Journal of Public Economics 235 (2024) 105138

Contents lists available at ScienceDirect

Journal of Public Economics

journal homepage: www.elsevier.com/locate/jpube

Short communication

Movin' on up? The impacts of a large-scale housing lottery in Uruguay[☆]

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A R T I C L E I N F O

JEL classification:
O18
I38
D72

Keywords:
Housing policy
Economic mobility
Political participation

A B S T R A C T

We report on a large-scale urban resettlement program in Uruguay. Under the program, thousands of low- to middle-income households were randomly assigned over the course of seven years to ownership of apartments in new buildings in more central areas and received a subsidy averaging $44,000 per household. We match applicants to comprehensive administrative data on employment, schooling, fertility, and voting over the decade after 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 affect school attendance. The relocation program did not result in transformative improvements in the lives of its beneficiaries, likely because of its minimum income requirements and the lack of strong spatial inequality in Uruguay.

1. Introduction

Neighborhoods are thought to have important effects on individuals' behavior and outcomes. Residential mobility programs that induce movement to a different neighborhood provide unusually clear insight into the role of context in determining welfare outcomes (Kling et al., 2005). The best-known place-based policy in the US (Moving to Opportunity, or MTO) had an arm that randomized access to housing vouchers in five US cities conditional on participants' moving to a less poor neighborhood. Studies on MTO have found modest short-term improvements (Katz et al., 2001; Ludwig et al., 2008; Jacob and Ludwig, 2012), followed by more transformative life-long benefits accruing to children who moved at a young age (Chetty et al., 2016), particularly girls (Ludwig et al., 2013).[1] However, only a handful of follow-up studies have been conducted (such as Bergman et al., 2019), and there are few rigorous studies of place-based programs outside of the US.[2] Given the importance of urban inequality and social mobility across the developing world, understanding the impact of such policies in a global context is critical.

We provide evidence based on a series of 187 independent lotteries conducted by the government of Uruguay's *Compra de Vivienda Nueva* ("Purchase of New Housing" or CVN) program whereby low- to middle-income families from peripheral neighborhoods were assigned to homes in newly constructed buildings closer to urban centers. These lotteries involved 4,883 households, with approximately one in six applicant households winning a place in a new building over all the lotteries. We match the individuals involved in this sequence of experiments to data from government sources, observing their formal employment status, fertility, public K–12 and university enrollment status, and voter turnout in local elections for all members of applicant families. The

☆ The authors gratefully acknowledge funding from the Fondo Maria Viñas (FMV_1_2014_1_103689), Agencia Nacional de Investigación e Innovación (ANII), Uruguay. The pre-analysis plan for this study (20171101AA) was filed with Evidence in Governance and Politics (EGAP) and can be found at https://osf.io/7rszw. We thank Javier Chiossi and Lihuen Nocetto for excellent research assistance. The authors declare no financial interests with respect to the study.

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[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); analyses of why MTO did not have larger impacts have centered on the fact that the beneficiaries chose to move to wealthier but still minority-dominated neighborhoods and so their peer groups may not have shifted (Clampet-Lundquist and Massey, 2008).

[2] Barnhardt et al. (2017) analyze the results of a randomized housing lottery in India after 14 years; they attribute their finding of null results of that program to the dislocation in social networks induced by the moves. 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 program most similar to ours is Brazil's "My House, My Life" program (PMCMV), which was found by Leape (2020) to have increased earnings by 13% and employment by 2% within four years.

https://doi.org/10.1016/j.jpubeco.2024.105138
Received 3 October 2022; Received in revised form 6 May 2024; Accepted 7 May 2024
Available online 21 May 2024
0047-2727/

first lotteries took place in 2009, and we have outcomes up through 2022, providing a relatively long post-treatment window to observe the evolution of outcomes as the winners moved, became homeowners, and adapted to their new neighborhoods. This setting represents a unique combination of large-scale experimentation, real-world government implementation, and universal capture of outcomes through administrative data.

The moves made by lottery winners entailed a meaningful improvement in their living conditions: the winners moved to larger and better houses in neighborhoods with higher property values and high school graduation rates—although, interestingly, not to neighborhoods with lower crime rates. 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) and made lottery winners titled homeowners in their new neighborhoods. Our analysis of the program results therefore also speaks to the literature suggesting that homeownership per se may promote community engagement, voting (DiPasquale and Glaeser, 1999; Hoff and Sen, 2005) and labor force participation (Hausman et al., 2022). Homeowners may be more likely to invest in their properties (Galiani and Schargrodsky, 2010) and to participate in voluntary organizations and in local politics (Cox, 1982; Blum and Kingston, 1984; McCabe, 2013; Kumar, 2022). Beyond simply relocating beneficiaries to new neighborhoods, the CVN program provided them with an asset transfer and a title to a home, and so our analysis here is closely related to other works that use experiments in financial subsidies to study homeownership (Engelhardt et al., 2010; Leape, 2020).

Despite the good statistical power, universal capture of outcomes, long duration of measurement, and substantial bundle of transfers implied by treatment in our setting, we uncover only relatively modest effects of the program. We use three distinct techniques to measure the impacts of exposure to treatment. First, we aggregate observations across all available post-lottery years for each outcome and examine the simple intention-to-treat (ITT) effect 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 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 using an instrumental variable local average treatment effect (IV/LATE) estimation considering the number of years since an individual became a lottery winner, which looks for a linear slope in the duration of treatment. None of the outcomes display strong and robust patterns across lottery years and estimation approaches, but the program does have detectable effects. For employment, the treatment effects across most lottery years are positive, and the pooled effect is significant, with a point estimate of a two-percentage-point improvement (over the control mean of 53%) in the likelihood of a family member's being formally employed. 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 that is never compensated by subsequent higher fertility and hence leads to suppressed subsequent fertility (approximately two-thirds of a percentage point below the annual fertility probability of 3.9% for the control group). Far from it being the case that home ownership encourages local political participation, we see a slight disruptive effect and an overall insignificant reduction in voting among lottery winners. Moving appears to have no effect on schooling enrollment (at either the public K–12 or university level).

In the final section of the paper, we try to account for our findings of relatively muted overall results. Our analysis suggests that two features of the program and context may be particularly consequential. First, receipt of a subsidized mortgage was conditional on recipients' meeting eligibility criteria with respect to their ability to pay, which served to screen out the poorest potential applicants. Thus, the universe of eligible applicants was lower-middle income, with few individuals from the poorest deciles of the local income distribution. Our examination of heterogeneity by baseline income level suggests that this feature of the targeting limited the program's overall impacts. Second, place-based mobility programs work precisely because they shift individuals across an urban gradient which is implicitly determined by the degree of inequality in the focal city. Exceptionally for Latin America, a region in which inequality is pervasive, Uruguay in general and Montevideo in particular are relatively egalitarian. The intersection of the screening criteria and Uruguay's urban geography resulted in the changes in neighborhood quality effectuated by the program being relatively modest. An implication is that geographic mobility programs may be particularly effective tools in places where inequality is a severe problem to begin with Korpi and Palme (1998). In the case at hand, the combination of low spatial inequality and targeting rules together undermined the effect of a geographic mobility program.

2. Design

2.1. The Compra de Vivienda Nueva program

In 2005, the National Housing Agency of Uruguay launched *Compra de Vivienda Nueva*, an initiative designed to aid families who sought to own a home but faced obstacles due to insufficient savings and an inability to meet the loan repayment criteria set by banks.[4] The program was targeted at families with at least one child under 18 years old or one disabled family member. The maximum monthly income a family could have varied with family size, but was 6.75 times the minimum wage (2,745 USD) for a family of four. Eligibility was restricted to families with 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 that they would 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 were screened out, but few of the very poorest households were included (see Fig. A.1 and A.2).[5] The median applicant fell in the third income decile and only 12% of applicants were in the poorest decile, while we observe no applicants with income above the city median (and only 6.75% of applicants falling in 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 approximately 30% 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 is larger for a) larger families and b) households with lower incomes.[6] The average lottery winner

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

[4] The program was designed to address two primary goals. First, it targeted families with children, prioritizing the well-being of infants and adolescents in housing policy. Second, it aimed to combat geographic fragmentation by providing new options for homeownership within the city, thereby promoting increased population density and socioeconomic diversity in urban areas already equipped with extensive public services.

[5] The National Housing Agency serves the latter population through alternative housing programs, such as rent subsidies and support for cooperative housing projects.

[6] The full set of requirements can be found at https://www.gub.uy/ministerio-vivienda-ordenamiento-territorial/politicas-y-gestion/compra-vivienda-nueva-construida-mvot.

purchased a house worth $64,000, was asked to make a 10% down payment from her 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' duration. The average total subsidy awarded to a winning household was $44,000, representing a very substantial investment in a single household and raising the bar for the scale of impacts that we would hope to see in a cost-effectiveness sense. Beneficiaries are not allowed to sell the home until they have completely paid off the mortgage. Furthermore, since the subsidy is applied to their monthly payments, they have a strong incentive to stay in the program apartment until the mortgage is fully settled.

The program experienced substantial excess demand, and so beneficiaries were randomly selected from among qualified applicants by lottery. When 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. Applicants were selected by means of a drawing of pieces of paper from a bowl, with all qualified applicants 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 2009 and 2016, the CVN program had 345 calls and 1,860 apartment units assigned. Among these calls, 187 were "competitive" lotteries, defined as a) lotteries that had more than one participant and b) lotteries in which not all participants won. A total of 1,255 apartment units were assigned in these competitive lotteries. 916 of these units were located in Montevideo and the remainder in the rest of the country.

Figure A.3 shows the locations of all apartment buildings constructed under CVN. Panel A shows the full locations 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 that they would move to if they won. Appendix Tables A.1 and A.2 show the ITT estimates of the extent to which winning the first lottery altered the location at which we last observe the individual to be living.[7] Table A.1 examines household characteristics, finding sharp improvements in housing quality (with the exception of water and electricity connections, which are close to universal anyway), with the number of bedrooms increasing by .36 and the share of households with written leases moving from 61% to 100%. Table A.2 examines attributes of the neighborhood rather than the residence and finds more mixed evidence of improvements. Socioeconomic metrics such as price per square meter, high school graduation, and an index of social deprivation all increase markedly for winners. Other attributes, such as most forms of crime and local homeownership rates, are no better in the new neighborhoods than the old, a point to which we return later (the rows at the bottom of the table provide the simple before/after averages for winning households). We can understand selection into the program by comparing applicant characteristics to national averages. Employing data from the 2011 round of the Uruguay national census, Appendix Table A.3 shows that applicants come from neighborhoods with higher rates of rentals than the national average and that the applicant households are substantially larger and even more likely to live in apartments than other households in their neighborhoods.

Analysis of the pooled lotteries is complicated by two forms of noncompliance. First, winners may decide not to participate in the program after winning the lottery. This is rare but did happen in 1.2% of cases; this is a typical form of noncompliance and can be handled simply by our estimating an ITT effect, ignoring this source of noncompliance. Second, households that lost in prior lotteries were free to join subsequent ones; 35% of the households in our data appear in more than one lottery, and 6% appear in more than three. Figure A.4 shows both forms of noncompliance; for every lottery year, the noncompliance in treatment is below 5%, and the subsequent retreatment rate among initial controls is around 10% for the first few cohorts and then drops below 5% for cohorts participating in lotteries after 2012.[8] We account for this issue using two strategies borrowed from the literature on US school lotteries, which share the same feature. First, we calculate the equivalent of an ITT effect by using only the outcome of the *first* lottery regardless of how many subsequent lotteries the household enrolled in. This estimate is structurally biased toward 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 the duration of treatment with the outcome of the first lottery entered (following Abdulkadiroğlu et al., 2011), which provides an estimate of the local average treatment effect (LATE).

2.2. Data and outcomes

The mapping of national ID numbers to government databases allows us to define five primary outcomes for the study. We measure **formal employment** by observing 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).[9] 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. The elections for which we observe voter participation relate to highly localized public good investments (parks, schools, street paving, sports fields, and so on). Participation in these elections therefore serves as an interesting metric of engagement with community issues.

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. The disruption of the move may interrupt family growth, but more interestingly, the program generates a Beckerian tradeoff: the increase in permanent income from the asset transfer would be expected to relax constraints and increase fertility, while the improvements in labor market opportunities from a more central urban location would raise the opportunity cost of time and hence decrease fertility. 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. 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 Uruguay. 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. We observe the years when individuals enter and exit these universities and so can construct a panel measure of their being currently enrolled for each year.

[7] We cannot track locations in a panel as we can outcomes, and so for those who never won a lottery or did not comply with treatment assignment (i.e., those who entered a subsequent lottery after losing or who did not move to a subsidized unit after winning), we use the location at the time of their last application; for those who win and comply, we use the location of the winning building. This outcome is analyzed by means of the same experimental treatment indicator (winning the first lottery entered) used in the impact analysis.

[8] Figure A.5 shows how the noncompliance changes across subsequent years for each initial lottery year.

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

Appendix Table A.4 provides summary statistics for our core data and illustrates the balance between treatment and control. It includes three types of variables: In the top panel, it uses the household information included on the program application forms. In the middle panel, we examine the pretreatment 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 eventually win to those who eventually lose. While the eventual winners are 5.5 percentage points more likely to be in rentals at the time of application and the treatment moves would be slightly more economically advantageous for them than for the control, critically, all of the outcome variables appear to be well balanced. This is particularly important as the researchers did not control the lottery mechanism.

3. Results

3.1. ITT estimation

Our first estimates of the impact of winning a CVN lottery use an ITT approach, 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 in which they appear during that year, but we use weights to remove the undue influence of these repeat observations and cluster 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 treatment outcomes (win, lose) equally important despite the fact that the lotteries rarely assigned close to 50% of the 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 treatment in that household's first lottery within pool p (where the pool refers to the group of lottery cohorts and their outcomes aggregated together in a given analysis). To improve statistical power, the regression includes a battery of covariates X_{ic} taken from the application data (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) and McKenzie (2012), 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 1. Every coefficient in the table is from a different regression; the different samples included in the lotteries 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. All outcomes are defined as the cumulative average across all observed post-treatment years. Each outcome is defined only 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.[10] The fertility effect appears consistent with disruption arising from the move; the negative coefficients appear only for years 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 of two-thirds of a percentage point in the likelihood of a birth, and the employment effect of .02 indicates one more week of formal employment over the entire post-treatment period. Thus, even where the pooled effects are significant, they are not large.[11] Voting similarly displays an apparent disruption effect, with political participation being lower for those who relocated recently, but the pooled effect is insignificant. For the two schooling outcomes, there are years for 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 impact in terms of employment increases and decreased fertility (as might be expected if female employment improves), no transformative benefits appear to have emerged.[12] Appendix Table A.5 provides the ITT results without covariates, and the results are very similar.

3.2. Event study estimation

The ITT results suggest the possibility of disruption effects closer to the time of lottery, but the averaging across post-treatment years inherent in that approach 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 the data for all lotteries and years (again repeating and deweighting 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 Fig. 1 for four of the primary outcomes. 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 averaged across post-treatment years, 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 averaged across lags to achieve statistical significance.

10 The row at the bottom of Table 1 gives the post-lottery control mean and so is different from that in Table A.4, which gives the pre-exposure mean.

11 In Appendix Table A.7, we look at fertility by examining the total number of children in 2017 rather than the probability of birth in any year. In Column (3) of this table, which presents the results corresponding to specification most similar to the above, we find a weak negative effect of -.01 children (equivalent to approximately 2 years of our per-year post-treatment effect), but given the much larger standard error on the total number of children, this effect is insignificant.

12 There is substantial heterogeneity in impacts across lottery years. To investigate the source of this heterogeneity, Appendix Table A.8 first runs a regression including interactions between lottery year and treatment and then reports the joint F-statistic on the difference between the year-specific effects, finding significant heterogeneity for all outcomes except employment (column (1)). It then controls for all the baseline applicant variables that we observe interacted with treatment, and it repeats the test of significant differences across years (column (2)). Because we lose significance for all outcomes except voting, the takeaway is that the cohort impacts are different because the participants in the cohorts are different in observable ways. These differences are plotted directly in Appendix Figure A.6, which shows the coefficient and confidence intervals for the coefficient on year dummies in a regression where the covariate is the outcome and the constant is omitted to obtain year-specific means.

Table 1
ITT estimates by lottery year.

	(1) Employment, 09–21	(2) Voting, 08–16	(3) Fertility, 09–16	(4) Schooling, 09–21	(5) University, 09–23
2009 Lottery Cohort	.0012	−.0161	.0018	−.0103	.0153
	(.023)	(.01)	(.006)	(.044)	(.013)
	[3,296]	[3,672]	[1,745]	[1,727]	[3,766]
2010 Lottery Cohort	.015	−.0141**	−.0009	.0433	.009
	(.027)	(.006)	(.007)	(.049)	(.009)
	[640]	[738]	[369]	[384]	[674]
2011 Lottery Cohort	.0111	−.0069	−.0052	.0516**	.0054
	(.017)	(.008)	(.006)	(.025)	(.007)
	[3,523]	[3,854]	[2,018]	[2,048]	[3,784]
2012 Lottery Cohort	.0057	.0094	.0123	.0139	−.0076
	(.016)	(.008)	(.009)	(.028)	(.008)
	[4,152]	[4,563]	[2,338]	[2,302]	[4,497]
2013 Lottery Cohort	.0477	−.0243***	−.0018	.0249	−.0026
	(.031)	(.009)	(.012)	(.05)	(.006)
	[1,023]	[1,142]	[608]	[600]	[1,193]
2014 Lottery Cohort	.1147***	.0188	−.0391***	−.003	−.0077
	(.027)	(.03)	(.009)	(.045)	(.008)
	[1,026]	[1,129]	[589]	[533]	[1,158]
2015 Lottery Cohort	.0303	−.035***	−.0319***	−.0476	.0086
	(.022)	(.01)	(.008)	(.04)	(.022)
	[1,483]	[1,628]	[830]	[755]	[1,641]
2016 Lottery Cohort	−.1027	−.1126***	.0034	.1369**	−.0384
	(.089)	(.035)	(.024)	(.069)	(.039)
	[253]	[280]	[163]	[128]	[303]
Pooled Sample	.0217**	−.0063	−.0069*	−.002	−.0052
	(.01)	(.005)	(.004)	(.017)	(.009)
	[15,502]	[17,006]	[8,755]	[7,933]	[13,250]
Pooled Average	0.53	0.05	0.04	0.40	0.08
Sample Eligibility	18 or more years of age	16 or more years of age	Between 15 and 49 years of age	Between 6 and 19 years of age	Between 17 and 40 years of age

Notes: Table presents intention-to-treat (ITT) impacts of the winning status in the first lottery entered for each cohort included. Outcome is cumulated across all available post-lottery years. Every coefficient is from a separate regression. Regressions include fixed effects for lottery and are weighted with 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 does 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 brackets; $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

3.3. LATE estimation

A related way of summarizing impacts is to ask whether the duration of exposure to the treatment of relocation to a new house, entry into homeownership, and receipt of a housing subsidy alters outcomes. Following Abdulkadiroğlu et al. (2011), we can estimate an IV/LATE equation where the endogenous variable of duration of treatment is instrumented by the outcome of the first lottery entered. 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 in calculating the post-exposure average, and for each lottery year, we pool all individuals who had been included in a lottery prior to that year. This means that by the time we examine the 2016 lottery (our last), we are effectively 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-stage least squares estimate is then:

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

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

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

The results are presented in Table 2. Again, every coefficient is from a different regression, but here, since the rows include all individuals involved in a lottery *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 that from Table 1 with the ITT estimates. One additional year of treatment increases the employment rate by .4 percentage points, 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.

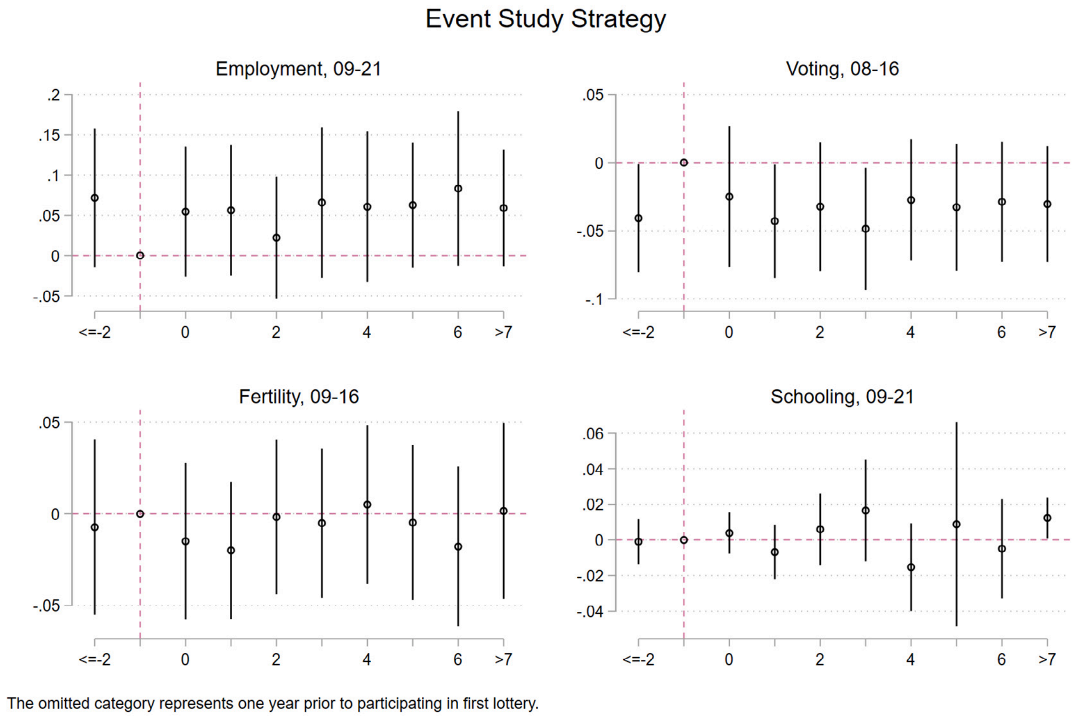

Fig. 1. Event study treatment effects.
The figure shows the first lead and the lags of treatment across the main study outcomes that are observable as panel data. The analyses pool all lottery cohorts and present point estimates and confidence intervals for post-treatment outcome years.

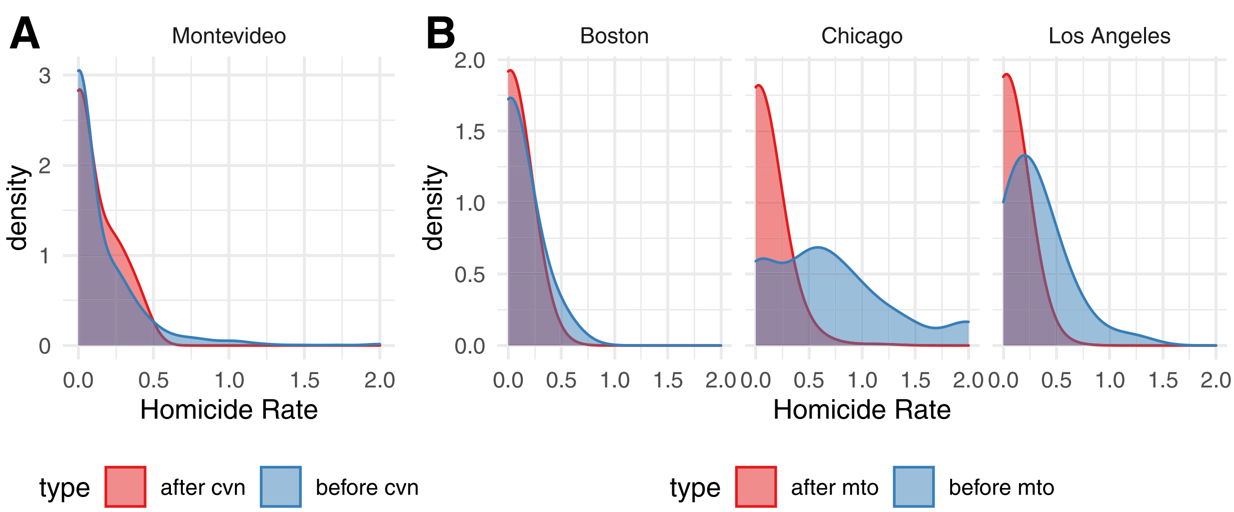

Fig. 2. Comparison of the Change in Census-Tract Homicide Distribution, CVN vs. MTO.
The figures compare the distribution of the homicide rate of the neighborhoods where program beneficiaries were located before and after the interventions for CVN (Panel A) and for MTO (Panel B). For MTO, we identify 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 are required criteria for eligibility.) Data on poverty and crime at the census-tract level for the 1990s come from Peterson and Krivo (2010).

Appendix Table A.6 provides the LATE results without covariates; the results are again very similar.

3.4. Treatment effect heterogeneity

We can contextualize the muted impacts found in this study through two types of heterogeneity analysis. The first of these is a household-level interaction analysis using the pooled ITT specification and interaction with application covariates as described in our pre-analysis plan. Table 3 shows that both employment and college enrollment impacts are larger for low-income households and that the employment impacts are more than twice as large for female-headed as male-headed households. We can calculate the mortgage subsidy that losers would have received if they had won, and so we can examine the heterogeneity in impacts over this variable in the same way. While the subsidy amount is partially determined by demographics (larger families receive larger subsidies), our heterogeneity analysis provides no evidence that making the program more generous would have improved outcomes; indeed, those receiving more appear to have worse schooling outcomes. The core result here is that the program would have had larger employment impacts if it had been targeted at poorer individuals, meaning that there

Table 2
LATE estimates by years treated.

	(1) Employment, 09–21	(2) Voting, 08–16	(3) Fertility, 09–16	(4) Schooling, 09–21	(5) University, 09–23
2009–2010 Cohorts		−.0085* (.0049) [4,410]	.0001 (.0029) [2,114]		.0334* (.0172) [674]
2009–2011 Cohorts	.0125** (.0062) [7,565]	−.0053* (.003) [8,264]	−.001 (.0018) [4,136]	.0061 (.011) [3,857]	.0034 (.0085) [4,458]
2009–2012 Cohorts	.0086** (.0043) [11,717]	−.0036* (.0019) [12,827]	−.0006 (.0014) [6,496]	.0034 (.0074) [5,987]	.0001 (.0048) [8,955]
2009–2013 Cohorts	.0063* (.0033) [12,740]	−.0025* (.0014) [13,969]	−.0006 (.0011) [7,111]	.0035 (.0055) [6,559]	−.0008 (.0032) [10,148]
2009–2014 Cohorts	.0054** (.0026) [13,766]	−.0016 (.0012) [15,098]	−.0009 (.0009) [7,723]	.0031 (.0043) [7,072]	−.0004 (.0024) [11,306]
2009–2015 Cohorts	.0047** (.0022) [15,249]	−.0015 (.0009) [16,726]	−.0009 (.0007) [8,586]	.0019 (.0036) [7,805]	−.0007 (.0019) [12,947]
2009–2016 Cohorts	.0041** (.0018) [15,502]	−.0012 (.0008) [17,006]	−.0009 (.0006) [8,755]	.0014 (.003) [7,933]	−.0007 (.0016) [13,250]
Pooled Average	0.52	0.05	0.04	0.39	0.06
Sample Eligibility	18 or more years of age	16 or more years of age	Between 15 and 49 years of age	Between 6 and 19 years of age	Between 17 and 40 years of age

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 included in a lottery prior to a given year. The first-stage relationship is the same across all regressions, and in turn, there should be one common F-statistic describing the strength of the relationship. However, as the sample for each cell varies, the F-statistic of the first stage also varies due to sampling variation. The smallest Kleibergen–Paap Wald F-statistics (KPF) observed for each column are 39,598, 10,232, 4,503, 50,115 and 5,116 and so indicate that the instrument is a strong one. Regressions include fixed effects for lottery and are weighted with randomization inverse propensity weights. Standard errors clustered at the household level are in parentheses, and number of observations for each analysis is in brackets. $^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$.

Table 3
Dimensions of applicant heterogeneity across main outcomes.

	(1) Employment, 09–21	(2) Voting, 08–16	(3) Fertility, 09–16	(4) Schooling, 09–21	(5) University, 09–23
Formal Income	−.0264** (.012) [15,502]	−.0028 (.005) [17,006]	−.0056 (.004) [8,755]	−.0168 (.018) [7,933]	−.016** (.008) [13,250]
Age at Lottery	−.0002 (.001) [15,502]	−.0003 (0) [17,006]	.0001 (0) [8,755]	−.0023 (.004) [7,933]	−.0005 (.001) [13,250]
Female HH Head	.0232** (.01) [15,502]	−.0016 (.005) [17,006]	.0042 (.004) [8,755]	.0053 (.017) [7,933]	−.0057 (.008) [13,250]
Subsidy Value	.0151 (.012) [15,172]	.0057 (.004) [16,640]	.0014 (.005) [8,558]	−.0376** (.017) [7,742]	.0119 (.009) [12,849]
Pooled Average	0.53	0.05	0.04	0.40	0.08
Sample Eligibility	18 or more years of age	16 or more years of age	Between 15 and 49 years of age	Between 6 and 19 years of age	Between 17 and 40 years of age

Notes: Heterogeneity analysis pooling all available data with the ITT strategy of Table 2 and showing only an interaction effect to measure treatment effect heterogeneity. Each cell represents an interaction term for a different regression. The dimension of heterogeneity is listed in the row title. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

is a tension between the economic requirements of a mortgage-based poverty alleviation program and its overall effectiveness.

We can examine heterogeneity across the spatial distance moved given that, for every applicant, we know both her neighborhood at the time of the lottery and also the neighborhood that she would have moved to if she had won, as shown in Appendix Table A.2. This distance is exogenous in that it is measured prior to randomization and captured in an identical manner for both treatment and control. We can take the value of the covariate Z_i at the location of the household at the time of application and that of Z_k at the location to which it would move if it won the lottery, and calculate d_{ikc}, the distance between these two values for household i in lottery group c and lottery j. The heterogeneity analysis is then conducted via the following regression:

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

Table A.9 presents the results of this analysis, focusing on the parameter τ which measures how the treatment effects differ across distance measure d_{ikc}. There is no heterogeneity over any spatial dimension for impacts on voting, K–12 schooling, or tertiary education. The results link fertility decisions to neighborhood education rates in an intuitive way, showing that women who see the largest increase in local high school completion rates see the largest drops in fertility. More surprisingly, this table demonstrates that employment responds most strongly to the treatment when individuals are relocated to *worse* neighborhoods, as measured by the infrastructure deprivation index, by high school graduation rates, or by homicide rates. This counterintuitive result is likely explained by the fact that, in Montevideo, the central district where most employment is located has higher crime than the suburban neighborhoods of the city. However, the absolute magnitudes of all these interaction effects is similar and small, suggesting that a one-standard-deviation increase in the neighborhood change would move employment by less than 5% of its control value of 53%.

An additional way of contextualizing the impact of this bundled program is provided in Appendix Table A.10, which shows both uninteracted terms for the distance interaction presented in the bottom row of Table A.9. Given the interaction with distance, we can think of the uninteracted treatment dummy as providing a linearized estimate of the impact of the program for someone who receives a new home in exactly the same location, or the effect with no spatial dislocation. For employment, we see an intercept approximately 60% larger than that for the ITT estimate in the bottom row of Table 1, suggesting that the economic dimension of the program (the need to pay the mortgage) drives the employment increase and the dislocation of the move attenuates this increase. For all other outcomes, the intercept term is within a percentage point of the ITT estimate. In general, the lack of significance in Table A.10 (and the substantial heterogeneity found in other dimensions of Table A.9) suggests that the physical distance of the move is not in this case the most powerful way of describing the change induced by winning the lottery.

Following on this realization, how much absolute variation in neighborhood quality do we see in the data? 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. Fig. 2 plots the distribution of homicide rates for the prior neighborhoods and the new locations in Montevideo (Panel A) and then their counterparts for 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 in Montevideo. This maps partly to the 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 the lower overall levels of crime (homicides per thousand are .12 in Montevideo, and average .2 in the MTO cities).[13]

The more striking feature of this picture, however, is that, in our study, the neighborhoods of the new buildings are actually have slightly *higher* homicide rates on average, while the MTO resulted in dramatic decreases in exposure to local crime. Kingsley and Pettit (2008) found that MTO-experiment movers saw a 72% reduction in their neighborhood violent crime rates (from 40 per 1k pop to 11) as a result of their first move. By contrast, in our study the substantial economic improvements in the new neighborhoods are not matched by lower crime rates.[14] 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, while the city center, toward which they moved, has higher crime rates. Indeed, while the correlation between the poverty rate and the homicide rate for the MTO cities is .45, for Montevideo, it is only .1. Consequently, two core features of urban geography appear to have played 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 second, 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

For a program that bundles relocation with home ownership and a substantial asset transfer, we find surprisingly muted impacts. Evaluations of MTO over a medium-term time frame corresponding to the horizon considered here were similarly disappointing, and so it may simply be the case that more time is required for benefits to manifest themselves. However, given that place-based programs have an impact by improving outcomes for those whose counterfactual outcomes would have been poor, we believe that our results emphasize two dimensions of this problem. First, such programs need to enroll and treat individuals who would have struggled financially and suffered from poor neighborhood quality 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 were treated under 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. Larger employment effects for female-headed households reinforce the impression of greater impacts for disadvantaged households. Given that attempts to target a home purchasing program with wealth requirements at poor individuals will likely severely limit the potential participant pool, such programs may do better to focus on rental subsidies, which can be more easily directed to the neediest households.

Second, to improve outcomes by moving an individual across space, these programs are fundamentally predicated on the degree of spatial inequality in the cities in which they operate. Ironically (given its setting in Latin America, long one of the most unequal regions in the world), this program was implemented in a country with limited overall inequality and in which the neighborhoods with the greatest economic

[13] Several of the MTO papers note the lower level of inequality and racial segregation in Boston relative to the other sites, particularly Chicago (Nguyen et al., 2017). However, it is also noted that beneficiaries in Boston moved to less segregated neighborhoods (de Souza Briggs et al., 2010) and to neighborhoods with good access to jobs (Turner et al., 2011) and these features may have sustained benefits there despite lower underlying inequality.

[14] Fig. 2 shows neighborhood-level changes, and indicates higher crime rates in the new neighborhoods. Table A.2 shows the same numbers but weighted by the number of applicants in each neighborhood in the bottom panel, and altered by the non-compliance in the top panel. The fact that these measures tilt toward a slight drop in crime from the program does indicate that demand is higher for individuals trying to leave high-crime neighborhoods.

opportunity also have higher crime. While the scalability of these programs is limited by constraints on the ability of the government to relocate large numbers of people, 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 geographic inequality to begin with.

Declaration of competing interest

None.

Data availability

We will make all replication data public.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jpubeco.2024.105138.

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