

The housing quality, income, and human capital effects of subsidized homes in urban India*

Tanu Kumar[†]

August 23, 2021

Forthcoming, Journal of Development Economics

Abstract

This paper measures the effects of a subsidized housing lottery in Mumbai, India wherein winners can choose to either live in or rent out the homes. After 3-5 years, winners experience better housing quality, greater reported income, and different attitudes when compared to non-winners. They also have higher education and employment rates, with effects concentrated among youth. This occurs even though winners live in neighborhoods with worse schools and lower employment rates than non-winners at the time of measurement. Overall, effects differ from those of other asset transfers and housing programs requiring relocation. Comparisons with existing studies indicate that the intervention shifts educational attainment and employment at least as much as conditional cash transfers that target these outcomes. The findings suggest that when in-kind transfers can be easily exchanged or sold, their benefits approach those of cash transfers.

Keywords: Housing, asset transfers, India, wealth, education, Mumbai

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

*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 A. Kumar (2020) uses the same research design but reports a different set of effects.

[†]The College of William and Mary. `tkumar[at]wm[dot]edu`

Introduction

Governments use many tools to make homeownership more affordable for citizens, including mortgage and home-price subsidies. One common policy is the subsidized sale of government-constructed homes to households. Such policies exist across countries including, but not limited to, India, Brazil, Uruguay, Nigeria, Kenya, Ethiopia, and South Africa. They are particularly common in India and are in every major city, including Delhi, Mumbai, Bengaluru, Kolkata, Chennai, Hyderabad, and Ahmedabad. What are their effects on household economic and social trajectories?

Existing studies on the effects of housing programs focus on housing or rental subsidies that require relocation, which means that the location or formality the new housing can drive effects. Barnhardt et al. (2017), for example, find that beneficiaries of a program in Ahmedabad suffer from broken social networks arising from the distance of the housing from their neighborhoods of origin. Picarelli (2019) and van Dijk (2019) find that in programs in South Africa and the Netherlands, respectively, the distance of the housing from labor markets negatively affects household economic outcomes. As many of these programs target households living in informal settlements, others have also found that relocation to housing for which beneficiaries do not have to spend time securing property rights may drive effects (e.g. Field, 2007; Franklin, 2020).

This paper presents the effects of a program in Mumbai that does not require relocation. Households can rent out the homes, and they can also resell them after 10 years. A lower bound on the subsidy that households (typically comprised of four individuals) might realize in the future ranges between 10,000-56,000 USD, depending on the apartment location. Rental income for households that do not relocate is, on average, 50 USD per month net of mortgage. This figure can further be interpreted as a lower bound on the value of housing benefits. The program thus generates both short-term housing benefits or income along with large shifts in permanent income. Furthermore, because households are able to choose whether to relocate, the program design prevents negative characteristics of the housing location from undermining the economic gains of the subsidy.

As implemented in Mumbai, the program allocates housing through a randomized lottery system and thus allows the causal identification of its effects among applicants. I estimate its reduced-form effects on those winning homes in 2012 and 2014 using an original survey of 834 households. I find

effects on reported income, housing quality, educational attainment, employment, and attitudes.

First, I discuss effects on housing quality, income, and non-housing asset ownership. I estimate positive effects on housing quality. These effects are similar to those found by Barnhardt et al. (2017). Unlike Barnhardt et al. (2017) and Picarelli (2019), however, I also detect an increase in reported income among winners. This could occur for many reasons, including employment effects and income from rent for those who choose not to live in housing. I detect no effects on investment in assets beyond housing.

There are large gains to education. The intervention increases individual years of education by about half a year over a control group mean of 10 years. The treatment effect reflects an increase in winners' likelihood of completing secondary and post-secondary education. These full sample effects are concentrated 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 15 percentage points (pp). Among household members who turned 21 after the lottery, the intervention also increases the likelihood of completing post-secondary education by 15 pp. These effect sizes are similar to those estimated for Mexico's *Progresa* (Parker and Vogl, 2018), and larger than those estimated for Ecuador's *Bono de Desarrollo Humano* (Araujo et al., 2016). These are conditional cash transfers that a) explicitly incentivize schooling and b) to which children are exposed to for most of their schooling

The intervention further increases employment among individuals by 4.4 pp over a mean employment rate of 46% in the control group. The subgroups among which I observe large education gains also have better employment outcomes, suggesting that the education gains drive employment gains. Overall average effect sizes are comparable to those that Parker and Vogl (2018) estimate for *Progresa*.

I also estimate effects on present bias and attitudes. Winners report feeling happier about their financial situations, expect better lives for their children, are more likely to plan to stay in the city permanently, and have slightly more "individualistic" attitudes. These changes might be driven by the change in households' short-term and long-term economic trajectories, as income and wealth transfers can ease cognitive and behavioral constraints. For example, recent work (e.g. Mani et

al. 2013; Haushofer and Fehr 2014) has found that the insecurity created by poverty can make it difficult to focus on long-term goals and lead to short-sighted behavior.

I next assess whether these effects are driven by opportunities in the apartment neighborhoods or changes to housing formality, two of the dominant mechanisms in the literature on subsidized housing programs. Winners on average live in neighborhoods with poorer school quality and lower rates of literacy and employment than non-winners, suggesting effects are not driven by relocating to areas with better employment and educational opportunities. Effects may also be driven by changes in housing formality, wherein households reallocate time from protecting property rights to working outside the home (Franklin, 2020). Yet treatment effects are smaller or even negative among those living in informal housing at baseline. This is likely because as predictors of moving show, winners living in informal housing are more likely to relocate to the worse neighborhoods. The heterogeneous effect further suggests that moving from formal to informal housing is unlikely to explain the overall average treatment effects.

There are many other possible mechanisms driving the findings. The multiple ways in which beneficiaries can consume the transfer underscore just how many mechanisms might be at work. They can choose any combination of three payout structures: 1) a stream of in-kind benefits for those who choose to live in the subsidized home; 2) cash benefits among those who choose to rent it out; or 3) lump-sum transfers through the eventual resale of the home. As a result, mechanisms could be related to shifts in short-term budget constraints, consumption smoothing in response to long-term shifts in permanent income, and cognitive or behavioral effects related to both the short- and long-term income shifts.

This paper is among the first of a housing program that does not require relocation, which may partially account for differences in findings from previous housing studies. It contributes to debates on cash vs. in-kind transfers (e.g. Thurow, 1974; Currie and Gahvari, 2008; Skoufias et al., 2008; Cunha, 2014; Gangopadhyay et al., 2015) by providing empirical evidence that in-kind transfers can work like cash transfers when markets for rental or resale work well. And indeed, this in-kind transfer shifts many outcomes typically of interest in studies of cash transfer policies. Moreover, the educational attainment and employment effects are at least as large as those of conditional cash

transfers that specifically target these outcomes. This comparison may not provide a full picture of the welfare gains this program generates, as general transfers tend to affect the same outcomes as those targeted by conditional programs, but at lower rates because households are spending in other areas as well (McIntosh and Zeitlin, 2018).

A key difference between this transfer and cash transfers previously studied is its size, particularly its potential size in the future. 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 potential wealth gains from the MHADA lottery are at least 10 to 55 times the magnitude of these transfers.

Yet these large transfers incur few direct costs on implementing governments because the sale price of the homes covers construction and marketing costs (Madan, 2016). Land-use laws may further limit the theoretically high opportunity cost of building subsidized homes on urban land. In the program studied here, for example, housing was constructed on land earmarked for social welfare purposes. An in-kind transfer of this size is possible because most of its (eventual) value derives from the opportunity it creates to participate in the real estate market.

Overall, this paper highlights the importance of studying this common policy that may facilitate asset accumulation and fundamentally change family trajectories. As these policies tend to be oversubscribed, they are often implemented through lottery systems, particularly in India. This feature should facilitate causally-identified future studies on long-term effects, mechanisms, and the effects of important program parameters.

The program

Across India, state-level housing development boards have spearheaded programs that sell, rather than rent, subsidized units to eligible households in every major city. I study the effects of one such program implemented by the Maharashtra Housing and Area Development Authority (MHADA) in Mumbai. 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 7,500 USD/year. Beneficiaries 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 1. 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 are permitted to choose the building for which they submit an application.

In 2012 and 2014, the EWS group could purchase a 269 square foot apartment for about 23,500 USD, while the LIG group could purchase a 403 square foot apartment for about 42,000 USD. Table 1 shows that these prices are 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 lies between about 10,000 USD and 56,000 USD.¹

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 roughly 50 USD. Households do not pay taxes on their dwelling for five years after possession.

Beneficiaries are selected through a randomized lottery. In response to public scrutiny over the selection process and concerns about corruption, the lottery is conducted using a protected computerized process that was implemented in 2010. Applicants also apply with their Permanent Account Numbers (PAN), which are linked to their bank accounts and allow the verification of income thresholds.² The winning sample is stratified by caste and occupation group (Table SI.2), as each lottery has quotas for these groups within which random selection occurs.

¹These 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.

²A PAN is equivalent to a taxpayer identification number.

Data collection

I estimate treatment effects on all outcomes using in-person household surveys of a sample of both winning (treatment) and non-winning (control) households. All winners from the EWS and LIG lotteries occurring in 2012 or 2014 were included in the sampling frame. As there were roughly 1,000 applicants for each apartment, I surveyed 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.³

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. As I dropped households using baseline addresses, I expect this procedure to be independent of treatment assignment. Indeed, in the sample remaining after mapping, I see similar proportions of winners and applicants in each caste/occupation category, lottery income category, and apartment building (Table SI.3). 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 SI.4 shows that there are relatively fewer Scheduled Tribe members and more General Population (i.e. Forward Castes) members in the mapped sample than in the full sample provided by MHADA.

From the mapped sample, I randomly selected 500 households from each treatment condition to survey. From September 2017-May 2018, I worked with a Mumbai-based organization to contact the households and conduct surveys. 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. Among winners, owner-occupiers were approached at the lottery apartments; the others were approached at the addresses listed on the application using the procedure devel-

³Applicants could apply for multiple lotteries at a time. 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 household was drawn more than once.

oped for non-winners. In all cases, we attempted to speak to the individual who had filled out the application for the lottery home (78% of respondents).⁴

The sample

This 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) in the treatment condition. The p-value for the difference in proportion contacted is 0.8. Full information on the number of households contacted in each stratum along with reasons for attrition can be found in Table SI.5.

Balance tests for fixed or baseline characteristics among the contacted sample can be found in Table 2. Winners and non-winners are similar across many fixed observable covariates, limiting concerns of corruption in the lottery or differential attrition across the treatment groups. Both treatment groups have an equal proportion of those belonging to the *Maratha* caste group, a dominant group in Mumbai and Maharashtra more generally. There is also balance on the date on which interviews were conducted. Additional balance tests are available in Appendix E.

EWS and LIG group membership places the highest earners in each category in the 47th and 94th percentile of annual income in Mumbai as reported in the India Human Development Survey-II (2016). With about 10 years of education on average, the sample is at the 61st percentile for years of education in Mumbai. Most live in dwellings with permanent floors (94%) 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 originally built for laborers, where many control group members live.

Estimation

I follow my pre-analysis plan 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

⁴In case a child had filled out the application, enumerators were instructed to speak to the household's main decision-maker.

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

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:

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

Households are “treated” if they win the lottery in the specific year for which they appear in the sample. While 8% of treated units did not purchase homes, I simply conduct an intent-to-treat (ITT) analysis. β can 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 use data from a household roster to estimate individual-level treatment effects. Regressions here include stratum-centered dummies and errors clustered at the household level. Winning the lottery has no measurable effect on household size.⁶ Results should, in any case, not be affected by changes in household composition because I drop all individuals born after the household-relevant lottery was conducted.

Results

Housing quality

Panel A first presents results for housing quality at the time of the survey. Most control group members have permanent or load-bearing roofs (78%), private taps (75%), and private toilets (60%). Nevertheless, I observe positive treatment effects for these variables, likely driven by those who relocate from poor quality housing. These effects are similar to the effects found for a relocation-based program in Ahmedabad (Barnhardt et al., 2017). They are also comparable to the effects of unconditional cash transfers on housing construction quality, particularly metal roof construction in rural Kenya (Haushofer and Shapiro, 2016).

⁶The control mean is 4, with a treatment effect of -0.19 (standard error of 0.16).

Income and assets

Panel B next presents effects on monthly income and asset ownership. For income, most respondents were unable to provide numbers for monthly earnings, but provided ranges instead. Enumerators thus placed respondents into income bins. Panel B shows treatment effects for being placed in a given income bin or higher. The results show a clear rightward shift in the income distribution for treated households. Treated households are 14, 19, and 20 pp more likely than non-treated households to earn over 10,000 INR (about 156 USD), 15,000 INR (about 234 USD), and 20,000 INR (about 312 USD) per month, respectively. Franklin (2020) similarly finds large and positive earnings effects of a South African government housing program on household earnings.

These effects could be driven by employment effects (see below) or the fact that landlords (50% of winners) receive rental income of about 3,250 INR (about 50 USD) net of mortgage. Households may also 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, possibly reflecting some ability to borrow against the accumulated equity or better knowledge about financial institutions, but this effect is no longer statistically significant after accounting for multiple testing (Table 3, Panel C).

I observe no measurable treatment effects for non-housing asset ownership (Panel B). Disaggregated effects on ownership of each of the 14 assets are available in Table SI.9.

Overall, it appears that treated households enjoy more monthly income than non-treated households, but are not investing this income into non-housing assets. This is at odds with other studies (e.g. Balboni et al. 2020) finding that cash or asset transfers ease constraints on purchasing important assets. One interpretation is that households in the study do not face substantial constraints to purchasing assets.

Another is that housing is the important asset in which households are choosing to invest. Households do, after all, have to pay substantial mortgages. The median monthly mortgage payment is about 9500 INR (148 USD). This payment represents a substantial increase in housing costs as the median difference between previous rents and mortgage payments is about 6500 INR (101 USD).

Yet another interpretation is that when constraints are eased, households, particularly those living in urban settings, may choose alternative avenues for investment. The following section, for

example, considers investment in education.

Education

Panels D-E (Table 3) present results for education-related variables measured at the individual- and household-levels. 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”.

First, I estimate that the mean years of education among winners is 0.61 years greater than the mean of 10 years for non-winners. At what margin do these gains occur? 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 modes at 0, 10, 12, 15 years of education (Figure SI.2). 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 reflects the barriers to continuing education past 10th grade that are high in India. Here, 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 itself a certification required for 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.⁸ Belonging to a household that has won the lottery increases the 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 (Table 4). It does not have an effect on beginning one’s education.

Effect sizes are larger among the youth subgroups moving through each of these barriers to education between the time at which the lottery was held and when they were surveyed. 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 4). These years

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

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

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 larger among those who turned 16 and 21 after winning, respectively. I estimate a roughly 15 pp (18%) increase in the likelihood of completing grade 10 among members of winning households who turned 16 after the lottery. I estimate a 15 pp (26%) increase in the likelihood of completing 15 years or more (post-secondary education) among members of winning households who turned 21 after the lottery. I observe no treatment effects on educational attainment among those who were older than 22, or school age, at the time of the lottery.

The intervention also affects school choice. At the household level, I estimate that parents of winners are about 8.6 pp (90.5%) and 8.9 (100%) less likely to report sending their sons and daughters, respectively, to public school than parents of non-winners. Here, asking if children attend a public (“government”) school is a common way to draw the distinction between public and private schools, as a private school can refer to any non-government school, from a prestigious international school to one run out of a private home (Harma, 2011). Generally, public schools are free and tend to be of lower quality than their private counterparts in urban India (Kingdon, 1996; De and Drèze, 1999). These results are unaccompanied by 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.

Imbalance in the age distribution for relevant cohorts does not appear to drive results. Table 2 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 SI.7). Additionally, selection into the sample, namely a more educated cohort of older respondents in treated households, cannot account for results as there are no measurable treatment effects on educational attainment among the older age cohort (Table 4). Furthermore, this pattern of selection would imply that older treatment group respondents are less likely to have lower levels of education and

⁹I measure age at the time of the survey, so age at the time of the lottery (age_t) could take on two values, $age_{\bar{t}}$ and $age_{\underline{t}}$, depending on the timing of the respondents’ birthdays. Tables in text present results assuming all individuals were $age_{\bar{t}}$ at the time of the lottery. Results using $age_{\underline{t}}$ are similar and presented in appendix G.

more likely to have higher levels of education. Yet Table SI.8 shows that there are no treatment effects on primary education (4 or fewer years) or tertiary education (more than 12 years) among those who were older than 22 at the time of the lottery.

The findings here differ markedly from those of Barnhardt et al. (2017), who find no effects on children's schooling, possibly due to poor facilities in the areas to which households had relocated. Existing studies of cash transfer programs provide benchmarks for the effect sizes found in the present study. 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, and find that the receipt of transfers increased secondary school completion rates by 1-2 pp over a base of 75%. Even while the housing lottery provides a much larger wealth transfer in the long-run, the median net rental income of 50 USD generates a present-term monthly cash transfer of a similar magnitude measured across a similar time period. 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 between 9-60 USD a month increased completion rates by 10-15 pp among those exposed to the program when young. In 3-5 years, then, the housing lottery increased high school completion at rates similar to a program that a) explicitly incentivized 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 increased college completion, an effect unseen (but measured) in the case of *Progresa*.

These effects on education are particularly important as investment in education allows families to increase the size of the transfers and pass them onto the next generation (Becker, 1964). Educational attainment is also a useful proxy for social and economic status in developing countries wherein informal labor markets and joint household production functions can make it difficult to measure individual income (Asher et al., 2020).

Employment

Panels D-E in Table 3 show that gains in educational attainment are accompanied by effects on individual and household employment. Individuals in winning households are 4.4 pp (9.6%) 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 7.7 pp (16%) positive effect on full-time work offset by a negative (but imprecisely estimated) effect on part-time labor. Here, full-time work is defined as working either 5 or 6 days a week. If the distinction between part-time and full-time labor is a rough proxy for wage and salaried labor, this set of results complements positive estimates of household-level effects on the main earner being salaried or having a government job (Table 3).

As with the gains to education, these effects on employment are larger among older youth. Model 1 in Table 5 first shows that older individuals are more likely to be employed; child labor is generally uncommon in this sample. 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 19.5 pp, or about 32.5% (Model 6). The likelihood of full-time employment among this subgroup increases by 21.9 pp, or 34.8%. 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 full-time jobs. Table SI.8 shows that there are no detectable treatment effects among those older than 22 at the time of the lottery.

How do these results compare to those of other studies? Picarelli (2019) and Barnhardt et al. (2017) report no effects on employment, in large part because (as they argue) the relocation tied to the housing benefits breaks social networks and puts households at distance from labor markets. While Franklin (2020) does find that the program in Cape Town increases household earnings, he finds no effects on employment because, he argues, the main effects for earning are concentrated among women who were already employed at baseline.

For comparison with studies of cash transfers, there are no measurable effects on employment in Araujo, Bosch, and Shady's (2016) study of BDH in Ecuador. In Parker and Vogl (2018)'s study of *Progresa*, while men in the control group were already employed at high rates, the intervention increased employment among women by 7-11 pp, effects of a similar size to those seen here.

Attitudes

Table 3's Panel F shows that the intervention increased winners' optimism about their financial futures. Winners are 20 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 8.7 pp more likely than non-winners to respond that they "would never leave" and roughly 7.3 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.

The mechanism here may be that both the shift in short-term budget constraints and the illiquid subsidy's impact on permanent income decrease present bias. The large size of the long-term transfer in particular may affect present bias, especially when compared to smaller, near-term transfers. 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). Lower levels of economic or financial stress could, in turn, affect decision-making (Mani et al., 2013). These insights are important in understanding the longer-term effects of any income or asset transfer, as they can facilitate investment in items with long-term payouts, such as education (Lavecchia et al., 2016).

The intervention may also affect individualistic or market-based attitudes. When asked if they believe that effort leads to much more/more/less/much less success, winners are 7.2 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 6.7 pp more likely to say "I make choices myself" rather than reporting taking guidance from family members, traditional values, or neighborhoods (Panel G). A plausible explanation is one proposed by Di Tella et al. (2007), who find that a program providing squatters with land titles in Buenos Aires led them to have more individualistic and materialistic beliefs, or what they describe as beliefs favoring the workings of a free market. One mechanism they propose for these findings is that winners perceive lower benefits to collective action and working with others than non-winners,

particularly as they have greater individual wealth.

Location-based opportunities and formality

I next investigate the role of relocation in the effects estimated above, with a focus on whether the effects seem to be driven by opportunities near, and the formality of, program housing. Overall, the analysis suggests that the results on education and employment are not driven by those who relocate or those who living in informal housing at baseline. Households face important tradeoffs between household quality and employment and education opportunities.

Opportunities

The results could be driven by owner-occupiers (50% of winners) who relocate to a new neighborhood and experience better labor market and educational opportunities. Indeed, Chetty et al.'s 2016 study on the United States' Moving to Opportunity (MTO) program finds that moving to a higher opportunity neighborhood increases college attendance and earnings among children below 13 when they move, suggesting that neighborhoods can play an important role in economic well-being and human capital accumulation.

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 lower rates of literacy and lower rates of full-time employment than non-winners (Table 3, Panel H). It also causes households to live in postal codes with a lower percentage of senior secondary schools (those that offer education through grade 12), schools less likely to be taught in English (a proxy for quality), and less likely to have offices for headmasters (a proxy for school size). Unlike MTO, the intervention provides households with the opportunity to move to generally poorer neighborhoods. Relocation and exposure to better educational contexts or labor markets thus seem to be unlikely explanations for the positive education and employment results.

Why, then, do we see results that differ so much from other studies (e.g. Barnhard et al., 2017; van Dijk, 2019) that also entail apartments won in worse neighborhoods? One possibility is that households here can choose whether to relocate; if the costs are too high, then they will not do

so. Predictors of moving (Table SI.13) show that scheduled castes and tribes, or those typically at the bottom of the socio-economic ladder, are less likely to relocate than others. This pattern is complementary to Barnhardt. et al.'s (2017) finding that relocation is costly for the poor. Nevertheless, those with makeshift roofs (a proxy for informal housing) are more likely to move; intuitively, the benefits of relocating to permanent housing is greater for this group. These regressions also include a standardized variable Change in Employment Rate, which measures the difference in the employment rate between the neighborhood in which a household's lottery apartment is located and its baseline neighborhood. The mean of this variable is -0.018, in line with the negative effects on employment opportunities described above. Across multiple model specifications, a one standard deviation increase in the employment rate of the apartment neighborhood relative to the baseline neighborhood is associated with 17-20 pp increase in the likelihood that a household will relocate. Households may be choosing whether to relocate depending on their predictions about how doing so would affect their well-being. The results are unlikely to be driven by a subsample of households moving from worse baseline neighborhoods to better neighborhoods, as there is no positive relationship between the indicator variable $I(\text{Change in Employment Rate} > 0)$, and the likelihood of relocating (Table SI.14).

Housing formality

Another possible mechanism is that the formal nature of the new housing allows households to shift time use from protecting property rights to education or working outside the home (Field, 2007; Franklin, 2020). Yet Table SI.15 shows that the treatment effect on individual years of education is smaller, even negative, among households with a makeshift roof (a proxy for informal housing) at baseline, and households with a makeshift roof at baseline are less likely to have salaried main earners. These effects are in line with the finding that those with makeshift roofs are more likely to relocate to the worse neighborhoods than those without. Furthermore, there is no effect on employment among those who were past school age at the time of the lottery (Table SI.8); if the housing formality mechanism were driving results, I would be likely to see effects on employment among older household members as well.

Conclusion

This paper finds that a commonly implemented housing program generates effects on housing quality, income, educational attainment, employment, and attitudes in Mumbai, India. I argue that the main function of the intervention is to transfer a large subsidy and housing benefits or their cash value to beneficiaries. The flexibility with which households can choose to consume the transfer means that several mechanisms may drive effects at once across the population; future studies of the ubiquitous program may be able to more clearly identify them.

This is a short-term study. A long-run study of this program will be essential to understanding its full potential. Furthermore, several important parameters, such as the cost, subsidy size, and characteristics of the beneficiary population will vary across instances of the intervention, further highlighting the importance of future studies.

Tables

Table 1: Lotteries included in the sample

Lottery ID	N winners	Year	Group	Neighborhood	Area ¹	Allotment price ²	Current price ³	Downpayment ⁴
274	14	2012	LIG	Charkop	402	42,581	78,125	235
275	14	2012	LIG	Charkop	462	48,922	93,750	235
276	14	2012	LIG	Charkop	403	42,679	78,125	235
283	270	2012	LIG	Malvani	306	30,261	43,750	235
284	130	2012	LIG	Vinobha Bhave Nagar	269	23,438	42,188	235
302	227	2014	EWS	Mankhurd	269	25,414	31,250	238
303	201	2014	LIG	Vinobha Bhave Nagar	269	31,848	42,188	394
305	61	2014	EWS	Magathane	269	22,883	78,125	238

¹ In square feet. Refers to "carpet area", or the actual apartment area and excludes common space.

² Price at which winners purchased the home with the cost converted from INR to 2017 USD. In 2017, about 64 rupees made up 1 USD. ³ Average sale list price of a MHADA flat of the same square footage in the same community. Data collected from magicbricks.com in 2017. ⁴ Cost converted from INR to 2017 USD.. Includes application fee of roughly 3 USD.

Table 2: Balance tests on household and individual characteristics as measured through a survey.

Variable	Control ¹	Treatment ²	s.e. 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
Date of interview ⁷	126.000	5.300	8.000	0.510
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 ⁴	0.150	-0.022	0.023	0.340
SC/ST ⁵	0.110	-0.029	0.021	0.165
<i>Maratha</i> ⁶	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, a historically disadvantaged social group.

⁶ A dominant group in Mumbai and Maharashtra more generally. ⁷ Refers to the day of the interview where the first interview was conducted on day 1.

Table 3: Reduced form treatment effects. N=834 unless otherwise noted.

Variable ¹	Control ²	Treatment effect ³	s.e. ⁴	Adjusted p ⁵
A: Housing quality				
Makeshift floor	0.019	0.011	0.016	0.490
Makeshift roof	0.220	-0.150	0.034	0.00
Private tap	0.750	0.130	0.039	0.001
Private toilet	0.600	0.250	0.043	0.000
B: Assets and monthly income				
> 1000 INR (15.60 USD)/month	0.990	0.003	0.009	0.710
> 5000 INR (78.13 USD)/month	0.970	0.011	0.021	0.670
> 10000 INR (156.25 USD)/month	0.860	0.140	0.030	0.000
> 15000 INR (234.38 USD)/month	0.700	0.190	0.051	0.000
> 20000 INR (312.50 USD)/month	0.540	0.200	0.063	0.003
> 25000 INR (390.63 USD)/month	0.410	0.130	0.066	0.100
> 30000 INR (468.75 USD)/month	0.240	0.078	0.060	0.310
Total non-housing assets	7.400	-0.120	0.230	0.670
C: Resources for emergency expenditures				
Savings	0.600	0.033	0.049	0.650
Family, friends and neighbors	0.550	0.030	0.050	0.650
Informal lender	0.012	0.005	0.012	0.650
Commercial bank	0.049	0.058	0.028	0.200
Don't know	0.036	-0.021	0.016	0.510
D: HH-level education and employment				
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
E: 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
F: Future-looking attitudes				
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 attitudes				
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: Ward level neighborhood characteristics⁷				
HH size	4.500	0.074	0.021	0.000
Sex ratio	0.850	-0.006	0.004	0.170
%Scheduled caste	0.064	0.001	0.003	0.780
%Scheduled tribe	0.010	0.000	0.000	0.780
%Literate	0.810	-0.010	0.003	0.002
%Working	0.400	-0.007	0.002	0.002
%Main workers	0.380	-0.007	0.002	0.002
%Marginal workers	0.023	0.000	0.000	0.430
I: Postal code-level school characteristics⁸				
%Senior secondary schools	0.120	-0.016	0.007	0.064
%Public schools	0.330	0.015	0.013	0.390
Mean # classrooms	8.300	-0.130	0.190	0.560
Mean # permanent classrooms	8.000	-0.200	0.190	0.400
% schools w/ office for headmaster	0.970	-0.011	0.003	0.000
% schools with library	0.980	-0.002	0.002	0.390
Mean # teachers w/ prof qualifications	14.000	0.051	0.400	0.900
%English medium	0.430	-0.030	0.013	0.064

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 Data from 2011 Indian Census. Measured for where households live at the time of survey. ⁸ Postal-code level data for 2017 from the Ministry of Human Resource Development, Government of India. Measured for where households live at the time of survey.

Table 4: 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(\geq 15 years)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
T	0.400 (0.150)	0.720 (0.390)	0.008 (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)
<i>Turned</i> ₆ ¹	-3.500 (0.240)			0.057 (0.017)						
<i>Turned</i> ₁₆	4.100 (0.240)					0.330 (0.042)				
<i>Turned</i> ₁₈	3.400 (0.260)							0.390 (0.051)		
<i>Turned</i> ₂₁	6.600 (0.270)									0.350 (0.050)
Older ²	4.100 (0.230)	1.700 (0.320)								
T×Older		-0.160 (0.450)								
T× <i>Turned</i> ₆				-0.016 (0.018)						
T× <i>Turned</i> ₁₆						0.093 (0.050)				
T× <i>Turned</i> ₁₈								0.110 (0.067)		
T× <i>Turned</i> ₂₁										0.110 (0.068)
Constant	6.700 (0.210)	9.100 (0.280)	0.940 (0.006)	0.930 (0.007)	0.510 (0.013)	0.490 (0.013)	0.320 (0.013)	0.300 (0.014)	0.260 (0.012)	0.230 (0.012)
Observations	3,170	3,170	3,170	3,170	3,170	3,170	3,170	3,170	3,170	3,170
R ²	0.290	0.076	0.047	0.049	0.053	0.088	0.058	0.110	0.058	0.110
Adjusted R ²	0.260	0.035	0.005	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*_{*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.

Table 5: Regressions of individual employment on the treatment indicator.

	<i>Dependent variable:</i>													
	Employed						Employed (full-time)				Employed (part-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)
$Turned_6^1$	-0.016 (0.012)	-0.470 (0.014)												
$Turned_{16}$	0.001 (0.025)		-0.450 (0.027)				-0.380 (0.037)				0.093 (0.043)			
$Turned_{18}$	0.140 (0.035)			-0.220 (0.052)			-0.170 (0.053)				0.063 (0.039)			
$Turned_{21}$	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)
$T \times Turned_6$		-0.023 (0.021)												
$T \times Turned_{16}$			0.058 (0.041)				0.051 (0.051)				0.017 (0.055)			
$T \times Turned_{18}$				0.065 (0.071)			0.049 (0.074)				-0.036 (0.049)			
$T \times Turned_{21}$					0.160 (0.068)			0.150 (0.062)				-0.010 (0.040)		
$T \times Older$						-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_{it} , 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.

Acknowledgements

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. Anushtubh Agnihotri, Caroline Brandt, Christopher Carter, Laura Jakli, Nirvikar Jassal, Michael Koelle, Curtis Morrill, Pranav Gupta, Carlos Schmidt-Padilla, Matthew Stenberg, Valerie Wirtschafter, 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.

References

- Araujo, M. C., Bosch, M., and Schady, N. (2016). Can Cash Transfers Help Households Escape an Inter-Generational Poverty Trap? Working Paper 22670, National Bureau of Economic Research.
- Asher, S., Novosad, P., and Rafkin, C. (2020). Intergenerational mobility in india: New methods and estimates across time, space, and communities.
- Baird, S., De Hoop, J., and Ozler, B. (2013). Income shocks and adolescent mental health. *Journal of Human Resources*, 48(2):370–403.
- Balboni, C., Bandiera, O., Burgess, R., Ghatak, M., and Heil, A. (2020). Why do people stay poor? Publisher: CEPR Discussion Paper No. DP14534.
- Barnhardt, S., Field, E., and 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. and 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.
- Chetty, R., Hendren, N., and Katz, L. F. (2016). The Effects of Exposure to Better Neighborhoods on Children: New Evidence from the Moving to Opportunity Experiment. *American Economic Review*, 106(4):855–902.
- Cunha, J. M. (2014). Testing paternalism: Cash versus in-kind transfers. *American Economic Journal: Applied Economics*, 6(2):195–230.
- Currie, J. and Gahvari, F. (2008). Transfers in cash and in-kind: Theory meets the data. *Journal of Economic Literature*, 46(2):333–83.
- De, A. and Dreze, J. (1999). *Public Report on Basic Education in India*. Oxford University Press, New Delhi.
- Desai, S. and 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]*, pages 02–16.
- Di Tella, R., Galiani, S., and Schargrodsky, E. (2007). The formation of beliefs: evidence from the allocation of land titles to squatters. *The Quarterly Journal of Economics*, pages 209–241.
- Fernald, L. C., Hamad, R., Karlan, D., Ozer, E. J., and Zinman, J. (2008). Small individual loans and mental health: a randomized controlled trial among South African adults. *BMC Public Health*, 8(1):409.
- Field, E. (2007). Entitled to work: Urban property rights and labor supply in peru. *The Quarterly Journal of Economics*, 122(4):1561–1602.

- Franklin, S. (2020). Enabled to work: the impact of government housing on slum dwellers in south africa. *Journal of Urban Economics*, page 103265.
- Gangopadhyay, S., Lensink, R., and Yadav, B. (2015). Cash or in-kind transfers? evidence from a randomised controlled trial in delhi, india. *The Journal of Development Studies*, 51(6):660–673.
- Harma, J. (2011). Low cost private schooling in India: Is it pro poor and equitable? *International Journal of Educational Development*, 31(4):350–356.
- Haushofer, J. and Fehr, E. (2014). On the psychology of poverty. *Science*, 344(6186):862–867.
- Haushofer, J. and 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. and 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.
- Kumar, T. (2020). Home-price subsidies increase claim-making in urban india. *Forthcoming, Journal of Politics*.
- Lavecchia, A. M., Liu, H., and Oreopoulos, P. (2016). Chapter 1 - Behavioral Economics of Education: Progress and Possibilities. In Hanushek, E. A., Machin, S., and Woessmann, L., editors, *Handbook of the Economics of Education*, volume 5, pages 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. and White, H. (1985). Some heteroskedasticity-consistent covariance matrix estimators with improved finite sample properties. *Journal of econometrics*, 29(3):305–325.
- Madan, U. (2016). Personal interview with the chief commissioner of Mumbai’s Metropolitan Region Development Authority.
- Mani, A., Mullainathan, S., Shafir, E., and Zhao, J. (2013). Poverty impedes cognitive function. *science*, 341(6149):976–980.
- McIntosh, C. and Zeitlin, A. (2018). Benchmarking a child nutrition program against cash: experimental evidence from Rwanda. *San Diego: University of California*.
- Ozer, E. J., Fernald, L. C., Weber, A., Flynn, E. P., and 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. and 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.
- Skoufias, E., Unar, M., and González-Cossío, T. (2008). The impacts of cash and in-kind transfers on consumption and labor supply: Experimental evidence from rural Mexico. *World Bank Policy Research Working Paper*, (4778).
- Ssewamala, F. M., Han, C.-K., and 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.
- Thurow, L. C. (1974). Cash versus in-kind transfers. *The American Economic Review*, pages 190–195.
- van Dijk, W. (2019). The Socio-Economic Consequences of Housing Assistance. *Working Paper*.