989 = Property Right 1998. Column (11) presents the matching estimate using the propensity score of the probability of attrition (with bootstrapped standard errors). The control variables are described in Appendix Table A.1. Absolute value of t statistics in parentheses. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Table A.9 – NUMBER OF OTHER RELATIVES (NO SPOUSE OR OFFSPRING OF THE HOUSEHOLD HEAD)											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Property Right	-0.68***	-0.53***	-0.55***	-0.56***	-0.68***	-0.68***	-0.68***		-0.90***		-0.70**
	(3.53)	(2.75)	(2.75)	(3.23)	(3.51)	(4.97)	(2.62)		(3.97)		(2.37)
Property Right Offer								-0.82***			
								(4.00)			
Property Right 1989										-0.36	
										(1.36)	
Property Right 1998										-0.85***	
										(3.92)	
Parcel Surface	-0.00		0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	
	(0.08)		(0.02)	(0.04)	(0.10)	(0.21)	(0.13)	(0.56)	(0.19)	(0.13)	
Distance to Creek	-0.08		-0.09	-0.02	-0.08	-0.08	-0.08	-0.12	-0.10	-0.13	
	(0.76)		(0.85)	(0.32)	(0.73)	(1.09)	(0.62)	(1.18)	(0.96)	(1.22)	
Block Corner	0.03		0.00	-0.06	0.03	0.03	0.03	-0.13	0.00	0.03	
	(0.10)		(0.01)	(0.26)	(0.12)	(0.16)	(0.13)	(0.46)	(0.01)	(0.11)	
Distance to Non-Squatted	-0.12		-0.12	-0.07	-0.12	-0.12**	-0.12	-0.11	-0.12	-0.12	
Area	(1.23)		(1.18)	(0.86)	(1.32)	(2.17)	(1.03)	(1.18)	(1.23)	(1.22)	
Age of Original Squatter<50	-0.35*			-0.43***	-0.35*	-0.35*	-0.35*	-0.32*	-0.36*	-0.36*	
	(1.80)			(2.59)	(1.94)	(2.07)	(2.84)	(1.68)	(1.87)	(1.89)	
Female Original Squatter	0.32*			0.26	0.32*	0.32	0.32*	0.33*	0.32	0.32*	
	(1.66)			(1.56)	(1.77)	(1.56)	(1.61)	(1.71)	(1.63)	(1.65)	
Argentine Original Squatter	-0.71			-0.53	-0.71	-0.71	-0.71	-0.73	-0.76	-0.75	
	(1.49)			(1.26)	(1.48)	(1.36)	(1.10)	(1.54)	(1.59)	(1.57)	
Years of Education of the	-0.10*			-0.09**	-0.10**	-0.10	-0.10*	-0.09*	-0.10*	-0.10*	
Original Squatter	(1.78)			(2.09)	(2.08)	(1.66)	(1.74)	(1.72)	(1.76)	(1.84)	
Argentine Father of the	0.97**			0.86**	0.97*	0.97*	0.97**	1.05***	1.00***	1.00***	
Original Squatter	(2.54)			(2.43)	(1.91)	(1.84)	(1.70)	(2.74)	(2.61)	(2.61)	
Years of Education of	-0.06			-0.07	-0.06	-0.06	-0.06	-0.06	-0.07	-0.05	
Original Squatter's Father	(0.65)			(0.93)	(0.51)	(0.62)	(0.59)	(0.74)	(0.75)	(0.63)	
Argentine Mother of the	-0.37			-0.31	-0.37	-0.37	-0.37	-0.37	-0.35	-0.40	
Original Squatter	(0.96)			(0.88)	(0.84)	(1.06)	(0.76)	(0.98)	(0.91)	(1.05)	
Years of Education of	0.03			-0.02	0.03	0.03	0.03	0.04	0.03	0.02	
Original Squatter's Mother	(0.30)			(0.24)	(0.29)	(0.34)	(0.29)	(0.45)	(0.37)	(0.25)	
Constant	2.56***	1.25***	1.63***	2.55***	2.56***	2.56***	2.56**	2.87***	2.79***	2.77***	
	(3.42)	(8.66)	(3.05)	(4.21)	(3.73)	(5.05)	(3.27)	(3.77)	(3.66)	(3.66)	
F-stat										2.81*	
Observations	313	313	313	425	313	313	313	313	313	313	290

Notes: The dependent variable is the number of household members excluding the household head, household head spouse and sons or daughters of the household head. The household is the unit of observation. Column (1) is summarized in Column 4 of Table 7. Column (2) includes no controls, and Column (3) only controls for parcel characteristics. Column (4) adds the observations for the San Martin neighborhood. Huber-White standard errors clustered at the block level are used in Column (5), and at the former owner level in Column (6). Column (7) considers block bootstrapped standard errors and significance levels by former owner. The reduced-form regression on the intention-to-treat variable Property Right Offer is displayed in Column (8). The 2SLS regression (instrumenting the treatment variable Property Right with the intention-to-treat variable Property Right Offer) is presented in Column (9). Column (10) shows separately the effect of early and late treatments. The F-stat tests the null hypothesis: Property Right 1989 = Property Right 1998. Column (11) presents the matching estimate using the propensity score of the probability of attrition (with bootstrapped standard errors). The control variables are described in Appendix Table A.1. Absolute value of t statistics in parentheses. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Table A.10 – NUMBER OF OFFSPRING OF THE HOUSEHOLD HEAD 5-13 YEARS OLD													
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Property Right	-0.17	-0.22	-0.23	-0.21*	-0.17	-0.17	-0.17		-0.12		-0.12		
	(1.18)	(1.51)	(1.56)	(1.66)	(1.23)	(0.99)	(0.77)		(0.72)		(0.75)		
Property Right Offer								-0.11					
								(0.72)					
Property Right 1989										-0.38*		-0.38*	
										(1.88)		(1.77)	
Property Right 1998										-0.06			-0.08
										(0.37)			(0.46)
Parcel Surface	0.00		0.00	-0.00	0.00	0.00	0.00	0.00	0.00	0.00			
	(0.14)		(0.24)	(0.21)	(0.14)	(0.14)	(0.12)	(0.10)	(0.17)	(0.18)			
Distance to Creek	0.01		-0.04	0.04	0.01	0.01	0.01	0.01	0.01	0.04			
	(0.10)		(0.49)	(0.86)	(0.11)	(0.16)	(0.11)	(0.12)	(0.16)	(0.53)			
Block Corner	-0.01		-0.01	-0.07	-0.01	-0.01	-0.01	-0.02	-0.01	-0.01			
	(0.06)		(0.07)	(0.39)	(0.06)	(0.06)	(0.06)	(0.12)	(0.03)	(0.06)			
Distance to Non-Squatted	0.03		0.00	0.07	0.03	0.03	0.03	0.03	0.03	0.03			
Area	(0.42)		(0.03)	(1.07)	(0.46)	(0.37)	(0.34)	(0.43)	(0.42)	(0.42)			
Age of Original Squatter<50	0.93***			0.76***	0.93***	0.93***	0.93***	0.94***	0.93***	0.94***			
	(6.48)			(6.28)	(5.95)	(13.02)	(10.67)	(6.54)	(6.50)	(6.56)			
Female Original Squatter	-0.15			-0.02	-0.15	-0.15	-0.15	-0.15	-0.15	-0.15			
	(1.03)			(0.19)	(1.01)	(1.00)	(0.90)	(1.01)	(1.02)	(1.02)			
Argentine Original Squatter	0.26			0.21	0.26	0.26	0.26	0.27	0.27	0.28			
	(0.72)			(0.69)	(0.71)	(0.78)	(0.74)	(0.76)	(0.75)	(0.79)			
Years of Education of the	-0.01			-0.01	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01			
Original Squatter	(0.35)			(0.17)	(0.36)	(0.46)	(0.45)	(0.35)	(0.36)	(0.31)			
Argentine Father of the	-0.29			-0.15	-0.29	-0.29	-0.29	-0.29	-0.30	-0.31			
Original Squatter	(1.03)			(0.57)	(0.94)	(1.00)	(0.88)	(1.02)	(1.05)	(1.09)			
Years of Education of	-0.13**			-0.13**	-0.13**	-0.13***	-0.13*	-0.13*	-0.13*	-0.13**			
Original Squatter's Father	(1.99)			(2.36)	(2.18)	(3.07)	(3.00)	(1.95)	(1.95)	(2.02)			
Argentine Mother of the	-0.22			-0.29	-0.22	-0.22	-0.22	-0.23	-0.23	-0.20			
Original Squatter	(0.78)			(1.12)	(0.61)	(1.56)	(0.92)	(0.81)	(0.80)	(0.70)			
Years of Education of	0.05			0.05	0.05	0.05	0.05	0.05	0.05	0.05			
Original Squatter's Mother	(0.70)			(0.74)	(0.64)	(0.53)	(0.54)	(0.70)	(0.69)	(0.75)			
Constant	1.17**	1.06***	1.07***	1.12**	1.17**	1.17***	1.17**	1.12**	1.12**	1.03*			
	(2.09)	(9.82)	(2.66)	(2.51)	(2.12)	(3.65)	(3.37)	(1.97)	(1.97)	(1.82)			
F-stat										2.19			
Observations	313	313	313	425	313	313	313	313	313	313	290	145	217

Notes: The dependent variable is the number of sons or daughters of the household head between 5 and 13 years old living in the house. The household is the unit of observation. Column (1) is summarized in Column 5 of Table 7. Column (2) includes no controls, and Column (3) only controls for parcel characteristics. Column (4) adds the observations for the San Martin neighborhood. Huber-White standard errors clustered at the block level are used in Column (5), and at the former owner level in Column (6). Column (7) considers block bootstrapped standard errors and significance levels by former owner. The reduced-form regression on the intention-to-treat variable Property Right Offer is displayed in Column (8). The 2SLS regression (instrumenting the treatment variable Property Right with the intention-to-treat variable Property Right Offer) is presented in Column (9). Column (10) is the regression summarized in Column 6 of Table 7, which shows separately the effect of early and late treatments. The F-stat tests the null hypothesis: Property Right 1989 = Property Right 1998. For treatment, early treatment and late treatment, respectively, Columns (11) through (13) present the matching estimates using the propensity score of the probability of attrition (with bootstrapped standard errors). The control variables are described in Appendix Table A.1. Absolute value of t statistics in parentheses. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Table A.11 – NUMBER OF OFFSPRING OF THE HOUSEHOLD HEAD 0-4 YEARS OLD

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Property Right	-0.07	-0.07	-0.07	-0.07	-0.07	-0.07*	-0.07		-0.00		-0.06		
	(1.03)	(1.05)	(1.01)	(1.09)	(0.91)	(1.80)	(1.25)		(0.04)		(0.71)		
Property Right Offer								-0.00					
								(0.04)					
Property Right 1989										-0.08		-0.05	
										(0.81)		(0.42)	
Property Right 1998										-0.07			-0.04
										(0.86)			(0.49)
Parcel Surface	0.00		0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00			
	(0.78)		(0.93)	(0.23)	(0.67)	(0.85)	(0.85)	(0.86)	(0.87)	(0.79)			
Distance to Creek	-0.01		0.00	0.02	-0.01	-0.01	-0.01	-0.00	-0.00	-0.01			
	(0.21)		(0.04)	(0.68)	(0.20)	(0.31)	(0.20)	(0.03)	(0.03)	(0.17)			
Block Corner	-0.03		0.00	-0.00	-0.03	-0.03	-0.03	-0.02	-0.02	-0.03			
	(0.32)		(0.04)	(0.05)	(0.37)	(0.31)	(0.29)	(0.24)	(0.24)	(0.32)			
Distance to Non Squatted	0.03		0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03			
Area	(0.74)		(0.96)	(0.99)	(0.75)	(0.64)	(0.52)	(0.74)	(0.74)	(0.73)			
Age of Original Squatter<50	0.15**			0.15***	0.15**	0.15***	0.15**	0.16**	0.16**	0.15**			
	(2.23)			(2.60)	(2.12)	(4.65)	(4.25)	(2.29)	(2.28)	(2.22)			
Female Original Squatter	0.04			0.10*	0.04	0.04	0.04	0.04	0.04	0.04			
	(0.55)			(1.70)	(0.58)	(0.80)	(1.12)	(0.57)	(0.57)	(0.55)			
Argentine Original Squatter	-0.28			-0.29*	-0.28*	-0.28***	-0.28*	-0.26	-0.26	-0.28			
	(1.65)			(1.94)	(1.85)	(4.87)	(1.44)	(1.55)	(1.55)	(1.64)			
Years of Education of the	0.01			-0.02	0.01	0.01	0.01	0.01	0.01	0.01			
Original Squatter	(0.51)			(1.01)	(0.59)	(0.66)	(0.57)	(0.50)	(0.50)	(0.51)			
Argentine Father of the	0.11			0.10	0.11	0.11	0.11	0.10	0.10	0.11			
Original Squatter	(0.81)			(0.81)	(1.09)	(1.38)	(1.38)	(0.74)	(0.74)	(0.80)			
Years of Education of	-0.00			-0.00	-0.00	-0.00	-0.00	0.00	0.00	-0.00			
Original Squatter's Father	(0.05)			(0.01)	(0.05)	(0.11)	(0.07)	(0.04)	(0.04)	(0.05)			
Argentine Mother of the	0.02			0.03	0.02	0.02	0.02	0.02	0.02	0.02			
Original Squatter	(0.16)			(0.27)	(0.20)	(0.34)	(0.20)	(0.11)	(0.11)	(0.16)			
Years of Education of	-0.03			-0.01	-0.03	-0.03	-0.03	-0.03	-0.03	-0.03			
Original Squatter's Mother	(0.89)			(0.17)	(0.91)	(0.95)	(0.85)	(0.94)	(0.94)	(0.88)			
Constant	0.32	0.33***	0.15	0.35*	0.32	0.32	0.32	0.25	0.25	0.31			
	(1.20)	(6.78)	(0.84)	(1.65)	(1.09)	(1.63)	(0.94)	(0.91)	(0.92)	(1.16)			
F-stat										0.01			
Observations	313	313	313	425	313	313	313	313	313	313	290	145	217

Notes: The dependent variable is the number of sons or daughters of the household head between 0 and 4 years old living in the house. The household is the unit of observation. Column (1) is summarized in Column 7 of Table 7. Column (2) includes no controls, and Column (3) only controls for parcel characteristics. Column (4) adds the observations for the San Martin neighborhood. Huber-White standard errors clustered at the block level are used in Column (5), and at the former owner level in Column (6). Column (7) considers block bootstrapped standard errors and significance levels by former owner. The reduced-form regression on the intention-to-treat variable Property Right Offer is displayed in Column (8). The 2SLS regression (instrumenting the treatment variable Property Right with the intention-to-treat variable Property Right Offer) is presented in Column (9). Column (10) is the regression summarized in Column 8 of Table 7, which shows separately the effect of early and late treatments. The F-stat tests the null hypothesis: Property Right 1989 = Property Right 1998. For treatment, early treatment and late treatment, respectively, Columns (11) through (13) present the matching estimate using the propensity score of the probability of attrition (with bootstrapped standard errors). The control variables are described in Appendix Table A.1. Absolute value of t statistics in parentheses. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Table A.12 – SCHOOL ACHIEVEMENT (OFFSPRING OF THE HOUSEHOLD HEAD 5-13 YEARS OLD)														
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Property Right	0.15	0.05	0.12	0.10	0.15	0.15	0.15	0.15		0.21		0. 02		
	(1.28)	(0.47)	(1.06)	(0.90)	(1.25)	(1.08)	(1.15)	(0.84)		(1.36)		(0.18)		
Property Right Offer									0.19					
									(1.36)					
Property Right 1989											0.42**		0.45**	
											(2.20)		(2.46)	
Property Right 1998											0.05			0.01
											(0.40)			(0.05)
Parcel Surface	-0.00		-0.00	0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00			
	(0.98)		(0.84)	(0.00)	(0.94)	(0.99)	(1.60)	(1.10)	(0.67)	(0.94)	(1.11)			
Distance to Creek	0.08		0.09	-0.00	0.08	0.08	0.08	0.08	0.09	0.08	0.04			
	(1.13)		(1.41)	(0.09)	(1.14)	(0.98)	(1.08)	(0.78)	(1.29)	(1.21)	(0.62)			
Block Corner	0.07		0.10	0.05	0.07	0.07	0.07	0.07	0.12	0.08	0.09			
	(0.40)		(0.69)	(0.34)	(0.40)	(0.42)	(0.52)	(0.33)	(0.69)	(0.46)	(0.55)			
Distance to Non-Squatted Area	-0.07		-0.05	-0.09	-0.07	-0.07	-0.07	-0.07	-0.08	-0.08	-0.08			
	(1.08)		(0.72)	(1.50)	(1.08)	(1.15)	(1.49)	(0.95)	(1.15)	(1.16)	(1.12)			
Male	-0.02	-0.03	-0.00	-0.03	-0.02	-0.02	-0.02	-0.02	-0.01	-0.02	-0.01			
	(0.18)	(0.30)	(0.02)	(0.28)	(0.15)	(0.13)	(0.18)	(0.17)	(0.06)	(0.18)	(0.11)			
Child Age	-0.11***	-0.12***	-0.11***	-0.11***	-0.11***	-0.11***	-0.11***	-0.11***	-0.11***	-0.11***	-0.11***			
	(5.37)	(5.50)	(5.27)	(5.67)	(5.25)	(5.76)	(7.45)	(6.54)	(5.37)	(5.38)	(5.28)			
Age of Original Squatter<50	-0.05			-0.06	-0.05	-0.05	-0.05	-0.05	-0.03	-0.05	-0.06			
	(0.34)			(0.52)	(0.34)	(0.35)	(0.58)	(0.43)	(0.24)	(0.37)	(0.46)			
Female Original Squatter	0.10			0.12	0.10	0.10	0.10	0.10	0.09	0.11	0.08			
	(0.82)			(1.15)	(0.81)	(0.76)	(0.61)	(0.63)	(0.75)	(0.88)	(0.66)			
Argentine Original Squatter	0.07			0.19	0.07	0.07	0.07	0.07	0.10	0.08	0.03			
	(0.21)			(0.66)	(0.23)	(0.26)	(0.36)	(0.29)	(0.30)	(0.25)	(0.10)			
Years of Education of the	0.05			0.07**	0.05	0.05	0.05**	0.05	0.05	0.05	0.05			
Original Squatter	(1.49)			(2.45)	(1.32)	(1.42)	(2.18)	(2.23)	(1.48)	(1.42)	(1.53)			
Argentine Father of the	-0.36			-0.29	-0.36	-0.36	-0.36	-0.36	-0.41	-0.38	-0.33			
Original Squatter	(1.46)			(1.18)	(1.64)	(1.52)	(1.65)	(1.12)	(1.61)	(1.53)	(1.36)			
Years of Education of	0.03			0.06	0.03	0.03	0.03	0.03	0.03	0.03	0.03			
Original Squatter's Father	(0.45)			(1.01)	(0.47)	(0.51)	(0.68)	(0.38)	(0.46)	(0.45)	(0.51)			
Argentine Mother of the	0.26			0.20	0.26	0.26	0.26*	0.26	0.26	0.26	0.22			
Original Squatter	(1.23)			(0.98)	(1.07)	(0.91)	(2.13)	(0.73)	(1.22)	(1.26)	(1.07)			
Years of Education of	0.02			0.00	0.02	0.02	0.02	0.02	0.01	0.02	0.00			
Original Squatter's Mother	(0.26)			(0.01)	(0.25)	(0.25)	(0.23)	(0.23)	(0.18)	(0.32)	(0.04)			
Constant	-0.24	0.11	0.10	-0.58	-0.24	-0.24	-0.24	-0.24	-0.33	-0.28	-0.07			
	(0.48)	(0.48)	(0.27)	(1.35)	(0.52)	(0.51)	(0.58)	(0.45)	(0.64)	(0.55)	(0.13)			
F-stat											3.19*			
Observations	273	273	273	355	273	273	273	273	273	273	273	254	134	204

Notes: The dependent variable is the difference between the school grade each child is currently attending or the maximum grade attained (if not attending school) minus the grade corresponding to the child age. The child is the unit of observation. Column (1) is summarized in Column 1 of Table 8. Column (2) only controls for child age and gender. Column (3) controls for child age, child gender and parcel characteristics. Column (4) adds the observations for the San Martin neighborhood. Huber-White standard errors clustered at the household level are used in Column (5), at the block level in Column (6), and at the former owner level in Column (7). Column (8) considers block bootstrapped standard errors and significance levels by former owner. The reduced-form regression on the intention-to-treat variable Property Right Offer is displayed in Column (9). The 2SLS regression (instrumenting the treatment variable Property Right with the intention-to-treat variable Property Right Offer) is presented in Column (10). Column (11) is the regression summarized in Column 2 of Table 8, which shows separately the effect of early and late treatments. The F-stat tests the null hypothesis: Property Right 1989 = Property Right 1998. For treatment, early treatment and late treatment, respectively, Columns (12) through (14) present the matching estimates using the propensity score of the probability of attrition (with bootstrapped standard errors). The control variables are described in Appendix Table A.1. Absolute value of t statistics in parentheses. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Table A.13 - SCHOOL ABSENTEEISM (OFFSPRING OF THE HOUSEHOLD HEAD 5-13 YEARS OLD)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Property Right	-0.39**	-0.34**	-0.39**	-0.29**	-0.39**	-0.39**	-0.39*	-0.39**		-0.45**		-0.39**		
	(2.43)	(2.38)	(2.54)	(2.14)	(2.14)	(2.21)	(1.83)	(1.43)		(2.17)		(2.41)		
Property Right Offer									-0.40**					
									(2.17)					
Property Right 1989											-0.55**		-0.73***	
											(2.12)		(2.79)	
Property Right 1998											-0.33*			-0.51**
											(1.86)			(2.46)
Parcel Surface	-0.00		0.00	0.00**	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00			
	(0.07)		(0.35)	(2.10)	(0.08)	(0.08)	(0.06)	(0.04)	(0.49)	(0.10)	(0.01)			
Distance to Creek	-0.16*		-0.13	-0.06	-0.16	-0.16	-0.16	-0.16*	-0.18*	-0.16*	-0.13			
	(1.72)		(1.51)	(0.97)	(1.09)	(1.07)	(1.08)	(0.79)	(1.89)	(1.78)	(1.43)			
Block Corner	0.11		0.11	0.15	0.11	0.11	0.11	0.11	0.00	0.10	0.10			
	(0.49)		(0.53)	(0.75)	(0.36)	(0.39)	(0.41)	(0.28)	(0.02)	(0.44)	(0.43)			
Distance to Non-Squatted Area	-0.04		-0.02	0.01	-0.04	-0.04	-0.04	-0.04	-0.04	-0.04	-0.04			
	(0.48)		(0.28)	(0.15)	(0.26)	(0.25)	(0.25)	(0.20)	(0.42)	(0.41)	(0.46)			
Male	-0.01	-0.02	-0.05	-0.02	-0.01	-0.01	-0.01	-0.01	-0.04	-0.01	-0.01			
	(0.05)	(0.17)	(0.32)	(0.16)	(0.06)	(0.06)	(0.05)	(0.04)	(0.24)	(0.05)	(0.09)			
Child Age	-0.03	-0.03	-0.03	-0.02	-0.03	-0.03	-0.03	-0.03	-0.03	-0.03	-0.03			
	(0.92)	(0.97)	(1.04)	(0.90)	(0.85)	(0.87)	(0.73)	(0.71)	(0.92)	(0.90)	(0.96)			
Age of Original Squatter<50	-0.16			-0.19	-0.16	-0.16	-0.16	-0.16	-0.19	-0.16	-0.15			
	(0.94)			(1.33)	(0.77)	(0.74)	(0.81)	(0.67)	(1.09)	(0.91)	(0.87)			
Female Original Squatter	0.18			0.14	0.18	0.18	0.18	0.18	0.21	0.18	0.20			
	(1.15)			(1.03)	(1.07)	(0.92)	(1.49)	(1.06)	(1.31)	(1.09)	(1.21)			
Argentine Original	-0.35			-0.04	-0.35	-0.35	-0.35	-0.35	-0.40	-0.36	-0.33			
Squatter	(0.79)			(0.11)	(0.91)	(0.92)	(1.44)	(0.59)	(0.90)	(0.83)	(0.74)			
Years of Education of the	-0.01			-0.02	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01			
Original Squatter	(0.19)			(0.45)	(0.18)	(0.19)	(0.20)	(0.20)	(0.21)	(0.14)	(0.20)			
Argentine Father of the	0.24			-0.02	0.24	0.24	0.24	0.24	0.32	0.26	0.23			
Original Squatter	(0.73)			(0.08)	(0.86)	(0.81)	(0.70)	(0.56)	(0.95)	(0.79)	(0.68)			
Years of Education of	-0.14*			-0.11	-0.14**	-0.14**	-0.14**	-0.14*	-0.14*	-0.14*	-0.14*			
Original Squatter's Father	(1.83)			(1.59)	(2.37)	(2.16)	(2.97)	(1.50)	(1.84)	(1.83)	(1.85)			
Argentine Mother of the	0.40			0.45*	0.40	0.40	0.40*	0.40	0.42	0.40	0.42			
Original Squatter	(1.44)			(1.76)	(1.64)	(1.54)	(1.80)	(1.27)	(1.48)	(1.41)	(1.50)			
Years of Education of	0.14			0.13	0.14	0.14	0.14**	0.14	0.16*	0.14	0.15			
Original Squatter's Mother	(1.52)			(1.62)	(1.46)	(1.39)	(2.19)	(1.35)	(1.70)	(1.47)	(1.61)			
Constant	1.21*	0.96***	1.17**	0.37	1.21	1.21	1.21	1.21*	1.36**	1.25*	1.10			
	(1.80)	(3.15)	(2.42)	(0.69)	(1.46)	(1.41)	(1.71)	(1.26)	(1.97)	(1.85)	(1.61)			
F-stat											0.65			
Observations	271	271	271	352	271	271	271	271	271	271	271	253	133	203

Notes: The dependent variable is the number of days each child missed school out of the last five days of classes. The child is the unit of observation. Column (1) is summarized in Column 3 of Table 8. Column (2) only controls for child age and gender. Column (3) controls for child age, child gender and parcel characteristics. Column (4) adds the observations for the San Martin neighborhood. Huber-White standard errors clustered at the household level are used in Column (5), at the block level in Column (6), and at the former owner level in Column (7). Column (8) considers block bootstrapped standard errors and significance levels by former owner. The reduced-form regression on the intention-to-treat variable Property Right Offer is displayed in Column (9). The 2SLS regression (instrumenting the treatment variable Property Right with the intention-to-treat variable Property Right Offer) is presented in Column (10). Column (11) is the regression summarized in Column 4 of Table 8, which shows separately the effect of early and late treatments. The F-stat tests the null hypothesis: Property Right 1989 = Property Right 1998. For treatment, early treatment and late treatment, respectively, Columns (12) through (14) present the matching estimates using the propensity score of the probability of attrition (with bootstrapped standard errors). The control variables are described in Appendix Table A.1. Absolute value of t statistics in parentheses. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Table A.14 – ACCESS TO CREDIT										
	Credit Card & Bank Account		Non-Mortgage Loan Received		Informal Credit		Grocery Store Credit		Mortgage Loan Received	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Property Right	-0.01		0.01		-0.06		0.01		0.02	
	(0.71)		(0.19)		(1.00)		(0.16)		(1.58)	
Property Right 1989		-0.01		0.01		-0.04		0.02		0.04***
		(0.41)		(0.24)		(0.50)		(0.31)		(3.19)
Property Right 1998		-0.02		0.00		-0.07		0.00		0.00
		(0.70)		(0.11)		(1.03)		(0.01)		(0.06)
Parcel Surface	0.00	0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	0.00	0.00
	(1.24)	(1.24)	(0.50)	(0.50)	(1.10)	(1.11)	(0.64)	(0.65)	(0.38)	(0.30)
Distance to Creek	0.02*	0.02*	-0.01	-0.01	-0.03	-0.03	-0.01	-0.01	0.00	-0.00
	(1.95)	(1.82)	(0.71)	(0.72)	(0.98)	(1.03)	(0.29)	(0.36)	(0.34)	(0.51)
Block Corner	-0.00	-0.00	-0.05	-0.05	-0.13	-0.13	0.02	0.02	0.02	0.02
	(0.07)	(0.07)	(1.06)	(1.06)	(1.60)	(1.59)	(0.30)	(0.30)	(1.37)	(1.41)
Distance to Non-Squatted	0.00	0.00	-0.03	-0.03	-0.00	-0.00	-0.03	-0.03	-0.00	-0.00
Area	(0.19)	(0.19)	(1.62)	(1.62)	(0.07)	(0.06)	(1.22)	(1.22)	(0.44)	(0.43)
Age of Original Squatter<50	0.01	0.01	0.02	0.02	0.10*	0.10	0.07	0.06	-0.00	-0.00
	(0.53)	(0.53)	(0.66)	(0.65)	(1.65)	(1.63)	(1.24)	(1.22)	(0.34)	(0.49)
Female Original Squatter	-0.00	-0.00	-0.05	-0.05	0.02	0.02	-0.08	-0.08	0.01	0.00
	(0.20)	(0.21)	(1.32)	(1.32)	(0.28)	(0.28)	(1.44)	(1.44)	(0.52)	(0.51)
Argentine Original Squatter	-0.00	-0.01	-0.01	-0.02	0.01	0.00	0.22*	0.22*	0.01	0.00
	(0.09)	(0.10)	(0.17)	(0.17)	(0.04)	(0.02)	(1.71)	(1.69)	(0.31)	(0.16)
Years of Education of the	0.01	0.01	-0.00	-0.00	-0.02	-0.02	-0.00	-0.01	0.00	0.00
Original Squatter	(1.34)	(1.33)	(0.25)	(0.25)	(1.06)	(1.07)	(0.33)	(0.34)	(0.70)	(0.62)
Argentine Father of the	-0.02	-0.02	-0.07	-0.07	0.15	0.15	-0.02	-0.02	-0.00	0.00
Original Squatter	(0.60)	(0.59)	(1.03)	(1.03)	(1.27)	(1.28)	(0.17)	(0.16)	(0.01)	(0.10)
Years of Education of	0.03***	0.03***	0.00	0.00	0.03	0.03	-0.02	-0.02	0.01	0.01*
Original Squatter's Father	(2.82)	(2.82)	(0.14)	(0.14)	(1.19)	(1.19)	(0.72)	(0.72)	(1.65)	(1.72)
Argentine Mother of the	0.03	0.03	0.10	0.10	-0.02	-0.02	-0.00	-0.00	0.00	0.00
Original Squatter	(0.77)	(0.76)	(1.34)	(1.33)	(0.17)	(0.18)	(0.03)	(0.04)	(0.24)	(0.11)
Years of Education of	-0.02*	-0.02*	0.04**	0.04**	0.04	0.04	0.01	0.01	-0.01*	-0.01*
Original Squatter's Mother	(1.75)	(1.75)	(2.09)	(2.08)	(1.31)	(1.29)	(0.53)	(0.52)	(1.68)	(1.79)
Constant	-0.14*	-0.14*	0.06	0.06	0.23	0.24	0.25	0.26	-0.03	-0.01
	(1.77)	(1.72)	(0.40)	(0.42)	(1.03)	(1.07)	(1.24)	(1.26)	(0.82)	(0.35)
F-stat		0.02		0.02		0.10		0.08		8.61***
Observations	312	312	312	312	302	302	312	312	312	312

Notes: Credit Card & Bank Account is a dummy variable that equals 1 if the household head has a credit card or bank account, and 0 otherwise. Non-Mortgage Loan Received and Mortgage Loan Received are dummy variables that equal 1 if the household has ever received from a bank, government, union, or cooperative, formal non-mortgage credit or formal mortgage credit, respectively, and 0 otherwise. Informal Credit is a dummy variable that equals 1 if the household has received informal credit from relatives, colleagues, neighbors or friends in the previous year, and 0 otherwise. Grocery Store Credit is a dummy variable that equals 1 if the household usually receives on trust credit from grocery stores, and 0 otherwise. The household is the unit of observation. Columns (1), (3), (5), (7), (9) and (10) are summarized in Table 9. The F-stat test the null hypotheses: Property Right 1989 = Property Right 1998. The control variables are described in Appendix Table A.1. Absolute values of t statistics are in parentheses. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Table A.15 – LABOR MARKET										
	Household Head Income		Total Household Income		Total Household Income per Capita		Total Household Income per Adult		Employed Household Head	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Property Right	-27.35		-43.56		1.04		-4.45		0.03	
	(1.10)		(1.27)		(0.13)		(0.38)		(0.63)	
Property Right 1989		-22.07		-32.71		8.91		-6.89		0.05
		(0.63)		(0.69)		(0.82)		(0.43)		(0.64)
Property Right 1998		-30.34		-49.85		-3.52		-3.04		0.02
		(1.06)		(1.27)		(0.39)		(0.23)		(0.43)
Parcel Surface	-0.10	-0.10	0.01	0.01	-0.01	-0.01	0.01	0.01	-0.00	-0.00
	(0.62)	(0.62)	(0.05)	(0.04)	(0.21)	(0.22)	(0.10)	(0.10)	(0.94)	(0.94)
Distance to Creek	8.25	7.45	13.69	11.98	2.83	1.59	0.66	1.05	-0.01	-0.01
	(0.63)	(0.55)	(0.76)	(0.64)	(0.69)	(0.37)	(0.11)	(0.17)	(0.32)	(0.38)
Block Corner	29.72	30.02	32.25	32.54	12.97	13.18	14.00	13.93	-0.11	-0.11
	(0.80)	(0.80)	(0.62)	(0.63)	(1.10)	(1.11)	(0.80)	(0.80)	(1.46)	(1.45)
Distance to Non-Squatted	0.59	0.73	10.95	11.20	0.72	0.91	1.29	1.23	0.06**	0.06**
Area	(0.05)	(0.06)	(0.64)	(0.66)	(0.19)	(0.23)	(0.22)	(0.21)	(2.24)	(2.23)
Age of Original Squatter<50	17.67	17.50	-17.51	-18.21	-12.50	-13.01	8.30	8.46	0.09*	0.08*
	(0.70)	(0.70)	(0.51)	(0.52)	(1.58)	(1.64)	(0.71)	(0.72)	(1.68)	(1.65)
Female Original Squatter	-50.45**	-50.21**	-62.87*	-62.54*	-8.49	-8.26	-14.47	-14.54	-0.10*	-0.10*
	(2.01)	(1.99)	(1.80)	(1.78)	(1.06)	(1.03)	(1.22)	(1.23)	(1.96)	(1.96)
Argentine Original Squatter	-15.10	-15.67	18.43	17.40	29.03	28.28	39.15	39.38	-0.05	-0.05
	(0.25)	(0.26)	(0.22)	(0.21)	(1.51)	(1.47)	(1.38)	(1.39)	(0.37)	(0.38)
Years of Education of the	3.20	3.14	9.52	9.44	5.29**	5.23**	4.13	4.15	0.01	0.01
Original Squatter	(0.46)	(0.45)	(0.99)	(0.98)	(2.40)	(2.37)	(1.27)	(1.27)	(0.59)	(0.58)
Argentine Father of the	-20.68	-20.47	-9.10	-8.62	-20.63	-20.28	-40.66*	-40.77*	0.07	0.07
Original Squatter	(0.44)	(0.44)	(0.14)	(0.13)	(1.38)	(1.36)	(1.85)	(1.85)	(0.67)	(0.68)
Years of Education of	4.36	4.40	23.45	23.51	2.88	2.93	-1.35	-1.36	0.01	0.01
Original Squatter's Father	(0.41)	(0.41)	(1.45)	(1.45)	(0.78)	(0.79)	(0.25)	(0.25)	(0.48)	(0.49)
Argentine Mother of the	20.29	19.78	-69.84	-71.01	-3.35	-4.20	3.87	4.14	0.00	0.00
Original Squatter	(0.44)	(0.43)	(1.09)	(1.10)	(0.23)	(0.29)	(0.18)	(0.19)	(0.05)	(0.03)
Years of Education of	-10.69	-10.80	-2.67	-2.88	-4.09	-4.24	-2.35	-2.31	-0.01	-0.01
Original Squatter's Mother	(0.92)	(0.93)	(0.16)	(0.17)	(1.09)	(1.13)	(0.42)	(0.41)	(0.40)	(0.41)
Constant	313.47***	316.34***	246.89*	253.17*	44.44	48.99	97.59**	96.18**	0.67***	0.68***
	(3.34)	(3.33)	(1.88)	(1.90)	(1.48)	(1.61)	(2.20)	(2.14)	(3.41)	(3.41)
F-stat		0.05		0.11		1.10		0.05		0.07
Observations	251	251	255	255	255	255	255	255	310	310

Notes: Household Head Income is the total income earned by the household head in the previous month. Total Household Income is the total income earned by all the household members in the previous month. Total Household Income per Capita is Total Household Income divided by the number of household members. Total Household Income per Adult is Total Household Income divided by the number of household members older than 16 years old. All income variables are measured in Argentine pesos. Employed Household Head is a dummy variable that equals 1 if the household head was employed the week before the survey, and 0 otherwise. The household is the unit of observation. Columns (1), (3), (5), (7), and (9) are summarized in Table 10. The F-stat test the null hypotheses: Property Right 1989 = Property Right 1998. The control variables are described in Appendix Table A.1. Absolute values of t statistics are in parentheses. * Significant at 10%; ** significant at 5%; *** significant at 1%.