

Transferring wealth: subsidizing homeownership increases investment in human capital*

Tanu Kumar[†]

June 20, 2020

Abstract

Governments often subsidize homeownership, thereby transferring wealth to households. I find that beneficiaries of a subsidized housing lottery in Mumbai reinvest this wealth in human capital. After 3-5 years, winners are more educated and more likely to be employed than non-winners, with effects concentrated among youth. Relocation to areas with better opportunities is unlikely to drive effects, as at the time of measurement, winners live in neighborhoods with worse schools and lower employment rates than non-winners. The results show that illiquid government transfers can change behavior in a short time.

JEL Codes: E24, I38, O18, H24, J62

How does subsidizing homeownership affect household economic trajectories? Governments in many countries use a variety of tools to make homeownership more affordable for citizens, including mortgage and home-price subsidies. These subsidies are wealth transfers to beneficiaries experienced in any combination of three payout structures: 1) through a stream of in-kind benefits for those who choose to live in the

*This project has been supported by the J-PAL Governance Initiative, the Weiss Family Program Fund for Development Economics at Harvard University, the Institute of International Studies at the University of California, Berkeley, and the American Political Science Association Centennial Center. Research has been approved by the Committee for Protection of Human Subjects at the University of California, Berkeley, protocol 2017-04-9808. A pre-analysis plan has been registered with EGAP here (<http://egap.org/registration/2810>). Deviations from the pre-analysis plan are explained in appendix. This paper uses the same research design and data as Kumar 2019. I am extremely grateful for Partners for Urban Knowledge Action Research and particularly Nilesh Kudupkar for assistance with data collection. Thank you to Pradeep Chhibber, Joel Middleton, Edward Miguel, and Alison Post for their mentorship and advice throughout this project. Anustubh Agnihotri, Caroline Brandt, Christopher Carter, Nirvikar Jassal, Curtis Morrill, Pranav Gupta, Carlos Schmidt-Padilla, Michael Koelle, Matthew Stenberg, and participants at DevPec 2019 also provided valuable comments. Most importantly, I thank the hundreds of survey and interview respondents who gave their time to this study.

[†]Travers Department of Political Science. `tkumar[at]berkeley[dot]edu`

subsidized home; 2) through cash benefits among those who choose to rent it out; 3) or lump-sum through resale of the home. Aside from transferring wealth directly, these interventions also help families purchase an asset that forms the cornerstone of wealth accumulation for many. In fact, home equity may be so fundamental to wealth that differential barriers to purchasing a home have been hypothesized to play a key role in intergenerational inequality (Oliver and Shapiro 2013). Learning about the ways in which beneficiaries spend these subsidies is fundamental to understanding their long-term effects.

Do beneficiaries re-invest their newfound wealth in human capital? Investment in education in particular might allow families to increase the size of these fortunes and pass them onto the next generation (Becker 1964). Bleakley and Ferrie (2016), for example, find that winners of a plot of land in Antebellum Georgia did not invest more in their children, and beneficiaries' descendants therefore did not have measurably different economic outcomes than those of non-beneficiaries. A substantial literature has attempted to estimate human capital effects of home subsidies in the United States, where homeowners can deduct much, if not all, of their mortgage interest from taxes (e.g. Richman, 1974; Essen et al., 1978; Green & White, 1997; Haurin et al., 2002; Dietz & Haurin, 2003; Cairney, 2005; Barker & Miller, 2009). Findings have been mixed, and many studies rely on selection-on-observables assumptions to circumvent problems of selection. Experimental studies from low- and middle-income countries often focus on housing or rental subsidies that require relocation, which means that the location of housing or broken social networks can drive effects (Barnhardt et al. 2017; van Dijk 2019; Franklin 2019; Picarelli 2019). Others, such as Franklin (2020) study programs that relocate those who live in slums, meaning baseline informality can also drive effects.

Many home subsidy programs, however, do not require relocation and favor middle-class citizens who do not live in slums. I measure the effects of one policy configuration common in India, the subsidized sale of government-constructed homes to middle-class households. These programs are found in every major city in India, including Delhi, Mumbai, Bengaluru, Kolkata, Chennai, Hyderabad, Ahmedabad, and are frequently offered in some form across smaller cities as well. I study a program in Mumbai, a city of over 20 million residents, which allocates the subsidized housing through a randomized lottery system.

Sale prices of similar homes suggest that a lower bound on the subsidy that beneficiaries might ultimately realize ranges between 10,000-45,000 USD. In a study of the long-run effects of cash transfers, Haushofer and Shapiro (2018) give households about 1,000 USD in seven monthly installments as part of their "large transfer" treatment arm. The wealth gains from the MHADA lottery are at least 10 to 45 times the magnitude of these transfers. Because the sale price of the homes covers construction and marketing costs, these programs incur few direct costs on implementing governments. Land-use laws might, in reality, limit the theoretically high opportunity cost of building subsidized

homes on urban land.

In the long term, these transfers are large. In a study of the long-run effects of cash transfers, Haushofer and Shapiro (2018) give households about 1,000 USD in seven monthly installments as part of their "large transfer" treatment arm. The wealth gains from the MHADA lottery are at least 10 to 45 times the magnitude of these transfers, but are illiquid because beneficiaries cannot resell the homes for ten years. In the short term, beneficiaries receive a flow of in-kind transfers in the form of housing benefits, or rental income which is, on average, 50 USD per month net of mortgage. Assuming rational decision-making, the value of housing benefits net relocation costs is at least the same for those who choose to relocate.

From September 2017 to May 2018, I surveyed 834 households of winners and non-winners of multiple lotteries that took place in 2012 and 2014. On average, individuals in winning households have over a half year more education than those living in non-winning households, over a base of 10 years. The treatment effect reflects an increase in winners' likelihood of completing secondary and post-secondary education.

The full sample effect reflects much larger effects among school-age children, or youth. Among household members who turned 16 after the lottery, the intervention increases the likelihood of beneficiaries continuing schooling past grade ten by 14 percentage points (pp). Among household members who turned 21 after the lottery, the intervention increases the likelihood of completing post-secondary education by 15 pp.

The intervention further increases levels of employment among individuals by 4.2 pp. Subgroups among which I observe large education gains also have better employment outcomes, suggesting that education gains drive employment gains. The effect size is 19.9 pp for youth who turned 21 after the intervention, or those who are old enough to have had completed their education in between the lottery and being surveyed. The overall employment effects represent a 7.5 percentage point increase in full-time labor and no measurable effect on part-time labor.

I suggest four possible mechanisms for these effects, including relocation, shifts in short- and long-term budget constraints, decreased present bias due to these shifts in budget constraints and improvements in housing quality, and changes in the perceived returns to education. Relocation to areas with better education and employment opportunities perhaps does not explain effects, as winners live on average in neighborhoods with poorer school quality and lower rates of literacy and employment than non-winners. It is more likely that the intervention alters behavior through the other mechanisms.

Existing studies of cash transfer programs provide benchmarks for the effect sizes. Araujo, Bosch, and Shady (2016) conduct a 10-year follow up of a cash transfer program in Ecuador (*Bono de Desarrollo Humano*, or BDH) providing households with children between 7-50 USD a month. When restricting the analysis to children living in households that began receiving transfers when they were young adults, they find that the

receipt of transfers increased secondary school completion rates by 1-2 pp, over a base of 75%. There are no measurable effects on employment. Even while the MHADA lottery provides a much larger wealth transfer in the long-run, the intervention generates a present-term monthly cash transfer of a similar magnitude. Nevertheless, effect sizes are much larger than for BDH.

Parker and Vogl (2018) find more comparable effects in a long-run study of Mexico's *Progresa* conditional cash transfer; a program providing 9.50-60 USD a month increased completion rates by 10-15 pp among those exposed to the program when young. While men in the control group are anyway employed at high rates, the intervention increases employment among women by 7-11 pp. In 3-5 years, then, the MHADA lottery increases high school completion and employment at rates similar to a program that a) explicitly incentivizes schooling and b) to which children were exposed for most of their schooling, rather than near the end of primary school or at the beginning of secondary school. It further increases college completion, an effect unseen (but measured) in the case of *Progresa*.

The results on education and employment, furthermore, do not provide a full picture of the welfare gains the MHADA program generates. McIntosh and Zeitlin (2018) suggest that general transfers may have effects on the same outcomes as those targeted by conditional programs, but at lower rates because households are spending in other areas as well. The relative flexibility in the use of MHADA benefits suggests that the program improves other aspects of household welfare as well, beyond the human capital gains measured here.

Because of their effect on permanent income, wealth and asset transfers may affect households differently than cash transfers. This study introduces a new context and transfer type to the relatively sparse literature on this type of intervention. The results in this study differ from those of other such studies for many potential reasons. The vehicle for the wealth transfer may affect results. Bleakley and Ferrie (2016), for example, find that winning a land lottery in Georgia, USA in 1832 did not increase the likelihood of *any* school attendance. Yet receiving land may increase the need for household labor on the farm, thereby increasing the opportunity cost of sending one to school. The context and target population are also important. Cesarini et al. (2016) find few human capital returns to a wealth shock in Sweden, but they argue that this is likely due in part to Sweden's strong social safety net, which does not exist in urban Mumbai. In low- and middle- income countries, other types of asset transfers provided as part of rural ultra-poor graduation programs (e.g. Banerjee et al. 2015) have received more attention, but these entail smaller wealth transfers to a poorer population.

This paper is among the first to study the effects of a common policy delivering large transfers that may fundamentally change the economic trajectories of families. Subsidizing homeownership is an initiative pursued by governments in wealthy, low-, and middle-income countries alike, yet causal identification of the effects of these policies is

difficult. Like the home mortgage interest deduction in the United States (see Glaeser and Shapiro 2003), these programs often benefit middle-class households rather than the poor. Studying their effects is thus essential to understanding the growth of inequality.

1 The program

Across India, state-level development boards have spearheaded programs that sell, rather than rent, subsidized units to eligible households in every major city in India. In 2015 India's federal government further announced a plan, Pradhan Mantri Awas Yojana (P-MAY, or "The Prime Minister's Housing Scheme"), to build 20 million affordable homes by 2022. Grants to subsidize the construction and sale of low-income housing by local municipal boards remain a central component of this policy.

I study the effects of an annual housing lottery run by the Mumbai Housing and Area Development Authority (MHADA), a subsidiary of the Maharashtra Housing and Area Development Authority that uses the same acronym. MHADA runs subsidized housing programs for economically weaker section (EWS) and low-income group (LIG) urban residents who 1) do not own housing, and 2) who have lived in the state of Maharashtra for at least 15 continuous years within the 20 years prior to the sale. Members of the EWS earn up to 3,200 USD/year. Members of the LIG earn up to 7,400 USD/year. Winners have access to loans from a state-owned bank, and most take out 15-year mortgages at 10-15% annual percentage rates. I include lotteries that took place in 2012 and 2014. Information about the area, cost, and downpayment for the apartments in the included lotteries can be found in Table SI.2.

Housing was constructed on land obtained from the city's dismantled textile industry. Figure SI.1 shows the location of the 2012 and 2014 EWS and LIG MHADA apartment buildings and households in the sample at the time of application. Households were permitted to choose the building for which they submitted an application.

The lottery homes were sold at a "fair price" that officials claim covers the costs of construction and marketing. Table SI.2 shows that these prices were small fractions of the market values of the homes; 3-5 years after the lottery, the difference between the apartment purchase price and list price for older MHADA apartments of the same size in the same neighborhood appears to lie anywhere between Rs. 661,700 (about 10,300 USD at 2017 conversion rates) to Rs. 2,869,015 (about 45,000 USD). These prices do not account for untaxed informal payments made above the list price, and are thus a lower bound on the potential value of the lottery homes.

Resale of the apartments is not permitted until 10 years after purchase, a rule enforced both by MHADA officials and homeowners' associations active in each lottery building. Households can, however, put the apartments up for rent. Half of households in the study have made this choice, and the median monthly rental income net of mortgage payments is Rs. 3000, or roughly 50 USD. Households do not pay taxes on their dwelling

for five years after possession.

2 Research design and data collection

Beneficiaries were selected through a lottery process. In response to extreme scrutiny over the selection process and concerns about corruption, the lottery was conducted using a protected computerized process that was implemented in 2010. Applicants also applied with their Permanent Account Numbers (PAN), which are linked to their bank accounts.¹ Before conducting the lottery, MHADA officials used the PAN numbers to check both whether individuals had applied multiple times for the same lottery number and whether or not they met the criteria for eligibility.

I estimate treatment effects on all outcomes based on in-person household surveys of a sample of both winning (treatment) and non-winning (control) households. All winners from the relevant year were included in the sampling frame. The winning sample was stratified by caste and occupation group (Table A.3), as each lottery had quotas for these groups within which random selection occurred. Note that applicants are able to apply for multiple lotteries within the same year, but only once per lottery.

As there are roughly 1,000 applicants for each apartment, I interviewed a random sample of non-winning applicants. MHADA provided phone numbers and addresses for both winners and a random sample of non-winning applicants drawn in the same stratified method used for the selection of winners. Recall that a stratum consists of a caste/occupation group within each lottery.

Households that had applied for multiple lotteries included in the study (either within a year or across years) would have a higher likelihood of appearing in either the treatment or control sample. The sampling procedure explicitly allowed for the possibility of the same household being drawn multiple times. If a household won lottery A but also was drawn in the sample of non-winners for lottery B, its data would have been included as a set of outcomes under treatment for lottery A and under control for lottery B. Ultimately, no households were drawn more than once.

I accessed a total of 1,862 addresses used at the time of application to the lottery. I first mapped them using Google Maps. I dropped Addresses that were incomplete (42), outside of Greater Mumbai (611), or could not be mapped (146). This left 531 and 532 control and treatment households, respectively with similar proportions of winners and applicants in each caste/occupation category, lottery income category, and apartment building (Table A.4). The mapping procedure favored wealthier applicants by dropping informal settlements and all who lived outside of Greater Mumbai, limiting my sample to urban applicants. Table A.5 shows that there are relatively fewer Scheduled Tribe

¹A PAN is issued by the Indian Income Tax Department to all eligible for an income tax. Its stated purpose is to minimize tax evasion.

members and more General Population (or Forward Castes) members in the mapped sample than in the full sample provided by MHADA.²

From the mapped sample, I randomly selected 500 households from each treatment condition to interview. From September 2017-May 2018, I worked with a Mumbai-based organization to contact the households and conduct interviews. The addresses and phone numbers provided by MHADA constituted the contact information for households at the time of application. Non-winners were attempted at these addresses. In cases where they had moved away, neighbors were asked for updated contact information, with which the enumerators once again attempted to contact non-winners. Among winners, owner-occupiers were approached at the lottery apartments; landlords were approached at the addresses listed on the application using the procedure developed for non-winners.

In all cases, we attempted to speak to the individual who had filled out application for the lottery home. In the case a child had applied for the home, enumerators were instructed to speak to the household's main decision-maker. Ultimately, 78% of respondents had reportedly completed the applications themselves.

To recap, here is a timeline of the events relevant to the study:

May 25, 2012: Winners of 2012 lottery announced

May 2013: Winners of 2012 lottery begin taking possession

June 25, 2014: Winners of 2014 lottery announced

June 2015: Winners of 2014 lottery begin taking possession

September 15, 2017-May 15, 2018: Surveys

2.1 The sample

The data collection process yielded a sample of 834, with 413 (82.6% contact rate) of the surveyed households in the control condition and 421 (84.2% contact rate) households in the treatment condition. Full information on the number of households contacted in each stratum along with reasons for attrition can be found in Table SI.7. I do not see strong evidence of differential rates of contact for control and treated units; the p-value for the difference in proportion contacted is 0.8.

Balance tests for fixed or baseline characteristics among the contacted sample can be found in Table 1. Winners and non-winners appear to be similar based on a number of fixed observable covariates, limiting concerns of corruption in the lottery or differential selection into the treatment groups. Both treatment groups have an equal proportion

²A scheduled tribe member is part of an officially designated group of socially and economically disadvantaged people in India.

of those belonging to the *Maratha* caste group, a dominant group in Mumbai and Maharashtra more generally. This is among the most politically powerful caste groups in Mumbai, and its members are therefore particularly likely to call in a favor and "win" the lottery. Additional balance tests are available in Appendix E.

I describe the sample as middle-class. EWS and LIG group membership is defined by annual income caps of Rs.192,000 and Rs.480,000, placing the highest earners in each category in the 47th and 94th percentile of annual income in Mumbai as collected by IHDS-II.³ Furthermore, with about 10 years of education on average, the sample is at about the 61st percentile for years of education in Mumbai based on the India Human Development Survey- II (IHDS-II), which was conducted in 2010 (Desai and Vanneman 2016). Most live in dwellings with permanent floors (97%) and roofs (78%). Yet there is room for improvement; only 60% have their own toilets, and 75% have their own private taps. Shared taps and toilets are common features in the Mumbai *chawls*, or cheap apartments built for laborers, where many control group members live.

3 Estimation

I follow my pre-analysis plan⁴ and estimate the treatment effect β , on i households or individuals across the pooled sample of lotteries (Equation 1). Y_i is the outcome, T_i is an indicator for treatment (winning the lottery), and ϵ_i is an error term.⁵ Given that randomization happened within strata, I include a set of centered dummies, $S_1 \dots S_l$ for each. Following Lin (2013), I allow for heterogeneous effects within the strata by interacting the centered stratum dummies with the treatment indicator:

$$Y_i = \alpha + \beta T_i + \sum_1^l \omega_l S_l + \sum_1^l \eta_l (T \times S_l) + \epsilon_i \quad (1)$$

I label households as "treated" if they win the lottery in the specific year for which they appear in the sample. While this study potentially suffers from two-sided noncompliance (8% of treated units did not purchase homes), I simply conduct an intent-to-treat (ITT) analysis. β can thus be interpreted as a weighted average of stratum-specific intent-to-treat effects. Given that randomization occurred at the household level, I compute standard errors using a heteroskedasticity-robust estimator (HC2) for standard errors (MacKinnon and White 1985). I make Benjamini-Hochberg (1995) corrections for the false discovery rate within "families" of outcomes.

For education and employment results, I analyze individual-level data based on a household roster to estimate individual-level treatment effects. This dataset drops all

³As in many cities with high levels of inequality, the income distribution in Mumbai is left skewed with a long right tail.

⁴Deviations from the pre-analysis plan are explained in appendix A.

⁵Covariate adjusted results using fixed characteristics yield similar standard errors (Table SI.12).

individuals born *after* the household-relevant lottery was conducted to mitigate post-treatment bias arising due to treatment effects on child-bearing.⁶ Regressions here include stratum-centered dummies, covariates, and errors clustered at the household level.

I estimate average treatment effects across owner-occupiers and landlords because the control group's counterfactual choice remain unknown. Predictors of moving can be found in Table A.12. The study is not powered to detect heterogeneous effects at the household level.

4 Results

Table 2, Panels A-B, present results for education and employment related variables measured at the individual and household levels. Household-level employment effects refer to the household's main earner. Household-level educational investment effects refer to whether an outcome holds for *any* of the sons or daughters; families with no children take on a value of "0". I find that positive effects on education and employment are particularly large among older youth. I find evidence to suggest that gains in employment arise from a higher likelihood of having full-time or even salaried work.

4.1 Education

First, I estimate that the average number of years of education among winners is about 0.60 years greater than the mean of 10 years for non-winners. The distribution of the individual years of education for those living in winning and non-winning households shows a multimodal distribution of educational attainment, with means at 0, 10, 12, 15 years of education (Figure 1). The modes at 0, 12, and 15 years represent barriers to beginning schooling, beginning post-secondary schooling, and beginning graduate schooling respectively.⁷ The mode at 10 years possibly reflects the barriers to continuing education past 10th grade that are particularly high in India. Students sit for national or state board exams (depending on their school's affiliation) at the end of grade 10. Only if they pass this exam can students advance past grade 10. Those who pass receive a Secondary School Certificate, which is in itself a certification that is often required for certain jobs. Stopping one's education at grade 10 can be the result of a failure to pass the exam or the decision to discontinue schooling; continuation of school after grade 10 should increase rates of both secondary school completion *and* rates of post-secondary school education.

Winning the housing lottery increases the likelihood of overcoming each of these barriers (Table 3).⁸ Belonging to a household that has won the lottery increases the

⁶Note that winning the lottery has no effect on childbirth. Results available upon request.

⁷In India, a bachelor's degree typically takes 3 years to complete.

⁸This analysis was not preregistered and can be considered exploratory.

likelihood of moving past grades 10 and 12 and completing post-secondary education by 7.1 pp (14%), 5.6 pp (17.6%), and 4.1 pp (15.9%), respectively. It does not have an effect on actually beginning one's education.

Effects are larger among youth subgroups. Figure 1 shows that effects are concentrated among individuals who were of secondary and post-secondary school age after the lottery, rather than younger or older individuals. The three panels for secondary and post-secondary school age children show a rightward shift in the distribution for educational attainment. In a regression analysis, I include an interaction with the treatment indicator and an indicator for whether each individual turned 6, 16, 18, and 21 in between being surveyed and the applicable lottery year (Table 3). These years were chosen with the assumption that most individuals complete 6, 16, 18, and 21 years of age in their first, tenth, twelfth, and fifteenth years of education.⁹

The program's effect on completing grades ten and college is stronger among those who turned 16 and 21 after winning, respectively (Table 3). I estimate a roughly 15 percentage point (45%) increase in the likelihood of completing grade 10 among members of winning households who turned 16 after the lottery. I estimate a 15 percentage point (43%) increase in the likelihood of completing post-secondary education among members of winning households who turned 21 after the lottery. Imbalance in the age distribution for the relevant cohorts cannot account for these results. Table 1 shows that winners are slightly older than non-winners. This difference appears to be concentrated among older individuals, but is not statistically significant for any age group (Table A.7).

At the household level, I estimate that parents of winners are about 8.4 pp (74%) less likely to report sending their children to public school than parents of non-winners; in India, asking if children attend a public ("government") school is a more common way to draw the distinction between public and private schools than by asking if children attend private schools. This is likely due to the extreme heterogeneity in the types of non-government providers of education in India; a private school can refer to a prestigious international school, or it could refer to a school run out of a private home (Harma 2011). In spite of this heterogeneity, public schools are free and tend to be of significantly lower quality than their private counterparts in urban India (Kingdon 1996; De and Drèze 1999). These results are not accompanied by any measurable effects on sending children to after-school tuition, a common practice in India. Note that effects do not differ for sons and daughters, but this may be due to social desirability bias in responses.

⁹I measure age at the time of the survey, so age at the time of the lottery (age_l) could take on two values, age_l and age_l , depending on the timing of the respondents' birthdays. For simplicity, tables in the text present results assuming all individuals were age_l at the time of the lottery. Individuals are coded to have turned X years old ($Turned_X$) after the lottery if age_s is greater than or equal to X and age_l is less than X. Results using age_l are similar and presented in appendix F.

4.2 Employment

Table 2 shows that gains in educational attainment are accompanied by effects on individual employment. Individuals in winning households are 4.8 pp (11.5%) more likely to be employed than those living in non-winning households. Employment here means having worked one hour or more in the past week. This effect can further be broken down into a 7.5 percentage point (15.6%) positive effect on full-time work offset by a negative (but statistically insignificant) effect on part-time labor.¹⁰ As increases in employment are concentrated among the same cohort experiencing gains in education, the fact that they are better educated may help them secure full-time jobs for which there is likely greater competition or higher skills requirements than part-time labor. If the distinction between part-time and full-time labor is a rough proxy for wage and salaried labor, this breakdown in results complements positive estimates of household-level effects on the main earner being salaried or having a government job (Table 2). The "main" worker is defined as the family's highest earner.

As with the gains to education, these effects on employment are particularly large among older youth. Model 1 in 4 first shows that individuals become more likely to be employed as they become older; child labor is generally uncommon in this sample. Models 2-6 further conduct an exploratory analysis to see whether effects are concentrated among among the same groups that benefitted from gains in educational attainment. Among the age cohort that turned 21 or had the opportunity to pass through college since the lottery, the likelihood of being employed increases by 19.9 pp, or about 45% (Model 6). The likelihood of full-time employment among this subgroup increases by 21.7 pp, or 48.8% . This increase is in line with the finding that belonging to a winning family increases the likelihood of this age cohort completing college; children are more likely to complete their education and, in turn, more likely to find jobs.

5 Mechanisms

The results could be driven by owner-occupiers who relocate to a new neighborhood and experience better labor market and educational opportunities as a result. I explore this possibility by estimating effects on household municipal ward and postal-code characteristics. The intervention leads winners to live, on average, in municipal wards with 0.34 standard deviation lower rates of literacy and 0.33 standard deviation lower rates of full-time employment than non-winners (Table 2, Panels C-D). The lottery causes households to live in postal codes with a lower percentage of senior secondary schools (those that offer education through grade 12), schools that are 0.22 standard deviations less likely to be taught in English (a proxy for quality), and 0.38 standard deviations less likely to have offices for headmasters (a proxy for school size). Unlike MTO, the intervention provides

¹⁰In India, most full-time employees work either 5 or 6 days a week.

households with the opportunity to move to generally poorer neighborhoods. Relocation and exposure to better educational contexts or labor markets seem to be unlikely explanations for the positive education and employment results.

5.1 Budget constraints

The intervention might increase educational attainment by shifting out short-term budget constraints. The decrease in sending children to public (as opposed to private) school, for example, is evidence that households are spending more on education (Table 2, Panel B). Haushofer and Shapiro (2018) find unconditional income transfers increase educational spending and improve educational outcomes, and there exists a close relationship between consumption and educational attainment in urban India (Figure SI.4).

The intervention is an illiquid increase in households' permanent income because of the rule prohibiting sale during the time of the study. But short-term budget constraints may shift outwards for a few reasons. Landlords receive rental income. See appendix D for positive but imprecisely measured effects on reported monthly income. Households may be able to borrow against the equity accumulated in the home. Winners report being 5 pp more likely to ask commercial banks for loans in cases of emergency, reflecting perhaps some ability to borrow against the accumulate equity or better knowledge about financial institutions, but this effect is no longer statistically significant after accounting for multiple testing (Table SI.4).

5.2 Housing quality

The in-kind housing transfer of housing benefits among owner-occupiers is visible in the lower incidence of makeshift roofs and shared taps and toilets (Table 2, Panel E). These benefits may decrease everyday stresses, such as queuing up for water or maintaining one's roof. This reduction in stress, as discussed below, may give beneficiaries the mental bandwidth to focus on the future.

5.3 Time horizons

The improved housing quality, shift in short-term budget constraints, and the illiquid subsidy's impact on permanent income might by decreasing present bias. Past research has found that income or wealth shocks can decrease stress and therefore increase time horizons (e.g. Baird et al. 2013; Fernald et al. 2008; Haushofer and Fehr 2014; Haushofer and Shapiro 2016; Ozer et al. 2011; Ssewamala et al. 2009). Decreased present bias may lead to greater investment in items with longer-term payouts, such as education. Behavioral deficits, particularly present bias, have been found to explain suboptimal choices in education (Lavecchia et al. 2016).

Table 2, Panel F shows that the intervention increased winners' optimism about their financial futures. Optimism may reflect lower levels of economic or financial stress, which could also affect economic choice (Mani et al. 2013). Winners are 19 pp more likely than non-winners to claim to be "happy" with the financial situation of the household. Winners appear to believe they will pass on their good fortune to their children, as they are roughly 12 pp more likely than non-winners to say "yes" when asked if their children will have better lives than them. Finally, they are about 8 pp more likely than non-winners to respond that they "would never leave" and roughly 7 pp less likely to say they are "unsure" when asked if would ever consider relocating from Mumbai, indicating the intervention increased time horizons and decreased uncertainty about the future.

Evidence of decreased present-bias can be found in effects on household healthcare consumption (Table 2, Panel H). Control and treatment households experience no detectable difference in the incidence of illnesses or severe illnesses in the month prior to the survey. Nevertheless, treatment households are more likely to report having visited some type of healthcare provider in the past month, particularly family members and non-medically certified individuals such as homeopathic doctors that are common throughout India (Das and Hammer 2014). These healthcare providers are cheap, or in the case of family members, may even be free. Thus changes in this reported behavior may reflect changes in present bias rather than simply shifts in budget constraints.

5.4 Returns to education

Finally, the intervention may increase the returns to education. This could be because as individuals become wealthier, they may derive greater utility from non-monetary gains to education that are higher on Maslow's (1943) hierarchy of needs, such as self-actualization. It could also be due to more individualistic or market-based values, which would increase the desire to invest in one's skills and future. When asked if they believe that effort leads to much more/more/less/much less success, winners are 7.3 pp more likely than non-winners to respond saying "more" or "much more." When asked about how they make important life decisions, such as those about careers, marriages, or education, winners are 7.4 pp more likely to say "I make choices myself" rather than reporting taking guidance from traditional values, families, or neighborhoods.

5.5 Mechanisms behind effects on employment

I observe an increase in full-time employment among precisely the same group of individuals exhibiting gains in educational attainment, namely older youth. If the gains in education are causing the effects on employment, then it would appear that increases in *post-secondary* education are affecting employment outcomes. These findings are surprising given the stylized fact that youth unemployment in India is highest among post-

graduates.¹¹

But the relationship between educational attainment and employment is one that will vary greatly across context and has yet to be fully explored in urban India, let alone Mumbai. This study was conducted from mid-2017 to early 2018, a period which saw a spike in unemployment rates among urban youth, particularly in the informal sector.¹² Some attribute this spike to a new national goods and services tax and a surprise "demonetization" initiative, which effectively cancelled a large portion of the national currency literally overnight. Returns to a college degree may have been higher during this period that was relatively favorable to formal businesses that did not rely as heavily on cash. This conjecture is supported by the results on full-time and salaried work.

6 Conclusion

I propose that the main function of a subsidized housing program in Mumbai, India is the transfer of wealth to eligible middle-class households. Through a survey of winners and non-winners of multiple housing lotteries that occurred in 2012 and 2014, I find this wealth transfer increases educational attainment and employment rates, particularly among youth. These effects occur even though winners tend to live in areas with lower levels of employment and worse schools, and are accompanied by changes in winners' attitudes about the future.

This is a short-term study. I find effects only on older youth, presumably because others are too young to display effects on educational attainment and employment outcomes. It is also too soon to measure effects on the children of youth themselves. As a result, a long-run study of this program will be essential to understanding the full potential of this program to change family trajectories. Furthermore, several important parameters, such as the cost, subsidy size, and characteristics of the beneficiary population will vary across instances of the intervention, highlighting the importance of future studies of other programs.

¹¹<https://www.businessinsider.in/indias-unemployment-rate-stands-at-13-2-among-graduates-and-post-graduates-cmie/articleshow/68517075.cms>

¹²<https://www.bbc.com/news/world-asia-india-47068223>

Figure 1: Distribution of individual years of education for the full sample and different age cohorts drawn using a Gaussian kernel.

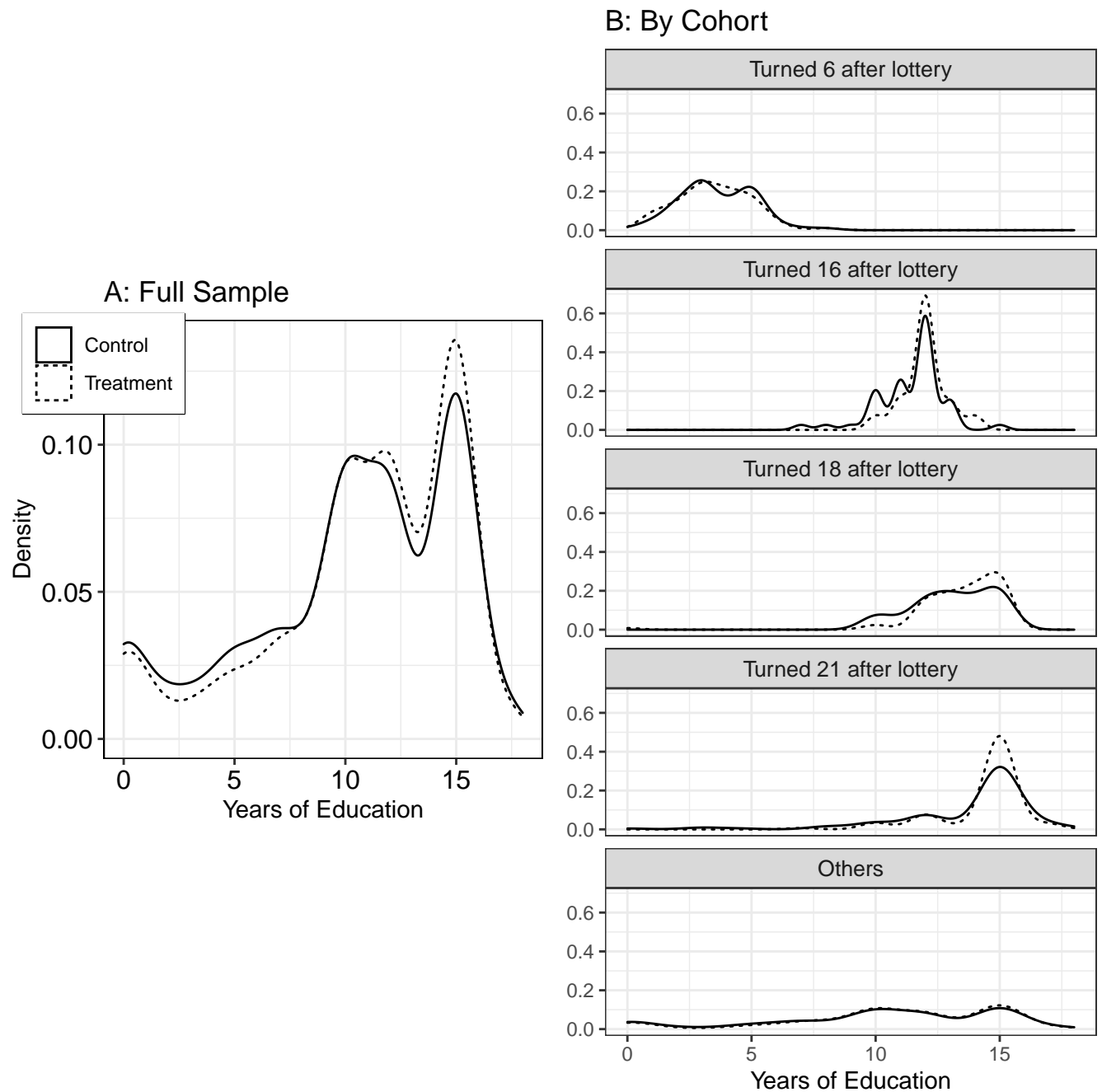


Table 1: Balance tests on household and individual characteristics as measured through a survey.

Variable	Control ¹	Treatment ²	sd ³	Pr(> t)
A: Household characteristics N=834				
OBC ⁴	0.150	-0.021	0.035	0.543
SC/ST ⁵	0.080	-0.018	0.026	0.499
<i>Maratha</i> ⁶	0.290	0.018	0.045	0.690
Muslim	0.090	0.090	0.029	0.852
Makeshift floor	0.031	0.028	0.019	0.136
Makeshift roof	0.039	0.001	0.018	0.945
Originally from Mumbai	0.810	0.062	0.039	0.114
From the same ward as the apartment	0.097	0.023	0.030	0.454
B: Individual characteristics N=3,170				
Age	36.000	0.095	0.574	0.869
Female	0.500	0.000	0.011	0.998
OBC	0.150	-0.022	0.023	0.340
SC/ST	0.110	-0.029	0.021	0.165
<i>Maratha</i>	0.270	0.024	0.032	0.457
Muslim	0.089	0.015	0.021	0.477
Makeshift floor	0.013	0.030	0.023	0.188
Makeshift roof	0.026	0.001	0.023	0.979
Originally from Mumbai	0.770	0.051	0.026	0.052
From the same ward as the apartment	0.095	0.030	0.021	0.154

¹ Intercept in an OLS regression of variable on treatment indicator. Each regression includes an interaction with the centered stratum-level indicator for randomization groups.

² Coefficient on variable in an OLS regression of each variable on treatment indicator.

³ HC2 errors, with errors clustered at the household level for individual results.

⁴ Other backward class caste group members. ⁵ Scheduled Caste/Scheduled Tribe, members of the lowest ranks of the caste system. ⁶ A dominant group in Mumbai and Maharashtra more generally.

Table 2: Treatment effects for main outcomes and outcomes related to mechanisms.

	Variable ¹	Control ²	Treatment ³	sd ⁴	Adjusted p ⁵
Main outcomes	A: Individual-level education and employment⁶				
	Years of education	10.000	0.610	0.230	0.018
	Working	0.460	0.044	0.026	0.120
	Working full-time	0.480	0.077	0.026	0.012
	Working part-time	0.092	-0.021	0.014	0.120
	B: HH-level education and employment⁷				
	Public school (sons)	0.095	-0.086	0.020	0.000
	Public school (daughters)	0.088	-0.089	0.018	0.000
	English medium school (sons)	0.280	0.022	0.046	0.700
	English medium school (daughters)	0.270	0.009	0.045	0.840
	After-school tuition (sons)	0.220	-0.037	0.039	0.520
	After-school tuition (daughters)	0.220	-0.031	0.040	0.560
	Main earner salaried	0.780	0.079	0.039	0.130
	Main earner govt. job	0.180	0.038	0.039	0.520
	Main earner formal sector job	0.096	0.053	0.034	0.260
Outcomes related to proposed mechanisms	C: Ward level neighborhood characteristics (control group SDs)^{7,8}				
	HH size	22.000	0.370	0.100	0.000
	Sex ratio	21.000	-0.160	0.100	0.170
	%Scheduled caste	2.200	0.024	0.086	0.780
	%Scheduled tribe	3.400	0.039	0.095	0.780
	%Literate	30.000	-0.370	0.110	0.002
	%Working	21.000	-0.380	0.110	0.002
	%Main workers	19.000	-0.350	0.110	0.002
	%Marginal workers	6.400	-0.093	0.093	0.430
	D: Postal code level school characteristics (control group SDs)^{7,9}				
	%Senior secondary schools	1.600	-0.210	0.091	0.064
	%public schools	2.300	0.100	0.090	0.390
	Mean # classrooms	3.900	-0.062	0.088	0.560
	Mean # permanent classrooms	3.900	-0.062	0.088	0.560
	% schools w/ office for headmaster	36.000	-0.400	0.100	0.000
	% schools with library	55.000	-0.110	0.088	0.390
	Mean # teachers w/ prof qualifications	3.300	0.012	0.091	0.900
	%English medium	3.100	-0.220	0.096	0.064
	E: Housing quality⁷				
	Makeshift floor	0.970	-0.008	0.017	0.650
	Makeshift roof	0.780	0.150	0.035	0.000
	Private tap	0.750	0.130	0.039	0.001
	Private toilet	0.600	0.250	0.043	0.000
	F: Future-looking attitudes⁷				
	Happy w/ financial situation	0.600	0.200	0.046	0.000
	Children will have better lives than them	0.560	0.120	0.048	0.022
	Would never leave Mumbai	0.770	0.087	0.039	0.032
	Unsure about leaving Mumbai	0.180	-0.073	0.036	0.042
	G: Individualistic attitudes⁷				
	Trusts others	0.740	-0.054	0.045	0.230
	Thinks effort leads to greater success	0.810	0.072	0.035	0.096
	Claims to make own decisions	0.130	0.067	0.036	0.096
	H: Healthcare⁷				
	N illnesses in the last month	0.730	0.006	0.250	0.980
	Homeopathic doctor	0.036	0.052	0.024	0.064
	Medically certified doctor	0.950	0.015	0.020	0.570
	Family member's advice	0.004	0.037	0.014	0.044

¹ Variable definitions for survey-based outcomes available in Table SI.1. ² Estimate for α in Equation 1.

³ Estimate for β in Equation 1. ⁴ HC2 errors, with errors clustered at the household level for individual results.

⁵ Benjamini-Hochberg adjusted p-values. ⁶ N=3,170 ⁷ N=834 ⁸ Data from 2011 Indian Census.

⁹ Postal-code level data for 2017 from the Ministry of Human Resource Development, Government of India.

Table 3: Regressions of individual completion of various years of education on the treatment indicator.

	<i>Dependent variable:</i>								
	Years of education	I(>0 years)		I(>10 years)		I(>12 years)		I(≥15 years)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
T	0.440 (0.160)	0.009 (0.009)	0.010 (0.009)	0.071 (0.018)	0.056 (0.019)	0.056 (0.019)	0.039 (0.021)	0.041 (0.017)	0.036 (0.017)
$Turned_6^1$	-7.000 (0.180)		0.057 (0.017)						
$Turned_{16}$	1.200 (0.150)				0.333 (0.042)				
$Turned_{18}$	1.900 (0.180)						0.387 (0.051)		
$Turned_{21}$	3.300 (0.190)								0.351 (0.050)
$T \times Turned_6$			-0.017 (0.018)						
$T \times Turned_{16}$					0.093 (0.050)				
$T \times Turned_{18}$							0.106 (0.067)		
$T \times Turned_{21}$									0.114 (0.068)
Constant	10.000 (0.120)	0.935 (0.006)	0.932 (0.007)	0.505 (0.013)	0.487 (0.013)	0.318 (0.013)	0.298 (0.014)	0.258 (0.012)	0.234 (0.012)
Observations	3,170	3,170	3,170	3,170	3,170	3,170	3,170	3,170	3,170
R ²	0.210	0.047	0.049	0.053	0.088	0.058	0.109	0.058	0.107
Adjusted R ²	0.180	0.006	0.007	0.012	0.048	0.017	0.069	0.018	0.068

All models include standard errors clustered at the household level and the treatment indicator interacted with mean-centered stratum dummies. ¹" $Turned_X$ " is an indicator for whether the individual completed X years of age in between the lottery and being surveyed, using age_t , or each individual's oldest possible age.

Table 4: Regressions of individual employment on the treatment indicator.

	<i>Dependent variable:</i>													
	Employed							Employed (full-time)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
T	0.042 (0.014)	0.038 (0.015)	0.051 (0.016)	0.045 (0.016)	0.035 (0.016)	0.058 (0.029)	0.082 (0.019)	0.077 (0.020)	0.069 (0.019)	0.082 (0.035)	-0.025 (0.012)	-0.020 (0.013)	-0.021 (0.013)	-0.021 (0.027)
Turned ¹	-0.016 (0.012)	-0.470 (0.014)												
Turned16	0.001 (0.025)		-0.450 (0.027)				-0.380 (0.037)				0.093 (0.043)			
Turned18	0.140 (0.035)			-0.220 (0.052)				-0.170 (0.053)				0.063 (0.039)		
Turned21	0.640 (0.036)				0.160 (0.045)			0.180 (0.044)				-0.008 (0.028)		
Older ²	0.570 (0.013)					0.410 (0.024)				0.330 (0.026)			-0.098 (0.022)	
TXTurned6		-0.023 (0.021)												
TXTurned16			0.058 (0.041)				0.051 (0.051)				0.017 (0.055)			
TXTurned18				0.065 (0.071)				0.049 (0.074)				-0.036 (0.049)		
TXTurned21					0.160 (0.068)				0.150 (0.062)				-0.010 (0.040)	
TXOlder						-0.021 (0.035)				-0.009 (0.038)				-0.0003 (0.028)
Constant	0.005 (0.012)	0.470 (0.011)	0.470 (0.011)	0.460 (0.011)	0.440 (0.011)	0.170 (0.020)	0.480 (0.014)	0.470 (0.014)	0.450 (0.014)	0.230 (0.024)	0.082 (0.009)	0.083 (0.009)	0.087 (0.009)	0.150 (0.020)
Observations	3,170	3,170	3,170	3,170	3,170	3,170	3,170	3,170	3,170	3,170	3,170	3,170	3,170	3,170
R ²	0.250	0.072	0.074	0.042	0.049	0.160	0.084	0.059	0.071	0.140	0.068	0.061	0.059	0.086
Adjusted R ²	0.220	0.031	0.034	0.0001	0.007	0.130	0.044	0.018	0.030	0.100	0.026	0.019	0.018	0.046

All models include standard errors clustered at the household level and the treatment indicator interacted with mean-centered stratum dummies.

¹ "Turned_X" is an indicator for whether the individual completed X years of age in between the lottery and being surveyed, using age_i , or each individual's oldest possible age. ² "Older" is an indicator for an individual being older than 21 at the time of the lottery.

References

- Araujo, M. C., Bosch, M., & Schady, N. (2016). *Can Cash Transfers Help Households Escape an Inter-Generational Poverty Trap?* Working Paper 22670, National Bureau of Economic Research.
- Baird, S., De Hoop, J., & Ozler, B. (2013). Income shocks and adolescent mental health. *Journal of Human Resources*, 48(2), 370–403.
- Banerjee, A., Duflo, E., Goldberg, N., Karlan, D., Osei, R., Parienté, W., Shapiro, J., Thuysbaert, B., & Udry, C. (2015). A multifaceted program causes lasting progress for the very poor: Evidence from six countries. *Science*, 348(6236), 1260–1279.
- Barker, D. & Miller, E. (2009). Homeownership and Child Welfare. *Real Estate Economics*, 37(2), 279–303.
- Barnhardt, S., Field, E., & Pande, R. (2017). Moving to Opportunity or Isolation? Network Effects of a Randomized Housing Lottery in Urban India. *American Economic Journal: Applied Economics*, 9(1), 1–32.
- Becker, G. S. (1964). *Human capital: A theoretical and empirical analysis, with special reference to education*. University of Chicago press.
- Benjamini, Y. & Hochberg, Y. (1995). Controlling the false discovery rate: a practical and powerful approach to multiple testing. *Journal of the Royal statistical society: series B (Methodological)*, 57(1), 289–300.
- Bleakley, H. & Ferrie, J. (2016). Shocking Behavior: Random Wealth in Antebellum Georgia and Human Capital Across Generations. *The Quarterly Journal of Economics*, 131(3), 1455–1495.
- Cairney, J. (2005). Housing Tenure and Psychological Well-Being During Adolescence. *Environment and Behavior*, 37(4), 552–564.
- Cesarini, D., Lindqvist, E., Ostling, R., & Wallace, B. (2016). Wealth, Health, and Child Development: Evidence from Administrative Data on Swedish Lottery Players. *The Quarterly Journal of Economics*, 131(2), 687–738.
- Das, J. & Hammer, J. (2014). Quality of Primary Care in Low-Income Countries: Facts and Economics. *Annual Review of Economics*, 6(1), 525–553.
- De, A. & Dreze, J. (1999). *Public Report on Basic Education in India*. New Delhi: Oxford University Press.
- Desai, S. & Vanneman, R. (2016). National Council of Applied Economic Research, New Delhi. India Human Development Survey (IHDS), 2005. ICPSR22626-v11. *Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor]*, (pp. 02–16).

- Dietz, R. D. & Haurin, D. R. (2003). The social and private micro-level consequences of homeownership. *Journal of Urban Economics*, 54(3), 401–450.
- Essen, J., Fogelman, K., & Head, J. (1978). Childhood Housing Experiences and School Attainment. *Child: Care, Health and Development*, 4(1), 41–58.
- Fernald, L. C., Hamad, R., Karlan, D., Ozer, E. J., & Zinman, J. (2008). Small individual loans and mental health: a randomized controlled trial among South African adults. *BMC Public Health*, 8(1), 409.
- Franklin, S. (2019). The demand for government housing: evidence from a lottery for 200,000 homes in Ethiopia.
- Franklin, S. (2020). Enabled to work: the impact of government housing on slum dwellers in south africa. *Journal of Urban Economics*, (pp. 103265).
- Glaeser, E. L. & Shapiro, J. M. (2003). The benefits of the home mortgage interest deduction. *Tax policy and the economy*, 17, 37–82.
- Green, R. K. & White, M. J. (1997). Measuring the Benefits of Homeowning: Effects on Children. *Journal of Urban Economics*, 41(3), 441–461.
- Harma, J. (2011). Low cost private schooling in India: Is it pro poor and equitable? *International Journal of Educational Development*, 31(4), 350–356.
- Haurin, D. R., Parcel, T. L., & Haurin, R. J. (2002). Does Homeownership Affect Child Outcomes? *Real Estate Economics*, 30(4), 635–666.
- Haushofer, J. & Fehr, E. (2014). On the psychology of poverty. *Science*, 344(6186), 862–867.
- Haushofer, J. & Shapiro, J. (2016). The Short-term Impact of Unconditional Cash Transfers to the Poor: Experimental Evidence from Kenya. *The Quarterly Journal of Economics*, 131(4), 1973–2042.
- Haushofer, J. & Shapiro, J. (2018). The long-term impact of unconditional cash transfers: experimental evidence from Kenya. *Busara Center for Behavioral Economics, Nairobi, Kenya*.
- Kingdon, G. (1996). The Quality and Efficiency of Private and Public Education: A Case-Study of Urban India. *Oxford Bulletin of Economics and Statistics*, 58(1), 57–82.
- Lavecchia, A. M., Liu, H., & Oreopoulos, P. (2016). Chapter 1 - Behavioral Economics of Education: Progress and Possibilities. In E. A. Hanushek, S. Machin, & L. Woessmann (Eds.), *Handbook of the Economics of Education*, volume 5 (pp. 1–74). Elsevier.
- Lin, W. (2013). Agnostic notes on regression adjustments to experimental data: Reexamining Freedman’s critique. *The Annals of Applied Statistics*, 7(1), 295–318.

- MacKinnon, J. G. & White, H. (1985). Some heteroskedasticity-consistent covariance matrix estimators with improved finite sample properties. *Journal of econometrics*, 29(3), 305–325.
- Mani, A., Mullainathan, S., Shafir, E., & Zhao, J. (2013). Poverty impedes cognitive function. *science*, 341(6149), 976–980.
- Maslow, A. H. (1943). A theory of human motivation. *Psychological review*, 50(4), 370.
- McIntosh, C. & Zeitlin, A. (2018). Benchmarking a child nutrition program against cash: experimental evidence from Rwanda. *San Diego: University of California*.
- Oliver, M. & Shapiro, T. (2013). *Black wealth/white wealth: A new perspective on racial inequality*. Routledge.
- Ozer, E. J., Fernald, L. C., Weber, A., Flynn, E. P., & VanderWeele, T. J. (2011). Does alleviating poverty affect mothers' depressive symptoms? A quasi-experimental investigation of Mexico's Oportunidades programme. *International Journal of Epidemiology*, 40(6), 1565–1576.
- Parker, S. W. & Vogl, T. (2018). *Do Conditional Cash Transfers Improve Economic Outcomes in the Next Generation? Evidence from Mexico*. Working Paper 24303, National Bureau of Economic Research. Series: Working Paper Series.
- Picarelli, N. (2019). There Is No Free House. *Journal of Urban Economics*, 111, 35–52.
- Richman, N. (1974). The Effects of Housing on Pre-school Children and Their Mothers. *Developmental Medicine & Child Neurology*, 16(1), 53–58.
- Ssewamala, F. M., Han, C.-K., & Neilands, T. B. (2009). Asset ownership and health and mental health functioning among AIDS-orphaned adolescents: Findings from a randomized clinical trial in rural Uganda. *Social Science & Medicine*, 69(2), 191–198.
- van Dijk, W. (2019). The Socio-Economic Consequences of Housing Assistance. *Working Paper*.