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SUBSIDIZED HOME–OWNERSHIP PROGRAMS, TRANSACTIONS COSTS, AND DOMESTIC VIOLENCE

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Subsidized Home–ownership Programs, Transaction Costs, and Domestic Violence*

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Abstract

We exploit the random assignment rule implemented by the government of the municipality of Salto (Argentina) in its program of social housing in order to identify the effect of the program on subsequent domestic violence. Beneficiaries receive a finished house in exchange for a long-term credit at a heavily subsidized rate, and are entitled to legal ownership after full payment. Using administrative records from the population of applicants, we find that subsidized home-ownership programs to low-income households are associated to an increase in reported domestic violence. We explore various potential mechanisms and we conclude that the empirical evidence only favors the mechanism of an increase in transaction costs associated to exiting a relationship.

Keywords: transaction costs, marriage, domestic violence.
JEL Classification Codes: K36, H31, J12.

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I Introduction

Housing programs to low-income households are widespread interventions in western countries. *Homeownership Voucher* in the United States, *Home Buyers' Plan* in Canada, *Right to Buy* in the United Kingdom, *Procrear* in Argentina, *Casa para Todos* in El Salvador, *Minha Casa Minha Vida* in Brazil, *Comprar tu Vivienda* in Chile, *Esta es tu Casa* in Mexico, are just a few examples.

Understanding the effects of programs that promote homeownership is of first-order interest from a policy perspective. There is an early literature showing the impact of homeownership on various outcomes, including education (Green and White, 1997), political involvement (DiPasquale and Glaeser, 1999). Rohe et al. (2001) and Dietz and Haurin (2003) provides surveys of the literature on the impact of homeownership. There is also a more recent literature that studies the impact of housing programs on labor market (Navarrete and Navarrete, 2016) and socioeconomic outcomes (Alzúa et al., 2016). The recent contribution by Alzúa et al. (2016) is the closest to our paper. They analyze the impact of a similar social housing program in Argentina and find a reduction in beneficiaries' registered employment for women and for beneficiaries over 50 years old. They also report a significant reduction in household size, an increase in inactivity, an increase in satisfaction with the house, an increase in housing improvements, mixed results on intra-household bargaining, an increase in the level of satisfaction with their daily activities, and no effect on labor informality.

Our paper contributes to the literature by providing evidence on the causal link between homeownership programs and domestic violence. We exploit the random assignment rule implemented by the government of the municipality of Salto (Argentina) in its program of social housing in order to identify the causal effect of the program on subsequent domestic violence. The social housing program consists in delivering a finished house located in the outskirts of an urban

center. The beneficiaries receive the house in exchange for a long-term credit at a heavily subsidized rate, and are only entitled to its legal ownership after full payment. Using administrative records on reported domestic violence for the population of applicants, we find that the program is associated to an increase in domestic violence.

Having established a causal link between the housing program and the subsequent increase in domestic violence, we explore the underlying mechanism behind this finding. The program implies an implicit wealth transfer for beneficiaries, and for beneficiary couples the program also implies an increase in the transaction costs associated to exiting the relationship. Given the various potential mechanisms that may be driving the effect we find, we go further and collect administrative data on labor market outcomes and fertility decisions. We find that there is no impact of the program on the probability of having a formal job and on fertility. We then derive a set of observational implications for the hypothesis that the mechanism is through the increase in the cost of exiting the relationship, and our findings provide support to this hypothesis. Thus, we conclude that the empirical evidence only favors the mechanism that the program makes it more costly for partners to exit a conflictive relationship.

The mechanism of the increase in transaction costs relates our paper to the literatures on divorce laws and marriages. There is an important literature that studies the link between transaction costs and domestic violence, with a focus on divorce laws. In an early contribution, Wardle (1994) states that "by facilitating and socially legitimating legal exits from severely repressive or abusive marriages, no-fault divorce culture may reduce or alleviate certain forms of violence." Empirical evidence on the link between divorce law and domestic violence was first provided by Dee et al. (2003) and Stevenson and Wolfers (2006), who reported mixed (un-conclusive) results. Stolzenberg and D'Alessio (2007) find a

negative association between the length of the divorce process and the incidence of ex-spouse victimization. Closer to our approach is Brassiolo (2016), who reports that violence decreases among couples who remained married when divorce becomes a more credible (cheaper) option.

The law and economics literature views marriage as a contract that makes it more costly for partners to exit their relationship than it would cost to exit if they were cohabitating (for an overview see Rowthorn and Dnes 2002; Matouschek and Rasul 2008). Transaction costs such as fees paid to divorce lawyers and legally imposed restrictions do not arise, or are at least severely mitigated, when cohabiting couples break up.[1] This increase in transaction costs generates more stable relationships, which has been argued to have a bunch of desirable effects. Being married is associated to better health, more happiness, higher wages, more wealth, and less drug use (Waite and Gallagher, 2000; Akerlof, 1998). Men who remain single tend to have lower incomes than married men, are less likely to be employed, and are more prone to drug use (Akerlof, 1998). Identifying the causal impact of an increase in transaction costs associated to marriage, however, is a difficult task since marriage is endogenous to most behavioral outcomes.[2] Our paper contributes to this literature by exploiting a housing program that provides an exogenous variation in transaction costs for cohabitating couples, thus allowing us to identify the impact of an increase in the transaction costs of exiting their relationship. We find a negative by-product associated to the increase in this transaction costs, namely, an increase in domestic violence.

The paper continues as follows. Section II describes the natural experiment and presents the data. Section III reports the results. Section IV concludes.

[1] Peters (1986) and Friedberg and Stern (2004) report evidence that transaction costs of divorcing are considerable.

[2] For example, Wydick (2004) finds that low-match-quality couples prefer cohabitation, while high-match-quality couples prefer marriage.

II Natural experiment and Data

In this section we describe the natural experiment, we present the data, and we provide evidence supporting the validity of the random assignment. We consider potential concerns associated to attrition, treatment-control balance of pretreatment characteristics, and non-compliance, we discuss the validity of the exclusion restriction, and we conclude that none of them are likely to undermine the main results of the paper.

The program

In October 2005, the Secretary of Health and Human Development in Salto (Buenos Aires, Argentina, approximately 34,000 inhabitants) launched a housing program targeted for low-income couples, either married or cohabitating. As an exception, singles were allowed to apply only if in charge of children.

The intervention consists in delivering a quality house located in the outskirts of the urban center of the city of Salto (see Figure 1). The beneficiaries receive the house in exchange for a long-term credit at a heavily subsidized rate, and are entitled to its legal ownership after full payment. The credit financed 100 percent of the house, without upfront payments, at a zero percent nominal rate (in a context of approximately 15 to 20 percent average annual inflation rate), in up to 600 monthly installments.

In April 2007, the 233 houses built as part of the program were allocated by means of a public lottery. To be eligible, applicants have to provide evidence of at least three years of residence in Salto. Couples had to be married or, if not married, they had to provide evidence of more than two years cohabitating. For those applicants that were owners of a house at the time of the application, they were required to provide evidence that the previous property was of lower

monetary value than the house provided by the program.

The program received 506 applications, and a total of 466 applicants met the eligibility requirements. Eligible applicants with disabilities, policemen, and fire-fighters were assigned the credit without participating in the general lottery. Given that our empirical strategy is based on the random assignment of the credit, in our empirical exercise we keep only the 445 applicant that participate in the lottery assignment.

On April 18th, 2007, the assignment of 233 beneficiaries was made by means of a public lottery. The procedure was as follows: each applicant was given a number and a notary was sequentially picking balls with numbers from a turning globe. In order to cover any eventual vacancy, after the assignment of the last beneficiary the procedure continued by assigning a ranking for substitutes using the same turning globe until there were no numbers left.

Table 1 presents the composition of our sample according to treatment status (treated and control) and type of applicant (single applicants and couples).

Beneficiaries received a quality finished house. The houses have 54 squared meters (approximately 581 squared feet), in plots of land of about 285 to 315 squared meters, two bedrooms, one bathroom, one kitchen, a living room, and a yard. Houses cannot be sold, leased, or rented until full payment.

A few months after the lottery assignment, by November 2007, each beneficiary signed a tenure contract and from then on they started to occupy the houses. All houses were occupied within a 2-year period after the assignment.

There are two main types of transitions to homeownership under this program depending in the previous housing situation. First, beneficiaries can be renters at the time of the application. Second, beneficiaries can come from a situation where they live with a hosting family. Even though in the two cases the program implies an implicit wealth transfer for beneficiaries, for previous renters it is not

clear the effect on disposable income (this depends on the amount of the previous rent compared to the amount paid for the mortgage). For beneficiary couples the program also implies an increase in the transaction costs associated to exiting the relationship.

Data

Data on reported domestic violence was provided by Salto's *Centro de Asistencia a la Víctima*. We have information on reported domestic violence for all women that applied to the program. The data on domestic violence corresponds to the period January 2001 to September 2015. That is, we have information on reported domestic violence before and after the intervention. The data distinguish between domestic violence with the partner with whom she applied to the program (Domestic violence with partner) or with any partner (Domestic violence). There is also information on the duration of domestic violence (Duration, in days) and the number of events of domestic violence (Events). Conditional on being involved in domestic violence, we have information on couple's total length between April 2007 and September 2015 (Years together).

We also have information on a set of pre-treatment characteristics. Income is the monthly income of the couple. Rent is a dummy variable that takes the value one for applicants that paid a rent in their previous residence. Exclusive use is a dummy variable that takes the value one for applicants that did not share their previous residence. Overcrowding is a dummy variable that takes the value one if there were more than three people per bedroom in their previous residence. Alone is a dummy variable that takes the value one for single applicants. Previous domestic violence is a dummy variable that takes the value one if there was domestic violence before April 2007. Previous domestic violence with partner is a dummy variable that takes the value one if the applicants were involved in

domestic violence before April 2007. Finally, Non-Attritors is a dummy variable takes the value one for those applicants that were alive and living in Salto by September 2015.

The database also includes administrative data (obtained from Argentina Social Security Agency, ANSES) on two additional outcomes: Participation in the formal job market (a dummy variable that takes the value one if the woman was registered as a formal worker between February 2014 and February 2015) and Fertility (a dummy variable that takes the value one if the woman gave birth to at least one child between February 2014 and February 2015 and she had no children with the couple who apply by 2005).

Summary statistics are reported in Table 2.

Balancing of pre-treatment characteristics

An implication of random assignment is that pre-treatment characteristics should be orthogonal to randomization status. We perform tests of balancing of pre-treatment characteristics by treatment status. As reported in Table 3, all pre-treatment characteristics are balanced between those assigned to the treatment group and those assigned to the control group. Additionally, we run a regression of the lottery assignment on the set of pre-treatment characteristics, and the pre-treatment characteristics are not jointly significant to explain randomization status (p-value of the joint F-test of 0.182).

Attrition

Attrition (the disappearance of an individual from our sample between the time of the lottery assignment in April 2007 and the date when the data was collected in September 2015) might undermine exogeneity. Two potential sources of attrition in our experiment: dying or moving out of Salto. The latter in particular is

potentially very relevant in our setting, since differential out migration from Salto according to assignment status could lead to biased estimates of the impact of the program. Fortunately, attrition in our sample is very low; there are only 6 applicants that either died or moved out from Salto in the period April 2007 to September 2015. In addition, as reported in Table 3, attrition is orthogonal to treatment assignment.[3] Thus, we conclude that attrition is unlikely to bias the main results.

Non-compliance

Another potential source of bias is non-compliance. Compliance in our pre- and post-attrition samples is extremely high, as reported in Table 4. This table reports the OLS estimates from a regression of being a beneficiary on randomized status (our instrumental variable, a dummy that takes value one if the woman was assigned to receive the credit thorough the lottery) and pre-treatment characteristics as covariates. We report results with and without attritors. First-stage estimates indicate that being randomly assigned to receive the credit increases the probability of actually receiving the credit in 91 percentage points.

We conclude that the lottery assignment of beneficiaries, the insignificance of attrition and non-compliance, and the balancing of pre-treatment characteristics indicate that results presented below are not subject to significant sources of selection bias.

III Empirical strategy and results

Figure 2 provides a preview of the results. It displays the cumulative proportion of women exposed to domestic violence in the treated group and in the control

[3] We use administrative data (the 2015 national ballot registry) and information provided by Secretary of Health and Human Development in Salto.

group for the period April 2007 (intervention) to September 2015 (collection of the data).[4]

The incidence of domestic violence is similar for the treated group and the control group until April 2011. By that time about 6 percent of women reported at least one episode of domestic violence. Between April 2011 and September 2015 the incidence of domestic violence starts to diverge: around 21 percent of beneficiary women reported at least one episode of domestic violence by September 2015, a figure that represents an increase of 15 percentage points. On the other hand, about 10 percent of non-beneficiary women report at least one episode of domestic violence (an increase of 4 percentage points).

Figure 2 reassures that the exclusion restriction holds, in the sense that the lottery has no direct impact on the outcome. This is a potential concern given that it has been reported that frustration has an impact on violence in general (Munyo and Rossi, 2013) and domestic violence in particular (Card and Dahl, 2011). In our setting, those applicants that are not assigned to the program may became frustrated by the outcome of the lottery and, therefore, the lottery may have an effect on domestic violence through channels other than the program. Two important findings support the conclusion that frustration is not a concern in our setting. First, frustration increases violent crime, which means that under the hypothesis of frustration we should observe an increase in domestic violence in the control group relative to the treated group, which is exactly the opposite effect as the one observed. Second, Munyo and Rossi (2013) and Card and Dahl (2011) report that the effect of frustration on violent behavior lasts for a short period of time. As observed in Figure 2, the gap in the incidence in domestic violence only started some time after the random assignment. Thus, the hypothesis that the increase in domestic violence is due to frustration associated to the bad outcome

[4] Computed as domestic violence with any partner after April 2007.

of the lottery assignment is not supported by the data.

Formally, we estimate the following regression model:

$$DomesticViolence_i = \alpha + \beta Beneficiary_i + \delta X_i + \varepsilon_i \quad (1)$$

where β is the casual parameter of interest, $Beneficiary_i$ refers to treatment assigned or treatment received by woman i, depending on the particular specification, X_i is the matrix of woman i pre-treatment characteristics, and ε_i is the usual error term.

Beneficiary is potentially endogenous in equation (1). The random assignment procedure described in Section II provides a source of exogenous variation for Beneficiary. However, as shown in , compliance with the lottery assignment is not perfect. To account for the presence of non-compliance, we use the randomly assigned beneficiary status as an instrument for the status actually observed. As shown in Angrist et al. (1996), the Two Stages Least Squares (2SLS) estimator recovers the Local Average Treatment Effect (LATE), a parameter that estimates the effect of receiving a loan for those women whose beneficiary status is influenced by the lottery assignment. Our estimation strategy is, hence, twofold. First, we estimate the Intention to Treat (ITT) parameter by estimating β in Equation (1) by Ordinary Least Squares (OLS), where $Beneficiary_i$ is the status assigned by the lottery. Second, we estimate the LATE parameter by estimating β in Equation (1) using 2SLS, where treatment received is instrumented with treatment assigned.

Results

Our estimates of the impact of being a beneficiary of the long-term credit to buy the house on reported domestic violence are presented in Table 5. We report estimates with and without controls. In all models our estimates indicate that being a beneficiary of the program significantly increases the probability a

woman being exposed to domestic violence. Columns (1) and (2) report reduced form estimates of Equation (1). ITT estimates suggest that being randomized to receive the subsidized house credit increases the probability that a woman experiences domestic violence in approximately 45 percent.

In columns (3) and (4) we instrument treatment received with treatment assigned and estimate Equation (1) using 2SLS. LATE estimates indicate that receiving a subsidized house increases domestic violence in approximately 50 percent. Thus, our instrumental variable results suggest that being a beneficiary of the program raises a complier woman's probability of being exposed to domestic violence by 7.37 percentage points, from 15.7 percent to 23.1 percent.

Even when our study relies on a well-documented randomization we conduct a false experiment to further test the exogeneity of our instrument. If the lottery was truly random, we should observe a statistical zero impact of receiving the loan on the probability of being involved in domestic violence episodes before the lottery assignment. As reported in Table 6, this is indeed the case.

Discussion and interpretation of the results

The random assignment of credits, the presence of very few non compliers, and the very low number of attritors suggests that the link between being a beneficiary of the program and the increase in reported domestic violence is causal.

There are various potential mechanisms driving the effect we find: (i) increase in reporting of domestic violence by beneficiaries; (ii) change in labor market outcomes (which potentially may affect bargaining power within households); (iii) change in fertility (since the change in family composition may affect conflict within households); and (iv) increase in transaction costs of exiting a relationship (associated to the co-ownership of a house that cannot be sold for a long period of time).

We first explore whether the increase in reported domestic violence is due to beneficiary women being more willing to report. According to Argentine law, the property of the house is not lost even if convicted for domestic violence. Even though the property is not at stake, one may be concern that women may want to keep the usufruct of the house. To derive an observational implication for the hypothesis of over-reporting we take advantage of the fact that, according to Argentine civil code, for couples with children under 18 years of age the woman is most likely to keep the usufruct of the house in the case of divorce or separation (until all children are 18). Thus, for couples with children there is no short-term advantage for a woman to report domestic violence. As shown in Table 7, our main results hold for the sub-sample of couples with children. This finding suggests that results are not driven by over-reporting of domestic violence by beneficiary women.

We then explore the hypothesis that the increase in domestic violence is due to the increase in transaction costs of exiting a relationship. Observational implications of this hypothesis are: (i) for single applicants there is no increase in transaction costs and therefore the interaction term between being treated and applying alone should be negative; (ii) an increase in domestic violence with the partner with whom she applied to the program; (iii) conditional on the existence of domestic violence, those couples beneficiaries of the program should last for longer; and (iv) domestic violence should last for longer.

As reported in Table 8, all estimates are in line with the observational implications of the hypothesis. As shown in column (1), the interaction term between being treated and applying alone is negative and significant, in line with the hypothesis of transaction costs. Estimates in column (2) indicate that the program increases domestic violence with the partner with whom she applied. Column (3) reports estimates restricted to couples involved in at least one episode of domestic

violence after April 2007. Estimates indicate that, conditional on being involved in domestic violence, beneficiaries stay longer with their couples. Finally, as reported in columns (4) and (5), the program increases the duration and the number of events of domestic violence.

We then explore other potential mechanisms. The wealth shock associated to being granted a heavily subsidized house credit may have an effect on intra-household dynamics. For instance, beneficiary households may have been re-scheduling their labor and/or fertility decisions in the face of the positive wealth shock.[5]

We first focus on the impact of the program on the labor market. This is potentially relevant since homeownership implies a reallocation that potentially may lead to a loss of labor market connections and a lower spatial mobility that could decrease beneficiaries' employment levels relative to renters. However, both reallocation effects and spatial mobility effects are unlikely in our setting given that the location of the houses granted by the program is within the urban center of Salto (a relative small city), and most beneficiaries were reallocated very close to their previous residence. Still, we report estimates of the impact of the program on formal labor market participation. As shown in column (1) of Table 9, being beneficiary of the program has no effect on the probability of having a formal job (in 2014).[6]

We also explore fertility decisions of beneficiary households. Estimates in column (2) suggest there is no impact of the program on the probability of having a first child in the period 2005 to 2015. This finding is in line with previous literature

[5] Aizer (2010) presents a household bargaining model that incorporates violence. She provides empirical evidence for a causal relationship between relative labor market conditions for women and female hospitalization as a result of assault. Her main finding is that a decrease in the wage gap between men and women can reduce violence against women.

[6] Alzúa et al. (2016) report a reduction in registered employment of more than 7 percentage points, especially for women and for beneficiaries over 50 years old, and no impact on labor informality. Navarrete and Navarrete (2016) find a decrease in employment by 4.1 percent.

(see Alzúa et al. 2016, who report no impact of a similar housing program on fertility decisions).

Overall, we find no effect of the program on formal labor market participation and fertility. In addition, there is evidence that results are not driven by an increase in reporting by beneficiary women. Thus, we conclude that the evidence only favors the mechanism that the program makes it more costly for partners to exit their relationship.

IV Final remarks

We exploit the random assignment rule implemented by the government of the municipality of Salto (Argentina) in its program of social housing in order to identify the effect of the program on subsequent domestic violence. Using administrative records from the population of applicants, we find that subsidized home-ownership programs to low-income households are associated to an increase in reported domestic violence. We explore various potential mechanisms (such as under-reporting, change in family composition, and labor market participation) and we conclude that the empirical evidence only favors the mechanism of an increase in transaction costs associated to exiting a relationship.

Worldwide, almost one third of women who have been in a relationship report that they have experienced some form of physical and/or sexual violence by their intimate partner in their lifetime. In addition, 38% of murders of women are committed by a male intimate partner (WHO, 2013). In this context, our paper contributes to the understanding of the causes of domestic violence, emphasizing the role of transaction costs.

Our paper has various policy implications. It suggests that policy makers should be alert to potential negative by-products associated to this type of pro-

grams, and that these negative by-products should be considered in the cost-benefit analysis. Our paper also highlights that the design of subsidized home-ownership programs in particular should explicitly take into consideration the possibility of facilitating exit from conflictive relationships.

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A Tables

Table 1: Composition of elegible applicants

	Single	Couples	Total
	(1)	(2)	(3)
Control	73	157	230
Treated	51	158	209
Total	124	315	439

Table 2: Summary Statistics

	Observations	Mean	Standard deviation
	(1)	(2)	(3)
Pre-treatment:			
Income	445	977	534.8
Rent	445	0.326	0.469
Exclusive use	445	0.560	0.497
Overcrowding	445	0.598	0.491
Alone	445	0.283	0.451
Previous domestic violence	445	0.063	0.243
Previous domestic violence with partner	445	0.027	0.162
Attrition:			
Non-attritors	445	0.987	0.115
Treatment:			
Beneficiary	445	0.483	0.500
Randomized as beneficiary	445	0.474	0.500
Outcomes:			
Domestic violence	439	0.196	0.397
Domestic violence with partner	439	0.105	0.307
Years together	46	4.630	2.961
Duration of domestic violence (in days)	439	52.55	274.5
Number of events	439	0.872	3.455
Participation in the formal job market	439	0.403	0.491
Fertility	439	0.107	0.310

Notes: Pre-treatment variables are observed in 2005, except for Previous domestic violence and Previous domestic violence with partner, which are observed for the period January 2001 to April 2007. Income is the monthly income of the couple. Rent is a dummy variable that takes the value one for applicants that paid a rent in their previous residence. Exclusive use is a dummy variable that takes the value one for applicants that did not share their previous residence. Overcrowding is a dummy variable that takes the value one if there were more than 3 people per bedroom in their previous residence. Alone is a dummy variable that takes the value one for single applicants. Previous domestic violence is a dummy variable that takes the value one if there was domestic violence before April 2007. Previous domestic violence with partner is a dummy variable that takes the value one if the applicants were involved in domestic violence before April 2007. Finally, Non-Attritors is a dummy variable takes the value one for those applicants that were alive and living in Salto by September 2015. Randomized as beneficiary is a dummy variable that takes the value for applicants that were assigned to receive a house. Domestic violence is a dummy variable that takes the value one if the woman was involved in domestic violence between April 2007 and September 2015. Years together is a count variable that measures couple's total length between April 2007 and September 2015. Duration of domestic violence (in days) is a count variable that measures the time lapse between the first and the last incident between April 2007 and September 2015. Number of events is a count variable that measures the number of incidents between April 2007 and September 2015. Participation in the formal job market is a dummy variable that takes the value one if the woman was registered as a formal worker between February 2014 and February 2015. Fertility is a dummy variable that takes the value one if the woman certifies at least one child between February 2014 and February 2015 and she had no children with the couple who apply in 2005.

Table 3: Pre-treatment characteristics and attrition

	E(control)	Randomized as beneficiary	p-value	Observations
	(1)	(2)	(3)	(4)
Pre-treatment:				
Household income	985.6	-18.20	0.720	445
Rent	0.299	0.056	0.207	445
Exclusive use	0.564	-0.010	0.839	445
Overcrowding	0.581	0.035	0.454	445
Alone	0.316	-0.070	0.103	445
Previous domestic violence	0.068	-0.012	0.619	445
Previous domestic violence with partner	0.030	-0.006	0.687	445
Joint F-test			0.182	445
Attrition:				
Non-attritors	0.982	0.008	0.481	445

Notes: Control refers to those couples that were not assigned the house by the lottery. Joint F-test p-value reports the p-value of a test of joint significance of pre-treatment characteristics to explain the random assignment to treatment. The p-value in column (3) corresponds to a test of differences in means by treatment assignment status.

Table 4: First stage

	Dependent variable: Beneficiary	
	(1)	(2)
Randomized as beneficiary	0.911***	0.914***
	(0.0196)	(0.0194)
Constant	0.0513***	0.0478***
	(0.0145)	(0.0141)
Observations	445	439
R-squared	0.828	0.834

Notes: Robust standard errors are shown in parentheses. Column (1) includes all the sample. Column (2) excludes attritors. ***Significant at the 1 percent level.

Table 5: Main results

	Dependent variable: Domestic violence			
	(1)	(2)	(3)	(4)
Randomized as beneficiary	0.0736*	0.0672*		
	(0.0381)	(0.0373)		
Beneficiary			0.0805*	0.0737*
			(0.0415)	(0.0405)
Observations	439	439	439	439
Porcentual change	46%	42%	51%	47%
Method	OLS	OLS	2SLS	2SLS
Controls	NO	YES	NO	YES

Notes: Robust standard errors are shown in parentheses. Models in columns (2) and (4) control for the set of pre-treatment characteristics (Income, Rent, Exclusive use, Overcrowding, Alone, and Previous domestic violence). In 2SLS models the instrument for Beneficiary is Randomized as beneficiary. All models include an intercept. Percentage change is defined with respect to the mean of the control group. *Significant at the 10 percent level.

Table 6: False experiment

	Dependent variable: Previous domestic violence			
	(1)	(2)	(3)	(4)
Beneficiary	-0.0132	-0.0166	-0.0071	-0.0116
	(0.0254)	(0.0260)	(0.0415)	(0.0172)
Observations	439	439	439	439
Controls	NO	YES	NO	YES

Notes: Robust standard errors are shown in parentheses. All models are estimated by 2SLS and include an intercept. The dependent variable in columns (1) and (2) is Previous domestic violence. The dependent variable in columns (3) and (4) is Previous domestic violence with partner. Models in columns (2) and (4) include controls for the set of pre-treatment characteristics (Income, Rent, Exclusive use, Overcrowding, and Alone). The instrument for Beneficiary is Randomized as beneficiary.

Table 7: Mechanisms (over reporting)

	Dependent variable: Domestic violence for couples with children			
	(1)	(2)	(3)	(4)
Randomized as beneficiary	0.1654***	0.1784***		
	(0.0467)	(0.0373)		
Beneficiary			0.1786***	0.1918***
			(0.0450)	(0.0480)
Observations	276	276	276	276
Porcentual change	59%	61%	61%	62%
Method	OLS	OLS	2SLS	2SLS
Controls	NO	YES	NO	YES

Notes: Robust standard errors are shown in parentheses. All models include only couples with children. Models in columns (2) and (4) control for the set of pre-treatment characteristics (Income, Rent, Exclusive use, Overcrowding, Alone, and Previous domestic violence). In 2SLS models the instrument for Beneficiary is Randomized as beneficiary. All models include an intercept. Percentage change is defined with respect to the mean of the control group. ***Significant at the 1 percent level.

Table 8: Mechanisms (increase in transaction costs associated to exiting a relationship)

	Domestic violence	Domestic violence with partner	Years together	Duration of domestic violence (in days)	Number of events
	(1)	(2)	(3)	(4)	(5)
Beneficiary	0.145***	0.0991***	2.747***	41.02	0.596*
	(0.0467)	(0.0310)	(0.787)	(26.38)	(0.307)
Beneficiary & Alone	-0.249***				
	(0.0879)				
Alone	0.079				
	(0.0575)				
Observations	439	439	46	439	439

Notes: Robust standard errors are shown in parentheses. The model in column (3) is conditional in Domestic violence with partner. All models are estimated by 2SLS, include an intercept, and control for all pre-treatment characteristics (Income, Rent, Exclusive use, Overcrowding, Alone, and Previous domestic violence with partner). The instrument for Beneficiary is Randomized as beneficiary. ***Significant at the 1 percent level. *Significant at the 10 percent level.

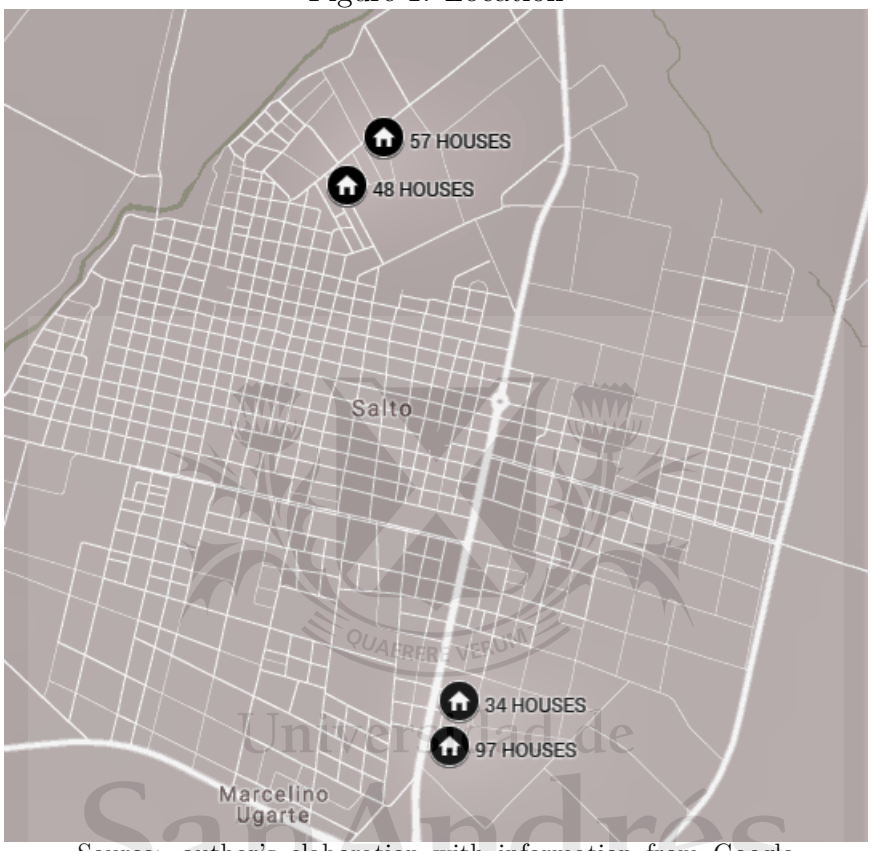
Table 9: Mechanisms (labor market and fertility)

	Participation in the formal job market	Fertility
	(1)	(2)
Beneficiary	-0.0516	-0.0298
	(0.0514)	(0.0308)
Observations	439	439

Notes: Robust standard errors are shown in parentheses. Both models are estimated by 2SLS, include an intercept, and control for pre-treatment characteristics (Income, Rent, Exclusive use, Overcrowding, and Alone). The instrument for Beneficiary is Randomized as beneficiary.

B Figures

Figure 1: Location



Source: author's elaboration with information from Google Maps.

Figure 2: Evolution of cumulative domestic violence since intervention