Inter-generational effects of titling programs: physical vs. human capital.

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Abstract

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

Keywords: home ownership, housing expenditure, Uruguay

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1. Introduction

In the volatile economies of Latin America, housing is the main saving mechanism for many families. Achieving homeownership is a milestone in a family's history and is probably the most secure way of transmitting family wealth to the next generation. Therefore, the impact of granting formal property rights is not only on the current generation, rather it may alter the future well being of their descendants.

Titling programs have been advocated as a powerful anti-poverty instrument. De Soto (2000) argues that the lack of formal property rights precludes the transformation of the wealth owned by the poor into capital. Granting formal property rights could allow the poor to collateralize the land and gain access to the credit market. According to de Soto this will start a virtuous circle going from titling to the credit market, from access to credit to investment in productive activities and from there to higher labor productivity and income. International organizations have taken very seriously this suggestion and have fostered land-titling programs throughout many developing countries. However, there is still a lack of rigorous empirical evidence to back income improvement effects of titling programs. The empirical work so far has find low support for the credit channel (Galiani and Schargrodsky 2006 and Field and Torero 2003). On a different transmission mechanism, Field (2007) finds that titling is associated with an increase in adult labor supply that the author attributes to a reduced need to protect their home from illegal occupants.

The inter-generational effects of titling programs are even less clear. On one hand following de Soto's argument, titling will allow families to attain higher income levels. Since education and health are normal goods, we should expect more investment in both of them and better education and health outcomes. On the other hand, titling makes home investment relatively more profitable either because the house and its improvements can now be sold[2] or because families can safely benefit from the house forever. This may incline families, especially poor families, to allocate more of their scarce resources in housing investment and away from human capital investment. In theory, titling programs have an income effect that favors investment in children's human capital and a substitution effect that operates in the opposite direction. The net effect remains ambiguous and a matter of empirical relevance.

In the study of titling, the main empirical methodological problem is to isolate the true effects of titling, separating them from other variables that are normally jointly determined. For instance: wealthier people are more likely to own their home and to have better socioeconomic indicators in general. In this paper, we use data from a natural experiment in Uruguay concerning nine neighborhoods called "comunidades" (communities).[3] The bottom line is that these communities were formed by ex ante homogeneous households and formal property rights could be assigned only to members of three communities due to reasons that are independent of any characteristics of the families living there or in the other communities. We used data collected from the

[2] Before titling houses could not be legally sold since the occupants are not the owners. However, in shantytowns sometimes houses are "sold" in informal markets.
[3] Di Tella, Galiani and Schargrodsky (2007) also use natural experiment with a similar number of observations.

families when they were assigned to the communities in the seventies and conducted a special survey to gather detailed current information.

We find that titling is associated with a higher probability of performing home improvement investments. This is in line with the empirical literature (see for instance Jimenez 1984, Besley 1995, Alston et al. 1996, Jacoby et al. 2002, and Brasselle et al. 2002). Field (2005) finds that that strengthening property rights in urban slums has a significant effect on residential investment and that most of it is financed without the use of credit. The author concludes that this is indicative of an increase in investment incentives related to lower threat of eviction. Following our main argument the increase in incentives to invest in housing in relative terms implies a reduction in the incentives to invest in other things. Indeed, we find that for *some* human capital (education and health) dimensions titling is associated with worse results for children of up to 16 years of age. The effects of titling on education have not received much attention yet. Galiani and Schargrodsky (2004) and Vogl (2007) report positive effects of titling on child nutrition and Gandelman (2008) reports positive effect on chronic diseases. We find it reasonable that with respect to nutrition the income effect will dominate the substitution effect and therefore the results of Galiani and Schargrodsky (2004) and Vogl (2007) are reasonable in terms of this paper. The results of Gandelman (2008) refer to the adult population.

The paper proceeds as follows. Section 2 presents a simple model of the income and substitution effect at place. Section 3 presents the data and estimation strategy and section 4 the results. Section 5 concludes

2. A simple model of titling inter-generational effects

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

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

The benefits from home investment are uncertain because property rights are insecure. There are two states of nature: with probability p the individual is not expropriated and

enjoys from utility level $u(K)$; with probability *(1-p)* the individual is expropriated, therefore her utility is $u(0)$. Utility derived from child's human capital is $v(H)$. The parent maximizes his expected utility form home investment and investment in his child education.

Therefore the problem to be solved is:

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

From the first order conditions we get:

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

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

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

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

An increase in income due to higher *p* translates into higher home investment *K* and

higher investment in the child's human capital, *H*. This is the pure income effect.

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

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

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

3. Data and estimation strategy

3.1. The natural experiment[4]

The Instituto Nacional de la Vivienda Económica (INVE, National Institute of Inexpensive Housing) was a public institution whose goal was to provide affordable housing solutions to low income families in Uruguay. In the mid-seventies the INVE built nine small neighborhoods to attend the housing needs of their objective population.

[4] For more institutional details on the communities see Gandelman (2008).

These neighborhoods received the name of “comunidades” (communities). In all cases purchase agreements were signed and implicit mortgage contracts were in place.[5]

In June 1977, a couple of years after the building of the communities, the INVE was eliminated by law and its goals and property were assigned to the state-owned mortgage bank Banco Hipotecario del Uruguay (BHU). Over the following decade there were various problems with the public management of the communities. Finally, the entire population of the communities stopped paying the required installments but no action was taken by any public institution.

On December 1987, a task group named to study the situation concluded that it would imply an excessive cost for the BHU to assume the management of the communities. Therefore it was in the best interest of the BHU to sell the houses to the current occupants at whatever price they were able to pay. By the end of 1988 the board of the BHU set a nominal price of approximately $100 for those occupants that could not prove to have made any previous payment. Those that made at least one previous payment just had to pay the housing titling expenses (approximately $20).

Although, the price was just a nominal figure, the selling of the houses and the assignment of the formal property rights to the occupants could be done only in three communities because in the other communities there were no registered plans (area maps with the land division among houses) at the Municipality of Montevideo. The

[5] See figure 1 in the appendix for a scan of an original mortgage agreement of one family from the Guayabos community.

architecture of the houses in all communities was basically the same. We could not find any justification with the authorities to explain why in some cases maps were registered and why in others they were not. Obviously, families were assigned to communities without knowing if maps were or would be registered at some point in the municipality.

Thus, the reason why the inhabitants of three communities were able to acquire formal property rights in the early nineties while the inhabitants of the other communities were not able to acquire formal ownership is not related to any one of their characteristics. Therefore we have an intention to treat and control groups to evaluate the effects of titling.

3.2. Definition of the treatment and control group

Table 1 summarizes aggregate information about the communities according to original information from the seventies. See figure 2 in the appendix for a map of Montevideo reflective the location of the communities.

Table 1. The communities			
Name	Address	Number houses	Area maps
19 de Abril	Camino Maldonado y Rosario	98	No
Independencia	E. Castro y A. Saravia	98	No
18 de Julio	A. Saravia y Trápani	34	Yes
Sarandí	Camino Carrasco y Oncativo	130	No
Rincón	Camino Carrasco y Oncativo	52	No
Grito de Asencio	Camino Carrasco y Oncativo	65	No
25 de Agosto	Irureta Goyena y Serratosa	52	Yes
Lavalleja	Camino Santos y Carcot	84	Yes
Guayabos	Camino Lecocq y A. Saravia	150	No

In order to evaluate the effects of land titling, we define a treatment group to whom formal property rights were assigned and a control group that did not attain ownership. The treatment is composed by a total of 71 households (314 individuals) from communities 18 de Julio, 25 de Agosto and Lavalleja that were awarded formal property rights. For the control group we extracted a random stratified sample of these communities. The sample of 165 houses implies an estimation error of approximately +-5%.

3.3. Pretreatment characteristics

In order to be sure that the exercises are adequately defined we need to show that the pre intervention characteristics of the intention to treat and control groups are reasonably

similar. We were able to locate the original files signed in the seventies for the communities built in INVE's land. In these files there was information on the following socioeconomic indicators of the original occupants: family composition, presence of children, age, income level and working status.[6]

Table 2 demonstrates that, indeed, *ex ante* the populations of the intention to treat and control groups were very similar.

Table 2. Pretreatment characteristics

		%female household heads	Rooms	Family members	% minors	Schooling Attendance	Employment rate	Income
Control	mean	44%	2,6	5,0	47%	81%	59%	471
	sd	(50%)	(1,0)	(2,7)	(27%)	(32%)	(31%)	(284)
Intention to treat	mean	46%	2,3	4,6	42%	85%	58%	449
	sd	(50%)	(0,9)	(2,5)	(28%)	(29%)	(31%)	(306)
Total	mean	45%	2,5	4,9	46%	82%	59%	466
	sd	(50%)	(0,9)	(2,6)	(28%)	(32%)	(31%)	(290)

3.4. Field work

The survey was conducted during February-March 2007 by a team of four welfare workers and one sociologist specially trained to deal with population of difficult socioeconomic environments. The no-response rate was 3%.

[6] Not all the files could be located. Overall we located 82% of the files of the communities according to the following detail: 19 de Abril 98%, Independencia 96%, 18 de Julio 85%, Sarandí, 71%, Rincón 94%, Grito

3.5. Estimation strategy

Once the exogeneity of the housing titling is established, the identification of the causal effects of land titling follows from simple econometric techniques. In this scenario there are two concerns that need to be addressed: no-compliance and attrition.

Although each family in the three intention to treat communities could have benefited from property rights only about half of them went trough the steps necessary to finally obtain formal rights. This no-compliance may be associated with personal or family characteristics (laziness, lack of knowledge of opportunities, family disputes, etc.) that may also impact on the variables under study.[7] In order to lessen the consequences of this situation we run in all cases two sets of regressions. First, in the OLS or Probit regressions (whether the dependent variable is continuous or binary) we use a dummy variable that takes the value 1 for the intention to treat communities. Second, we run instrumental variables regressions were we instrument the treatment (availability of property rights) with the completely exogenous intention to treat (all houses in the 18 de Julio, 25 de Agosto and Lavalleja communities).

By 1995, it was known which communities could benefit from transfers of property rights. Those that arrived to the treatment or control communities after that date may have different characteristics than those that arrived before and may also differ among the treatment and the control communities. Therefore we have to use data only of those that

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de Asencio 89%, 25 de Agosto 90%, Lavalleja 69% and Guayabos 70%.

[7] See Gandelman (2009) for a study on the characteristics of those that effectively acquired formal property

arrived before 1995.

4. Results

4.1. Housing investment

Tables 3 and 4 present summary statistics and regression results for home investment.[8] First, we defined an overall housing quality index that takes the value 0 when the house has none of ten possible construction problems and adds 1 point to each problem present in the house. The problems considered were: humidity in the roof, leaks, wall fissures, problems in doors or windows, floor fissures, problems with wall or roof plaster, problems with the ceiling, lack of natural light, lack of ventilation and humidity in the foundation. The index ranges from 0 to 10 with a median value of 4. Thus, the median household in the survey has 40% of the housing quality problems considered. We find no statistically significant differences attributable to formal property rights.

A direct question gathered information on the reforms performed in the house. In the intention to treat communities 95% of the houses performed some type of reform while in the control communities 87% of the houses performed reforms. The most common reforms are in the rooms and in the bathroom. The regression results show that titling is associated with a statistically significant higher probability of performing reforms in the house. The results are significant for reforms in rooms but not for bathrooms, roof, walls

rights and those that did not.

[8] Table A1 in the appendix presents the marginal effects of all the regressions included in this paper.

or the floor.

According to the marginal effect of the probit regression the increase in the probability of a reform is of 8% for those in the intention to treat community and the marginal effect of titling according to the instrumental variables estimation is 13.3%. The range of the marginal effect for reform in rooms is much wider. According to the probit regression the marginal effect of titling is 12% and according to the instrumental variables is 29%. Even in the lower estimate the effect is very large compared to less than 50% of houses that have made reforms in their house rooms.

Table 3. Summary statistics: home investment

		Problems	Reforms	Reform in:				
				Rooms	Bathroom	Roof	Walls	Floor
Control	Average	4.00	0.87	0.42	0.45	0.10	0.04	0.24
	Observations	140	140	140	140	140	140	140
Intention to treat	Average	4.06	0.95	0.47	0.52	0.08	0.08	0.25
	Observations	106	106	106	106	106	106	106
Total	Average	4.02	0.91	0.44	0.48	0.09	0.06	0.24
	Observations	246	246	246	246	246	246	246

Table 4. Regression results: home investment

	Problems	Reforms	Reforms in:				
			Rooms	Bathroom	Roof	Walls	Floor
Estimation method	*OLS*	*Probit*	*Probit*	*Probit*	*Probit*	*Probit*	*Probit*
Treat intention	0.057	0.540	0.331	0.173	-0.091	0.346	0.060
	(0.344)	(0.249)**	(0.166)**	(0.162)	(0.226)	(0.256)	(0.176)
Constant	4.000	1.133	-0.545	-0.126	-1.282	-1.718	-0.720
	(0.226)***	(0.135)***	(0.112)***	(0.106)	(0.144)***	(0.188)***	(0.117)***
Observations	246	246	246	246	246	246	246
R-squared	0.00	0.033	0.01	0.00	0.00	0.02	0.00
	Instrumental variables						
Estimation method	*OLS*	*Probit*	*Probit*	*Probit*	*Probit*	*Probit*	*Probit*
Treat	0.133	1.183	0.764	0.410	-0.232	0.668	0.123
	(0.811)	(0.482)**	(0.381)**	(0.381)	(0.536)	(0.547)	(0.412)
Constant	4.000	1.053	-0.542	-0.126	-1.282	-1.598	-0.713
	(0.226)***	(0.184)***	(0.107)***	(0.106)	(0.145)***	(0.133)***	(0.108)***
Observations	246	246	246	246	246	246	246

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

4.2. Children's education

Tables 5 and 6 report summary statistics and regression results for education variables for the children between 6 and 16 years old living in the communities. The overall attendance rate is somewhat below the average city attendance rate but in line with the attendance rate of the population of similar socioeconomic strata. The average attendance rate (primary and secondary schooling) in the control communities is 91% and in the intention to treat communities it is 84%. The probit estimation shows that the larger schooling attendance of the control community is significant at the 10% significance

level with a marginal effect of 6.5%. The instrumental variables estimation shows no significant differences between those with and without formal property rights.

To measure the advancement in the educational system, we constructed an educational gap indicator. The gap is measured as the age of the student minus the years of formal schooling minus five years. The average gap is about half year meaning that on average one every two children in the communities is one year behind of what he is supposed to be according to his age. We found no evidence of differences in the educational gap that could be attributable to titling.

We considered two additional measures of education that imply direct expenses for the families. Overall, 94% of the children of the communities attend the free of charge public school system. We found that 8% of the children in the control community and 4% in the intention to treat communities attend a private school. According to the regression estimates this difference is statistically significant with marginal effects of –4.7% and –7.6% in the probit and instrumental variables probit estimations. The marginal effect in comparison with the unconditional probability of attendance to a private school is very large.

Finally, we asked the families if they sent their kids to “extra-lessons”. The most common of these extra lessons are support in homework resolution and languages (especially English). About one out of four of the children in the communities went to some extra-lessons. The regression results show significant differences at any traditional

significant level with marginal effects above 20%.

Table 5. Summary statistics: children's education

		School attendance	Gap	Private School	Extra-classes
Control	Average	91%	0.45	8%	36%
	Observations	154	187	160	128
Treat intention	Average	84%	0.44	4%	15%
	Observations	154	174	144	112
Total	Average	88%	0.44	6%	26%
	Observations	308	361	304	240

Table 6. Regression results: children's education

	School attendance	Gap	Private School	Extra-classes
Estimation method	*Probit*	*OLS*	*Probit*	*Probit*
Treat intention	-0.323	-0.012	-0.419	-0.669
	(0.187)*	(0.052)	(0.245)*	(0.184)***
Constant	1.335	0.449	-1.397	-0.360
	(0.142)***	(0.036)***	(0.144)***	(0.113)***
Observations	308	361	304	240
R-squared	0.01	0.00	0.02	0.05
		Instrumental variables		
Estimation method	*Probit*	*OLS*	*Probit*	*Probit*
Treat	-0.585	-0.029	-1.017	-1.324
	(0.376)	(0.122)	(0.535)*	(0.310)***
Constant	1.231	0.449	-1.379	-0.318
	(0.100)***	(0.036)***	(0.169)***	(0.122)***
Observations	308	361	304	240

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

4.3. Children's health

Most of the population in the communities depends on the free of charge Public Health System for health care. Only 2% of the children have rights to be attended in a HMO. Tables 7 and 8 present health variables summary statistics and regression results for children of 16 years and younger. We asked if they had access to partial private health assistance (Emergency Medical Services). The firms that provide this Emergency Services in Uruguay are all private. Typically, they take care of in the site emergency treatment and transport to a Hospital. Sometimes they also provide some types of ambulatory health care. We found titling to be associated with a larger probability of having rights to an Emergency Medical Service. In terms of our argument this is the only dimension where the income effect dominates the substitution effect.

Overall 17% of children under 17 years of age felt ill during the previous 30 days. According to our regression there is a statistically significant difference between those with formal property rights and those without them. The marginal effect according to the probit regression is 7% and according to the instrumental variables probit regression it is 18%. We asked if after feeling ill they went to see a medical doctor and found no significant differences (not reported). The fourth column of Table 7 reports that a larger percentage of children in the control communities went for a medical control (without feeling ill) in the previous 30 days (44% vs 36%) but this difference did not turn out to be significant. In many cases, after medical appointments medicines are recommended or studies requested by the health professional. We found that for 30% of the children in the

intention to treat communities the parents were not able to get the recommended medicines. In the control communities this percentage is a much lower 8%. The regression estimates show that this difference is statistically significant and imply very large marginal effects above 20%. We found that 41% of the children in the control communities had a dentist appointment in the previous 6 months while in the intention to treat communities only 30% of the children had. This difference is statistically significant at a 5% confidence level.

Table 7. Summary statistics: health

		Emergency services	Felt ill last month?	Control medical appointment	Lack required medicines	Dentist appointment 6 months
Control	Average	5%	14%	44%	8%	41%
	Observations	187	187	187	24	187
Treatment intention	Average	10%	21%	36%	30%	30%
	Observations	174	174	174	20	174
Total	Average	8%	17%	40%	18%	36%
	Observations	361	361	361	44	361

Table 8. Regression results: health					
	Emergency services	Felt ill last month?	Control medical appointment	Lack required medicines	Dentist appointment 6 months
Estimation method	*Probit*	*Probit*	*Probit*	*Probit*	*Probit*
Treat intention	0.350	0.267	-0.212	0.859	-0.274
	(0.198)*	(0.157)*	(0.134)	(0.471)*	(0.136)**
Constant	-1.612	-1.085	-0.141	-1.383	-0.237
	(0.151)***	(0.114)***	(0.092)	(0.368)***	(0.093)**
Observations	361	361	361	44	361
R-squared	0.02	0.01	0.01	0.08	0.01
			Instrumental variables		
Estimation method	*Probit*	*Probit*	*Probit*	*Probit*	*Probit*
Treat	0.771	0.608	-0.487	1.836	-0.618
	(0.411)*	(0.345)*	(0.302)	(0.768)**	(0.288)**
Constant	-1.539	-1.064	-0.140	-1.194	-0.224
	(0.110)***	(0.097)***	(0.093)	(0.247)***	(0.096)**
Observations	361	361	361	44	361

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

5. Conclusions

In this paper we claim that the inter-generational effects of titling programs are ambiguous. The argument provided by de Soto (2000) implies that the larger income generated by the transfer of formal property rights should at least in part translate into larger investment in childrens human capital. Therefore, besides the potential higher inheritance value that the children may receive they should have more education and better health care. This typical income effect may be upset by a substitution effect. As

suggested by the empirical work in the area, larger land security is likely to induce more home investment. But at the same time it alters the family resource allocation in favor of physical assets and against human capital investment. The net inter- generational effect of titling programs is a matter of empirical research.

Using data from a natural experiment in Uruguay we found that titling is effectively associated to higher probabilities of investing in home improvements especially reforming the rooms of the house. On the other hand several dimension of education investment in children could be worsened due to titling (school attendance, decision to send children to private schools and to lessons beyond regular schooling). We also found worse indicators for some health outcomes in children (feeling sick, lack of medicines, dental assistance).

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

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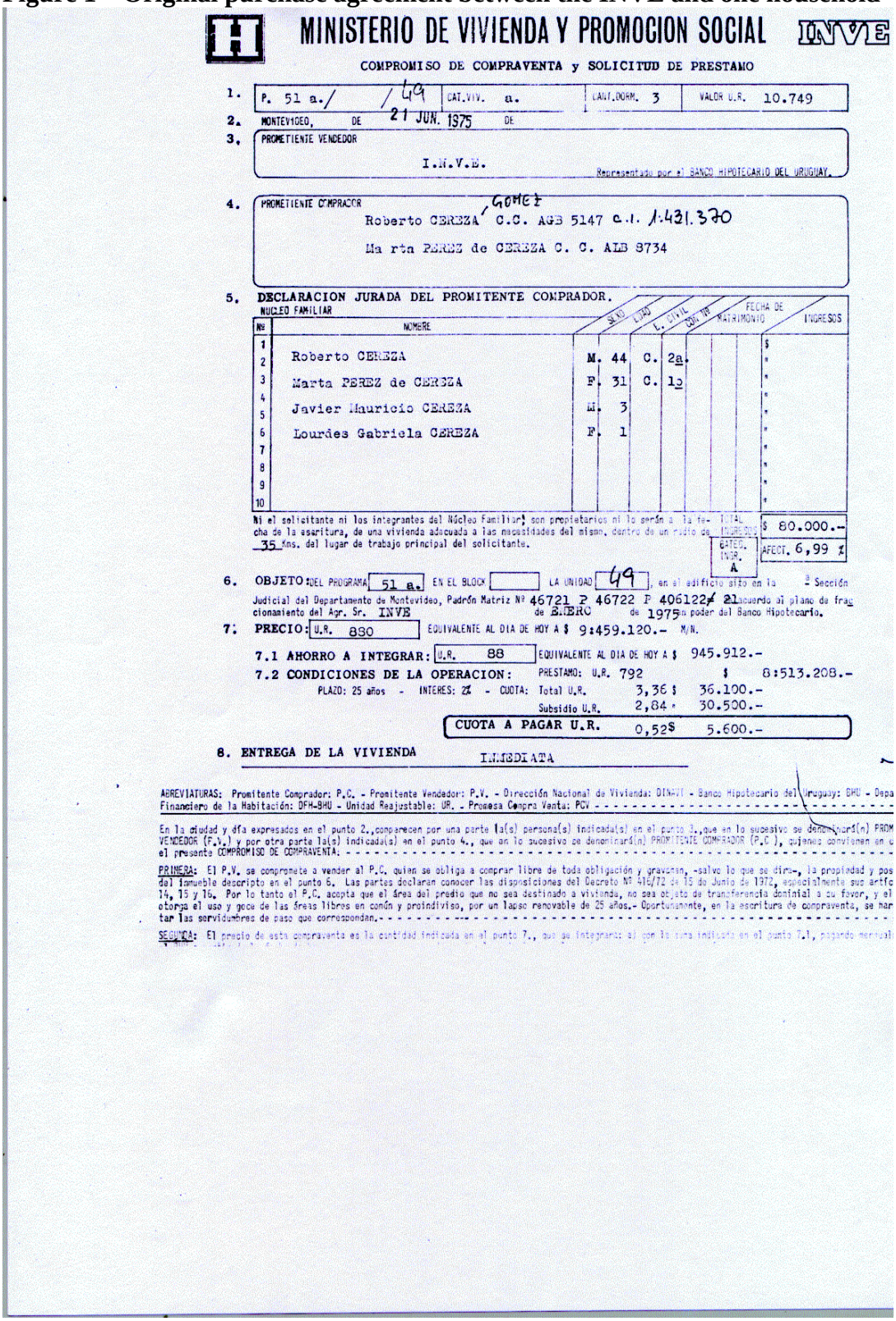
Appendix

Figure 1 – Original purchase agreement between the INVE and one household



MINISTERIO DE VIVIENDA Y PROMOCION SOCIAL — INVE

COMPROMISO DE COMPRAVENTA y SOLICITUD DE PRESTAMO

1. P. 51 a./ 49 | CAT.VIV. a. | CANT.DORM. 3 | VALOR U.R. 10.749
2. MONTEVIDEO, DE 21 JUN. 1975 DE
3. PROMETIENTE VENDEDOR: I.N.V.E. Representado por el BANCO HIPOTECARIO DEL URUGUAY.
4. PROMETIENTE COMPRADOR: Roberto CEREZA / GOMEZ C.C. AGB 5147 c.i. 1.431.370; Marta PEREZ de CEREZA C. C. ALB 8734
5. DECLARACION JURADA DEL PROMITENTE COMPRADOR. NUCLEO FAMILIAR

Nº	NOMBRE	SEXO	EDAD	ESTADO CIVIL	CONYUGE	FECHA DE MATRIMONIO	INGRESOS
1							$
2	Roberto CEREZA	M.	44	C.	2a.		*
3	Marta PEREZ de CEREZA	F.	31	C.	1o		*
4	Javier Mauricio CEREZA	M.	3				*
5	Lourdes Gabriela CEREZA	F.	1				*
6							*
7							*
8							*
9							*
10							*

Ni el solicitante ni los integrantes del Núcleo familiar son propietarios ni lo serán a la fecha de la escritura, de una vivienda adecuada a las necesidades del mismo, dentro de un radio de 35 kms. del lugar de trabajo principal del solicitante. TOTAL INGRESOS $ 80.000.- CATEG. INGR. A AFECT. 6,99 %

6. OBJETO: DEL PROGRAMA 51 a. EN EL BLOCK ____ LA UNIDAD 49, en el edificio sito en la 3ª Sección Judicial del Departamento de Montevideo, Padrón Matriz Nº 46721 P 46722 P 406122 P 21 acuerdo al plano de fraccionamiento del Agr. Sr. INVE de ENERO de 1975 en poder del Banco Hipotecario.
7. PRECIO: U.R. 880 EQUIVALENTE AL DIA DE HOY A $ 9:459.120.- M/N.
   7.1 AHORRO A INTEGRAR: U.R. 88 EQUIVALENTE AL DIA DE HOY A $ 945.912.-
   7.2 CONDICIONES DE LA OPERACION: PRESTAMO: U.R. 792 $ 8:513.208.-
   PLAZO: 25 años - INTERES: 2% - CUOTA: Total U.R. 3,36 $ 36.100.-
   Subsidio U.R. 2,84 " 30.500.-
   CUOTA A PAGAR U.R. 0,52 $ 5.600.-
8. ENTREGA DE LA VIVIENDA INMEDIATA

ABREVIATURAS: Promitente Comprador: P.C. - Promitente Vendedor: P.V. - Dirección Nacional de Vivienda: DINAVI - Banco Hipotecario del Uruguay: BHU - Departamento Financiero de la Habitación: DFH-BHU - Unidad Reajustable: UR. - Promesa Compra Venta: PCV

En la ciudad y día expresados en el punto 2., comparecen por una parte la(s) persona(s) indicada(s) en el punto 3., que en lo sucesivo se denominará(n) PROMITENTE VENDEDOR (P.V.) y por otra parte la(s) indicada(s) en el punto 4., que en lo sucesivo se denominará(n) PROMITENTE COMPRADOR (P.C.), quienes convienen en celebrar el presente COMPROMISO DE COMPRAVENTA:

PRIMERA: El P.V. se compromete a vender al P.C. quien se obliga a comprar libre de toda obligación y gravamen, -salvo lo que se dirá-, la propiedad y posesión del inmueble descripto en el punto 6. Las partes declaran conocer las disposiciones del Decreto Nº 416/72 de 15 de Junio de 1972, especialmente sus artículos 14, 15 y 16. Por lo tanto el P.C. acepta que el área del predio que no sea destinado a vivienda, no sea objeto de transferencia dominial a su favor, y el P.V. otorga el uso y goce de las áreas libres en común y proindiviso, por un lapso renovable de 25 años.- Oportunamente, en la escritura de compraventa, se harán constar las servidumbres de paso que correspondan.-

SEGUNDA: El precio de esta compraventa es la cantidad indicada en el punto 7., que se integrará: a) con la suma indicada en el punto 7.1, pagando mensualmente

Figure 2 – Map of the treatment and control communities

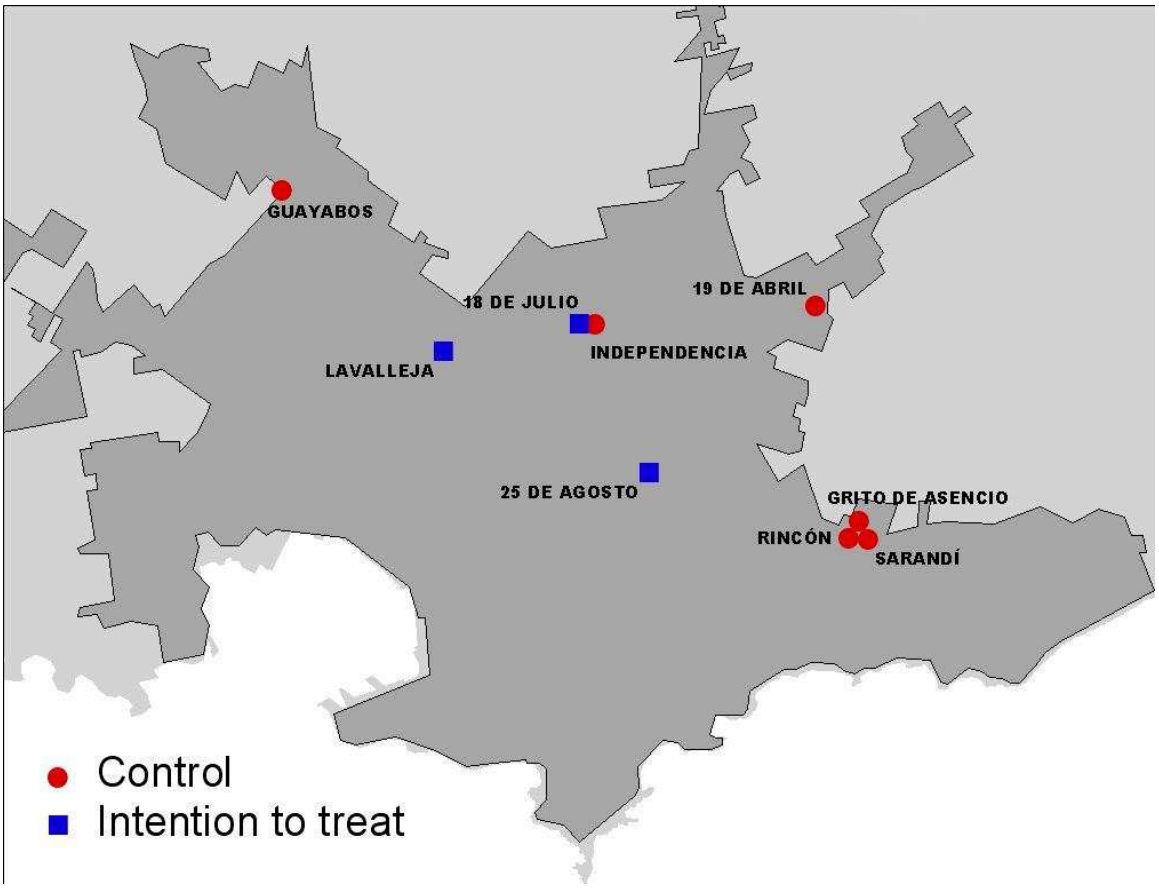


Table A1. Marginal effects

Home investment

	Problems	Reforms	Reforms in:				
			Rooms	Bathroom	Roof	Walls	Floor
Estimation method	*OLS*	*Probit*	*Probit*	*Probit*	*Probit*	*Probit*	*Probit*
Treat intention	0.057	0.081	0.122	0.069	-0.015	0.042	0.019
Unconditional probability		0.914	0.344	0.480	0.093	0.058	0.243
	Instrumental variables						
Treat	0.133	0.133	0.294	0.162	-0.035	0.121	0.040
Unconditional probability		0.898	0.344	0.480	0.093	0.070	245

Children's education

	School attendance	Gap	Private School	Extra-classes
Estimation method	*Probit*	*OLS*	*Probit*	*Probit*
Treat intention	-0.065	-0.012	-0.047	-0.208
Unconditional probability	0.880		0.055	0.251
	Instrumental variables			
Treat	-0.150	-0.029	-0.076	-0.325
Unconditional probability	0.864		0.054	0.276

Children's health

	Emergency services	Felt ill last month?	Control medical appointment	Lack required medicines	Dentist appointment 6 months
Estimation method	*Probit*	*Probit*	*Probit*	*Probit*	*Probit*
Treat intention	0.050	0.068	-0.082	0.217	-0.102
Unconditional probability	0.074	0.170	0.404	0.160	0.356
	Instrumental variables				
Treat	0.160	0.181	-0.179	0.623	-0.211
Unconditional probability	0.084	0.174	0405	0.195	0.362