

JUE INSIGHT: THE UNINTENDED EFFECT OF ARGENTINA'S SUBSIDIZED HOMEOWNERSHIP LOTTERY PROGRAM ON INTIMATE PARTNER VIOLENCE

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JUE Insight: The Unintended Effect of Argentina’s Subsidized Homeownership Lottery Program on Intimate Partner Violence

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Abstract

We study a natural experiment in Argentina, where low-income women were selected through a lottery system to receive a house and a heavily subsidized long-term mortgage. We exploit the random assignment to estimate the causal link between subsidized homeownership programs and intimate partner violence (IPV). Our analysis utilizes administrative records of the population of women applicants to assess the impact of homeownership on IPV, differentiating between women under joint-ownership contracts with their partners and those under single-ownership contracts. We find that the program causes an increase in IPV for women under joint-ownership contracts and a decrease in IPV for women under single-ownership contracts. Our results highlight the importance of considering the design of subsidized homeownership programs and explicitly incorporating measures to facilitate exit from conflicting relationships.

Keywords: Household Economics, Single Parent, Spouse, Family Law, Homeownership, Subsidized Housing, Social Housing

JEL: H31, J12, K36, R21, R28

1. Introduction

About 90 percent of adults worldwide lack access to formal housing finance, and the demand for new housing units is expected to reach 3 billion people by 2030.¹ As a result, subsidized homeownership programs have become prevalent interventions aimed at assisting low and middle-income households on a global scale.²

Subsidized homeownership programs predominantly target low and middle-income individuals and households. However, within this demographic, intimate partner vio-

¹ See [World Bank \(2016\)](#). This number accounts for almost 40 percent of the world’s current population.

² Providing access to housing is one of the targets under the Sustainable Development Goals. A non-exhaustive list of examples includes Home Buyers’ Plan in Canada; Right to Buy in the United Kingdom; *Casa para Todos* in El Salvador; *Pradhan Mantri Awas Yojana* in India; *Minha Casa Minha Vida* in Brazil; Housing Choice Voucher in the US; *Comprar tu Vivienda* in Chile; *Esta es tu Casa* in Mexico; Home Ownership for the People Scheme in Singapore; *Baukindergeld* in Germany.

lence (IPV) is notably prevalent.³ IPV typically involves asymmetrical dynamics, with males perpetrating violence against females. Globally, nearly one-third of women who have been in a relationship report experiencing some form of physical and/or sexual violence by their intimate partner during their lifetime. Moreover, close to half of the women who have experienced IPV also report sustaining injuries as a result of this violence.⁴ Still, IPV remains understudied in economics as a potential negative consequence of housing programs.⁵ While recognizing a body of work beyond economics, we aim to address the existing gap by contributing to the literature through a robust natural experiment.

Our paper studies the link between being a beneficiary of a subsidized homeownership program and IPV. Specifically, we focus on a housing policy implemented in Argentina, where low-income women were selected through a public lottery to receive a house along with a highly subsidized long-term mortgage. The program offered two options: joint-ownership with the cohabiting partner or single-ownership solely by the women. To conduct our study, we collected administrative data on IPV experienced by women who applied to the program, considering any partner they had. Our findings reveal that the program’s impact on IPV varies depending on the type of ownership contract. For women under joint-ownership contracts, we observe an increase in IPV. Conversely, for women under single-ownership contracts, there is a decrease in IPV. We reinforce the validity and quality of the natural experiment by thoroughly examining potential caveats, such as attrition, non-compliance, selection bias, reporting bias, and the limitations associated with a small sample size.

Previous studies have discussed the significance of transfers in poverty alleviation programs, encompassing various types such as in-kind, skills-based, conditional, and unconditional cash transfers.⁶ However, an aspect that has been overlooked in previous

³See [Riley and Brunson \(2018\)](#), [Shepherd \(2019\)](#), and *Primer estudio nacional sobre violencias contra la mujer en Argentina*. In this paper, we follow international standards in Argentinian Law and we define IPV as a subset of domestic violence. Within this paradigm, IPV is an event involving a woman’s conflict with past or current partners.

⁴[WHO \(2013\)](#); [WB \(2017\)](#); [WHO \(2021\)](#).

⁵We acknowledge the existence of relevant literature outside the discipline. For instance, studies in public health ([Yakubovich and Maki, 2022](#); [Yakubovich et al., 2022](#)), criminology ([Boggess and Chamberlain, 2021](#)), psychology ([Bullock et al., 2020](#); [Baker et al., 2010](#)), social policy ([Menard, 2001](#); [Raphael, 2001](#)), sociology ([De Keseredy et al., 1999](#)), and social work ([Malos and Hague, 1997](#)) have explored the relationship between housing and IPV, highlighting the higher incidence of IPV among low and middle-income households.

⁶For an overview of the literature, see [Thurow \(1974\)](#); [Smeeding \(1977\)](#); [Bruce and Waldman \(1991\)](#); [Gahvari \(1994\)](#); [Marton and Wildasin \(2007\)](#); [Angelucci \(2008\)](#); [Currie and Gahvari \(2008\)](#); [Hoynes and Schanzenbach \(2009\)](#); [Baird et al. \(2011\)](#); [Gertler et al. \(2012\)](#); [De Mel et al. \(2012\)](#); [Baird et al. \(2013\)](#); [Bobonis et al. \(2013\)](#); [Hidrobo and Fernald \(2013\)](#); [Skoufias et al. \(2013\)](#); [Blattman et al. \(2014\)](#); [Cunha \(2014\)](#); [Hidrobo et al. \(2014\)](#); [Gangopadhyay et al. \(2015\)](#); [Green et al. \(2015\)](#); [Aizer et al. \(2016\)](#); [Haushofer and Shapiro \(2016\)](#); [Hidrobo et al. \(2016\)](#); [Cunha et al. \(2018\)](#); [Lieber and Lockwood \(2019\)](#); [Edmonds and Theoharides \(2020\)](#); [Bandiera et al. \(2020\)](#); [Cahyadi et al. \(2020\)](#); [Millán et al. \(2020\)](#); [Heath et al. \(2020\)](#); [Sedlmayr et al. \(2020\)](#); [Brooks et al. \(2022\)](#).

research is the liquidity of transfers, which plays a crucial role in understanding the impacts of transfers, income, and wealth shocks. One contribution of our paper is to emphasize the importance of liquidity by providing clear and causally identified evidence on the effects of an illiquid in-kind transfer on IPV. Our findings indicate that policies that transfer illiquid assets to women have the potential to reduce IPV, aligning with previous research highlighting the relationship between transfers and empowerment (Bobonis et al., 2013; Roy et al., 2015, 2019). Furthermore, our results align with the perspective that the impact of interventions on women’s empowerment and IPV tends to be ambiguous on average (Ngo and Wahhaj, 2012; Doepke and Tertilt, 2019; Heath et al., 2020). Given this context, it becomes essential for researchers to delve into the relevant heterogeneity underlying such interventions to assess which groups of recipients may face a disadvantaged position. In our study, we show that women who receive the program under a joint-ownership contract are particularly vulnerable to violence, and this heterogeneity stems from the lack of liquidity associated with the transfer.

Within the discipline of economics, researchers have extensively examined homeowners and their communities to gain insights into homeownership (Dietz and Haurin, 2003; Coulson and Li, 2013). For example, studies have explored the academic performance of homeowners’ children (Green and White, 1997; Aaronson, 2000; Haurin et al., 2002), homeowners’ wealth accumulation (Di et al., 2007; Turner and Luea, 2009; Wainer and Zabel, 2020), sense of community engagement (Engelhardt et al., 2010; Hausman et al., 2022), and degree of responsible citizenship (DiPasquale and Glaeser, 1999). In our contribution to this literature, we utilize a natural experiment that enables us to establish a causal relationship. Our analysis provides compelling evidence of one negative and one positive outcome associated with homeownership. Specifically, an increase and a decrease in IPV among women who receive the house under joint-ownership and single-ownership contracts, respectively.

Our research contributes to the existing literature on the “house-lock-in” effects associated with homeownership. Oswald (1996) examines the potential relationship between homeownership and increased unemployment or underemployment, primarily due to the significant costs involved in relocating to areas with better job opportunities. Oswald’s ideas have given rise to the notion that homeownership, by reducing the likelihood of moving, could be associated with lower wages and higher unemployment rates.⁷ The concept of house-lock-in, arising from the ownership of illiquid real estate assets, has gained considerable attention in understanding these dynamics.

The legal implications of winning the lottery for partners share similarities with the effect of the marriage contract on increasing the costs of ending the partnership. The law and economics literature considers marriage as a contract that imposes higher costs for partners to dissolve their relationship compared to cohabitation alone (Rowthorn

⁷ See Oswald (1999); Green and Hendershott (2001); Munch et al. (2006); Coulson and Fisher (2009); Head and Lloyd-Ellis (2012); Zabel (2012); Blanchflower and Oswald (2013); Wolf and Caruana-Galizia (2015); Laamanen (2017); Broulíková et al. (2020).

and Dnes, 2002). The marriage contract serves as a commitment device in the marriage market, introducing transaction costs that discourage partners from exiting the partnership (Matouschek and Rasul, 2008). This commitment device creates a lock-in effect within the marriage market, resulting in more stable relationships with various desirable outcomes.⁸ However, studies have found that decreases in transaction costs through unilateral divorce laws are associated with reduced IPV (Dee, 2003; Stevenson and Wolfers, 2006; Stolzenberg and D'Alessio, 2007; Brassiolo, 2016). Identifying the causal impact of increased transaction costs associated with marriage is challenging due to the endogenous nature of marriage in relation to most relevant outcomes. We contribute to this literature by exploiting a housing program that provides exogenous variation in increasing the costs associated with exiting a relationship, and our results are consistent with previous findings studying unilateral divorce laws. Specifically, an increase in transaction costs resulting from joint ownership of a real estate asset leads to a rise in IPV. The homeownership contract triggers a similar lock-in effect as the marriage contract.

2. Background

2.1. Institutional Context

In October 2005, the Secretary of Health and Human Development in Salto (Buenos Aires, Argentina, approximately 34,000 inhabitants) launched a housing program targeted at low-income couples, either married or cohabiting (Joint ownership). If not married or cohabiting, women in charge of children were allowed to apply for a single-ownership contract (Women ownership).

The intervention consisted in delivering a house located close to the urban center of the city of Salto (see Figure B1). The national government plans, the provincial government oversights, the municipal government implements the program, and construction is outsourced to private firms. Beneficiaries received a finished house. The houses were located within the city limits. House's size is 54 square meters, in plots of land of around 300 square meters. Houses have two bedrooms, one bathroom, one kitchen, a living room, and a yard.

The assignment of 233 subsidies and houses was made by means of a public lottery on April 18th, 2007. The procedure was as follows. First, among women not married or cohabiting, each woman was given a number and a notary was sequentially picking balls with numbers from a turning globe until completing the assignment of 10% of the houses. Then, the notary repeated the procedure among the remaining eligible applicants.⁹ In order to cover any eventual vacancy, after the assignment of the last beneficiary the procedure continued by assigning a ranking for substitutes.

⁸ Being married is associated with better health, more happiness, higher wages, more wealth, and less drug use (Waite and Gallagher, 2000). 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).

⁹ Importantly, women's ability to determine whether their application is classified as joint-ownership

The loan financed 100 percent of the house, without upfront payments, in 600 monthly installments, and at a zero percent nominal rate. In 2007 the inflation rate was 20 percent, and the expectation was that inflation was on an increasing path. Expectations turned out to be accurate, with an inflation rate of around 100 percent as of 2022. Mortgage finance is offered by the state to beneficiary households as part of the program through the Bank of the Province of Buenos Aires (*Banco Provincia*).¹⁰

Beneficiaries of the program are required to sign three contracts to become the legal owners of the unit: an awarded contract that certifies the legal winners of the house; a mortgage agreement that establishes the terms and conditions of the home-loan repayment obligations; and a tenure contract that grants the usufruct of the house in favor of the national government.¹¹ Houses cannot be sold, leased, or rented until full payment.

In sum, beneficiary households received the house in exchange for a long-term mortgage at a heavily subsidized rate, and they are entitled to full property rights only after complete repayment of the mortgage. In the meanwhile, they can live in the unit under the tenure contract but they cannot sell or rent the property until obtaining complete property rights, which happens after full mortgage repayment. A few months after the lottery assignment, by late 2007 and early 2008, beneficiaries started to occupy the houses. To the best of our knowledge, there is no publicly available individual-level information on the exact occupancy date.

The program received 506 applications, and 466 applicants met the eligibility requirements. Eligible applicants with disabilities, police officers, and firefighters were assigned the credit without participating in the lottery. Given that our empirical strategy is based on the random assignment of the credit, we keep only the 445 applicants who participate in the lottery.

Table A1 shows the composition of our sample according to treatment status (treatment and control) and strata category (joint-ownership and women single-ownership applications). Overall, we end up with 209 units in the treatment group and 228 in the control group in our main sample of non-attritor eligible applicants. Of those 437 units, we observe 290 applications from joint-ownership applicants and 147 applications from women single-ownership. Within strata, we observe 150 units in the treatment group and 140 units in the control group among joint-ownership applications, and 59 units in the treatment group and 88 units in the control group among women single-ownership applications.

or single-ownership is limited due to the mechanistic categorization process based on observable characteristics and supported by legal documentation.

¹⁰ Back-of-the-envelope calculations indicate that the present-discounted value of the subsidy represents more than 90 percent of the market value of the asset.

¹¹ Winners of the lottery are entitled to full property rights only after complete repayment of the mortgage. Until that contingency, the mortgage is in favor of the national government. In the event that beneficiaries stop meeting their mortgage obligations, the national government retains the right to foreclosure and eviction.

There are three main types of individuals eligible to homeownership under this program depending on their baseline housing situation. First, beneficiaries can live in an informally-owned housing unit characterized by poor-quality construction and lack of basic services such as sanitation services, supply of clean water, electricity, and law enforcement. We define this subgroup as “Homeless”. Second, beneficiaries can come from a situation where they live as guests with a hosting family (i.e., multi-generational households). We define this subgroup as “Multi-generational households”. Third, beneficiaries can be renters without sharing spaces at the time of the application. We define this subgroup as “Renters”.

In addition, the ownership of a real estate asset and a long-term term home-loan has a non-negligible amount of legal obligations associated. It involves three different legal documents, two contracts, and a mortgage, effectively increasing the cost of the dissolution of the union in the case of joint-ownership applications winning the lottery. This is not the case for single-ownership applications winning the lottery, and it is irrespective of the previous homeownership status.¹²

2.2. Data

Data on reported IPV was provided by Salto’s *Centro de Asistencia a la Víctima* (henceforth, CAV). We had access to all criminal records since CAV’s establishment in January 2001 until the data was collected in September 2015. This means that we have detailed reported IPV for all women that applied to the program conditional on they living in Salto. In other words, we have population records on reported IPV for the period January 2001 to September 2015.¹³ We define IPV as a binary variable that equals 1 for all women whose name and ID number in the housing database match a name and ID number in the administrative IPV record, and 0 otherwise. Our IPV operational definition implies a binary variable that measures, for a given woman, if there is a record of IPV at CAV involving her. Importantly, our main measure of IPV is an extensive measure because it takes into account all partners and not only the partner in the program, and the measure encompasses all other possible administrative sources available because CAV is the governmental agency in charge of the centralization of IPV records. If the original complaint is not made at CAV but at the police department or the woman-specialized police department, the CAV intervenes to follow up on the relationship. If a woman enters the hospital and the doctor certifies her injuries, the woman-specialized police department intervenes as well as the CAV to follow up on the relationship.

There is one limitation with the administrative data that posits an additional challenge. Since our measure of IPV is self-reported, there could be reporting bias. Treatment may increase awareness of violent behaviors and may change the extent of reporting as opposed to the actual incidence of the underlying phenomenon of interest.

¹² See Appendix C.1 for more details about the housing program.

¹³ Notice that our database includes only IPV claims initiated by women.

This is an important issue, and it is the reason why many relevant papers in the field of IPV use objective measures of violence that are not subject to reporting bias.¹⁴ We do not have data from ambulance calls, hospital, or insurance records that might be better than CAV’s reports to capture the real extent of IPV. To deal with this concern, we complement this data with a victimization survey conducted between November 2016 and March 2017, which is arguably less subject to reporting bias. It is generally accepted that self-reported victimization surveys are a suitable method for identifying victimization not reported to the authorities.¹⁵ Moreover, the victimization survey was designed to be comparable with the administrative data, and thus we followed the same criteria from national regulations and the World Health Organization to define our main outcome of interest.¹⁶

Data on the housing program, randomization, and treatment status comes from the municipal authorities in charge of the program at the *Secretaría de Desarrollo Humano* (Secretary of Human Development). We had access to records of the registration to the program. We also collected information about a set of baseline characteristics from the registration files. Summary statistics are reported in [Table 1](#). We show summary statistics separately for joint- and women single-ownership applications. At baseline, women’s age average in joint-ownership applications is 29 years old, 4 years younger than women in single-ownership applications on average. Although having children is a necessary condition for observing women in single-ownership applications, almost all women in joint-ownership applications had children at baseline. Both groups are different in civil status composition as well. Among joint-ownership applications married women are the first minority, with 38% of the share. Separated, divorced, and widow women account for the remaining 5%. Among women single-ownership applications separated is the first minority, with 30% of the share. Widow, divorced, and married follows with 10% of the share.¹⁷ Turning to the residence status at baseline, both groups look similar from the standpoint of their housing needs. Almost 5% of joint-ownership applicants were homeless, and 7% among women in single-ownership applications. Roughly half of the applicants lived in a multi-generational household,

¹⁴ For example, see [Aizer \(2010\)](#) or [Card and Dahl \(2011\)](#).

¹⁵ The telephone survey was conducted by MBC MORI, a public opinion consultant with more than 20 years of experience. They follow standard protocols and techniques for phone calls to maximize reach and guarantee the security of the interviews.

¹⁶ Using this purposely designed survey that follows the Argentinian law to capture the different types of violence, we can distinguish between psychological violence or emotional abuse (any type of involuntary isolation or coercive impediment employed by a woman’s intimate partner that affects her emotional stability and self-esteem), physical violence (any act committed by a woman’s intimate partner that affects her physical stability as stipulated in the legal system), sexual violence (incidental sexual abuse and forced performance of sexual acts to obtain economic benefits for the benefit of a woman’s intimate partner), and economic violence (if a woman has problems if she wants to work, if she has been forced her to ask for money, or if she is not part of the economic decisions of the household).

¹⁷ Notice that single-ownership refers to the tenure type and not to the civil status of the women.

one-third of applicants lived in a rented house, and more than half of the households were overcrowded in our sample at baseline. One in five houses had a gas connection, and one in three houses had sewers, but nine out of ten houses had a water connection, and almost all had electricity connection at baseline.¹⁸

3. Econometric Framework

3.1. Research Design

We want to estimate the causal effect of homeownership on IPV. Taking into consideration the randomization procedure implied by the housing lottery, there are two different samples: joint-ownership applicants and single-ownership applicants.

The randomization procedure implied by the housing lottery is analogous to random assignment within strata. Whenever the program is as good as conditionally randomly assigned within strata, we estimate separate models to isolate the causal effect within strata. Therefore, we estimate the effect of subsidized homeownership programs among joint-ownership applications ($\omega = 0$), and single-ownership applications ($\omega = 1$) with a linear probability regression model within strata:

$$IPV_i(\omega) = \alpha(\omega) + \beta(\omega)D_i(\omega) + \varepsilon_i(\omega) \quad (1)$$

for $\omega \in \{0, 1\}$ where the dependent variable $IPV_i(\omega)$ is a binary measure of IPV that takes the value of one for those women with IPV after April 2007 (when the lottery took place), and zero otherwise. The main independent variable $D_i(\omega)$ is a binary variable that equals 1 if the woman receives the house and the loan, and zero otherwise. ε_i is the heterogeneity unobserved by the researcher. There is one observation for each woman that applied to the program. We estimate separate models for joint-ownership applications which we refer to as **Joint ownership** and we represent with $\omega = 0$, and single-ownership applications which we refer to as **Women ownership** and we represent with $\omega = 1$.

We wish to interpret the regression coefficient $\beta(\omega)$ in terms of the causal effects of $D_i(\omega)$ on $Y_i(\omega)$, and the random assignment provides a source of exogenous variation for receiving the house and the loan. The random assignment process makes the estimation of causal effects straightforward. We employ instrumental variables and instrument treatment received $D_i(\omega)$ with treatment assigned $Z_i(\omega)$. We show in the paper the LATE obtained by estimating Equation (1) with instrumental variables. We will use the linear probability model for interpretation.

¹⁸See [Cardinale Lagomarsino and Rossi \(2023\)](#) for the data and code to reproduce our findings. The material is also accessible in the following URL: <https://data.mendeley.com/datasets/2zs27x5dsm/1>.

3.2. *Validity and Quality of the Research Design*

We start by testing the validity of the research design. An implication of random assignment is that baseline characteristics should be independent of randomization status. We perform tests of balancing baseline characteristics by treatment assignment status. We show the tests for each stratum separately in [Table 2](#). Both characteristics of the women and their previous residence status are consistent with the experiment validity: 12 out of 14 variables are balanced among joint-ownership applications, and all 14 variables are balanced among single-ownership applications. [Table 2](#) shows that around 8 percent of women with single-ownership applications reported experiencing violence at baseline. Moreover, 16 percent of women with single-ownership applications are in cohabiting relationships. It is essential to underscore that this does not necessarily imply that half of the cohabiting women with single-ownership applications experienced violence at baseline. Various factors may contribute to these statistics. For instance, a woman with a single-ownership application might have encountered violence at baseline with a non-cohabiting partner or a cohabiting partner who is not the father of her children.¹⁹

Additionally, we run a regression of the lottery assignment on the set of baseline characteristics, and the baseline characteristics are not jointly significant to explain randomization status. The p-value of the joint F-test is 0.132 among joint-ownership applications and 0.549 among single-ownership applications.

Two potential sources of attrition (the disappearance of an individual from our sample after the lottery assignment) might undermine exogeneity in the natural experiment: dying or moving out of Salto. Differential outmigration from Salto according to assignment status could lead to biased estimates of the impact of the program. We use the information provided by the Secretary of Health and Human Development in Salto and we further complement it with administrative data from the 2015 national ballot registry for the 2015 Presidential Election. This last source is particularly useful because Argentina has elections every other year, voting is mandatory, and citizens vote in the polling station closest to their legal address. The Electoral Justice prepares the national ballot registry six months before the date of the elections. Voters can request modifications free of charge up to 20 days before the elections. Moving out of a city and keeping the old address in the ballot registry is costly. The year of an election, citizens must cast their vote at the minimum twice. First, the mandatory, simultaneous, and open primaries. Second, the general election. Moreover, when there are Presidential Elections, citizens may have to cast their vote three times if there is a runoff election, and even four times if the state-level elections are not held on the same date as the national elections. The turnout rate in the first round of the 2015 Presidential Election

¹⁹ All results are based on within-strata comparisons. This means that any observed disparities between strata, including variations in baseline violence, do not affect the overall conclusions drawn from our research. Indeed, in [Table 4](#) we show that the main findings remain consistent even after controlling for the set of baseline characteristics.

was 81 percent. Those who do not turn out to vote are included in the Registry of Offenders of the National Electoral Chamber. They may reasonably justify their absence to the National Electoral Justice, otherwise, they must pay a fine. We report in [Table 2](#) the results from the attrition check using both sources of information. Attrition is a binary variable that takes the value one for those applicants that died or moved out from Salto by September 2015. The level of attrition in our sample is very low, and attrition is orthogonal to treatment assignment.

Finally, another potential source of bias is non-compliance. Compliance in our pre- and post-attrition samples is extremely high, as reported in [Table 3](#). This table reports the OLS estimates from a regression of being a beneficiary on randomized status (our instrumental variable, a binary that takes value one if the woman was assigned to receive the credit by means of the lottery) and baseline characteristics as covariates. We report results without attriters. Our first-stage estimates indicate that being randomly assigned as a beneficiary increases the probability of actually receiving the house by 91 percentage points among joint-ownership applications, and ranging between 90 and 93 percentage points among single-ownership applications. We also report heteroskedasticity robust [Kleibergen and Paap \(2006\)](#) Wald rk F-statistic test statistics for weak instruments, which suggest a very strong first stage within strata.

We conclude that the lottery assignment of beneficiaries, the insignificance of attrition and non-compliance, and the balancing of baseline characteristics indicate that results presented following the identification strategy below are not subject to significant sources of selection bias. Nonetheless, we show results together with tests of the exclusion restriction.

4. Results

4.1. Administrative Data

Our estimates of the impact of being a beneficiary of the long-term mortgage to buy the house on IPV are presented in [Table 4](#), columns 1 to 4. We show estimates with and without covariates. In all models, our estimates indicate that being a beneficiary of the program significantly increases the probability a woman reports being exposed to IPV to the local authorities among joint-owners and decreases the probability a woman reports being exposed to IPV among single-owners. For the inference procedure, we show Huber–White standard errors that are robust to heteroskedasticity ([White, 1980](#)).²⁰

The estimates are statistically different from zero and statistically different from each other at conventional levels. A potential concern is that an unobserved time-varying shock that occurred in one of the treated neighborhoods time after the treatment could have caused the surge in IPV (there are two neighborhoods in which the

²⁰See in [Table A2](#) p-values that do not rely on asymptotic theory or distributional assumptions: a non-parametric permutation test that computes the null distribution of the test statistic under minimal assumptions (random assignment and no treatment effect). Results are similar. See [Anderson \(2008\)](#) and [Young \(2018\)](#) for more details.

new houses are located). To account for the possibility that a local shock to one of the treatment neighborhoods may be driving the results, we estimate separate effects by strata-neighborhood. The estimated coefficients have the same sign and statistical significance, thus suggesting that the effect is not arising from location-specific unobserved shocks (see [Table A3](#)).

In addition, we show in [Table 5](#) results from an exercise in which we analyze the IPV files with more detail to understand the nature and intensity of the conflict, and we create additional variables from the administrative records. We distinguish IPV with the partner the woman had at baseline from IPV with any partner (our measure of interest so far), this variable is called *IPV with partner* (column 1). We obtain information on how many days the claim is followed up by CAV employees before closing the file and we name this measure *Record days* (column 2). Similarly, we distinguish if the complaint in the file explicitly mentions physical violence with the assumption that every complaint filed without physical violence is associated with an underlying level of emotional violence. We split our measure of IPV in the administrative data into *IPV (emotional)* in column 3, and *IPV (Physical)* in column 4.

In sum, we find that joint-owners beneficiaries of the program are almost twice more likely to report IPV than those joint-ownership applications without receiving the program. Among women on single-ownership applications, there is strong evidence against an increase in IPV reports and at least weak evidence in favor of a decrease in IPV reports.²¹ The results estimated in the administrative data suggest a 180% increase among joint-owners and a 50% decrease among women single-owners in the probability of experiencing IPV.

4.2. Survey Data

An important question regarding the interpretation of our findings is whether the difference in IPV rates within strata is due to differences in the incidence of IPV and/or to reporting being correlated with treatment status. We conducted a purposely designed victimization survey to further explore whether reporting bias is driving the results we observe.

Even though we targeted the universe of eligible women, we only managed to collect a sample of 122 women (28 percent of the eligible population), 61 randomized as beneficiaries and 61 randomized as non-beneficiaries.²² [Table A4](#) reports summary statistics for non-attriters, those applicants answering the survey, and attriters. Survey attrition is high, but treatment status is balanced in the survey sample, and survey

²¹ These results we find are robust to different tests to the exclusion restriction to validate the natural experiment. The significance and magnitude of results are robust to controlling for the set of baseline characteristics (see [Table 4](#)). There is no effect on IPV as measured before the intervention (see [Table 2](#)). Alternative models yield the same qualitative results (see columns 5 and 6 in [Table 5](#) for probit and differences estimators, respectively).

²² In this sub-sample there is full compliance, and therefore Beneficiary and Randomized as beneficiary are the same variables.

attrition is balanced in all baseline characteristics including previous IPV in 2005. In contrast, attrition is not balanced in the main outcome of interest from the administrative data in the women-ownership group. The sub-sample of the survey is selected in the sense that those women with IPV in the administrative data are more likely to answer the survey. If they are in the joint-ownership sub-group, the difference is small in size and not statistically different from zero. If they are in the women-ownership sub-group, the difference is larger in size and statistically different from zero. That is, we have a selection problem in our outcome as measured in the administrative data in the women-ownership sub-group, and hence our sample is not representative of the population data. Selecting into taking the survey is an important factor that posits an additional challenge to the task of disentangling if the effect we observe is due to the incidence or reporting of IPV happening. Recall that our results in the administrative data for the women-ownership subgroup imply a decrease in IPV for beneficiaries of the program. To mitigate the concern and deal with selection, we build a propensity score using the baseline characteristics to predict survey participation, and then we show in [Table 4](#), columns 5 to 8, results weighted by the inverse probability of participating in the survey.²³ Our findings consistent with previous estimates from reports in the administrative data. We find that the program causes an increase in IPV for women under joint ownership contracts. Among women under single-ownership contracts, we can confidently rule out this increase in IPV. Furthermore, we find evidence in favor of a notable reduction in IPV.

5. Interpretation of the Results

In spite of the aforementioned challenges, we show that our results are consistent for the two sub-groups using two independent sources of measurement for IPV. Results using the administrative data and the survey data are also aligned in predicting the ordering of the sub-groups in terms of IPV incidence. Women beneficiaries of the joint-ownership program are the group with the greatest incidence of IPV in the administrative and survey data. The second most disadvantaged group in terms of IPV are those women in single-ownership applications but not beneficiaries of the program. Finally, both single-ownership beneficiaries and joint-ownership applications not beneficiaries are less disadvantaged in the relative ordering of IPV. The former sub-group is slightly better if we look into the survey data.

We contrast our results with the prevalent views about violence. The evidence is consistent with the exposure view of IPV, by-which violence grows with the time the partners spend together ([Chin, 2012](#)). Among joint-ownership applicants, results are consistent with the instrumental or extractive view, by-which violence may be a tool to control women’s resources ([Macmillan and Gartner, 1999](#); [Bloch and Rao, 2002](#); [Heath, 2014](#)). Still among joint-ownership applicants, as long as the joint-ownership implies a

²³ Unweighted results are qualitatively the same (see [Table A5](#)).

better situation than before the assignment for the woman relative to the man in terms of outside options, results are consistent with the status threat view or backlash view by-which some men might resort to violence to assert their dominance when they feel that their status is threatened (Angelucci, 2008; Luke and Munshi, 2011).

Results for women under single-ownership applications are consistent with the stress or scarcity view, by which a positive shock should lead to a decrease in IPV by alleviating scarcity and/or stress. The literature on transfers discussed previously suggests that poverty relief from transfers might reduce IPV (Bobonis et al., 2013; Haushofer and Shapiro, 2016; Heath et al., 2020).

The intrinsic or expressive view of violence in this context suggests that IPV is an intrinsic source of utility for the perpetrator. The expressive view of violence, with violence as a normal good, has associated a substitution effect among types of violent activities. This substitution effect has been documented in studying assaults and intimidation (Dahl and DellaVigna, 2009). Household bargaining models can incorporate violence with the underlying assumption that violence is a normal good, and it can be substituted with other violent activities. The evidence for the intrinsic view is not consistent with our findings. If higher bargaining power increased women’s ability to lower IPV, we would expect the substitution effect to kick in: IPV should decrease in both groups and not only among women under single-ownership contracts.

6. Conclusion

We study a natural experiment in Argentina, where low-income women were selected through a lottery system to receive a house and a heavily subsidized long-term mortgage. We exploit the random assignment to estimate the causal link between subsidized homeownership programs and intimate partner violence. We find that women who are joint-owners and beneficiaries of the program are more likely to report IPV. In contrast, women who receive the program solely in their own names are less likely to report IPV.

Our results suggest that the liquidity of a transfer may be an overlooked factor when studying the effects of transfers, as well as income and wealth shocks more broadly. Our analysis unveils unintended consequences of a widespread housing policy. The asymmetry of these unintended consequences based on tenure type exemplifies the limitations of a one-size-fits-all policy approach. Therefore, our results emphasize the importance of incorporating explicit provisions in the design of subsidized homeownership programs to facilitate exit from conflicting relationships. Furthermore, the results for women under single-ownership applications suggest that policies transferring illiquid assets to women may help reduce IPV.

Understanding the connections between homeownership and IPV is a crucial step towards improving not only housing policies but also broader initiatives aimed at effectively reducing gender-based violence.

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Tables

Table 1: Summary statistics

	Joint ownership		Women ownership	
	Mean (1)	Standard deviation (2)	Mean (3)	Standard deviation (4)
<i>Women characteristics</i>				
Age	29.27	8.632	33.53	9.978
Children	0.959	0.198	1	0
Cohabitation	1	0	0.161	0.369
Never married	0.554	0.498	0.584	0.495
Married	0.388	0.488	0.007	0.082
Separated	0.041	0.198	0.302	0.461
Divorced	0.014	0.116	0.034	0.181
Widowed	0.003	0.058	0.067	0.251
<i>Previous residence</i>				
Homeless	0.048	0.213	0.074	0.262
Multi-generational	0.418	0.494	0.483	0.501
Renter	0.323	0.468	0.329	0.471
Gas connection	0.218	0.413	0.215	0.412
Water connection	0.867	0.340	0.906	0.293
Electricity connection	0.993	0.082	0.966	0.181
Sewers	0.286	0.453	0.369	0.484
Overcrowding	0.609	0.489	0.570	0.497
<i>Research design</i>				
Randomized to housing	0.514	0.501	0.403	0.492
House	0.514	0.501	0.430	0.497
Attrition (admin)	0.014	0.116	0.013	0.115
Observations	294	294	149	149

Notes: An observation is an application, either a joint ownership application or a woman ownership application. Every observation has a female applicant associated but not all observations have a male applicant associated. Data comes from the municipal authorities in charge of the program at the *Secretaría de Desarrollo Humano* (Secretary of Human Development). All variables but Age are binary. All variables were observed at baseline. Age is a continuous variable recording the age of the women at baseline. Children indicate if the woman associated with the application has at least one child. Cohabitation indicates if the woman associated with the application has a cohabitation partner. Never married, Married, Separated, Divorced, and Widowed is a set of indicators for civil status. Homeless indicates whether the household associated with the application has experienced homelessness for at least a year. Multi-generational indicates whether the household associated with the application includes two or more adult generations (ages 25 or older) or a “skipped generation” which consists of grandparents and their grandchildren younger than 25. Renter indicates whether the previous residence of the household associated with the application was a rented property. Gas connection, Water connection, Electricity connection, and Sewers, indicate if the previous residence of the household associated with the application had a connection to the gas network, tap or piped water network, electric network, and sewers. Overcrowding indicates if the household associated with the application had a ratio of persons per bedroom greater or equal to three. Randomized to housing indicates whether the household associated with the application was assigned by lottery to receive the house and the loan. House indicates if the household associated with the application received the house and the loan. Attrition (admin) indicates if the woman associated with the application died or moved out from the municipality after the lottery day.

Table 2: Balance and attrition

	Joint ownership			Women ownership		
	Control mean (1)	Lottery (2)	p-value (3)	Control mean (4)	Lottery (5)	p-value (6)
Age	29.68 (0.783)	-0.811 (0.642)	0.424	32.01 (1.004)	3.772 (1.338)	0.0250
Children	0.944 (0.019)	0.0290 (0.013)	0.208	- -	- -	-
Cohabitation	- -	- -	-	0.180 (0.041)	-0.0460 (0.044)	0.442
Never Married	0.622 (0.041)	-0.132 (0.041)	0.022	0.607 (0.052)	-0.0570 (0.065)	0.496
Married	0.315 (0.039)	0.142 (0.041)	0.012	- -	- -	-
Separated	0.056 (0.019)	-0.029 (0.023)	0.270	0.270 (0.047)	0.097 (0.077)	0.219
Divorced	0.00700 (0.007)	0.0130 (0.011)	0.336	0.0340 (0.019)	0 (0.023)	0.990
Widow	0 (0.000)	0.00700 (0.007)	0.318	0.0790 (0.033)	-0.0290 (0.034)	0.479
Homeless	0.0420 (0.017)	0.0110 (0.018)	0.658	0.0560 (0.025)	0.0440 (0.039)	0.343
Multi-generational	0.385 (0.019)	0.0660 (0.023)	0.255	0.517 (0.053)	-0.0840 (0.065)	0.320
Renter	0.301 (0.038)	0.0440 (0.039)	0.425	0.292 (0.048)	0.0910 (0.063)	0.254
Gas connection	0.189 (0.033)	0.0560 (0.035)	0.243	0.225 (0.044)	-0.0250 (0.052)	0.719
Water connection	0.853 (0.030)	0.0280 (0.026)	0.488	0.899 (0.032)	0.0180 (0.036)	0.713
Electricity connection	0.993 (0.007)	0 (0.007)	0.969	0.955 (0.022)	0.0280 (0.017)	0.309
Sewers	0.280 (0.038)	0.0120 (0.037)	0.825	0.360 (0.051)	0.0240 (0.063)	0.770
Overcrowding	0.594 (0.041)	0.0280 (0.040)	0.623	0.551 (0.053)	0.0490 (0.064)	0.552
Attrition	0.0210 (0.012)	-0.0140 (0.007)	0.297	0.0110 (0.011)	0.00500 (0.017)	0.787
IPV at baseline	0.0640 (0.021)	-0.0180 (0.017)	0.515	0.0800 (0.029)	0.00500 (0.037)	0.911
Joint F-test			0.142			0.549
Observations	143	151	294	89	60	149

Notes: Columns (1) and (4) report the mean for the control group of each stratum, with standard errors in parenthesis. Columns (2) and (4) show the difference in the means among control groups and those who were selected by the lottery to receive the program, with standard errors in parenthesis. The p-value in columns (3) and (6) corresponds to a test of differences in means computed with heteroskedastic-robust standard errors. At the bottom of the table, columns (3) and (6) display the p-value from a test of joint significance of the baseline characteristics after an OLS regression with treatment assignment in the dependent variable and baseline characteristics as independent variables. IPV at baseline is a binary measure of IPV that takes the value of one for those women with IPV between January 2001 (when the CAV started its operations) and April 2007 (when the lottery took place), and zero otherwise. We use the sample of non-attritor-eligible applicants to compute IPV at baseline.

Table 3: First stage

	Dependent variable: Beneficiary			
	Joint ownership		Women ownership	
	(1)	(2)	(3)	(4)
Randomized as beneficiary	0.917*** (0.024)	0.913*** (0.024)	0.909*** (0.034)	0.915*** (0.038)
Dependent variable mean	0.514	0.514	0.430	0.430
Kleibergen-Paap F statistic	1522	1390	701	743
R-squared	0.841	0.847	0.815	0.861
Control variables	No	Yes	No	Yes
Observations	290	290	147	147

Notes: Heteroskedastic-robust standard errors in parentheses. Dependent variable equals 1 if the woman is a beneficiary of the program and zero otherwise. The instrument and main independent variable is “Randomized as beneficiary” and equals 1 if the woman was assigned by the lottery as a beneficiary of the program and zero otherwise. The set of control variables is age, age squared, and binary variables to indicate never married, married, divorced, widowed, guest, renter, previous house, gas connection, water connection, electricity connection, sewers, overcrowding, and IPV at baseline. For joint owners, it also includes an indicator for children. For women owners, it also includes an indicator for cohabitation. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: Main results

	Dependent variable: IPV							
	Joint ownership		Women ownership		Joint ownership		Women ownership	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Beneficiary	0.173*** (0.049)	0.196*** (0.047)	-0.113 (0.070)	-0.116* (0.069)	0.072 (0.122)	0.069 (0.127)	-0.608*** (0.172)	-0.501*** (0.146)
Control group mean	0.114	0.114	0.239	0.239	0.386	0.386	0.887	0.887
Control variables	No	Yes	No	Yes	No	Yes	No	Yes
Sample	Admin	Admin	Admin	Admin	Survey	Survey	Survey	Survey
Observations	290	290	147	147	85	85	37	37

Notes: Heteroskedastic-robust standard errors in parentheses. The 437 observations correspond to the sample of eligible non-attritor women. Dependent variable equals 1 if the women reported IPV in the administrative data and zero otherwise in columns 1 to 4. Dependent variable is equal to 1 if the woman reported IPV in the survey and zero otherwise in columns 5 to 8. The main independent variable equals 1 if the woman is a beneficiary of the program and zero otherwise. The instrument equals 1 if the woman was assigned by the lottery as a beneficiary of the program and zero otherwise. The set of controls is the same as in Table 3. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Further results in the administrative data

	IPV with partner (1)	Record days (2)	IPV (emotional) (3)	IPV (physical) (4)	IPV (probit) (5)	IPV (Δ) (6)
<i>Panel A: Joint ownership</i>						
Beneficiary	0.164*** (0.042)	57.635 (58.048)	0.084*** (0.029)	0.112*** (0.041)	0.200*** (0.049)	0.193*** (0.050)
Dependent variable mean	0.141	158.166	0.059	0.138	0.197	0.141
Control group mean	0.071	147.764	0.021	0.093	0.114	0.050
Observations	290	290	290	290	290	290
<i>Panel B: Women ownership</i>						
Beneficiary	-0.032* (0.019)	-97.282** (42.410)	-0.023 (0.045)	-0.093 (0.059)	-0.096 (0.069)	-0.116** (0.069)
Dependent variable mean	0.020	78.259	0.068	0.129	0.197	0.116
Control group mean	0.034	111.773	0.068	0.170	0.239	0.159
Observations	147	147	147	147	147	147

Notes: Heteroskedastic-robust standard errors in parentheses. The dependent variables in the columns are as follows: (1) IPV with partner, (2) Record days, (3) IPV (emotional), (4) IPV (physical), IPV, and IPV (difference). All coefficients but column (5) are from a 2SLS estimation. Column (5) is an instrumental variables probit. All regressions include the same set of controls as in Table 3. *** p<0.01, ** p<0.05, * p<0.1.

Online Appendixes

Appendix A. Tables

Table A1: Composition of non-attritors eligible applicants

	Joint ownership (1)	Women ownership (2)	(3)
Control	140	88	228
Treatment	150	59	209
Total	290	147	437

Notes: Columns 1 and 2 split the sample of non-attritor-eligible applicants between those women that had or would have been assigned jointly with their partner as beneficiaries, and those women that had or would have been assigned as the only beneficiaries of the program. Rows show the composition of the sample according to treatment status.

Table A2: Main results and permutation tests

	Dependent variable: IPV			
	Joint ownership		Women ownership	
	(1)	(2)	(3)	(4)
Randomized as beneficiary	0.159 (0.001) {0.000} [0.000,0.001]	0.179 (0.000) {0.000} [0.000,0.001]	-0.103 (0.110) {0.144} [0.138,0.151]	-0.107 (0.117) {0.126} [0.119,0.133]
Control group mean	0.114	0.114	0.239	0.239
Control variables	No	Yes	No	Yes
Observations	290	290	147	147

Notes: In parentheses, heteroskedastic-robust p-values. In curly brackets, p-values estimated with Monte Carlo simulations and 10,000 permutations. In square brackets, the 95% confidence intervals associated with the estimated p-values. Dependent variable equals 1 if the woman is a beneficiary of the program and zero otherwise. The main independent variable is “Randomized as beneficiary” and equals 1 if the woman was assigned by the lottery as a beneficiary of the program and zero otherwise. The set of control variables is the same as in Table 3. Although the Huber–White standard errors are asymptotically consistent, our samples are moderate and the occurrence of violence is a relatively rare event. The standard errors may be misleading, particularly an issue if the normality assumption does not hold in the underlying data. To address this concern, in addition to the t-based confidence intervals, we test our results against a permutation-test-based approach.

Table A3: Main results by neighborhood

	Dependent variable: IPV			
	Joint ownership		Women ownership	
	(1)	(2)	(3)	(4)
<i>Panel A: Location group 1</i>				
Beneficiary	0.214*** (0.061)	0.239*** (0.058)	-0.108 (0.077)	-0.148* (0.079)
Dependent variable mean	0.192	0.192	0.195	0.195
Control group mean	0.116	0.116	0.224	0.224
Observations	224	224	118	118
<i>Panel B: Location group 2</i>				
Beneficiary	0.106* (0.061)	0.129** (0.053)	-0.097 (0.092)	-0.060 (0.092)
Dependent variable mean	0.150	0.150	0.223	0.223
Control group mean	0.118	0.118	0.244	0.244
Observations	200	200	112	112

Notes: Heteroskedastic-robust standard errors in parentheses. The 437 observations correspond to the sample of eligible non-attritor women. Dependent variable equals 1 if the women reported IPV in the administrative data and zero otherwise. The main independent variable equals 1 if the woman is a beneficiary of the program and zero otherwise. The instrument equals 1 if the woman was assigned by the lottery as a beneficiary of the program and zero otherwise. The set of controls is the same as in Table 3. *** p<0.01, ** p<0.05, * p<0.1.

Table A4: Survey attrition

	Joint ownership			Women ownership		
	Survey mean (1)	Attrition (2)	p-value (3)	Survey mean (4)	Attrition (5)	p-value (6)
Age	28.46 (0.850)	1.139 (0.618)	0.279	31.97 (1.617)	2.072 (0.946)	0.268
Children	0.965 (0.020)	-0.008 (0.014)	0.752	1 (0.000)	0 (0.000)	-
Cohabitation	1 (0.000)	0 (0.000)	-	0.162 (0.061)	-0.001 (0.035)	0.984
Never married	0.529 (0.054)	0.0350 (0.034)	0.585	0.676 (0.078)	-0.122 (0.047)	0.181
Married	0.447 (0.054)	-0.083 (0.033)	0.191	- -	- -	-
Separated	0 (0.000)	0.057 (0.016)	0	0.270 (0.074)	0.042 (0.044)	0.623
Divorced	0.024 (0.017)	-0.014 (0.007)	0.434	0 (0.000)	0.045 (0.020)	0.025
Widow	0 (0.000)	0.005 (0.005)	0.319	0.054 (0.038)	0.017 (0.024)	0.698
Homeless	0.047 (0.023)	0.001 (0.015)	0.977	0.081 (0.045)	-0.010 (0.024)	0.851
Guest	0.435 (0.054)	-0.024 (0.034)	0.710	0.459 (0.083)	0.032 (0.047)	0.740
Renter	0 (0.050)	0.041 (0.033)	0.493	0.297 (0.076)	0.042 (0.045)	0.634
Gas connection	0.224 (0.045)	-0.008 (0.029)	0.878	0.324 (0.078)	-0.146 (0.036)	0.091
Water connection	0.906 (0.032)	-0.054 (0.025)	0.179	0.865 (0.057)	0.055 (0.026)	0.380
Electricity connection	1 (0.000)	-0.010 (0.007)	0.158	0.973 (0.027)	-0.009 (0.018)	0.787
Sewers	0.400 (0.053)	-0.161 (0.030)	0.009	0.351 (0.080)	0.024 (0.046)	0.796
Overcrowding	0.612 (0.053)	-0.004 (0.034)	0.948	0.541 (0.083)	0.040 (0.047)	0.675
IPV, at baseline	0.059 (0.026)	-0.005 (0.016)	0.864	0.108 (0.052)	-0.035 (0.025)	0.537
IPV	0.212 (0.045)	-0.022 (0.027)	0.681	0.351 (0.080)	-0.206 (0.034)	0.018
Multivariate joint F			0.126			0.273
Observations	85	209	294	37	112	149

Notes: Columns (1) and (4) report the mean for those who participated in the survey with standard errors in parenthesis. Columns (2) and (4) show the difference in the means among attritors and those who took the survey, with standard errors in parenthesis. The p-value in columns (3) and (6) corresponds to a test of differences in means computed with heteroskedastic-robust standard errors. At the bottom of the table, columns (3) and (6) display the p-value from a test of joint significance of the baseline characteristics after an OLS regression with treatment assignment in the dependent variable and baseline characteristics as independent variables. IPV at baseline, IPV statistic and the multivariate joint F use the sample of non-attritor-eligible applicants.

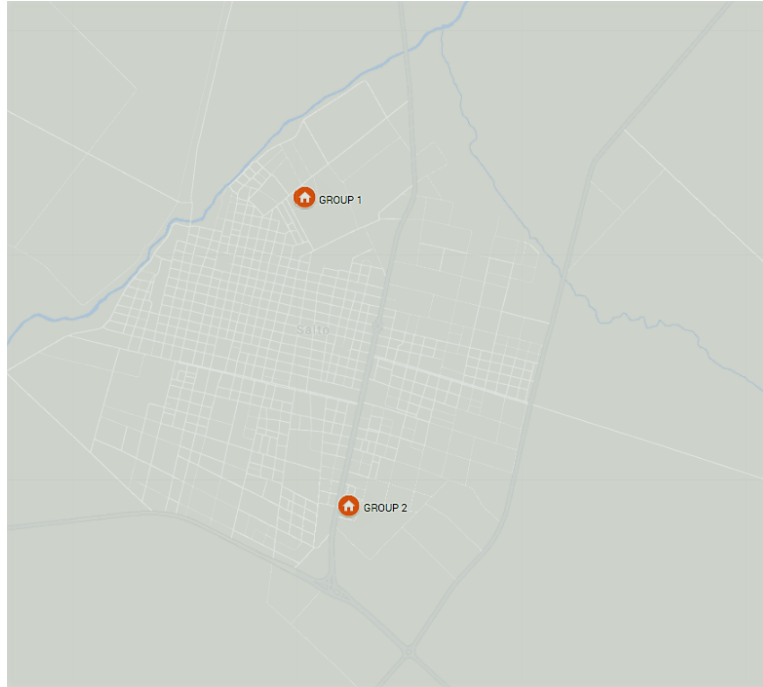
Table A5: Main results in the survey, unweighted

	Joint ownership		Women ownership	
	(1)	(2)	(3)	(4)
Beneficiary	0.046 (0.118)	0.063 (0.126)	-0.502*** (0.176)	-0.402*** (0.147)
Dependent variable mean	0.412	0.412	0.595	0.595
Control group mean	0.390	0.390	0.800	0.800
Control variables	No	Yes	No	Yes
Observations	85	85	37	37

Notes: Heteroskedastic-robust standard errors in parentheses. The 122 observations correspond to the sample of eligible non-attritor women who answered the survey. Dependent variable is equal to 1 if the woman reported IPV in the survey and zero otherwise. The main independent variable equals 1 if the woman is a beneficiary of the program and zero otherwise. The instrument equals 1 if the woman was assigned by the lottery as a beneficiary of the program and zero otherwise. The set of controls is the same as in Table 3. *** p<0.01, ** p<0.05, * p<0.1.

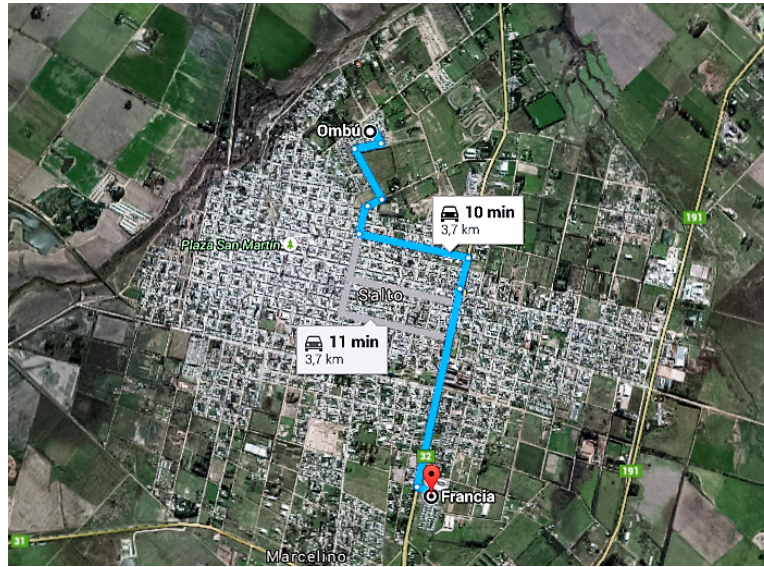
Appendix B. Figures

Figure B1: Location of the houses



Note: The map shows the location of the two neighborhoods where the houses are located. The average distance between centroids of Groups 1, and 2 is 9 minutes by car, 11 minutes biking, and 40 minutes walking. The average distance from the centroid of Group 1 to the main square of the city is 5 minutes by car or biking, and 16 minutes by walking, and 5 minutes by car. The average distance from the centroid of Group 2 to the main square of the city is 7 minutes by car, 8 minutes biking, and 33 minutes walking. Source: Google Maps.

Figure B2: Distances from the houses to the city center



Note: The average distance between the centroid of Group 1 *Francia*, and 2 *Ombú* is 10 minutes by car, 11 minutes biking, and 40 minutes walking. The average distance from the centroid of Group 1 to the main square of the city is 5 minutes by car or biking, and 16 minutes by walking. The average distance from the centroid of Group 2 to the main square of the city is 7 minutes by car, 8 minutes biking, and 33 minutes walking. Source: Google Maps.

Appendix C. Institutional Background

Appendix C.1. The Housing Program

Details. To be eligible, applicants had to provide evidence of at least three years of residence in Salto. Unmarried couples had to provide evidence of at least two years of cohabitation. 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. Given the relatively low value of the houses provided by the program, in practice, this requirement implies that previous owners were informal owners of units characterized by poor-quality construction and lack of basic services such as sanitation services, supply of clean water, electricity, and law enforcement. In our setting, relocation effects and spatial mobility effects are unlikely given that the location of program houses is within the urban center of Salto (a relatively small city), and most beneficiaries were reallocated near their previous residence. [Figure B2](#) shows the two neighborhoods where the houses are located. The average distance from a house to the main square of the city is 6 minutes by car, 7 minutes by biking, and 24 minutes by walking.

A Program After the 2001 Crisis. The initiative of the Argentine national government was a response to the economic and political crisis that broke out by the end of 2001. The programs had different versions for housing emergency, construction, improvement, urbanization of slums and settlements. In this paper we focus on the construction version of the program, the Federal Housing Construction Plan, then every reference to the program or the policy should be interpreted accordingly. Bringing back housing into the public policy agenda in 2004 was a drastic change. The housing policy pursued multiple objectives after the crisis. In fact, the stated purpose of the program has three goals: boost employment, reduce the housing deficit, and reactivate the local economy by mobilizing the construction sector. The program aimed to alleviate the housing needs in population centers with sizable housing deficits and without access to the real estate market. All these goals were put into practice through the Federal Housing Programs, an initiative with centralized organization, and execution delegated to the provincial and municipal governments.

Housing Policy in Argentina. In Argentina, housing is a key element in private and public investment decisions. In a country subject to recurrent economic stress, housing is attractive as a secure way to save and increase wealth within the household. The national government allocates a large amount of public resources to facilitate access to housing, and low-income households are an important target. Since the beginning of the 1970s, the Argentine state has maintained a strong intervention in the housing market with subsidies to mortgage interest rates (demand side), and directly building affordable housing (supply side). Convolutional institutional arrangements involve different levels of government, national, provinces, and increasingly over time the municipalities.

In 1972, the national government created a National Housing Fund (*Fondo Nacional de Viviendas*, FONAVI), the main source of financing for housing policy from then onwards. The Law 19.929 is the establishment of the fund, which was financed originally by salary contributions, and later with a fuel tax. During a first period of centralization (1972-1991) the decisions of location, design, target population, among other elements, were on federal level of government. The provinces, through their agencies, the Provincial Housing Institutes (*Institutos Provinciales de la Viviendas*, IPV), were only executing the directives of the national government. In 1992, Law 24.130 was enacted, and the power of housing policy was delegated to the provinces. The FONAVI began a decentralization process by which the national government only retained control over the use of funds.

The 2002-2004 period was characterized by a sharp contraction in housing production as a result of the socioeconomic and political crisis at the end of 2001. This crisis would also indicate the end of FONAVI. In 2004, the Federal Housing Programs replaced FONAVI as the main Argentine housing policy. The emergence of these programs also entails the partial reversal of the decentralization process to the provinces. The central government defines the operation and destination of the resources regarding the FP, which it executes directly through the provincial and even municipal governments. The budget of the FP is not determined automatically as it happens with the FONAVI, but it is rather annually decided based on the budget.

In summary, along with a process of increasing decentralization, which accelerated in 1992 with the decentralization of the FONAVI to the provinces, the responsibilities of subnational governments in the design and execution of national housing policy grew until the economic and political crisis of 2002. After that, the national government once again took on a preponderant role in determining the use of public resources allocated to the sector through the Federal Housing Programs, in which the participation of the provinces was reduced and, in the specific case of the Province of Buenos Aires, the municipalities have a more active role as executors of the guidelines of the national government. Indeed, FP will stand out for delegating project implementation and execution tasks to local governments, thus municipalities had a central role in the execution of housing policies. If a municipality wants a program to be implemented, local governments must design intervention projects, manage their financing, prepare the specifications and call for public bidding. They must also inspect the works, certify the physical progress, administer the resources and resolve the beneficiary selection system. At the same time, the national government selects, finances and audits the projects presented by the municipalities. The Provincial Housing Institutes supervise the progress of the works, guide and supervise the adjudication process, recover the investment, and take care of the property rights. In any case, the new role of local governments generated a great diversity in the implementation and execution of the project in each municipality. In order to apply the FP in their jurisdictions, each municipality proceeded to create various instances, secretariats and sub-secretaries, thus configuring various styles and procedures of action.

Budget and Scale. The construction version of the Federal Housing Plan is the largest-scale housing policy developed by the public sector in the country so far in the 21st century. In 2004, the national government launched the first round and allocated a non-reimbursable financing fund to build 120,000 houses for an equivalent of \$1.29 billion.²⁴

In 2005, total public investment was 60 percent capital transfers, 28 percent direct real investment, and 12 percent for public firms. Around half of the capital transfers were budgeted for housing projects for the period 2005-2007. Capital transfers grew by 75 percent in nominal terms from 2004 to 2005, while the annual inflation rate in 2004 was around 6 percent.

In 2007, the national government launched the second round of the program and allocated an equivalent of \$5.49 billion to finance the construction of 300,000 houses. Public investment in the FP accounts for 30 percent of capital transfers in 2007.

Appendix C.2. Violence Against Women and IPV

Legislation. In Argentina, one of the most relevant pieces of legislation to understand the background is the Integral Protection of Women Law 26.485, sanctioned on March 11th, 2009. Other earlier efforts against domestic violence are National laws 23.179, 24.417, 25.087, and 26.364.²⁵ National Law 24.417 in 1994 is the closest antecedent, a law of Protection Against Family Violence. Twenty-five years later, law 26.485 is a more complete and updated regulatory framework than law 24.417.²⁶

Since National Law 26.485 is groundbreaking within the institutional framework, we borrow from it the definition of violence against women, types, and modalities. National Law 26.485 was complemented, updated, and modified by more than ninety documents since its sanction.²⁷ The general dispositions in Title 1 about definitions, modalities, and types, remain largely in line with state-of-the-art international standards as per the World Health Organization.

²⁴ Amount in 2004 dollars. Information from the Argentinian Budget Law ([link](#)).

²⁵ Law 23.179 in 1985 is the approval of the Inter-American Convention to abolish all forms of discrimination against women. Law 25.087 in 1999 is an amendment to the Criminal Code on Crimes Against Sexual Integrity. Law 26.364 in 2008 is to prevent and ban sexual slavery and provide help to its victims.

²⁶ The former has four times more articles and ten times more words than the latter.

²⁷ Following important regulatory documents is an amendment about penalties for gender violence to the Criminal Code and Procedural Criminal Code (National Law 26.791), regulation about sexual slavery (National Law 26.842), the incorporation of the National Registry of Generic Data Related to Sexual Integrity Crimes (National Law 26.879), the incorporation of the Lawyers College for Gender Violence Victims (National Law 27.210), the program Educate in Equality (National Law 27.234), an amendment of the National Criminal Code (National Law 27.352), an amendment to the National Civil and Commercial Code (National Law 27.363), an economic compensation for children of women victims of femicide or homicide in the context of domestic violence (National Law 27.452), and “Micaela Law” of mandatory training on gender matters to all employees of the public sector (National Law 27.499). See [here](#) for more details.

Article 4 defines violence against women as “any behavior, action or omission, which directly or indirectly, both in the public and private spheres, based on an unequal power relationship, affects their life, freedom, dignity, physical, psychological, sexual, economic or patrimonial integrity as well as your personal safety. It includes those perpetrated by the State or by its agents. For the purposes of this law, indirect violence is considered to be any conduct, action, omission, disposition, criterion or discriminatory practice that places women at a disadvantage with respect to men.”

Article 5 specifies different types of violence against women. Physical violence is defined as against the woman’s body causing pain, damage, or risk of producing it, and any other form of abuse or aggression that affects her physical integrity. Psychological violence is defined as causes of emotional damage and decrease in self-esteem, or harms and disturbs the full personal development, or seeks to degrade or control their actions, behaviors, beliefs, and decisions, through threats, harassment, restriction, humiliation, dishonor, discredit, isolation manipulation. It also includes blaming, constant surveillance, demand for obedience, submission, verbal coercion, persecution, insult, indifference, abandonment, excessive jealousy, blackmail, ridicule, exploitation, and limitation of the right of movement or any other means that harms their psychological health and self-determination. Sexual violence is defined as any action that implies the impediment in all its forms, with or without genital access, of the right of women to decide voluntarily about their sexual or reproductive life through threats, coercion, use of force or intimidation, including rape within marriage or other related or family relationships, whether or not they live together, as well as forced prostitution, exploitation, slavery, harassment, sexual abuse and trafficking in women. Economic and patrimonial violence is defined as causing an impairment in the economic or patrimonial resources of women. This is a more diluted concept, but some examples are described. The disturbance of the possession, possession, or ownership of women’s property. The loss, theft, destruction, retention, or improper distraction of objects, work instruments, personal documents, goods, values, and patrimonial rights. The limitation of the economic resources destined to satisfy her needs or deprivation of the indispensable means to live a dignified life. The limitation or control of her income, as well as the perception of a lower salary for the same task, within the same workplace. Symbolic violence is defined as performed through stereotyped patterns, messages, values, icons or signs transmit and reproduce domination, inequality, and discrimination in social relations, naturalizing the subordination of women in society.

Article 6 establishes the modalities of violence. “For the purposes of this law, modalities refers to how different types of violence against women manifest in different environments and include domestic violence, institutional violence, labor violence, violence against reproductive freedom, obstetric violence, and media violence.” In this paper, we zoom into domestic violence defined as “violence exercised against women by a member of the family group, regardless of the physical space where it occurs, that damages dignity, well-being, physical, psychological, sexual, economic or patrimonial integrity, freedom, including reproductive freedom and the right to full development of

women”. Family group is understood as that originating in kinship, whether by consanguinity or by affinity, marriage, *de facto* unions and partners or couples. Includes current or past relationships. Importantly, cohabitation among past or current partners is not required for violence among partners to be considered IPV.

In this paper, we will follow the definition in article 4, and the types in article 5, and we will focus on the domestic violence modality as defined in article 6. We further narrow our definition and we focus only on IPV, which is any type of domestic violence against women perpetrated by an intimate partner. We also follow article 6 and include any current or past intimate partnership (from marriages to more informal sexual partnerships). Again, cohabitation among past or current partners is not required for the definition of IPV.

Institutions. The institutions in charge of domestic violence are family civil courts (family civil judges). These civil courts have auxiliary entities. Two more institutions defined at the national level are the National Council for the Minor and the Family, which must provide law-technical support in cases required by the competent courts, and a specialized police force within the Argentine Federal Police, which is incorporated to act in aid of the family civil courts. At the local level, the family civil courts are entitled to budget information and counseling centers for physical and psychological violence. These centers, a key institutional actor in our research, have the purpose of advising and guiding violence victims through the judiciary system.

Local counseling centers receive a wide range of complaints. The most common complaints are categorized as follows. First, domestic violence complaints, physical and/or psychological violence. Second, non-fulfillment of parenthood obligations set forth by the applicable law. Third, sexual abuse or harassment, whether domestic or not.