Movin' on Up?
The Impacts of a Large-Scale Home Ownership Lottery in Uruguay

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Abstract

We report on a large-scale urban resettlement program in Uruguay. Over the course of seven years the program randomly assigned thousands of low-income households to ownership of apartments in new buildings in middle-class areas, including a subsidy averaging $44,000 per household. We match applicants to comprehensive administrative data on employment, schooling, health, fertility, and voting over the decade subsequent to the move. We find that the program led to a small decline in fertility for women and a two percentage point increase in formal employment, but did not drive school attendance. Likely due to the lack of strong spatial inequality in Uruguay, this relocation program did not result in transformative improvements in the lives of its beneficiaries.

Keywords: Housing policy, economic mobility, political participation
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1 Introduction

One of the most fundamental changes that the government can induce in the life of its citizens is to relocate them in space. Residential mobility programs have long been of interest to social scientists because they provide an unusually clear window into the role of context in driving human behavior (Kling et al., 2005). The best-known place-based policy in the US (Moving to Opportunity, or MTO) randomized access to housing vouchers in five US cities conditional on moving to a less poor neighborhood. Studies of MTO found modest short-term improvements (Katz et al., 2001; Ludwig et al., 2008; Jacob and Ludwig, 2012), but then more transformative life-long benefits accruing to children who moved young (Chetty et al., 2016), particularly for girls (Ludwig et al., 2013).[1] However, little new experimental evidence has emerged since the MTO study was conducted, and there are few rigorous studies of place-based programs outside of the US.[2] Given the importance of urban inequality and social mobility, understanding the broader impact of such policies is critical.

We provide evidence based on a series of 187 independent lotteries conducted by the government of Uruguay's *Compra de Vivienda Nueva* program to assign subsidized mortgages to poor families from peripheral neighborhoods, allowing them to buy an apartment in newly constructed buildings closer to urban centers. These lotteries involve 4,883 households, and we have national ID numbers for all 15,502 individuals who lived in these households at the time they applied for the housing lottery. A data agreement with the Uruguayan government allows us to view formal employment status, fertility, public K-12 and university enrollment status, as well as voter turnout in local elections for the universe of lottery participants. The first lotteries took place in 2009 and we have outcomes up through 2020, providing a relatively long post-treatment window to observe the evolution of outcomes as the winners move, become home owners, and adapt to their new neighborhoods. We also have GIS locations for the neighborhood at the time of application as well as the location of the new building, and so can look for signs of heterogeneity expressed in terms of how large the potential improvement in neighborhood would be. This environment represents a unique combination of large-scale experimentation, real-world government implementation, and universal capture of outcomes using administrative data.

Roughly one in six applicant households win a place in a new building over all the lotteries. This move entails a meaningful improvement in the quality of housing and local life; for example the average lottery winner moved to neighborhoods with property values that were one third higher,

[1] Early evidence from the Gautreaux program in Chicago suggested substantial benefits to children and mothers from moving away from high-poverty neighborhoods (Rosenbaum, 1995); evidence on why MTO may not have had a larger impact has centered on the fact that the beneficiaries chose to move to neighborhoods that were wealthier but still minority-dominated and so may not have shifted peer groups (Clampet-Lundquist and Massey, 2008).

[2] Barnhardt et al. (2017) analyze a randomized housing lottery in India after 14 years; they attribute null results of that program to the dislocation in social networks induced by the move. Franklin (2019) examines a housing lottery in Ethiopia that moved slum-dwellers to large apartments on the periphery of the city, finding decreases in conflict but no change in labor supply or earnings. The most similar program to ours is Brazil's My House, My Life (PMCMV) program, which was found by Leape (2020) to have increased earnings by 13% and employment by 2% within four years.

voter turnout rates that were almost 2 percentage points higher, housing formality goes from less than half to 100%, and the number of bedrooms rises by more than .5. Unlike most US programs, CVN provided a very large subsidy to purchase the new unit (68% of the purchase price of the unit and 76% of the monthly payment, for an average of $44,000 per winning household), as well as making them titled homeowners in their new neighborhoods. The existing literature leads us to believe that this ownership dimension will prove conducive to the economic incentives of families to invest in their property (Galiani and Schargrodsky, 2010), as well as to participate politically (Rohe 2001). For example, homeowners are more inclined than renters to participate in voluntary organizations and in politics at the local level (Cox, 1982; Blum and Kingston, 1984; McCabe, 2013; Kumar, 2022). Hence the program provides beneficiaries with a bundle that includes an asset transfer and a title to a home as well as locating them in a new neighborhood.

Despite the excellent statistical power, universal capture of outcomes, long duration of measurement, and bundle of transfers implied by treatment, we discover only relatively modest effects of the program. We use three distinct techniques to measure the impacts of exposure to treatment. First, we cumulate outcomes across all post-lottery years available for each outcome and examine the simple Intention to Treat as a function of treatment status in the first lottery entered. Second, we use an event study approach to measure the year-on-year effects of entry into the treatment, which allows us to focus on the possibility of short-term disruption effects that might dissipate or reverse in the longer run.[3] Finally, we follow Abdulkadiroğlu et al. (2011) and Cullen et al. (2006) in estimating an IV/LATE on the number of years an individual had been a lottery winner, which looks for a linear slope in duration of treatment. None of the outcomes display patterns across lottery years and estimation approaches that are strong and robust, but the program does have detectable effects. For employment, treatment effects across most lottery years are positive and the pooled effect is significant, with a point estimate indicating a two percentage point improvement in the likelihood of a family member being formally employed (over a control mean of 53%). Each additional year of treatment increases employment rates by .4 percentage points. For female family members, winning the lottery results in a small drop in fertility rates which is never compensated for by subsequent higher fertility and hence leads to suppressed subsequent fertility (about two thirds of a percentage point below a control annual fertility probability of 3.9%). Far from it being the case that home ownership encourages local political participation, we see a slight disruptive effect and an overall insignificant suppression of voting among lottery winners. Moving appears to have no effect on schooling enrollment (either public K-12 or universities we observe) in this context.

In the final section of the paper we try to unpack the explanation for these relatively muted overall results. Our analysis suggests that two features of the program and context may be particularly implicated: first, the provision of a subsidized mortgage requires eligibility requirements

[3]For example, Kling et al. (2005) finds disruptive short-term effects of MTO on the schooling of adolescent boys, and Gay (2012) shows that MTO depresses voting among adults as a result of dislocation effects.

around ability pay that screen out the poorest potential applicants, meaning that the study sample is lower-middle income but contains few individuals from the poorest deciles of the local income distribution. Examination of heterogeneity by baseline income levels suggests that this feature of the targeting limits overall impacts. Secondly, place-based mobility programs work precisely because they shift individuals across an urban gradient whose slope is implicitly determined by the degree of inequality in the city in which they operate. Ironically for Latin America—a region in which inequality is pervasive—Uruguay generally and Montevideo specifically are relatively egalitarian. The intersection of the screening criteria and the urban geography of Uruguay result in a program for which changes in neighborhood quality are relatively modest. In one important dimension, namely local violent crime rates, the intervention actually moves households towards neighborhoods with slightly worse outcomes. The implication is that geographic mobility programs will be a particularly effective tool in places where inequality is a severe problem to begin with. The intersection of targeting rules and context (Korpi and Palme, 1998) simply does not feature the primitive inequality necessary to make moving a household across the city a powerful intervention.

2 Design

2.1 The *Compra de Vivienda Nueva* Program

In 2005, the Uruguayan government developed the *Compra de Vivienda Nueva* (CVN) program with an eye to promoting home ownership while providing housing opportunities in central urban areas. The concern at that time was that the conventional approach of building housing in peripheral areas simply reproduced segregation rather than addressing the structural problems that reduce the quality of life in marginal communities. Consequently the design of CVN focuses on bringing the marginalized poor into the geographic center of the city, and on providing a pathway to home ownership with the idea that the stability and enfranchisement from the title provides a more durable pathway to the middle class.

The program was targeted at families with at least one child under 18 years old or one disabled member. The maximum monthly income a family may have varies with family size, but it is 6.75 minimum wages (2,745 USD) for a family of four. Eligibility was restricted to families that have sufficient savings to make an initial down payment of 8-10% of the market value of a new apartment and sufficient income to afford the monthly mortgage payments. To prove they will be able to afford the monthly payments, families needed to demonstrate that they had a monthly income of between 557 and 1,336 USD in Montevideo, or between 523 and 2,092 USD in the rest of the country. As we might expect from this mixture of progressive targeting and minimum payment requirements, wealthy households are screened out and yet few of the very poorest households are included. (See figure A.2). The median applicant comes from the third income decile; 12% of applicants are from the poorest decile and we observe no applicants with income above the city median (and only 6.75%

of applicants from the fourth decile). According to our estimation, between 2010 and 2016, the target population of households that met the income requirements and had at least one minor child included around thirty percent of Uruguay´s total population.

Monthly payments are calculated based on families' financial capacity, but never exceed 25% of the family´s income. The mortgage is fixed-rate and has a maximum 25-year term. The quantity of the subsidy therefore varies by household and will be larger for a) larger families and b) households with lower incomes.[4] The average winner of a lottery was purchasing a house worth \$64,000, was asked to make a 10% down payment from their own savings, and would have had an unsubsidized mortgage payment of \$337 per month. The payments were subsidized by 76%, leaving the average winner with a post-subsidy mortgage payment of \$80 per month on a loan averaging 15 years. The average total subsidy received for a winning household is \$44,000, representing a very substantial investment in a single household and raising the bar for the scale of impacts we would hope to see in a cost-effectiveness sense.

The program experienced substantial excess demand, and so access to the CVN units and the associated subsidized mortgages was randomly assigned by lottery. When the units in a new building became available, the Housing Ministry (MVOTMA) put out a call for applications, specifying the location of the building and the number of bedrooms in the apartments. The number of units in each call for applications varied depending on the number of units of a given size in the new building. Interested families were then invited to apply. After evaluating whether all registered families complied with the program guidelines, MVOTMA conducted a public lottery among eligible applicants, by drawing pieces of paper from a bowl with all participants invited to be present in person. If a given family did not win the lottery for a given call, it could register again for subsequent housing lotteries.

Between 2008 and 2016 the CVN program distributed 1,617 units; 776 of which were located in Montevideo and the remainder in the rest of the country. In this period, there were 345 calls and 1,860 apartment units were assigned. 187 of the calls were 'competitive', defined as being a) lotteries that had more than one participant, and b) lotteries in which not all participants won. 1,248 apartment units were assigned in these competitive lotteries. Figure A.3 shows the location of all the apartment buildings that were constructed under CVN. Panel A shows the complete location of all buildings in the city of Montevideo, while Panel B provides an example, for a single lottery, of the locations of the individuals at the time of application and the location of the building they would move to if they won. Table A.1 provides summary statistics on the change in living conditions experienced by those who won the lotteries; the average number of bedrooms increases by .5, the share living with another family falls from 27% to 0, and the share with formal housing rises from 45% to 100%.

Analysis of the pooled lotteries is complicated by two forms of non-compliance. First, winners

[4]The full set of requirements can be found at https://www.mvotma.gub.uy/programas-permanentes-por-postulacion/comprar/vivienda-construida.

may decide not to participate in the program after winning the lottery. This is rare but has happened in 1.7% of cases; this is a typical form of non-compliance and can be handled simply by estimating an Intention to Treat Effect ignoring this source of non-compliance. Second, households who lost in prior lotteries are free to join subsequent ones; 35% of the households in our data appear in more than one lottery and 3% appear in more than three. Figure A.4 shows both forms of non-compliance; for every lottery year the non-compliance in the treatment is below 5%, and the subsequent re-treatment rate among initial controls is 20% for the first cohort, 10% for the next three, and then drops below 5% for cohorts lotteried after 2012.[5] We account for this issue using two strategies borrowed from the literature on US school lotteries, which share the same issue. First, we calculate the equivalent of an Intention to Treat (ITT) effect by using only the outcome of the *first* lottery regardless of how many subsequent lotteries are enrolled in. This estimate is structurally biased towards zero and so provides a lower bound on the absolute magnitude of the true treatment effect (Cullen et al., 2006). Second, we can instrument for winning any given lottery with the outcome of the first (following Abdulkadiroğlu et al. (2011)), which effectively inflates the ITT above to provide an estimate of the Local Average Treatment Effect (LATE).

2.2 Data and Outcomes

The initial data for the project consists of the application information that was filed with CVN by beneficiaries in order to be eligible to participate in a lottery. The average age of the adults in each applicant household was 36 years old. 59.4 percent of the families that applied to the program were renters while the remainder lived under various informal non-ownership arrangements (e.g. living in a relative´s house).

The mapping of national ID numbers to government databases allow us to define five primary outcomes for the study. First, we can measure **formal employment** by seeing whether working-aged adults are covered by the national worker health insurance scheme (*Sistema Nacional Integrado de Salud*, or SNIS; employers are required to register employees under SNIS).[6] This variable is observed from 2010-2020 and is defined for all adults over the age of 18.

Second, we examine **voting behavior** by matching adults of voting age to the registry of participation in Montevideo's local council and participatory budget elections in 2008, 2011, 2013 and 2016. Our measure records whether an individual voted either on any participatory local budget proposals, or in a local council election. Voting in these local elections is voluntary for all residents of Montevideo sixteen years and older (while voting in national elections is mandatory), and the issues on which they are voting are highly localized public good investments (parks, schools, street paving, sports fields, and so on). Participation in these elections is therefore an interesting metric of engagement with community issues.

[5]Figure A.5 shows how the non-compliance changes across subsequent years for each initial lottery year.

[6]SNIS covers the whole household of any formally registered employee, and so this variable effectively measures 'any household member employed' and will not be affected by a second adult gaining formal employment.

Third, we measure **female fertility** by matching women aged 16-40 to data from the national birth registry, which includes the ID of the mother. This allows us to measure the probability of birth in any year or the cumulative number of births over any interval. This outcome is observed from 2010-2016.

Fourth, we measure public school enrollment by matching all children aged 6-19 in lottery households to the enrollment records of the public primary and secondary school system. However, only 80% of children in Uruguay attend public schools, so this measure is imperfect in that it would, for example, measure a child moving from a public to a private school as dropping out. Nonetheless we expect this to be a strong measure of enrollment in this context, given that eligible households are low income by definition and hence more likely to rely on the public school system. This outcome is observed from 2013-2020.

Finally, we measure **tertiary enrollment** by matching college-age youth to the enrollment records of the two major universities in the capital of Montevideo, Universidad de la República and Universidad Católica del Urugauy. These are the two most elite universities in the country, and so represent a reasonable measure of treatment effects on access to high-quality tertiary education. This outcome is observed only in 2016; we define this outcome for all youth aged less than 19 at the time of lottery and more than 18 in 2016.

Because we have the GPS locations of the household of residence at the time of application as well as the location of the apartment they would move to, we can also define a number of covariates that describe the heterogeneity of the move households undertake if they win. These include the difference in the property prices across old and new neighborhoods as well as the difference in homicide rates.[7] These differences can be calculated both for winners and losers of the lottery (e.g., the change they would have experienced if they won) and so can be used as interaction variables for a standard experimental analysis of impact heterogeneity. We lack real estate price data for the entire country and we have homicide data only for the city of Montevideo, meaning that we can examine only 182 and 124 lotteries, respectively, when we interact with these covariates to look for signs of heterogeneity.

Table 1 provides summary statistics for our core data, as well as illustrating the balance between treatment and control. It includes three types of variables; in the top panel it uses the information on households that is included on the program application forms. In the middle panel we examine the pre-treatment outcomes from the institutional data used later in the paper. In the bottom panel we construct metrics of the change in neighborhood characteristics that would occur if the individual won the lottery (post-pre) and compare those who will win to those who will lose. The table presents 22 comparisons and finds one to be significantly different at the 95% level, as we would expect by random chance. While the eventual winners are 5.5 pp more likely to be in rentals at the time of application, critically all of the outcome variables appear to be well-balanced. While

[7] We have also examined other dimensions such as physical distance travelled and differences in political participation and found no evidence of heterogeneity, so they are omitted from our final analysis.

the eventual winners would move to higher-theft neighborhoods than losers, the effects on the other metrics of crime (homicide, assault, and IPV) all move in the opposite direction and so there does not appear to be any systematic difference in the types of neighborhood changes that would be experienced by winners and losers. This is particularly important as the researchers did not control the lottery mechanism.

Table 1: Summary Statistics and Balance

	Control	Treatment	Difference
Information from Application Forms			
Monthly Family Formal Income	23,218.88	21,133.41	-532.075
	(8,511.75)	(8,230.81)	(363.818)
Any Formal Income	0.16	0.15	-.001
	(0.37)	(0.36)	(.019)
Two Titleholders	0.57	0.60	.005
	(0.50)	(0.49)	(.025)
Minor At Lottery	0.43	0.42	.001
	(0.49)	(0.49)	(.006)
Number of Minors At Lottery	1.41	1.40	.005
	(0.66)	(0.65)	(.023)
Origin: House	0.44	0.44	.011
	(0.50)	(0.50)	(.023)
Origin: Rental	0.60	0.54	-.055**
	(0.49)	(0.50)	(.024)
Origin: Number of Bedrooms	1.95	1.95	.005
	(0.82)	(0.81)	(.036)
Origin: Had Restroom	0.81	0.81	-.019
	(0.39)	(0.39)	(.02)
Origin: Under Construction	0.25	0.19	.004
	(0.43)	(0.39)	(.02)
Pre-Treatment Outcomes			
Never Unemployed	0.62	0.59	-.034
	(0.49)	(0.49)	(.029)
Never Missed School	0.11	0.11	-.028
	(0.31)	(0.32)	(.026)
Voted	0.05	0.05	-.001
	(0.22)	(0.22)	(.01)
Fertility	0.11	0.11	.013
	(0.32)	(0.32)	(.02)
University	0.41	0.42	.001
	(0.49)	(0.49)	(.029)
Neighborhood Characteristics, Post-Pre			
Homicide Change Z Score	0.00	0.02	.025
	(1.00)	(0.95)	(.033)
Theft Change Z Score	-0.01	0.00	.02
	(1.08)	(1.13)	(.036)
Assault Change Z Score	-0.01	-0.00	.017
	(1.08)	(1.14)	(.036)
IPV Change Z Score	-0.00	-0.02	-.002
	(1.08)	(1.14)	(.038)
2004 Voter Turnout Change Z Score	-0.02	0.01	.024
	(0.99)	(0.97)	(.056)
2009 Voter Turnout Change Z Score	-0.03	0.04	.065
	(0.98)	(0.96)	(.055)
2014 Voter Turnout Change Z Score	-0.04	0.04	.058
	(0.97)	(0.95)	(.051)

Notes: Table presents tests of balance for lottery winners versus losers. Panel A examines household data from the application forms, Panel B uses the institutional data to examine pre-lottery outcomes, and Panel C looks at the change in neighborhood characteristics that would have been induced by winning the lottery. Difference column shows the beta on a treatment indicator representing the lottery outcome of a household the first time it is observed. Standard errors in parentheses, weighted means and standard deviations reported. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

3 Results

3.1 Intention to Treat Estimation

Our first approach to estimating the impact of winning a CVN lottery is to use an Intention to Treat, cumulating outcomes across all post-lottery years up to the last observation possible for each outcome and for each lottery cohort. To avoid endogeneity from re-entry into subsequent lotteries for losers, we retain the treatment status assigned in the first lottery entered in that year. To preserve the balance of subsequent experiments we include individuals as many times as the number of lotteries they appear in during that year, but we use weights to remove the undue influence of these repeat observations, as well as clustering standard errors at the household level. We also utilize Horvitz-Thompson experimental inverse propensity weights based on the success rates in each lottery (these weights make each of the two potential outcomes equally important despite the fact that lotteries rarely assigned close to 50% of subjects to treatment); the product of these two weights is used in analysis. The confluence of these issues means that for a given household, the 'first lottery' treatment variable, the cumulated outcome, and the weights can all vary depending on the lottery cohort being considered.

We observe the post-treatment outcome Y_{itc} for each household i, each outcome time period t, and each lottery c. This outcome is explained with a binary indicator T_{icp} which indicates whether a household present in lottery c was assigned to the treatment in that household's first lottery within pool p (where pool refers to the group of lottery cohorts and their outcomes being aggregated together in a given analysis). To improve statistical power the regression includes a battery of covariates taken from the application data βX_{ic} (formal and informal income, number of individuals who would be on the title, number of minors in the household, quality of home and type of tenancy) and a set of indicator variables for each lottery (the randomization groups) α_c.

Following Cullen et al. (2006), the ANCOVA estimate of the ITT regression is then:

$$Y_{itc} = \alpha_c + \delta T_{icp} + \beta X_{ic} + \rho Y_{i0c} + \epsilon_{itcp} \tag{1}$$

The results of this estimation are presented in Table 2. Every coefficient in the table is from a different regression; the different samples lotteried in each year are in the rows, and the outcomes in the columns. The bottom row pools together all of the samples and provides the highest-powered test on the aggregate sample. With the exception of the 'University' outcome, which we only observe in 2016, all outcomes are defined as the cumulative average across all observed post-treatment years. Each outcome is only defined for the relevant group of individuals given in Section 2.2.

Beginning with the pooled results, we see statistically significant decreases in fertility and increases in employment arising from the treatment.[8] The fertility effect appears consistent with

[8] The row at the bottom of Table 2 gives the post-lottery control mean and so is different from Table 1 which gives the pre-exposure mean.

Table 2: ITT Estimates by Lottery Year

	(1) Employment, 09-21	(2) Voting, 08-16	(3) Fertility, 09-16	(4) Schooling, 09-21	(5) University, 2016
2009 Lottery Cohort	.0012	-.0161	.0018	-.0103	-.1472***
	(.023)	(.01)	(.006)	(.044)	(.052)
	[3,296]	[3,672]	[1,745]	[1,727]	[812]
2010 Lottery Cohort	.015	-.0141**	-.0009	.0433	.2432***
	(.027)	(.006)	(.007)	(.049)	(.084)
	[640]	[738]	[369]	[384]	[196]
2011 Lottery Cohort	.0111	-.0069	-.0052	.0516**	.0159
	(.017)	(.008)	(.006)	(.025)	(.051)
	[3,523]	[3,854]	[2,018]	[2,048]	[899]
2012 Lottery Cohort	.0057	.0094	.0123	.0139	-.06
	(.016)	(.008)	(.009)	(.028)	(.057)
	[4,152]	[4,563]	[2,338]	[2,302]	[931]
2013 Lottery Cohort	.0477	-.0243***	-.0018	.0249	.1909***
	(.031)	(.009)	(.012)	(.05)	(.073)
	[1,023]	[1,142]	[608]	[600]	[213]
2014 Lottery Cohort	.1147***	.0188	-.0391***	-.003	.295***
	(.027)	(.03)	(.009)	(.045)	(.1)
	[1,026]	[1,129]	[589]	[533]	[173]
2015 Lottery Cohort	.0303	-.035***	-.0319***	-.0476	.1659*
	(.022)	(.01)	(.008)	(.04)	(.099)
	[1,483]	[1,628]	[830]	[755]	[229]
2016 Lottery Cohort	-.1027	-.1126***	.0034	.1369**	.7206
	(.089)	(.035)	(.024)	(.069)	(.647)
	[253]	[280]	[163]	[128]	[25]
Pooled Sample	.0217**	-.0063	-.0069*	-.002	-.0013
	(.01)	(.005)	(.004)	(.017)	(.03)
	[15,502]	[17,006]	[8,755]	[7,933]	[3,478]
Pooled Average	0.53	0.05	0.04	0.40	0.42

Notes: Table presents Intention to Treat impacts of the winning status in the first lottery entered for the cohort lotteried in each. Outcome is cumulated across all available post-lottery years. Every coefficient is from a separate regression. Regressions include fixed effects for lottery, are weighted using randomization inverse propensity weights. The Pooled Sample result in the final row includes all lottery years. Note that the number of observations within a column will not sum to the pooled sample due to re-entry. Employment: Avg Number of Years without gap in Employment, 09-21. Voting: Avg Participation Across Post-Treatment Years , 08-16. Fertility: Avg Births Across Post-Treatment Years, 09-16. Schooling: Avg Number of Years without gap in Schooling, 09-21. University: Attended in 2016. Standard errors in parentheses, and number of outcomes per regression in hard brackets; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

disruption arising from the move; the negative coefficients appear only shortly after the lottery (2014 and 2015 lotteries for 2016 outcomes). However, both coefficients are small in absolute magnitude; the fertility effect implies a decrease in fertility of less than one percentage point, and the employment effect of .02 would indicate one more week of formal employment over the entire post-treatment period. So even where the pooled effects are significant they are not large. Voting similarly displays an apparent disruption effect with political participation being lower for those who have relocated recently, but the pooled effect is insignificant. The two schooling outcomes have years in which we see a significant positive effect, but the pooled impact is very small and insignificant. Hence while the ITT approach provides some glimmers of benefits in terms of employment and decreased fertility (as might be expected if female employment improves), no transformative benefits have occurred.

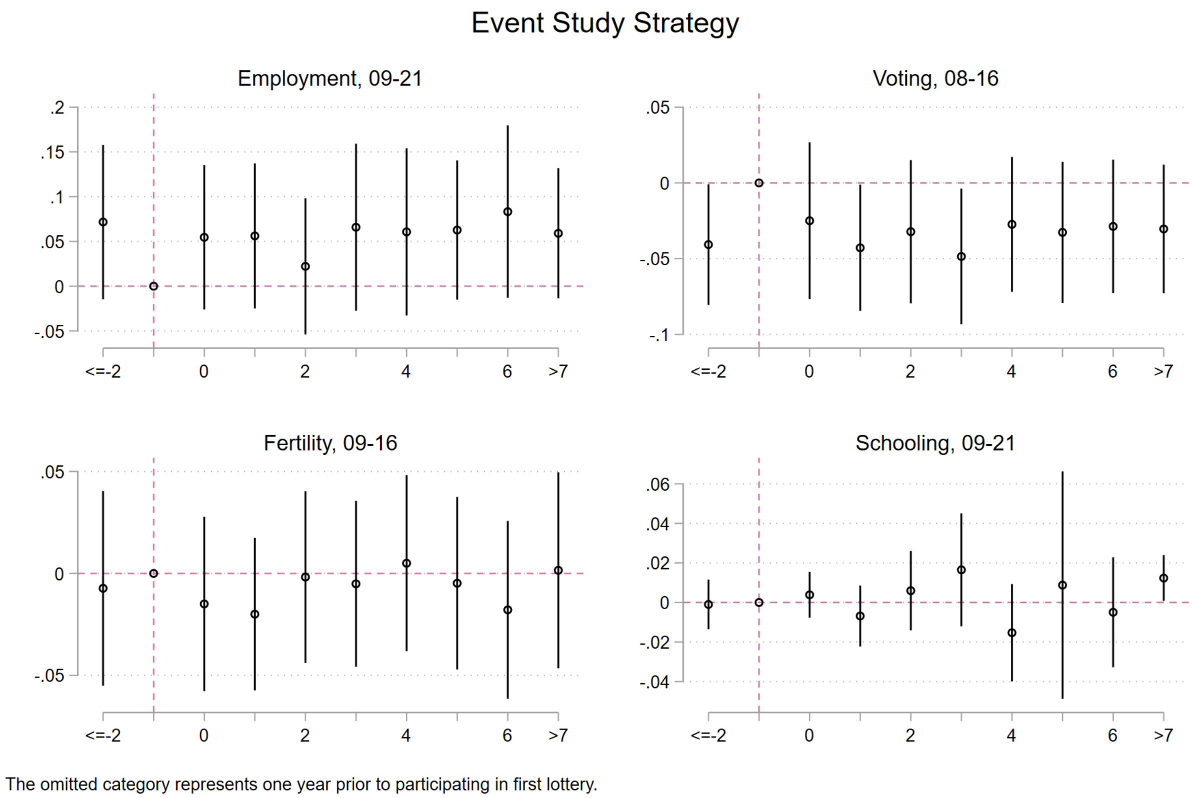
3.2 Event Study Estimation

The ITT results suggest the possibility of disruption effects closer to the time of lottery, but the pooling of samples and years inherent in that analysis structure may mask these effects. To gain a more fine-grained view on the dynamics of outcomes around the time of the lotteries, we can pool together all years and all observations (again repeating and de-weighting individuals participating in more than one lottery) and for each year and lottery cohort define the leads and lags of treatment. Along with fixed effects for lottery cohort and year of observation, this then gives an experimental estimate of the time path of treatment effects. The results of this exercise are presented in Figure 1 for the four outcomes that are observed each year and hence amenable to this form of analysis. Schooling displays no clear temporal pattern, and employment appears to rise slightly in the year of the move and to stay elevated in a manner that is similar across years (but significant only when pooled, as we saw in the prior table). Fertility and particularly Voting both display some sense of disruption, with voting weakly disrupted in lags 1-3, and fertility somewhat below trend in the two years after the move. Nonetheless, the overall impacts are sufficiently muted as to be undetectable in this year-by-year format, and must be pooled to achieve statistical significance.

3.3 Local Average Treatment Effect Estimation

A related way of summarizing impacts is to ask whether the duration of exposure to the treatment of a new house alters outcomes. An attractive way of summarizing the effects of duration of exposure is to estimate an IV/LATE equation pool refers to the group of lottery enous) outcome of the first lottery entered, following Abdulkadiroğlu et al. (2011). We now include each household only once, while continuing to define the sample as the universe of households ever exposed to a competitive lottery. For each outcome we always employ the full range of available years for calculating the post-exposure average, and for each lottery year we pool all individuals who had been lotteried prior to that year. This means that once we are examining the 2016 lottery (our last) we are effectively

Figure 1: Event Study Treatment Effects



The figure shows the first lead and the lags of treatment across main study outcomes that are observable as panels. The analyses pool all lottery cohorts and present point estimates and confidence intervals for post-treatment outcome years.

estimating the pooled regression that uses the maximum available sample size and the maximum available number of post-treatment years.

The endogenous variable for the LATE estimation is D_{itp}, the duration for which household i has been treated as of time t in pool p. We can instrument for the duration with the interaction between the lottery fixed effects and whether the household won its first lottery in that pool. Treatment in subsequent lotteries is ignored in the instrument but included in the duration of treatment D_{itp}, so the coefficient δ_L gives the LATE estimate of one additional year of actually receiving the program.

The Two State Least Squares estimate is then:

$$D_{itp} = \alpha_c + \gamma_c(\alpha_c * T_{icp}) + \nu X_{ic} + \psi Y_{i0c} + \mu_{itcp} \tag{2}$$

$$Y_{itc} = \alpha_c + \delta_L \hat{D}_{itp} + \beta X_{ic} + \rho Y_{i0c} + \epsilon_{itcp} \tag{3}$$

This analysis is weighted using only the lottery treatment percentage-based inverse propensity weights, and standard errors are clustered at the household level.

Table 3: LATE Estimates by Years Treated

	(1) Employment, 09-21	(2) Voting, 08-16	(3) Fertility, 09-16	(4) Schooling, 09-21	(5) University, 2016
2009-2010 Cohorts		-.0085*	.0001		-.0598**
		(.0049)	(.0029)		(.0257)
		[4,410]	[2,114]		[1,008]
2009-2011 Cohorts	.0125**	-.0053*	-.001	.0061	-.0203
	(.0062)	(.003)	(.0018)	(.011)	(.0164)
	[7,565]	[8,264]	[4,136]	[3,857]	[1,907]
2009-2012 Cohorts	.0086**	-.0036*	-.0006	.0034	-.0123
	(.0043)	(.0019)	(.0014)	(.0074)	(.0118)
	[11,717]	[12,827]	[6,496]	[5,987]	[2,838]
2009-2013 Cohorts	.0063*	-.0025*	-.0006	.0035	-.0084
	(.0033)	(.0014)	(.0011)	(.0055)	(.009)
	[12,740]	[13,969]	[7,111]	[6,559]	[3,051]
2009-2014 Cohorts	.0054**	-.0016	-.0009	.0031	-.0057
	(.0026)	(.0012)	(.0009)	(.0043)	(.0071)
	[13,766]	[15,098]	[7,723]	[7,072]	[3,224]
2009-2015 Cohorts	.0047**	-.0015	-.0009	.0019	-.0039
	(.0022)	(.0009)	(.0007)	(.0036)	(.0059)
	[15,249]	[16,726]	[8,586]	[7,805]	[3,453]
2009-2016 Cohorts	.0041**	-.0012	-.0009	.0014	-.0027
	(.0018)	(.0008)	(.0006)	(.003)	(.005)
	[15,502]	[17,006]	[8,755]	[7,933]	[3,478]

Notes: Analysis is a LATE instrumenting for years treated in 2016 with the outcome of the first lottery a household participated in within each observation window. Each coefficient is the output from a separate regression, cumulating across all cohorts that had been lotteried prior to a given year. Regressions include fixed effects for lottery, are weighted using randomization inverse propensity weights. Standard errors clustered at the household level are in parentheses, and number of observations for each analysis is in hard brackets. * p¡0.10 ** p¡0.05 *** p¡0.01.

Results are presented in Table 3. Again every coefficient is from a different regression, but here since the rows include all individuals lotteried *prior to* the named year, the sample cumulates as we move down and the 2016 row contains all available observations. Despite posing the impact question in terms of years of exposure to the move, the takeaway from this table is quite similar to the ITT Table 2. One additional year of treatment increases the employment rate by .4 pp, but in the cumulated regressions in the bottom row none of the other outcomes display treatment effects on number of years treated. As with the ITT results, the employment effect is significant but small in absolute magnitude.

3.4 Heterogeneous Treatment Effects

We can contextualize the muted impacts found in this study through two types of heterogeneity analysis. The first of these is the standard interaction analysis, as described in our pre-analysis plan. Relevant heterogeneity may exist at the household level (the income of the household, or the presence of a female household head), the individual level (age at the time of lottery), or across the neighborhood change induced by a lottery win (measured either in terms of property prices or of changes in crime rates). Accordingly, in Appendix tables A.2 through A.6 we sequentially analyze the five primary outcomes for the study, with these dimensions of heterogeneity examined separately in the columns of each table. The specification uses the same cumulative ITT approach as in Table 2, including all observations for whom we observe the heterogeneity covariate at the time of application.

Analysis by the individual- and household-level attributes is straightforward; the counterfactual neighborhood-level changes are somewhat more non-standard and are only enabled by the fact that for all control households we see the place they would have moved to had they won. That is, we can take the value of the covariate Z_i at the location of the household at the time of application, and Z_k at the location to which they would move if they won the lottery, and calculate d_{ikc}, the distance between these two values for household i in lottery group c and lottery j. The analysis of heterogeneity is then conducted via the following regression:

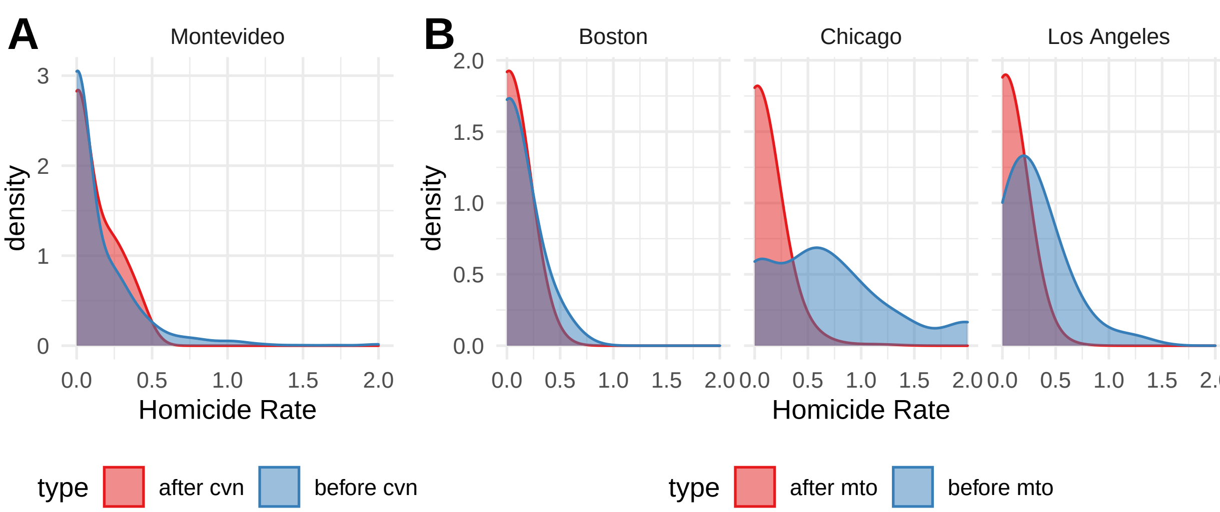
$$Y_{itc} = \alpha_c + \delta T_{icp} + \beta X_{ic} + \chi d_{ikc} + \tau(T_{icp} d_{ikc}) + \epsilon_{itcp} \quad (4)$$

The coefficient τ measures the extent to which the impact of the treatment is heterogeneous across distance d_{ikc} , and now δ measures a kind of 'pure' impact of home ownership that would occur if there were no change at all for the beneficiaries across the distance dimension included in the regression. We can include multiple distance interactions simultaneously in order to understand the differential impact of one kind of distance holding others constant.

Table A.2 examines employment, the most straightforward economic outcome, and finds heterogeneity that helps to contextualize the overall results of the study. In it, we find that households with the *lowest* initial incomes are the ones seeing the largest employment boost from the program. Similarly, female headed households see more than double the increase in employment as those with male heads. These heterogeneity results suggest that the program would have had larger employment impacts if it had been targeted at poorer individuals, meaning that there is a tension between the economic requirements of a mortgage-based poverty alleviation program and its overall effectiveness. The remaining heterogeneity tables typically have a negative coefficient on the income interaction but are not significant.

The final column of Table A.2 examines the change in crime rates across neighborhoods. It uncovers the surprising result that moving to a higher-crime neighborhood actually *increases* employment rates. A deeper examination of this issue helps to contextualize our study and how this place-based program may have differed from its better-known antecedents in the US. Figure 2 plots the distribution of homicide rates for the prior neighborhoods and the new locations in Montevideo (Panel A) and then in Boston, Chicago, and Los Angeles (Panel B). Two facts are striking from these pictures. First, there is substantially more inequality in crime rates within the US cities than there is in Montevideo. Partially this maps to higher overall levels of economic inequality in the US (the Gini coefficient of census-tract poverty rates in Montevideo is .28, while it is .39 in Chicago, .32 in Boston, and .35 in Los Angeles), and partly to lower overall levels of crime (homicides per thousand are .12 in Montevideo, and average .2 in the MTO cities).

Figure 2: Comparing the Change in Census Tract Homicide Distribution, CVN vs. MTO



The figures compare the distribution of the homicide rate of the neighborhoods where program beneficiaries were located before and after the interventions for CVN (Panel A) and for MTO (Panel B). For MTO, we identify the relevant census tracts through the program guidelines. The "pre" distribution includes tracts with more than 40% of their population under the poverty line; the "post" distribution includes tracts with less than 10% of their population under the poverty line. (Both required criteria for eligibility). Data on poverty and crime at the census tract level for the 1990s comes from Peterson and Krivo (2010).

The more striking feature of this picture, however, is that in our study individuals are actually moving to higher-crime neighborhoods on average, while MTO results in dramatic decreases in exposure to local crime. Kingsley and Pettit (2008) find that MTO experimental movers saw a 72% reduction in neighborhood violent crime rate (from 40 per 1k pop to 11) as a result of their first move. For Montevideo, the overall change is a 7% *increase* in crime [From 55.6 to 59.8]. In fact, the median CVN winner moved to a census tract with a homicide rate that was 27% higher than where they previously lived. How can this be the case? The answer lies in the different urban geography of Montevideo, in which the peripheral neighborhoods from which individuals moved are typically relatively safe, towards the city center where crime rates are higher. Indeed while the correlation between the poverty rate and the homicide rate in the MTO cities is .45, in Montevideo it is only .1. Consequently two core features of urban geography appear to play important roles in determining the outcome of this program: first, the relatively muted gradient of inequality across neighborhoods limited the extent to which relocation could achieve a transformative effect, and secondly movements that were positive in economic space showed muted improvements (or even deterioration) in terms of the level of crime to which winners were exposed.

4 Conclusion

Place-based programs have an impact by improving outcomes for those whose counterfactual outcomes would have been poor. Our results emphasize two dimensions of this problem. First, they need to be enrolling and treating individuals who would have struggled in the absence of the intervention. Our results suggest that the most economically deprived households gained most in terms of employment from moving to wealthier neighborhoods, but the home purchase/mortgage requirement in the program means that relatively few of these neediest households received the program. Hence the intention to treat of this intervention appears to be limited by the fact that the marginal treatment effects for many of the wealthier individuals were modest. This may suggest that, the independent benefits of home ownership notwithstanding, the income bar imposed by the equity purchase dimension of the design limited overall impacts, and such programs may do better to focus on rental subsidies that can be more easily targeted at the neediest households.

Secondly, to improve outcomes by moving an individual across space these programs fundamentally rely on the degree of spatial inequality in the cities in which they operate. Ironically for Latin America, long one of the most unequal regions in the world, this program worked in a country that has limited overall inequality, and in which the neighborhoods with the greatest economic opportunity also have higher crime. The policy implication is that place-based programs induce a treatment effect proportional to the spatial gradient across which individuals can be moved inside a city, and hence will be most effective in urban areas that suffer from high inequality to begin with.

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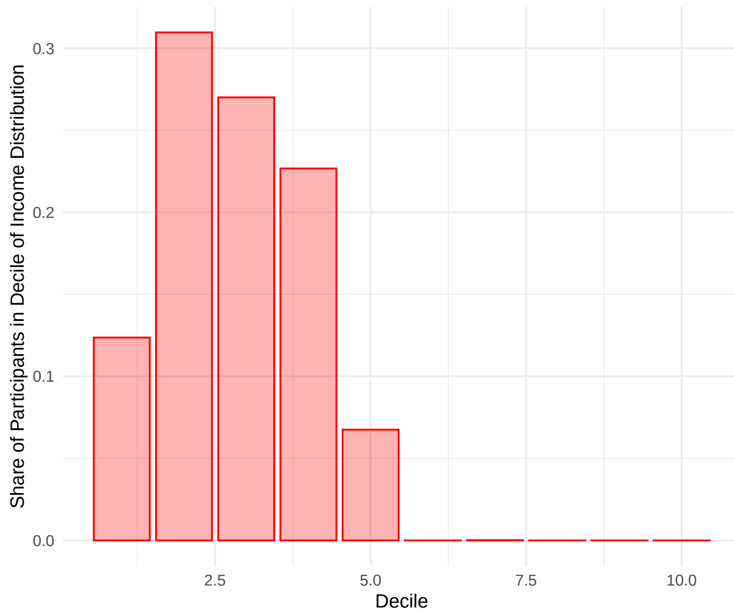
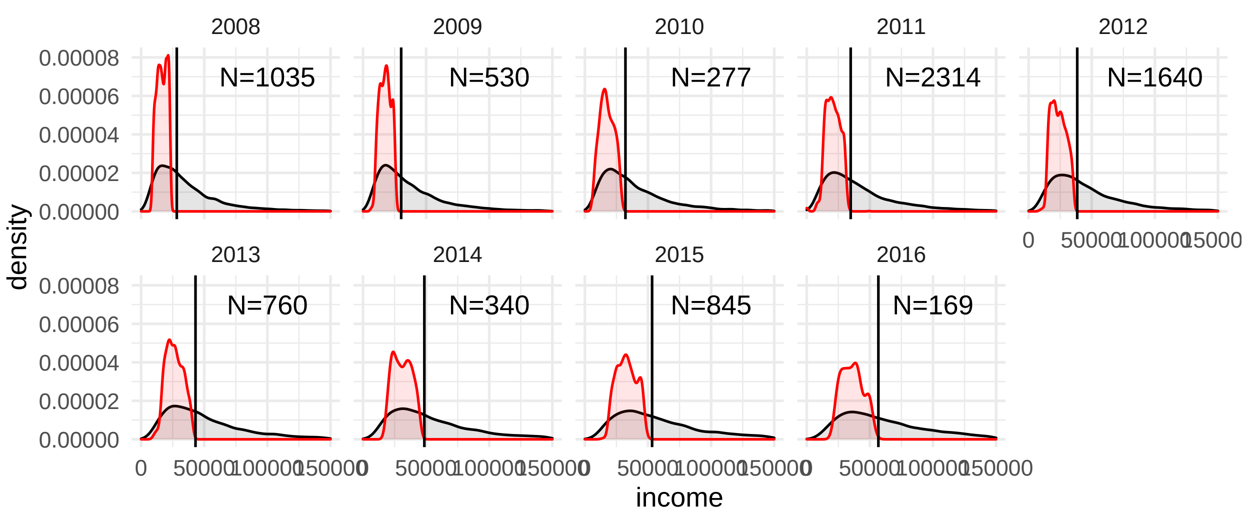
Appendix A Online Appendix

Figure A.1: Share of Applicants by Deciles of Montevideo's Income Distribution



Data on Montevideo's income distribution comes from the *Instituto Nacional de Estadística*.

Figure A.2: Applicant Pool and City-Wide Income Distributions, by Year



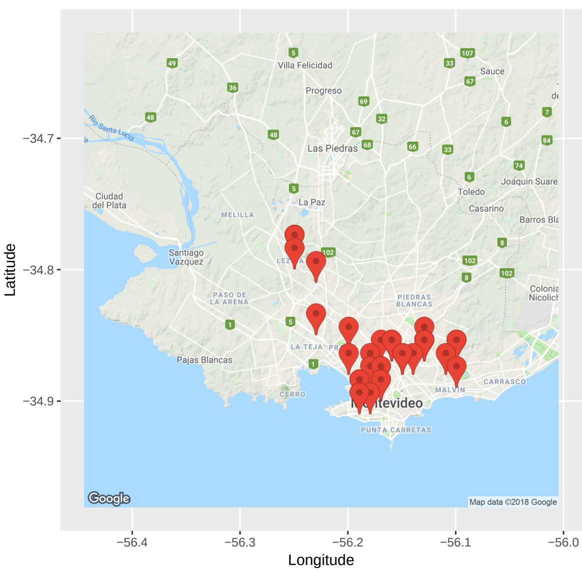
PDF of applicants in red; city in black. vertical line marks median of city-wide income distribution. Data on Montevideo's income distribution comes from the *Instituto Nacional de Estadística*.

Table A.1: Change in Living Conditions

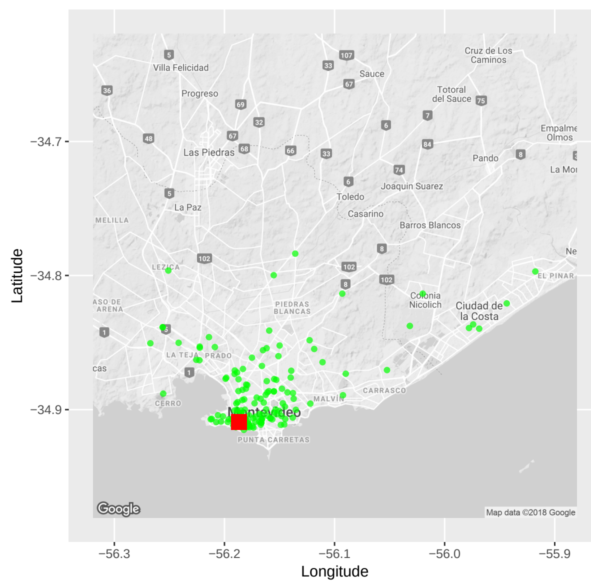
	Pre-Treatment Mean	Post-Treatment Mean	Difference
Housing Quality			
Has Own Bathroom	0.84	1	0.16
			(0.01)
Has Own Kitchen	0.84	1	0.16
			(0.01)
Connected to Water Network	0.99	1	0.01
			(0.01)
Connected to Energy Network	0.97	1	0.02
			(0.01)
Connected to Sewage Network	0.83	1	0.17
			(0.01)
Housing Quality Index	8.86	10	1.14
			(0.03)
Housing Size			
Number of Bedrooms	1.96	2.52	0.56
			(0.03)
Housing Stability			
Shares Housing with Another Family	0.27	0	-0.27
			(0.01)
Formal Housing Arrangement	0.45	1	0.55
			(0.01)
Neighborhood Quality			
Price of Square Meter (Uruguayan Pesos)	3,828	4,492	664
			(155)

The table describes the housing characteristics for winning families ($N = 1,255$) at the time of application and after treatment. Data on housing quality and housing stability was self-reported by families at the time of application to the program. To obtain data on neighborhood quality, we geocoded applicants' addresses as well as the addresses of the apartment buildings where the applicants relocated and used official data on housing prices to calculate the mean price of square meter in the relevant neighborhood.

Figure A.3: Location of Montevideo Apartment Buildings

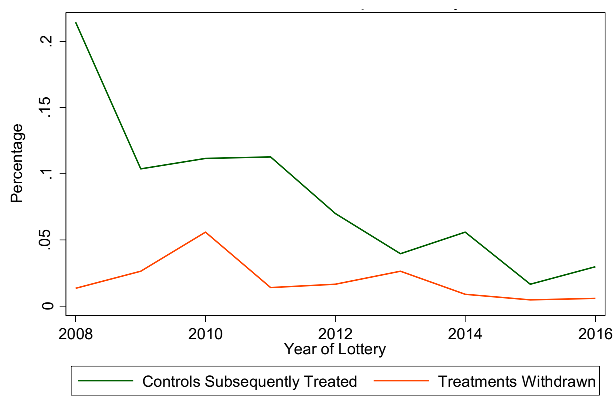


(a) Study Building Locations in Montevideo



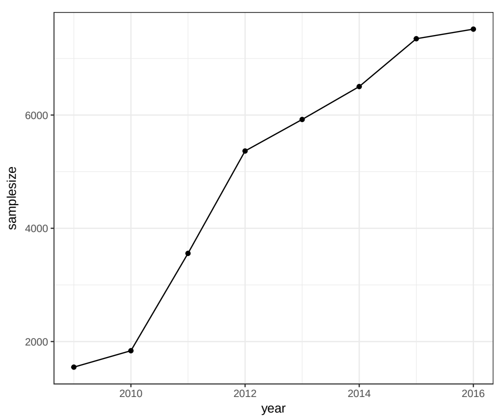
(b) Detail of Prior Residences for One Lottery

Figure A.4: Two-Sided Non-Compliance by Year

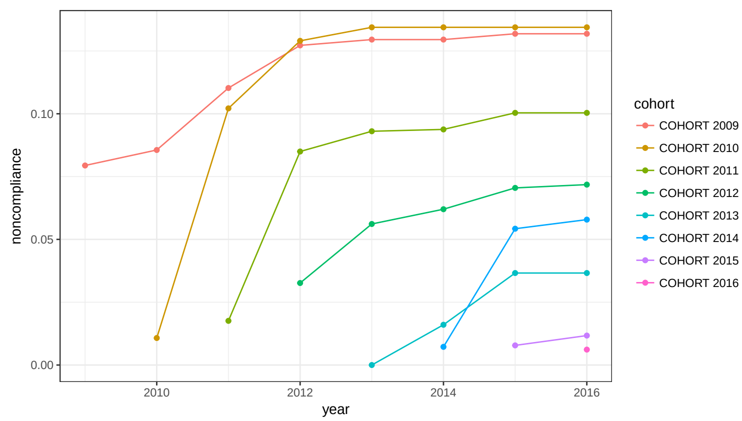


The figure shows the over-time evolution of each type of non-compliance. Controls subsequently treated gives the fraction of individuals lotteried in each year who lost their initial lottery but then apply for a subsequent one. Treatments withdrawn is the fraction of winners in each year who do not occupy the CVN apartment for any reason.

Figure A.5: Differential Non-Compliance and Sample Size



(a) Sample Size Growth



(b) Non-Compliance in the Control Group by Year Cohort

Panel A shows the simple growth in the number of households who have participated in a competitive lottery by year. Panel B shows the evolution of re-entering subsequent lotteries among losers of first lotteries for each year.

Table A.2: Employment Heterogeneity

	(1) Formal Income	(2) Age at Lottery	(3) Female HH Head	(4) Property Price Change	(5) Homicide Change
Treatment	0.0183	0.0297	0.0214**	0.0308***	0.0422***
	(0.0112)	(0.0309)	(0.0103)	(0.0104)	(0.0119)
Heterogeneity Dimension	0.109*	0.00212***	0.0214***	-0.00633	-0.00442
	(0.0653)	(0.000543)	(0.00656)	(0.00543)	(0.00603)
Interaction Term	-0.0264**	-0.000219	0.0232**	-0.0102	0.0353***
	(0.0118)	(0.000901)	(0.0101)	(0.0105)	(0.0113)
Control Group Y-Mean	0.5301	0.5301	0.5301	0.5309	0.5436
Control Group Het. Mean	-0.0213	33.0612	-0.0151	0.0016	0.0159
Observations	13,428	13,428	13,428	11,599	10,347
R^2	0.2057	0.2068	0.2077	0.2171	0.2179

Notes: Analysis of heterogeneity on the Employment outcome, pooling together all available data using the ITT strategy of Table 2, and including an interaction effect to measure heterogeneous treatment effects. The dimension of heterogeneity is listed in the column title, and the third row provides the measure of heterogeneity. * p¡0.10 ** p¡0.05 *** p¡0.01.

Table A.3: Voting Heterogeneity

	(1) Formal Income	(2) Age at Lottery	(3) Female HH Head	(4) Property Price Change	(5) Homicide Change
Treatment	-0.00657	0.00409	-0.00651	-0.00969*	-0.0146**
	(0.00509)	(0.00879)	(0.00470)	(0.00505)	(0.00593)
Heterogeneity Dimension	0.0280	0.000670***	0.00735*	-0.00166	0.00166
	(0.0221)	(0.000190)	(0.00379)	(0.00290)	(0.00317)
Interaction Term	-0.00278	-0.000332	-0.00160	-0.00338	-0.00421
	(0.00483)	(0.000275)	(0.00534)	(0.00476)	(0.00549)
Control Group Y-Mean	0.0492	0.0492	0.0492	0.0512	0.0580
Control Group Het. Mean	-0.0261	31.0715	0.0079	0.0053	0.0154
Observations	14,710	14,710	14,710	12,693	11,319
R^2	0.0974	0.0984	0.0978	0.1011	0.0874

Notes: Analysis of heterogeneity on the Voting outcome, pooling together all available data using the ITT strategy of Table 2, and including an interaction effect to measure heterogeneous treatment effects. The dimension of heterogeneity is listed in the column title, and the third row provides the measure of heterogeneity. * p¡0.10 ** p¡0.05 *** p¡0.01.

Table A.4: Fertility Heterogeneity

	(1) Formal Income	(2) Age at Lottery	(3) Female HH Head	(4) Property Price Change	(5) Homicide Change
Treatment	-0.00811*	-0.0108	-0.00717*	-0.00466	-0.00128
	(0.00415)	(0.00763)	(0.00397)	(0.00415)	(0.00504)
Heterogeneity Dimension	-0.0140	-0.00235***	-0.0118***	0.00238	-0.00223
	(0.0441)	(0.000268)	(0.00283)	(0.00211)	(0.00211)
Interaction Term	-0.00563	0.000136	0.00425	-0.00127	-0.00316
	(0.00447)	(0.000246)	(0.00363)	(0.00424)	(0.00632)
Control Group Y-Mean	0.0411	0.0411	0.0411	0.0409	0.0398
Control Group Het. Mean	-0.0587	29.3007	0.1869	0.0016	-0.0082
Observations	7,606	7,606	7,606	6,552	5,835
R^2	0.1254	0.1443	0.1282	0.1377	0.1422

Notes: Analysis of heterogeneity on the Fertility outcome, pooling together all available data using the ITT strategy of Table 2, and including an interaction effect to measure heterogeneous treatment effects. The dimension of heterogeneity is listed in the column title, and the third row provides the measure of heterogeneity. * p¡0.10 ** p¡0.05 *** p¡0.01.

Table A.5: School Heterogeneity

	(1) Formal Income	(2) Age at Lottery	(3) Female HH Head	(4) Property Price Change	(5) Homicide Change
Treatment	-0.00528	0.00943	-0.00284	-0.000252	0.00530
	(0.0181)	(0.0368)	(0.0170)	(0.0187)	(0.0224)
Heterogeneity Dimension	0.246***	-0.0274***	-0.000377	0.00549	-0.00735
	(0.0804)	(0.00176)	(0.0190)	(0.0103)	(0.00982)
Interaction Term	-0.0168	-0.00228	0.00531	0.0000918	0.00820
	(0.0182)	(0.00372)	(0.0173)	(0.0188)	(0.0205)
Control Group Y-Mean	0.4033	0.4033	0.4033	0.3990	0.3850
Control Group Het. Mean	-0.1165	8.4381	0.1388	-0.0089	-0.0133
Observations	6,827	6,827	6,827	5,885	5,211
R^2	0.1599	0.2464	0.1576	0.1657	0.1717

Notes: Analysis of heterogeneity on the Schooling outcome, pooling together all available data using the ITT strategy of Table 2, and including an interaction effect to measure heterogeneous treatment effects. The dimension of heterogeneity is listed in the column title, and the third row provides the measure of heterogeneity. * p¡0.10 ** p¡0.05 *** p¡0.01.

Table A.6: College Heterogeneity

	(1) Formal Income	(2) Age at Lottery	(3) Female HH Head	(4) Property Price Change	(5) Homicide Change
Treatment	0.00643	0.150	-0.00348	-0.00129	-0.0258
	(0.0348)	(0.125)	(0.0303)	(0.0320)	(0.0372)
Heterogeneity Dimension	-0.116	-0.00299	0.0325	-0.0200	0.0447**
	(0.235)	(0.00625)	(0.0216)	(0.0185)	(0.0202)
Interaction Term	0.0197	-0.00811	0.00247	-0.00365	-0.0523*
	(0.0354)	(0.00652)	(0.0288)	(0.0310)	(0.0297)
Control Group Y-Mean	0.4170	0.4170	0.4170	0.4209	0.4544
Control Group Het. Mean	-0.2208	18.5633	0.1511	0.0195	0.0060
Observations	2,894	2,894	2,894	2,512	2,168
R^2	0.2201	0.2217	0.2212	0.2413	0.2315

Notes: Analysis of heterogeneity on the College attendance outcome, pooling together all available data using the ITT strategy of Table 2, and including an interaction effect to measure heterogeneous treatment effects. The dimension of heterogeneity is listed in the column title, and the third row provides the measure of heterogeneity. * p¡0.10 ** p¡0.05 *** p¡0.01.