Housing Vouchers, Income Shocks and Crime: Evidence from a Lottery

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Abstract

Employing exogenous variation in randomized wait-list positions assigned using a lottery, we identify the causal effects of Section 8 housing vouchers on arrests of adult household heads. Based on administrative records from Houston, we find that voucher receipt has no effect on the likelihood of arrest. Even among the groups with the highest propensities for crime, the vouchers have no impact. Income effects for these adults are particularly large relative to neighborhood effects, leading us to believe that this large income shock does little to alleviate financial pressures that could lead to crime.

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1 Introduction

The federally-funded Housing Choice Voucher Program (HCVP) provides rent support to about 2.1 million U.S. households living in non-government housing, which is around 43% of all households receiving federal rental assistance (Center on Budget and Policy Priorities 2012 and 2013). The program, often simply called "Section 8," is designed to provide an in-kind transfer to low-income families and individuals and allow them to reside in areas otherwise unaffordable. The program is means-tested, and participating families receive a rent subsidy that is paid directly to their landlords.

In this paper, we examine the effect of Section 8 vouchers on crime committed by adult heads of household. Housing vouchers could affect crime through two major channels: income transfer effects and neighborhood effects. Income transfers can improve the quality of a family's housing and relieve financial pressures that could otherwise cause recipients to seek illicit income. Alternatively, income transfers could also provide the funds or leisure time necessary to participate in illegal activities. Voucher receipt could also affect criminal involvement by changing neighborhood influences. Moving to a better neighborhood could reduce crime via positive peer effects or social norms, or it could increase crime by providing easier and wealthier targets.

Empirically, neighborhood effects are not substantial in this setting. Nearly 30 percent of voucher recipients stayed in the same Census Tract, and those who did move to a different Tract moved to ones with only slighter lower crime rates and marginally better economic characteristics.

The change to a family's income due to voucher receipt can be substantial, as the quantity of the voucher is equal to around 60 percent of baseline income on average for our sample. Sizeable income shocks could have meaningful impacts on crime. Empirically, large one-time or annual payments result in reductions in property crime for both recipients and communities (Watson et al., 2019; Chioda et al., 2016; Mejia and Camacho, 2014; Street, 2019). Notably, even smaller, temporal fluctuations in family income due to monthly transfer programs, even in-kind programs, has been shown to impact crime (see for example Foley, 2011; Carr and Packham, 2019; Evans and Moore, 2011; Dobkin and Puller, 2007).

The Section 8 program is ideal for studying the impacts of large income shocks on recipients. Understanding the causal effects of income transfer programs is generally challenging because individuals select to participate in these programs. Eligible families that opt to participate may also take other steps to better their lives, creating a substantial source of selection bias. Often, Section 8 housing vouchers are given out via randomized lottery because there are usually more applicants than vouchers, introducing a random component to participation in the program. This allows us to look specifically at the impact of the introduction of a large, monthly transfer on a recipient.

In this paper, we exploit the exogenous variation in randomized wait-list positions assigned using a lottery in order to identify the causal effects of Section 8 vouchers on arrests of adult household heads. The lottery we study was administered by the Houston Housing Authority (HHA), and we link the voucher applicants to arrest records from the Houston Police Department (HPD) to determine whether voucher receipt has an effect on arrests for various types of crimes. Houston is a novel place to study housing programs. The Moving to Opportunity (MTO) experiment, a landmark field experiment with a treatment arm that included Section 8 vouchers, focused on 5 large urban centers (Baltimore, Boston, Chicago, Los Angeles and New York), and much of the most well-known work on Section 8 also comes from Chicago and New York as well. Houston exhibits a number of characteristics not possessed by the other cities previously studied in the housing literature. For example, Houston residents are particularly impacted by transportation concerns and the need for climate control.

This random variation in Section 8 voucher allocation (on its own and within the MTO context)[1] has been relied upon for identification of effects on a host of youth outcomes (see for example Jacob et al., 2014, 2013; Mills et al., 2006; Kling et al., 2005; Ludwig et al.,

[1]See Appendix A for a thorough description of MTO, a comparison of the MTO and Section 8 programs, and the related literature.

2001; Katz et al., 2001) as well as adult labor market outcomes (Jacob and Ludwig, 2012; Chyn, 2018). More recently, research has been done on potential reforms to the HCVP and resulting effects on different subgroups of voucher recipients (Geyer, 2017).[2]

Current research on Section 8 and crime, specifically, has focused on outcomes for individuals who were juveniles at the time their families received vouchers. Jacob et al. (2014) find little evidence that youth outcomes are affected, and although they do find statistically significant effects on the social cost of crimes committed by these juveniles, the magnitudes are relatively small. It is possible that these effects would not be the same for adults, especially given that adults experience income effects much more acutely than youths.

As adult heads of household are most likely in charge of family finances, any reduction in the portion of resources going towards rent will cause them to experience the income shock directly. Unless that income is made available to the children in the household, even small neighborhood effects are likely to dominate income effects for juveniles. For this reason, we believe that a main contribution of this paper is that we focus on the criminal outcomes of the adults receiving Section 8 vouchers.[3] Notably, the majority of recipients are women, who are particularly sensitive to family finances, and whose criminal activities have been shown to respond to within-month variation in income (due to SNAP schedules) in Carr and Packham (2019).

Like the existing literature on Section 8, we depend on a randomized lottery that created a wait-list for vouchers for our identification. We estimate the effects using intent-to-treat and two-stage least squares models that are identified using the relative timing of voucher receipt, which is determined by the lottery. To support the assumption that wait-list positions are indeed random, we perform empirical tests for differences in pre-lottery characteristics

[2]Researchers have also used the Gautreaux Program (a precursor of MTO) (Popkin et al., 1993), random assignment into public housing (Oreopoulos, 2003), or Hurricane Katrina (Hussey et al., 2011; Kirk, 2012) to study household mobility and crime.

[3]Leech (2013) uses NLSY data to study the relationship between self-reported voucher receipt and self-reported violent altercations for young adult heads of household receiving vouchers. She finds that voucher receipt is associated with reduced violent altercations, but that this association is not present in the subsample of black recipients.

across applicants with high and low lottery numbers. The relationships between pre-lottery characteristics and wait-list positions are consistent with wait-list randomization. We also perform a test for differential attrition between applicants assigned low and high lottery numbers and find no evidence of such.

Results indicate that arrests for criminal activity are generally unchanged by voucher receipt, although we are only able to rule out reductions in arrests overall of over 10.5 percent with 95 percent confidence. The results are the similarly statistically indistinguishable from zero even among subgroups typically considered to have a higher proclivity for crime, namely men and individuals with a prior arrest record.

Take-up is particularly low for this housing voucher lottery, so we also consider just the subsample of individuals who eventually use a voucher to actually "lease up." Both the take-up rate and composition of leasers are consistent over time, so we are confident that those who lease-up later are a good comparison for those who lease-up earlier. For this population, we actually find an increase in violent crime arrests, which appears to be driven by the groups with higher likelihoods of involvement in crime - individuals with a criminal history and males.

The paper proceeds as follows. In Section 2 we present background on the Housing Choice Voucher Program both federally and in Houston, and we further discuss the likely mechanisms behind our results. In Section 3, we describe the data, and in Section 4 we explain our empirical approach. In Section 5, we present results, including various subgroup analyses and robustness tests. In Section 6, we conclude.

2 Background

2.1 Housing Choice Voucher Program

The Section 8 Housing Voucher Program is the largest housing assistance program in the U.S. The vouchers are federally funded, and the U.S. Department of Housing and Urban Development (HUD) allocates the funds to local housing authorities and sets eligibility standards across the nation. HUD requires that participants' incomes fall below 80 percent of the median family income in the area, adjusting for family size, and stipulates that 75 percent of new voucher recipients' incomes be less than 30 percent of the local median family income (Center on Budget and Policy Priorities, 2013).

Voucher recipients must also be citizens or of other eligible immigration statuses, and local housing authorities can deny eligibility for a history of criminal activity (U.S. Department of Housing and Urban Development, 2001; Houston Housing Authority, 2013). In practice, individuals with a conviction for a drug or violent crime in the past 5 years are considered ineligible by HHA. Continued eligibility is assessed annually, creating incentives for recipients to take steps to maintain their eligibility such as avoiding criminal convictions. Recipients are allowed to use their vouchers in any U.S. city with the program in place, although, according to HHA, less than 10 percent of their voucher recipients move to a different city.

New program participants are allowed to use vouchers at their current (pre-voucher) address (provided that apartment complex/property is Section 8 approved) or use the vouchers to move to a new address. Local housing authorities submit the subsidies directly to the recipients' private market landlords. The subsidy amount is determined by a number of factors. The local housing authority calculates the number of bedrooms the family will need based on family size and composition and computes the family's adjusted income (calculated according to HUD's regulations). Families must contribute 30 percent of their adjusted income to rent at a minimum, and they receive a set "maximum subsidy" amount calculated by the local housing authority as a percentage of fair market rent in a city for a given number of bedrooms. Even if the rental rate at the family's desired apartment is less than 30% of their adjusted income plus the "maximum subsidy" the family still must pay the 30% of income, and the subsidy will be reduced. Families are not allowed to contribute more than 40% of adjusted income towards rent (U.S. Department of Housing and Urban Development, 2001).

2.2 The Houston Lottery

The Houston Housing Authority (HHA) serves around 60,000 Houstonians, over 80 percent of whom are participants in the Housing Choice Voucher Program. HHA opened its wait-list and accepted applications from December 11, 2006, to December 27, 2006; they received over 29,000 applications. All applicants were assigned a randomized lottery number (denoting wait-list position) regardless of whether they met the eligibility criteria. Vouchers were then extended to the applicants as sufficient funding became available starting with the lowest lottery numbers. We use the term "voucher service process" to refer to the process of obtaining a voucher that begins when the applicant's lottery-generated wait-list position is reached and ends when the applicant leases-up with a voucher. Not all applicants complete the voucher service process. The lottery and voucher service processes are outlined in Figure A1. Once an applicant's wait-list position was reached, he or she was sent a voucher screening packet from HHA, and the verification process began. After their eligibility was verified, families were required to sign a lease for a unit that meets Section 8 requirements in order to enroll into the program.

The average time between HHA sending the initial packet and the recipient leasing up with the voucher was 6 months. Because the speed of this process varied by applicant, the vouchers were not issued in perfect sequential order. A few lottery numbers were serviced too far out of order for this to be the likely cause as well. According to HHA, there were no priority groups in the lottery, and applicants who were serviced out of sequence do not share any common characteristics. However, because we use the randomized lottery number as the basis of identification, not the actual date the screening packet was sent or the date of move-in, our estimates should not be biased by any non-sequential servicing of lottery numbers.

The first vouchers were issued in July 2007. However, the majority of vouchers were serviced starting in 2009, and HHA had sent screening packets to almost all the applicants by October 2012. Overall, only about 23% of applicants ever leased-up using a voucher.

The low lease-up is a result of applicants dropping out at every step of the voucher service process. Based on the last known application statuses, close to 60 percent of the verification packets were not returned to HHA by the families.[4] 2.7 percent of the applicants were found to be ineligible after verification and about 2.9 percent of them were unable to sign a lease in time, and their voucher expired.

2.3 Potential Mechanisms

Empirically, the neighborhoods in which voucher recipients use vouchers are only marginally different than those in which they lived at the time of the lottery (see Panel A of Table 1). Around 14 percent of voucher recipients did not move and instead used the voucher at their address listed on their application; nearly 30 percent stayed in the same Census Tract. Also, the median distance moved is only 3.01 miles.

In fact, Ellen et al. (2012) even show that voucher recipients move into relatively high crime areas within cities. Similarly, we find that families do not move into neighborhoods that experience substantially less crime. We do find that within the set of applicants who lease up using vouchers, those with lower demonstrated and likely criminal propensities (individuals without any previous arrests and women) move to neighborhoods that have greater improvements in economic characteristics than the corresponding higher propensity groups.

The income effects, on the other hand, appear to be quite large. The voucher paid an average of $627 toward rent every month. Only 1.71% of program participants were living in public housing at the time of the lottery, indicating that this aid represents a new transfer to the majority of the families and not merely a change in the form of housing aid. This evidence suggesting that the income effects may dominate neighborhood effects is in line with a number of past Section 8 studies (e.g. Jacob and Ludwig, 2012; Jacob et al., 2014; Ellen et al., 2016).

[4] We suspect that some of this was due to HHA's lack of screening pre-application, but even in Chicago, where they did pre-screen, take-up was only 40 percent.

Voucher program participants may internalize this income change in two ways: as a shock to their family income directly and as an impetus to reevaluate the way that members divide their time between labor and leisure.

Foremost, the increase in family income can improve the quality of housing that families can afford, and many will increase the quality of their housing resources. Although we find little evidence of changing neighborhood quality, housing quality could improve, even within apartment complexes if families are able to switch to larger units. Crowding in units or in complexes can lead to an increased likelihood of conflict between residents, so this improvement could reduce interpersonal crimes like assault.

Additional resources could also alleviate financial pressures, which would reduce the recipients' motivations to be involved in a crime that can lead to financial gains, such as selling illegal drugs or theft. For example, Watson et al. (2019) find that the annual payments made through Alaska's Permanent Fund Dividend lead to an 8% decrease in property crime during the two weeks after payment. Similarly, studies of Columbia's "Familias en Accion" program (Mejia and Camacho, 2014), Brazil's "Bolsa Família" program (Chioda et al., 2016) and the 1996 Welfare Reform in the US (Hannon and DeFronzo, 1998) find that these programs reduced financially-motivated crime by increasing cash transfers to low-income families.

Conversely, additional funds can be spent on complements to crime such as drugs, alcohol, and weapons. These effects are also substantiated in the income shocks literature. Watson et al. (2019) find that the same payment that reduces property crime also increases substance-use incidents involving law enforcement in the 4 weeks after individuals receive these payments. Both Dobkin and Puller (2007) and Evans and Moore (2011) show that mortality (often related to drug use) increases after receipt of government transfer payments, and Hsu (2016) and Carr and Packham (2020) find that domestic violence responds to fluctuations in monthly transfer payments. The entry of these new residents and their newly increased income could lead to spillover effects on communities, as well. This is a particularly challenging empirical question, as the residents locate endogenously into their new neighborhoods. Leveraging Hurricane Katrina as a shock to whether cities saw an influx of new residents, studies have shown modest increases in crime (Hussey et al., 2011; Varano et al., 2010).

The theoretical implications of an in-kind transfer on labor decisions are similarly ambiguous because they depend on the shape of each recipient's indifference curves. If this additional income affords recipients the opportunity to take additional leisure time, they could use it to participate in crime or for positive activities like education or child-rearing. The means-tested structure of the program may also play a role in reducing labor force participation, as an increase in adjusted income is met with a reduction in benefits awarded.

Empirically, Section 8 voucher receipt does, in fact, cause lower labor force participation rates and earnings (Jacob and Ludwig, 2012; Carlson et al., 2012), and a similar effect has been detected for Food Stamps (Hoynes and Schanzenbach, 2012). Jacob et al. (2014) suggest that a reduction in parental labor force participation may help explain why they find little effect of Section 8 vouchers on youth outcomes, while other research focused on aid programs with a work incentive component finds positive outcomes for youth (e.g. Dahl and Lochner, 2012; Duncan et al., 2011; Milligan and Stabile, 2011).

In the context of this study, some of the large income shock could be eroded if families do choose to work less, and therefore have lower earnings. Earnings are unlikely to fall by more than the amount of the voucher, but if they are diminished even a small amount, then the income shock will be smaller than the full voucher amount.

Because so many of the recipients of Section 8 are single mothers, we find it plausible that they reduce hours worked upon receiving the voucher. The additional funds may allow these parents to work a more reasonable number of hours (as opposed to working multiple jobs as many low-income parents do) to spend more time with their children. This possible reduction of the income effect through reduced labor market earnings combined with more time spent at home with children are a plausible set of factors contributing to the null effect we find.

While all of these factors are likely to contribute to effects in one direction or another, there is another mitigating factor that may play a significant role - that recipients can lose their voucher or be deemed ineligible if they have been convicted of certain types of crime. We think that the size of the transfer makes this rule particularly salient to households, and may prevent both those waiting for voucher service and those already receiving a voucher from committing crimes.

3 Data

The Houston Housing Authority provided us with information on all voucher applicants, applicants who successfully enrolled into the program after signing a new lease (whom we call "leasers"), and participants in the program in 2014 ("current residents"). The data provided on these three groups are for heads of household only, and they contain different variables, explaining some of the variation in samples sizes throughout.

The confidential data on applicants include lottery numbers, the number of bedrooms needed (calculated based on family size and composition), their address at the time of the lottery, and the date on which HHA sent the voucher screening packet. We also observe the name and birth date of the head of household, which we use to match the HHA data to arrest records. For leasers, we know the lease start date on which they began using a voucher to pay part of their rent. For current residents, we also know their race, homeless status at the time of admission, the address of the unit at which they were using a voucher in 2014, the voucher amount, and the portion of the rent paid by the family.

HHA assigned lottery numbers up to 29,327, but we limit our sample to those living in Houston at the time of the lottery. Additionally, there are a small number of duplicate applicants (77 applicants total); we assign them their lowest lottery number. We also drop applicants with lottery numbers greater than 24,000 because the lease-up rate is much lower among these applicants indicating a probable change in the voucher service process after that point. (Appendix Figure A2 displays take-up rates across lottery numbers, highlighting the precipitous drop-off.) The resulting sample size is 19,621.

We geocode the addresses listed on the applications for all applicants and the voucher-use addresses for the current residents group.[5] We link the successfully geocoded addresses (some were poor quality) to Census Tracts and police divisions in order to generate measures of neighborhood characteristics.

The distribution of addresses indicates that the current residents had not moved to different parts of the city on the whole, which helps to alleviate concerns about differential crime reporting rates between pre-lottery and voucher-use neighborhoods. (See heatmaps in Appendix Figure A3.) Panel B of Table 1 shows the differences between the neighborhoods at the time of the lottery and the neighborhoods in which current residents used vouchers in 2014 (for the group of leasers for whom we have geocoded 2014 voucher-use addresses as well as pre-lottery addresses).

We report median rent in 2012 from the American Community Survey, and we see that voucher-use addresses are in Census Tracts with only $39.86 higher monthly median rent. We report demographic and socioeconomic characteristics of the Census Tracts from the 2010 census and crime rates from 2000 to 2005 for the police divisions. The voucher-use neighborhoods are somewhat better off in terms of quality parameters such as unemployment rate, household income, poverty rate and crime rates. These differences in neighborhoods are minimal; for example, voucher-use neighborhoods had on average 1.5 fewer crimes per year per 1000 residents than the pre-lottery neighborhoods, which is a 1.1 percent improvement. This is particularly interesting given that HHA offered higher reimbursement rates for rent in less economically disadvantaged neighborhoods.

Based on the minimal changes to neighborhoods, we believe that any impact of the vouchers in this context can be most reasonably attributed to the income shock induced by

[5]We are grateful to Texas A& M GeoServices for providing us with this service.

an annual rent subsidy of more than \$7,500 on average.[6]

Moreover, if we assume that voucher recipients were paying the median rent in their pre-lottery address Census Tracts (\$767) because they contribute on average \$205 towards rent once they use a voucher, they are spending \$562 less on housing per month. This could be an upper bound because negative selection into housing quality prior to the voucher would imply that they were paying less than the median rent in their pre-lottery Census Tracts.

However, the difference in the average median rent between pre- and post-voucher Census Tracts is only \$40. Unless the majority of the families moved to units with rents much higher than the median, these numbers suggest modest increases in housing consumption and sizeable income shocks. That said, there is considerable scope for heterogeneity, and we do in fact document some across gender and criminal history in Appendix Tables A7 and A8, indicating that females and household heads with no past arrests do in fact opt into more improved neighborhoods than males and previously-arrested household heads.

We match the HHA data to arrest records provided by the Houston Police Department (HPD). The arrest records are reported at the time of booking and include information on the most serious offense as well as the arrestee's name, birthdate, and reported home address. We match the HHA and HPD data using name and birthdate, and we perform secondary matches using the Levenshtein distance and Soundex code of each name for unmatched records.[7] The arrest records range from January 1990 to November 2011.[8] We also use the matched arrest records to create measures of criminal activity in the period before the

[6] All lottery applicants have their voucher serviced in this implementation, and as far as we know, there is no difference in the duration over which families receive aid across lower and higher lottery numbers. Therefore, the comparison is between people who get the same treatment 2 years later vs. earlier. Because low-income families are usually highly credit constrained, we argue that present value comparisons of the transfers are not necessary in this setting.

[7] For the arrest records that are unmatched by name and birthdate, we calculate the Levenshtein distance for the first and last names, if the sum of the Levenshtein distances is less than 3, conditional on an exact birthdate match, we accept the match. For example, conditional on having the same birthday, this would allow us to match "Michael" to "Micheal." For the records that are still unmatched, we perform an exact soundex code match. Undoubtedly, there is room for mismatch, but since we do not expect the match quality to be correlated with lottery number, it should not be a concern for identification.

[8] The Houston Police Department has denied our requests for additional data, so we are not able to extend the panel further into the post-lottery period.

participants applied to the lottery and a quarterly panel of arrests for the study period after the program commenced (from quarter 1 of 2007 to quarter 2 of 2011).

We consider arrests of any type and specifically categorize violent offenses, drug and alcohol offenses, and financially-motivated offenses. We use the FBI's UCR classification for violent crime, and include any offenses mentioning drugs or alcohol in the drug and alcohol category. For financially-motivated crimes we add other potentially financially-motivated crimes to the the FBI's definition of property crimes, namely we include robbery in this category as well, in order to focus on the underlying motivation.[9,10]

We measure arrests as a binary indicator for whether the individual was arrested. The pre-lottery crime measures are constructed for the 5 years prior to the lottery, and we create an additional binary indicator for whether the applicant was arrested at least once between 1990 and 2006.

Table 2 reports pre-lottery descriptive statistics. We report them for a number of groups: all applicants, low and high lottery numbers (applicants with lottery numbers below and above the median), leasers, and non-leasers. If the low and high lottery number groups are different on important measures, it could indicate that HHA gave preference to some groups in lottery number assignment. We report the leasers and non-leasers to show that the two groups are generally similar in observable characteristics.

The first panel of Table 2 pertains to the lottery implementation. In the analysis that follows, treatment is defined as leasing-up using a voucher. Intuitively, the "voucher service quarter" (intent-to-treat) is the quarter during which the applicant should have leased-up according to his or her lottery number. We determine whether the individual's lottery number has been serviced by a given quarter based on his or her lottery number relative to

[9]A complete list of all offenses and crime categories are provided in Appendix Table A1.

[10]Because many robberies have underlying financial motivation, we have put them in the same category as burglary and other financially-motivated crimes. In practice, whether we include robbery in this definition does not matter, as it is only a small proportion in this category. Appendix Tables A2 and A4 replicate the main results for the full sample and leasers and include a property crime category according to the UCR definition.

the numbers serviced by that point.[11] On average, leasers take approximately 6 months to complete the voucher service process and actually lease-up using the voucher, so we define the "voucher service quarter" as 2 quarters after the family's lottery number was reached to capture the impact of lease-up. The low lottery numbers were serviced about 1.5 years (5.8 quarters) before the high lottery numbers on average.

The average applicant was around 36 years old at the time of the lottery and required just over two bedrooms (indicating that the average family size was between 2 and 6) (U.S. Department of Housing and Urban Development 2001). Around 94% of residents are black, and using 2012 voting records from the Harris County Tax Assessor's office, we estimate that around 79% of applicants are female.[12] Less than 1% of residents were homeless at the time of admission to the program. The number of observations varies for race and homeless status because they are only available for current (2014) HHA voucher recipients.

The third panel pertains to the criminal history of applicants. Around 20% of applicants were arrested during the 1990-2006 period, and approximately 9% of applicants had been arrested in the 5 years prior to the lottery.[13] There are no statistically significant differences between applicants with high and low lottery numbers in criminal history.

Using the geocoded pre-lottery addresses, we find that applicants lived in Census Tracts with around 47% black residents. The mean unemployment rate in those Census Tracts was around 11 percent and the mean of median family income was approximately $35,000. The mean poverty rate was quite high at nearly 30 percent. Voucher applicants with higher

[11]Since the lottery numbers were not serviced in perfect sequential order, we cannot determine the voucher service quarter associated with a lottery number by simply using the smallest and largest lottery number serviced in a quarter. Additionally, for approximately 1,900 applicants, there is no recorded date of screening packet issue. As a workaround, within each quarter from 2007 to 2011, we take the lottery number at the 75th percentile of the numbers called in that quarter to be the last number called in that quarter. We assign the next lottery number as the first number called in the subsequent quarter.

[12]We calculate the percentage of Harris County voters whose reported gender is "male" for each unique first name in the list of registered voters. If there are at least 5 individuals with a given name, and 70% or more are listed as males, the name is assigned the gender "male." If 30% or less are listed as male, we classify the name as "female." Applicants with unmatched or ambiguous names are omitted from the gender subgroup analysis.

[13]HHA performs criminal background checks on all adult family members to ensure that they have "no drug-related or violent criminal history during the past 5 years" (p. 18, Houston Housing Authority (2013)). HHA obtains conviction records, so applicants who were arrested but not convicted would be eligible.

lottery numbers lived in Census Tracts with slightly higher unemployment rates. Voucher applicants lived in police divisions with an annual average of 133 crimes per 1000 residents. On average, nearly 60 of these crimes were property crimes and only 13 were violent. The similarity between these groups indicates that pre-lottery characteristics are distributed randomly across lottery numbers and suggests that the lottery was in fact random.

4 Identification and Methods

In this study, we identify the effect of housing vouchers on criminal involvement using a lottery. The lottery randomized the order of the wait-list from which applicants were called to receive their vouchers. HHA called almost all of the lottery numbers over the study period, and we use the group of applicants whose lottery number was yet to be called as a control group.

We focus on a quarterly repeated cross-section model. This allows us to treat the lottery numbers called in each quarter as the "winners" in that quarter's lottery. We can conceptualize this much like a randomized control trial with stratified random assignment.

To estimate the impact of Section 8 vouchers on arrests, we estimate both intent-to-treat models that measure the effect of voucher service and two-stage least squares models that measure the effect of voucher use. To estimate the first stage, we use an indicator for whether individual i had leased up using a voucher by quarter t, called $Post\ Lease-Up_{it}$, as the outcome variable in an ordinary least squares regression:

$$Post\ Lease-Up_{it} = \rho + \pi * Post\ Voucher\ Service_{it} + \Gamma X_i + \lambda_t + \epsilon_{it} \qquad (1)$$

In the above equation, $Post\ Voucher\ Service_{it}$ is a dummy variable equal to one if we predict individual i's voucher has been serviced by quarter t (i.e. their lottery number had been called). The first stage captures the rate of lease-up and the relationship between timing of actual lease-up and timing of voucher service (recall that although most lottery numbers were generally called in sequential order, the time taken to complete the verification process and start a new lease could vary).

We estimate all models using quarter fixed effects (λ_t) as well as robust standard errors that are clustered at the individual level. The quarter fixed effects are important because the probability of treatment varies across lotteries, and macroeconomic fluctuations are particularly salient during the study period, possibly in ways related to the probability of treatment in a quarter. The vector X_i contains individual-level controls for criminal history, age and a measure of family size. With the exception of 2SLS models, we use ordinary least squares to estimate the models.

To consider the impact on arrests, we estimate intent-to-treat (ITT) regressions of the following form:

$$Outcome_{it} = \rho + \pi * Post\ Voucher\ Service_{it} + \Gamma X_i + \lambda_t + \epsilon_{it} \tag{2}$$

The results should be interpreted as the effects of potential lease-up based on lottery number and can be rescaled by the first stage to recover a local average treatment effect. We estimate the intent-to-treat effects using a number of arrest outcomes: whether individual i was arrested for crimes of any type, violent crimes, drug and alcohol crimes, and financially-motivated crimes in quarter t. Appendix Table A1 contains a list of crimes that fall into each category.

Turning to the two-stage least squares models, the first stage is described by Equation (1) and the second stage regression can be described by:

$$Outcome_{it} = \delta + \alpha * Post\ \widehat{Lease}\ Up_{it} + \Gamma X_i + \lambda_t + v_{it} \tag{3}$$

The results from these models can be interpreted as the effects of treatment on the treated (TOT) and do not need to be rescaled as the intent-to-treat estimates do. The coefficient α represents this direct effect of voucher use. Again, we estimate the model using quarter fixed effects (λ_t) as well as robust standard errors that are clustered at the individual level.

Our identifying assumption is that the relative timing of voucher service is exogenous. That is, we assume that the low lottery number applicants had similar propensities to commit a crime as those with higher lottery numbers. This is a reasonable assumption, and we perform various tests and robustness checks to support that randomization was successful.

We test for differences in composition over lottery numbers using all the observable characteristics in our data. Specifically, we examine the extent to which demographic and criminal history variables are correlated with "voucher order," i.e. lottery number or voucher service quarter (where the first quarter of 2007 is indexed to one). The relationship is estimated empirically according to the following equation:

$$Control_i = \gamma + \beta * Voucher\ Order_i + u_i \tag{4}$$

We test for correlations between voucher order and demographic and family characteristics, criminal history and neighborhood characteristics. We also perform this test graphically, by plotting the local averages of these observed characteristics against lottery number. An absence of a trend could suggest that the lottery numbers were indeed random.

Since our analysis spans data over multiple years, our results can be biased if applicants with low or high lottery numbers were to non-randomly leave the Houston area such that we cannot observe if they were arrested. Therefore, we perform a test of attrition. Finding no relationship between lottery number and a proxy for Houston residence would indicate that the results are not being driven by attrition.

We investigate the possibility of dynamic effects over time as well and to explore potential mechanisms and policy implications, we also replicate our main analysis for pairs of subgroups related to propensity for crime. Because the proportion of applicants who eventually receive housing assistance through the program (or "lease up") is particularly low in this setting, we also report results for just the group of applicants who do receive assistance through the program ("leasers"). Acknowledging the different identifying assumption, we perform separate tests for identification and robustness for this sample as well.

5 Results

5.1 Tests of Identifying Assumptions

Identification of the model comes from the assumption that the relative timing of voucher service was exogenous. That is, individuals with lower lottery numbers had similar propensities to commit a crime as those with higher lottery numbers. Because the wait-list position and, in turn, the order of voucher service was determined by a randomized lottery, this is a reasonable assumption. Nevertheless, we test this assumption empirically.

We test for correlations between observable characteristics and both lottery number and voucher service quarter. If the identifying assumption holds, we expect to see no correlations between these measures and demographic or criminal history variables. For instance, if the most motivated applicants secured lower numbers through manipulation of the lottery mechanism, we would see a negative correlation between lottery number and indicators of stability (e.g. they would be older and have less substantial criminal history).

Figure 1 represents these relationships graphically for criminal history (probability of past arrests, past violent arrests, past drug arrests, and past financial arrests) and demographic variables (age and number of bedrooms). Each hollow square represents a local average of the variable for a bin of about 1000 applicants. If lottery number is truly random, the local averages for applicants should exhibit a flat relationship, which they do. We take this as support for the identification assumption.

Table 3 reports the results of the empirical tests. Column 2 contains the results from 24 separate regressions using lottery number as the independent variable as described by Equation 4. Similarly, the regressions that generate column 3 all use indexed voucher service quarter as the independent variable. Each row is labeled for the covariate used as the dependent variable.

There are no significant relationships between lottery number or voucher service quarter and criminal history measures (perhaps the most important determinants of future arrests). There are 3 neighborhood characteristics which appear to vary with these timing measures. The individuals with higher lottery numbers come from Census Tracts with higher unemployment rates. They also come from police divisions with higher crime rates overall and for violent crimes. These differences are too small to be economically significant. For example, if we consider 2 families whose vouchers were serviced 2 years apart (the maximum difference in timing), we would expect the later-served family's original neighborhood to only have 1.06 (0.8% of the mean) additional crimes per 1000 population annually. As an additional check, we also estimate the main models with and without these controls and show that the results are invariant, indicating that timing of voucher service is orthogonal to these characteristics.

5.2 Effect of Voucher Service on Lease-Up and Arrests

Before examining the effect of voucher receipt on criminal outcomes, we first document that families are likely to lease-up when we predict that their vouchers were serviced. Our ability to use lottery variation to identify effects hinges on the extent to which the lottery predicts the timing of lease-up.

Table 4 contains the main results, including those from the first stage regressions in column 1. Results are obtained by estimating Equation 1, as described above, and we report the coefficient on $Post\ Voucher\ Service_{it}$. Results indicate that an applicant had actually leased-up in 19.6% of the person-quarters after voucher service, and the estimate is statistically significant at the 1% level. Because the proportion of applicants who lease-up is only 23%, the small magnitude of this coefficient is both expected and reasonable.

The remaining columns in Table 4 contain the arrest outcome results. Column 2 reports the mean of each outcome variable from the year preceding the lottery (2006); we refer to it as the "pre-lottery mean" or "PLM." In columns 3 and 4, we estimate Equation (2) without and with controls. The estimates are unresponsive to their inclusion, indicating that the relative timing of voucher service is unrelated to these observable characteristics and, we expect, to unobservable characteristics. The last column (5) contains results from the two-stage least squares model described by Equations 1 and 3. Each row is labeled for the outcome variable for which the results are generated.

Results show no evidence that voucher service and lease-up affect arrests for all types of crimes combined. All the coefficients are statistically insignificant. Due to the small first stage and lack of statistical power, we are only able to rule out medium-sized effects. For example, using the ITT model with controls we can rule out with 95 percent confidence a reduction of over 0.0694 percentage points and an increase of over 0.116 percentage points in annual likelihood of arrest, which correspond to 10.5 percent and 17.6 percent, respectively. When we consider the TOT effects from the 2SLS model, estimates are significantly less precise due to the small first stage, only allowing us to rule out a reduction over 53.68 percent and an increase over 90.05 percent with 95 percent confidence.

We also look at arrests for specific types of crimes that are likely to be affected by voucher receipt: violent crimes, drug and alcohol crimes, and financially-motivated crimes. These results are also indistinguishable from zero. We attribute the lack of significance to limited statistical power given the low proportion of applicants leasing up.

Nonetheless, we can rule out effect sizes present in some of the literature when comparing them to our relatively more precise ITT effects.[14] For example, we can rule out reductions over 0.0264 percentage points in violent crime arrest probability with 95 percent confidence (and increases over 0.0308). This decline corresponds to 26.4 percent of the pre-lottery mean, and although this is a large percentage (and the lower bound of the TOT confidence interval is a 134 percent reduction), it is smaller than the 30-50 percent reductions in juvenile violent crime caused by both treatment arms in MTO documented by Ludwig et al. (2001).

[14] We believe that results from the leasers sample are the most informative TOT estimates given our lack of precision due to low take-up. TOT effects for the full sample are especially noisy for subgroups of crimes and household head characteristics, but we report them for transparency.

It is larger, though, than the 15 percent decrease in juvenile crime caused by the main MTO treatment arm in Kling et al. (2005), although the "Section 8" treatment arm has no discernible impact on juvenile crime overall.

We are also unable to rule out even medium-sized effects on financially-motivated arrests, though, as our 95 percent confidence intervals on the point estimates range from -0.0254 to 0.0461 percentage points, which when compared to the pre-lottery means is -22.25 percent to 41.89 percent (and the TOT confidence interval is -114 percent to 214 percent). Watson et al. (2019) find a decrease in property crime around 8 percent which we are unable to rule out.

We also consider subgroups and report results for the subgroup of individuals who eventually lease-up in response to this lack of precision.

There are a number of reasons to expect different types of individuals to respond differently to the vouchers. It is reasonable to postulate that if the voucher makes individuals more likely to commit a crime, those who have a higher propensity for crime will respond more strongly. We compare applicants who have been arrested in the past to those who have not because they have demonstrated such a propensity for crime. Then, we compare males to females because males are more likely to be arrested in general and in our sample. Last, we compare individuals who are younger (30 years old or younger) to those who are older because younger individuals have a higher propensity for crime in general.

Table 5 contains results for the subgroups. Panel A contains first stage results for the subgroups, and all estimates are large and similar to the first stage for the complete sample. The following panels contain results from an intent-to-treat model including individual-level controls, similar to the results in column 4 of Table 4.[15]

Overall, criminal history and gender do not impact the effect of voucher service on arrest

[15]We report ITT results for subgroups and further robustness tests. We do this for a number of reasons. First, we think that voucher service may have an effect even when the individuals do not lease-up, and using this reduced form IV allows us to not restrict the impact of the voucher service to the channel of lease-up. Second, we think that the "leasers only" analysis in Section 5.4 is the best measure of the TOT in this study. More practically, focusing on ITT maximizes our limited statistical power, and the TOT models cannot be used in the dynamic effects analysis in Section 5.3.

outcomes, and estimates are again very noisy, but we are able to use these estimates to compare to other literature with gendered effects specifically. For females, we can rule out a reduction of over 12.33 percent for all arrests, and a reduction of over 9.8 percent for financially-motivated arrests with 95 percent confidence, although we cannot rule out even sizeable increases (29.4 percent and 74 percent for all arrests and financially-motivated arrests, respectively). For comparison, Carr and Packham (2019) find increases in financially-motivated crimes the week before SNAP receipt for women of around 25 percent (also in an ITT model), indicating that obtaining resources could lead to reductions of a similar size.

Table 5 does provide some evidence that age may have an impact on responses to the vouchers. Older individuals are more likely to be arrested for a financially-motivated crime due to voucher service, but younger individuals are not. In fact, the coefficients for all crime types are positive for older individuals and negative for younger individuals (except for drug offenses, which are positive for both). Although effects are only statistically distinguishable from zero for financially-motivated offenses, the signs of the coefficients support a consistent effect by age. Older individuals are less likely to have young children at home, so to the extent that children incapacitate their caregivers, older individuals will not experience this effect.

5.3 Dynamic Effects of Voucher Service on Crime

In line with the MTO effects found for juveniles by Kling et al. (2005), one might also expect differential effects by how long an individual has been treated. Table 6 contains the results from ITT models that allow for the effect of voucher service to vary over time. Specifically, we estimate effects for six month bins, including the time during which the family's voucher is being processed (to measure any possible announcement effects) and the six months previous to that. If the coefficients from the pre-treatment period were not close to zero and statistically insignificant, that would cast doubt on the identifying assumption that the relative timing of treatment is random.

We find that there are no dynamic effects related to how recently the family's voucher was serviced. For all types of crime, we see no indication that there were any effects pre-treatment. Importantly, it also rules out a "best behavior" dip in criminality just before voucher service. None of the announcement effects are statistically different from zero.

5.4 Leasers: Effect of Voucher Service on Lease-Up and Arrests

Because we have low take-up rates in our sample of applicants, we also present results for just the sample of individuals who eventually lease up with a voucher. For this model, our identifying assumption is that the relative timing of voucher service among those who eventually lease up was exogenous. That is, we assume that within the group of leasers, individuals with low lottery numbers (who leased up earlier) had similar propensities to commit a crime as those with higher lottery numbers (who leased up later).

Despite the lottery number and the associated wait-list position being assigned randomly, we need to be wary of the possibility that there could be non-random selection in leasing up over time. For instance, as more lottery numbers are serviced, if only the "stable" families remain at the same address as the one on their application, receive the voucher packet, and successfully enroll into the program, then we could see the rate of enrollment fall over time and the late-leasers could be different from the early-leasers.

Probably the most straightforward support we have for this identification strategy comes from the constant take-up rate over time shown in Figure 2. We would expect lower take-up rates over time (and hence over lottery numbers) if these two groups were substantially different.

For compositional changes to occur over time within the leasers sample without any detectable change in lease-up rate, any reduction in the number of a certain "type" of leaser would have to be exactly offset by an increase in the number of another "type" of leaser. While this seems unlikely, we can also test for similarity between these groups directly. We present visual evidence of similarity in Appendix Figure A4, and there is no apparent relationship between controls and voucher order. Appendix Table A3 contains the results of an empirical test in which we estimate Equation 4 using demographic and family characteristics, criminal history and neighborhood characteristics as the dependent variable. This is the same test contained in Table 3 for the full sample, and results are similar in that they show some economically insignificant differences in neighborhoods. There is only one statistically significant correlation between individual characteristics and voucher order. This effect is on the number of bedrooms, but it is not economically significant. It predicts that the individual with the highest lottery number (24,000) would require 0.11 more bedrooms than the individual with the lowest lottery number. There are no significant relationships between lottery number or voucher service quarter and criminal history measures.

In Table 7, we report that in 86.6% of the person-quarters after voucher service, the leasers had previously leased-up. The large magnitude of this first stage result means that the intent-to-treat estimates will be very close to the local average treatment effects in the leaser sample.

In the leaser sample, we find no effects on crime overall or drug crimes. Overall, our 95 percent confidence intervals on the ITT model range from -0.15 percentage points to 0.23 percentage points, which is -23.4 percent to 35 percent relative to the pre-lottery mean. Because the first stage is so large, the TOT 95 percent confidence intervals are similar at -0.18 percentage points to 0.27 percentage points (-27.0 percent to 40.8 percent). Drug arrests are even less precise with a TOT 95 percent confidence interval of -35.5 percent to 64.6 percent relative to the pre-lottery mean.

We do find statistically significant effects on violent crimes. The magnitude of said effect indicates that voucher service (ITT) increases the quarterly probability of violent crime arrest by 0.0690 percentage points, and voucher use (TOT) increases the quarterly probability of violent crime arrest by 0.0796 percentage points. Comparing this estimate to the mean pre-lottery quarterly probability of violent crime arrest (from 2006), it represents a two-fold increase. In absolute terms, these results suggest an increase of 2.76 violent crime arrests per 1000 leasers annually. For the full sample of leasers, this indicates 12.4 additional violent crimes per year. Unless these were geographically concentrated, this is unlikely to make a noticeable difference in a city the size of Houston. While the coefficients for financially-motivated crime arrests are positive and large, they are not statistically distinguishable from zero.

As no other mobility studies have found an increase in violent crime, this represents a departure from the literature, although it is not implausible, given results indicating negative outcomes for transfer recipients of other types (Dobkin and Puller, 2007; Evans and Moore, 2011).

These results for violent crimes are driven by subgroups with a higher propensity for crime - individuals with a past criminal history and males. Effects are also larger for older individuals, who normally have lower crime rates. The older individuals in our sample are more likely to be male and have a criminal history, so the overlap between these groups may be driving this effect. Subgroup results are in Table 8. The first stage coefficient on whether the individual is using the voucher to pay rent as predicted by the voucher service variable is nearly 0.9 for all subgroups, indicating a strong first stage.

We can also consider whether the groups driving the effects in the leaser sample are experiencing different changes to their neighborhood characteristics after voucher receipt. In Tables A7 and A8 we replicate Table 1 split by the criminal history and gender of the leasers. We show that both males and individuals with a more substantial criminal history are less likely to move to better neighborhoods than women and individuals who have never been arrested.

Individuals who have been arrested before signing up for the lottery select into neighborhoods with younger residents and higher poverty rates. Males also select into younger neighborhoods and places with lower income, higher unemployment and lower rents, relative to females. Although many other differences between the neighborhood changes across these high and low propensity for crime groups are not statistically significant, they tell a consistent story - that these groups are less likely to use the voucher to move to neighborhoods that improve significantly upon their pre-lottery neighborhoods.

Appendix Figure A5 presents the dynamic effects on crime using an event study framework for the leasers. The main results for the leasers for violent crime appear to be driven by quarters 2 or more post-treatment. The coefficient on this time period is larger, positive and statistically significant on the 5% level. Drug and alcohol crimes exhibit a similar pattern although the effect in quarters 2 and 3 is only significant at the 10% level.

To ascertain the possibility of estimating statistically significant effects of voucher service and receipt on violent crime arrests in the leaser sample completely by chance, we conduct a placebo exercise using pre-lottery data on arrests. We perform this exercise using pre-period data from 2000-2006.[16] Our actual study period is from 2007 quarter 1 to 2011 quarter 3. Preserving the order of lottery numbers and the sequence of voucher service, we move the study period window by one quarter in each of the placebo tests. We preserve the ordering of voucher service to test whether the arrest outcomes for early and late leasers could diverge from each other as a statistical artifact in the absence of treatment. In each test, we assign a quarter from quarter 1 of 2000 to quarter 2 of 2002 to act as the starting quarter of the study period.

Figure A6 displays results from this placebo test.[17] For each type of crime, we plot the estimated coefficients against the range of placebo start quarters (square markers). We also indicate the estimated coefficient from the actual program start date for comparison (diamond markers). For all types of crime, the placebo estimates are close to zero and often

[16]In order to prevent overlapping with the real treatment period, while preserving our 18 month sample window, the last placebo voucher service start date we can use is Q2:2002. We are hesitant to go back further than 2000 because the individuals would be significantly younger.

[17]Another ostensible placebo test would be to verify that those who never lease-up are unaffected when their vouchers are serviced. Overall, there are no effects on non-leasers, but the ineligible group (4 percent of all applicants) are more likely to experience declines in criminal arrests after voucher service. (See Appendix Table A5 for these results.) This group has a more substantial criminal history at baseline, which may have caused their ineligibility. This could reflect incarceration, that their ineligibility may be related to an improved economic situation and therefore lower need, or it could be caused by this group internalizing this additional cost of crime and altering their criminal activities. Because this group is very small, 2.7 percent of all applicants, this relationship does not drive any results.

change signs across quarters. This is in sharp contrast to the actual results for violent crime, and to a degree drug crime, indicating that our results are outside of the usual level of variation in arrests across the early and late leaser groups.

5.5 Test for Attrition

One potential concern for our study is attrition. That is, to the extent that individuals with low lottery numbers are more or less likely to move out of Houston than individuals with high numbers, our results could be biased. For example, if individuals who receive high lottery numbers are more likely to leave Houston and commit crimes elsewhere that are not measured in our data, then our zero result could be hiding a real decrease in arrests.

We empirically test whether applicants with lower lottery numbers and earlier voucher service quarters are more or less likely to have stayed in Houston than those with higher numbers and later voucher service quarters. We proxy for continued Houston residence with whether the applicant was registered to vote in the City of Houston in 2012 and whether he or she voted in the 2012 general election. Specifically, we estimate an analog of Equation 4 to test for a relationship between when a leaser's voucher was serviced and whether he or she stayed in the city.

We show the raw data in Figure 3; it plots voter registration (hollow squares) and actual voting (solid diamonds) in 2012 against lottery numbers. Each dot represents a local average for a bin of about 1090 lottery numbers. There is no discernible correlation between lottery number and either voting outcome. This suggests that heads of household whose numbers were called early in the sample period were no more or less likely to be in Houston several years later than those whose numbers were called late in the sample period. Appendix Table A6 contains the results of the empirical test, and all reported coefficients are small and not statistically different from zero. We conduct a similar test for the leaser sample as well. The plots of voter registration and actual voting in 2012 against lottery numbers is shown in Figure A7.

6 Conclusion

Section 8 housing vouchers can provide a substantial change in resources for recipient families. Because resource scarcity is a driver of crime, this substantial increase in resources from the vouchers could have an influence on the criminal involvement of the heads of household.

With the relative timing of enrollment determined exogenously by a lottery, we believe we have an ideal setting to isolate the impacts of a housing-based resource shock on adults. However, we find no indication of a decline in criminal arrests for these adult voucher recipients. These results are at odds with studies of other transfer programs. For example, other studies of Section 8 that focus on juveniles find reductions in violent crime (Ludwig et al., 2001; Katz et al., 2001; Kling et al., 2005), and although our estimates of violent crime are imprecise for the overall sample, we can often rule out these effects. When we consider the population of leasers only, we even find an increase in violent crime arrests that is driven by men and individuals who have been arrested before.

It is fair to say that these two groups are not the primary targets of the Section 8 program. Like many transfer programs, Section 8 targets families and many recipients are single mothers. Potential recipients can also be deemed ineligible based on their criminal history, although not universally so.

Male-headed households and individuals with a criminal history are each a small portion the program participants (with substantial overlap in our data). Mothers, on the other hand, are a group that is typically not involved in crime, and the women in our sample have low baseline arrest rates, even relative to other women in urban Texas.

As these groups are driving effects indicating an increase in arrests in the leasers analysis, we think that this points towards substantial heterogeneity in the channels through which voucher receipt affects crime.

We show that the females and individuals who have not been arrested before are more likely to use the voucher to improve their neighborhoods. We suspect that by doing so, they may also upgrade the quality of their housing resources and therefore their income shock may be smaller than the full voucher amount.

The Section 8 program has a number of characteristics which have been deliberately designed to mitigate some of the potential negative impacts of large income shocks. First, families can be deemed ineligible if their head of household has been convicted of a serious crime in the last 5 years, and participants can lose their voucher for new convictions. Second, the voucher reimbursement amounts (at least in Houston) are greater in less economically disadvantaged areas, creating incentives for families to move to better neighborhoods that they may have considered out of reach financially even with the standard voucher reimbursement. Our results show that these rules may be having the desired effect in Houston, and that the negative impacts of large income shocks may be avoidable through careful program design.

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Tables and Figures

Table 1: Comparison of application and voucher use addresses for recipients

Panel A: Voucher Use Characteristics	Mean	Observations	
Distance moved in miles	4.7	1693	
Post voucher rent	828	2974	
Rent paid by voucher	627	2974	
Rent paid by resident	206	2965	
Percent leasers living in public housing before	1.71	4510	
Panel B: Neighborhood Characteristics	**Pre-Lottery Address**	**Voucher Use Address**	**Difference**
Census Tract Characteristics			
Percent male	48.14	48.00	-0.14 (0.12)
Percent white	30.67	34.38	3.71 *** (0.53)
Percent black	54.06	47.93	-6.13 *** (0.68)
Percent over 18 years	70.52	69.39	-1.13 (0.17)
Median age	31.88	30.79	-1.09 *** (0.15)
Median rent	767.25	807.35	39.86 *** (5.04)
Unemployment rate	12.25	11.03	-1.22 *** (0.15)
Median household income	33282.02	35942.3	2660.28 *** (358.51)
Median family income	37797.2	39666.11	1868.91 *** (411.90)
Percent poverty	29.14	27.03	-2.11 *** (0.34)
Observations	1693	1693	
Police Division Characteristics (Annual rates per 1000 population)			
Crime rate	134.87	133.35	-1.52 *** (0.64)
Murder rate	0.16	0.16	-0.003 *** (0.00)
Violent crime rate	13.46	13.17	-0.29 *** (0.10)
Property crime rate	57.92	58.08	0.16 (0.25)
Observations	1097	1097	

Notes: Statistics are shown for voucher recipients for whom both pre and post-lottery addresses were available and geocodable. Crime rates at the police division level are from 2000 to 2005.
Significance: * 10% level; ** 5% level; *** 1% level

Table 2: Descriptive statistics

	Observations	All	Low Lottery Numbers	High Lottery Numbers	Difference	Leasers	Non-leasers
Lottery Variables							
Lottery Number	19621	11982	5979	17984	12006*** (50)	11852	12021
Voucher Service Quarter	19621	13	10	16	-6*** (0)	13	13
Leased Up with Voucher	19621	0.23	0.23	0.23	0.003 (0.006)	1	0
HHH Demographic Characteristics - as on the application to HHA							
Age (in years)	19621	36.3	36.3	36.4	-0.07 (0.21)	35.3	36.7
Number of Bedrooms	19621	2.2	2.2	2.2	-0.01 (0.01)	2.2	2.1
Male	16614	0.15	0.15	0.15	-0.002 (0.006)	0.10	0.16
Female	16614	0.79	0.79	0.79	0.001 (0.006)	0.84	0.77
Black	2974	0.94	0.94	0.94	-0.002 (0.009)	0.94	0.96
White	2974	0.03	0.03	0.03	-0.001 (0.007)	0.03	0.03
Other race	2974	0.02	0.03	0.02	0.003 (0.006)	0.03	0.02
Homeless at the time of admission	2974	0.004	0.004	0.004	0.001 (0.002)	0.001	0.025
HHH Criminal History - measured 5 or more years prior to the lottery							
Arrested in 5 years prior to lottery	19621	0.09	0.09	0.09	0.003 (0.004)	0.09	0.09
Violent offense in 5 years prior	19621	0.02	0.02	0.02	-0.001 (0.002)	0.02	0.02
Drug offense in 5 years prior	19621	0.02	0.02	0.02	0.002 (0.002)	0.02	0.03
Financial offense in 5 years prior	19621	0.02	0.02	0.02	0.002 (0.002)	0.02	0.02
Arrested between 1990 and 2006	19621	0.18	0.18	0.18	-0.001 (0.006)	0.20	0.18
Neighborhood Characteristics - based on the address at the time of the lottery							
Percent black in Census Tract	15933	0.47	0.47	0.47	0.005 (0.004)	0.53	0.45
Poverty rate in Census Tract	15931	0.28	0.28	0.28	0.001 (0.002)	0.29	0.27
Median Rent in Census Tract	15913	779	778	779	-1 (3)	774	780
Median Household Income in Census Tract	15931	35300	35272	35329	-57 (222)	33816	35739
Unemployment rate in Census Tract	15933	11.38	11.31	11.45	0.14* (0.087)	12.04	11.18
Crime Rate per 1k population	12788	132.8	132.4	133.1	-0.71 (0.45)	135.1	132.1
Violent Crime Rate per 1k population	12788	13.0	12.9	13.0	-0.08 (0.06)	13.4	12.9
Property Crime Rate per 1k population	12788	58.5	58.4	58.6	-0.2 (0.2)	58.6	58.4
Total applicants per group		19621	9810	9811		4510	15111

Notes: Lottery numbers are classified as low or high based on whether they are below or above the median (11960). HHH stands for Household Head. As described in the text, gender could only be imputed for 16614 applicants. Race and other demographic characteristics are only available for applicants who were participating in the HCVP when we obtained the data (2974). Neighborhood characteristics are available only for those applicants whose pre-lottery addresses were geocodable and matched to a census tract (15933) or police division (12788). Neighborhood crime rates are annual rates reported at the police division level from 2000 to 2005 provided by HPD, who also supplied the arrest data. Census tract attributes are from the American Community Survey.
Significance: * 10% level; ** 5% level; *** 1% level

Table 3: Test of randomization

		Independent variables	
Dependent variables	Observations	Lottery number/1000	Voucher service quarter
Arrested in 5 years prior to lottery	19621	-0.0002	-0.0005
		(0.0003)	(0.0006)
Violent offence in 5 years prior	19621	0.0000	0.0002
		(0.0001)	(0.0003)
Drug offence in 5 years prior	19621	-0.0001	-0.0002
		(0.0002)	(0.0003)
Financial offence in 5 years prior	19621	-0.0001	-0.0003
		(0.0001)	(0.0003)
Number of arrests in 5 years prior	19621	-0.0002	-0.0006
		(0.0005)	(0.001)
Number of violent arrests in 5 years prior	19621	0.0001	0.0002
		(0.0002)	(0.0003)
Number of drug arrests in 5 years prior	19621	-0.0002	-0.0004
		(0.0002)	(0.0005)
Number of financial arrests in 5 years prior	19621	-0.0001	-0.0003
		(0.0002)	(0.0004)
Arrested between 1990 and 2006	19621	0.0001	-0.0002
		(0.0004)	(0.0008)
Age	19621	0.0085	0.004
		(0.0147)	(0.0306)
Number of bedrooms	19621	0.0001	-0.0001
		(0.001)	(0.0021)
Male	16614	0.0002	0.0002
		(0.0004)	(0.0008)
Female	16614	-0.0001	-0.0001
		(0.0005)	(0.0009)
Black	2974	0.0002	0.0003
		(0.0006)	(0.0013)
White	2974	-0.0001	0.0001
		(0.0005)	(0.001)
Other race	2974	-0.0001	-0.0004
		(0.0004)	(0.0009)
Homeless at the time of admission	2974	-0.0003	0.000
		(0.0002)	(0.0004)
Percent black in Census Tract	15933	-0.0004	-0.0006
		(0.0003)	(0.0007)
Median Rent in Census Tract	15913	0.05	0.0867
		(0.195)	(0.405)
Poverty rate in Census Tract	15931	-0.0001	-0.0001
		(0.0001)	(0.0003)
Unemployment rate in Census Tract	15933	0.0118*	0.0249*
		(0.0063)	(0.013)
Median Household Income in Census Tract	15931	3.513	12.49
		(15.97)	(33.13)
Crimes per 1k population	12788	0.0613*	0.133**
		(0.0322)	(0.0673)
Violent Crimes per 1k population	12788	0.0072*	0.0155*
		(0.0043)	(0.009)
Property Crimes per 1k population	12788	0.0168	0.0347
		(0.0142)	(0.0296)

Notes: Each cell represents a separate regression, estimating equation 4 with the listed covariate as the dependent variable. Unit of observation is an individual. Column 2 shows the coefficients of lottery number scaled down by 1000 and column 3 shows coefficients of the quarter in which the voucher is serviced (where Q1:2007 is indexed to one). Robust standard errors are presented in parentheses.
Significance: * 10% level; ** 5% level; *** 1% level

Table 4: Effect of vouchers on crime - By crime type

	First stage (with controls)	PLM	ITT (without controls)	ITT (with controls)	2SLS (with controls)
Voucher use	0.196***				
	(0.00419)				
All arrests		0.0066	0.000316	0.000235	0.00120
			(0.000484)	(0.000474)	(0.00242)
Violent arrests		0.001	0.0000167	0.0000220	0.000112
			(0.000146)	(0.000146)	(0.000744)
Drug arrests		0.0017	0.000143	0.000112	0.000574
			(0.000217)	(0.000215)	(0.00110)
Financial arrests		0.0011	0.000121	0.000108	0.000549
			(0.000181)	(0.000180)	(0.000921)
Observations	353178		353178	353178	353178
Individuals	19621		19621	19621	19621

Notes: Each cell represents a separate regression. Unit of observation is a person-quarter. Column 1 contains results where the dependent variable is an indicator for post lease-up. The pre-lottery means (PLM), mean of quarterly probability of arrest in the crime category from the year 2006, are shown in column 2. Intent-To-Treat effects are shown in columns 3 and 4. Two-stage least squares estimates are shown in column 5. The dependent variables in columns 3 to 5 are dummy variables indicating an arrest in the person-quarter for the particular category of offense. Controls include age at the time of the lottery, number of bedrooms and a dummy indicating an arrest in the crime category in the 5 years prior to the lottery. Robust standard errors, clustered at the individual level, are presented in parentheses.
Significance: * 10% level; ** 5% level; *** 1% level

Table 5: Effect of vouchers on crime - Subgroup analysis

	Criminal history		Gender		Age at the time of application	
	Past arrest	No past arrest	Males	Females	< 31 years	> 30 years
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: First stage						
Post Voucher Service	0.210***	0.193***	0.136***	0.205***	0.219***	0.177***
	(0.01)	(0.0046)	(0.0102)	(0.0052)	(0.00645)	(0.00545)
Panel B: All arrests						
Post Voucher Service	0.00188	-0.00015	0.000679	0.000406	-0.000196	0.000592
	(0.002)	(0.000369)	(0.00188)	(0.000509)	(0.000814)	(0.000547)
Pre-lottery Mean	0.0362	0	0.0158	0.0048	0.0087	0.0048
Panel C: Violent arrests						
Post Voucher Service	-0.000118	0.0000393	0.000176	-0.0000294	-0.0000354	0.0000667
	(0.000596)	(0.000119)	(0.000677)	(0.000147)	(0.000251)	(0.000168)
Pre-lottery Mean	0.0054	0	0.002	0.0007	0.0015	0.0006
Panel D: Drug arrests						
Post Voucher Service	0.00110	-0.000127	0.000763	0.000046	0.00000981	0.000203
	(0.000986)	(0.000144)	(0.00103)	(0.000206)	(0.000342)	(0.000273)
Pre-lottery Mean	0.0095	0	0.0051	0.0011	0.0017	0.0017
Panel E: Financial arrests						
Post Voucher Service	0.00106	-0.000105	-0.000232	0.000321	-0.000251	0.000403**
	(0.000742)	(0.000147)	(0.000565)	(0.000214)	(0.000315)	(0.000203)
Pre-lottery Mean	0.006	0	0.0017	0.001	0.0016	0.0006
Observations	64098	289080	44622	236088	158976	194202
Individuals	3561	16060	2479	13116	8832	10789
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each column within a panel represents a separate regression estimating ITT models within a subgroup. While panel A presents the first stage effects, panels B to E present the ITT effects on being arrested in the person-quarter for the particular crime category. Pre-lottery mean is the mean of quarterly probability of arrest in the crime category for the particular subgroup from the year 2006. Controls include age at the time of the lottery, number of bedrooms and a dummy indicating an arrest in the crime category in the 5 years prior to the lottery. Robust standard errors, clustered at the individual level, are presented in parentheses.
Significance: * 10% level; ** 5% level; *** 1% level

Table 6: Effect of vouchers on crime - By duration

	All arrests	Violent	Drug	Financial
Quarters from voucher service	(1)	(2)	(3)	(4)
-3 and -4	0.0000146	-0.0000377	0.0000133	-0.000101
(6 months prior to announcement)	(0.000574)	(0.000204)	(0.000297)	(0.000222)
-2 and -1	0.000254	0.000224	0.000268	0.000141
(announcement effect)	(0.000618)	(0.000226)	(0.000331)	(0.000241)
0 and 1	-0.000429	-0.000236	0.0000207	0.000314
(first 6 months after voucher service)	(0.000684)	(0.000188)	(0.000312)	(0.000293)
2 and 3	-0.0000279	0.000263	0.000410	-0.000376
(next 6 months after voucher service)	(0.000875)	(0.000307)	(0.000438)	(0.000324)
4 and beyond	0.00140	0.000283	0.000241	0.000342
($>$ 1 year since voucher service)	(0.000869)	(0.000272)	(0.000411)	(0.000311)
Observations	353178	353178	353178	353178
Individuals	19621	19621	19621	19621
Quarter FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: Each column represents a separate regression estimating ITT models with the independent variable split by duration from voucher service. The dependent variables in columns 1 to 4 are dummy variables indicating an arrest in the person-quarter for any offense, violent offense, drug or alcohol related offense, and financially motivated offense respectively. Controls include age at the time of the lottery, number of bedrooms and a dummy indicating an arrest in the crime category in the 5 years prior to the lottery. Robust standard errors, clustered at the individual level, are presented in parentheses.
Significance: * 10% level; ** 5% level; *** 1% level

Table 7: Effect of vouchers on crime - By crime type for leaser sample

	First stage (with controls)	PLM	ITT (without controls)	ITT (with controls)	2SLS (with controls)
Voucher use	0.866***				
	(0.00376)				
All arrests		0.0055	0.000431	0.000395	0.000456
			(0.00100)	(0.000990)	(0.00114)
Violent arrests		0.0007	0.000701*	0.000690*	0.000796*
			(0.000361)	(0.000359)	(0.000414)
Drug arrests		0.0012	0.000168	0.000215	0.000248
			(0.000379)	(0.000376)	(0.000434)
Financial arrests		0.0007	0.000189	0.000155	0.000179
			(0.000442)	(0.000439)	(0.000506)
Observations	81180		81180	81180	81180
Individuals	4510		4510	4510	4510

Notes: Each cell of each column represents a separate regression. Unit of observation is a person-quarter. Column 1 contains results where the dependent variable is an indicator for post lease-up. The pre-lottery means (PLM), mean of quarterly probability of arrest in the crime category from the year 2006, are shown in column 2. Intent-To-Treat effects are shown in columns 3 and 4. Two-stage least squares estimates are shown in column 5. The dependent variables in columns 3 to 5 are dummy variables indicating an arrest in the person-quarter for the particular category of offense. Controls include age at the time of the lottery, number of bedrooms and a dummy indicating an arrest in the crime category in the 5 years prior to the lottery. Robust standard errors, clustered at the individual level, are presented in parentheses.
Significance: * 10% level; ** 5% level; *** 1% level

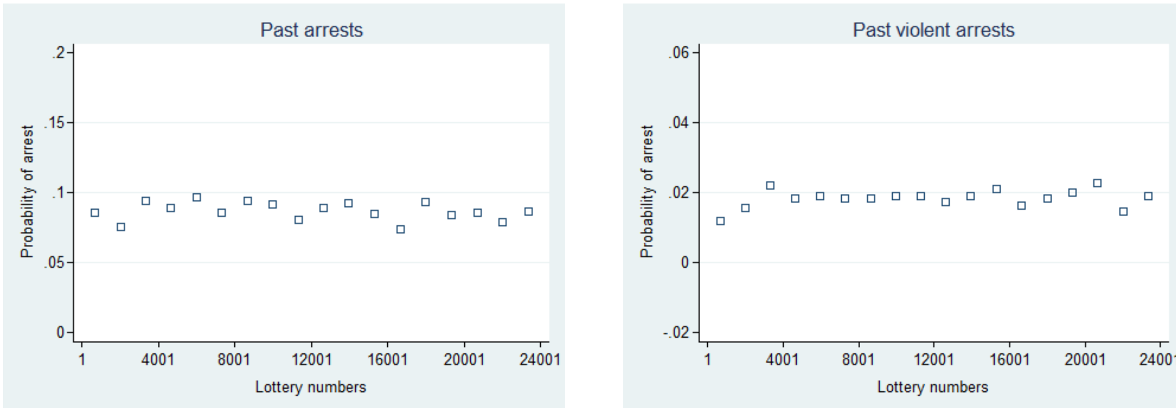
Table 8: Effect of vouchers on crime - Subgroup analysis for leaser sample

	Criminal history		Gender		Age at the time of application	
	Past arrest	No past arrest	Males	Females	$<$ 31 years	$>$ 30 years
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: First stage						
Post Voucher Service	0.870***	0.865***	0.879***	0.862***	0.868***	0.863***
	(0.00773)	(0.00428)	(0.0131)	(0.00453)	(0.00484)	(0.00577)
Panel B: All arrests						
Post Voucher Service	0.00214	0.00000763	-0.00173	-0.000318	0.00163	-0.000917
	(0.00357)	(0.000868)	(0.00462)	(0.000995)	(0.00157)	(0.00123)
Pre-lottery Mean	0.0281	0	0.0174	0.0039	0.0049	0.0061
Panel C: Violent arrests						
Post Voucher Service	0.00262**	0.000229	0.00394*	-0.0000570	0.000563	0.000743*
	(0.00129)	(0.000318)	(0.00224)	(0.000320)	(0.000579)	(0.000428)
Pre-lottery Mean	0.0037	0	0.0013	0.0005	0.0005	0.0009
Panel D: Drug arrests						
Post Voucher Service	0.000435	0.000170	-0.00159	0.000184	0.000472	0.00000824
	(0.00151)	(0.000288)	(0.00213)	(0.000358)	(0.000369)	(0.000646)
Pre-lottery Mean	0.0062	0	0.006	0.0008	0.0018	0.0006
Panel E: Financial arrests						
Post Voucher Service	0.00160	-0.000191	-0.00151	0.000436	-0.000141	0.000409
	(0.00166)	(0.000365)	(0.00155)	(0.000467)	(0.000774)	(0.000440)
Pre-lottery Mean	0.0037	0	0.0007	0.0006	0.0004	0.001
Observations	15858	65322	6732	58446	39546	41634
Individuals	881	3629	374	3247	2197	2313
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

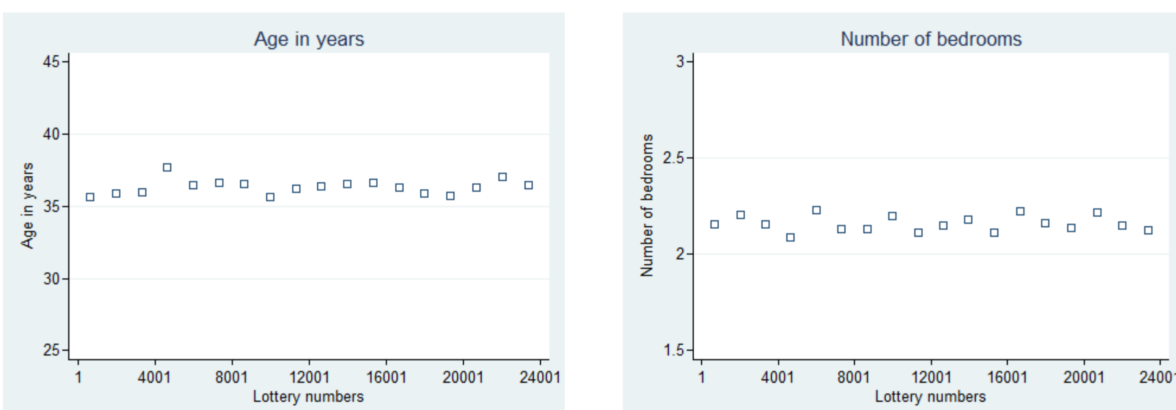
Notes: Each column within a panel represents a separate regression estimating ITT models within a subgroup. While panel A presents the first stage effects, panels B to E present the ITT effects on being arrested in the person-quarter for the particular crime category. Pre-lottery mean is the mean of quarterly probability of arrest in the crime category for the particular subgroup from the year 2006. Controls include age at the time of the lottery, number of bedrooms and a dummy indicating an arrest in the crime category in the 5 years prior to the lottery. Robust standard errors, clustered at the individual level, are presented in parentheses.
Significance: * 10% level; ** 5% level; *** 1% level

Figure 1: Test of randomization - Distribution of pre-lottery characteristics

(a) Criminal history

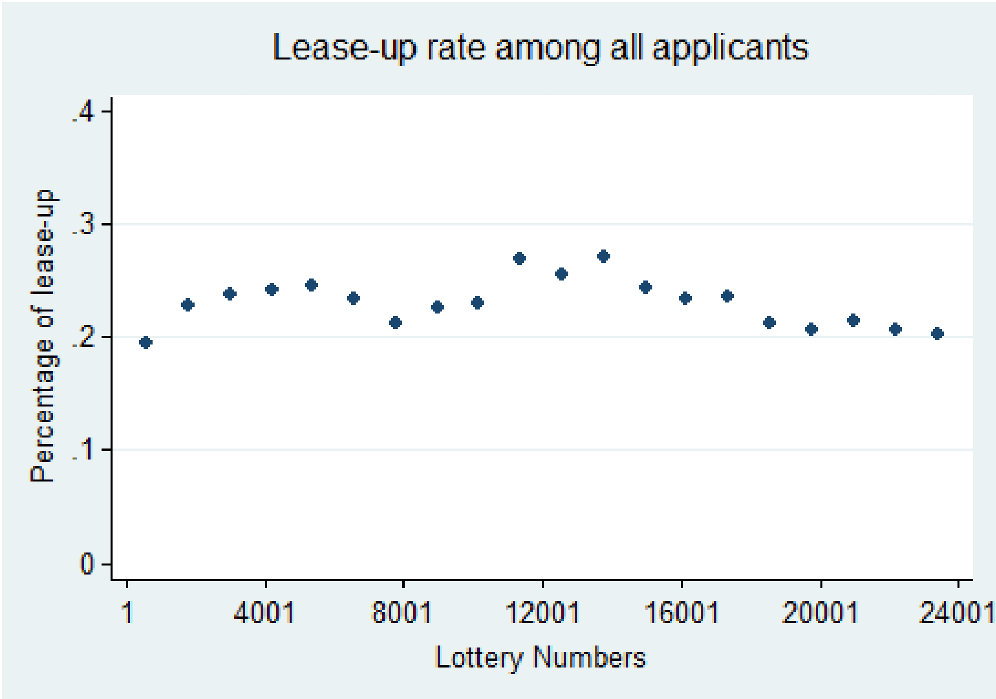


(b) Demographics



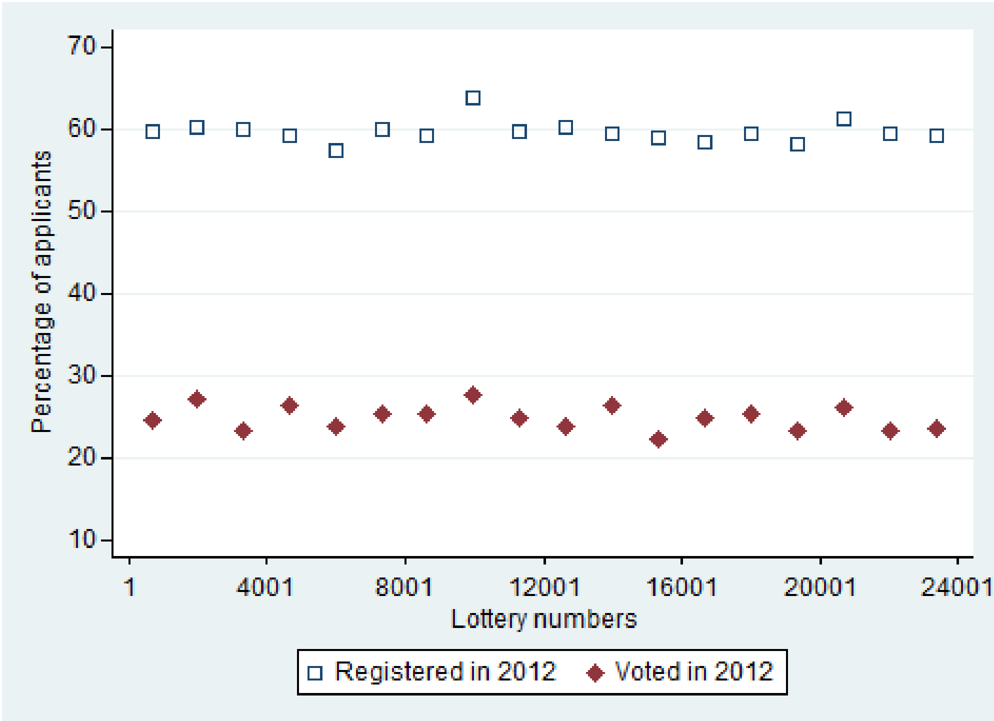
Notes: Each square represents the local average of the variable within lottery number bins of about 1090 applicants. Criminal history variables represent the probability of arrest in the crime category between 2002 and 2006 (5 years prior to the lottery).

Figure 2: Lease-up rates across lottery numbers



Notes: Each marker represents the percentage of lease-up within bins of about 980 applicants.

Figure 3: Test for attrition - Voter registration and voting in 2012



Notes: Each marker represents the local percentage of applicants that were registered to vote or who voted in Houston in 2012 within bins of about 1090 applicants.

ONLINE APPENDIX

A Moving to Opportunity

The largest body of work studying housing mobility programs has come from the Moving to Opportunity experiment, often simply called "MTO." MTO was a social experiment that randomized families living in public housing into either a control group or one of two treatment groups that were offered housing vouchers. Because MTO families were already receiving housing assistance through public housing at the time of the intervention, they did not experience an income effect as large as the one experienced by ordinary Section 8 voucher recipients (as studied here). Because the primary mechanism is so different and such a large income shock is likely to have a profound impact on these adults, results from MTO studies may not be representative of the effects for typical Section 8 participants.

Nonetheless, in this Appendix, we describe the program and the most closely related literature.

Although the Moving to Opportunity experiment gave vouchers to treatment group families, there are a number of notable differences between the regular Section 8 program and MTO. Understanding these differences is important for interpreting and comparing results from empirical studies focusing on these two programs. MTO researchers recruited only public housing residents to participate in the experiment and split them into three groups. The first (the "MTO experimental group") received vouchers and was only allowed to use them in Census Tracts with low poverty rates. The second group was simply given vouchers that could be used anywhere without restrictions. This group was called the "Section 8 experimental group." The third was a control.

MTO experimental families experienced significant improvements in neighborhood likely due to the program's restriction on local poverty rates in Census Tracts into which families relocated (Katz et al., 2001; Kling et al., 2005). In comparison, the MTO Section 8 experimental group experienced smaller improvements in neighborhood quality (Kling et al., 2005). However, in the case of the regular Section 8 voucher recipients in our sample, we find the voucher-use neighborhoods to be only marginally better than their pre-lottery neighborhoods. For example, while the HHA voucher recipients moved to Census Tracts with a 7.5% lower average poverty rate, the MTO experimental group participants moved to Census Tracts with a 66% lower poverty rate, and the Section 8 experimental group participants saw a 38% reduction in poverty rate one year after the intervention (Kling et al., 2005).

MTO required the families to move and provided little, if any, additional financial gains to them. Section 8, on the other hand, provides a substantial income transfer, and HUD does not allow local housing authorities to place restrictions on neighborhoods in which recipients can live while receiving vouchers. While we don't have information on the Section 8 participants' reasons for applying for the program, it is well documented that MTO families cite a desire to get away from gangs and drugs as the main reason for volunteering (e.g. Kling et al., 2005). This concern is likely addressed by the neighborhood change facilitated by MTO as well as the exit from public housing. On the other hand, Section 8 voucher receipt may have little effect on exposure to criminal activity if the families do not choose to move to better neighborhoods. The populations opting into these two programs are also likely to be quite different due to incongruous motivations.

For juveniles, the MTO literature suggests that both reported behavioral problems and violent crime arrests may be reduced when families receive vouchers (Ludwig et al., 2001; Katz et al., 2001; Kling et al., 2005), but there is also some evidence that property crime arrests may actually increase for male youth (Ludwig et al., 2001; Kling et al., 2005). Sciandra et al. (2013) perform a longer run follow-up study of adult outcomes for the juvenile MTO recipients and show that the effects observed immediately after the intervention diminish over time.

The only paper in this literature that looks at criminal outcomes for adults is a study by Ludwig and Kling (2007). In this paper, the authors analyze the effects of various neighborhood characteristics on crimes committed by adult and juvenile recipients using MTO treatment by site interactions as instruments for new neighborhood attributes. Their results suggest that moving to a new neighborhood that is racially segregated has the largest effect on criminal involvement.

Table A1: Classification of crimes into categories

Category	Included crimes
Violent	Assault, Aggravated Assault, Arson, Kidnapping, Murder, Robbery, Sexual Assault
Drug	Alcohol related offenses, DUI, Manufacture, Possession or Sale of contraband products
Financial	Auto Theft, Burglary, Robbery, Gambling, Shoplifting, Theft, White Collar crimes (Forgery, Fraud etc.)
Unclassified	Minor traffic offenses, Carrying/Discharging prohibited weapons, Criminal Mischief, Criminal Trespassing, Evading arrest, Indecent behavior/exposure, Prostitution related arrests

Table A2: Effect of vouchers on crime - By crime type

	First stage (with controls)	PLM	ITT (without controls)	ITT (with controls)	2SLS (with controls)
Voucher use	0.196***				
	(0.00419)				
All arrests		0.0066	0.000316	0.000235	0.00120
			(0.000484)	(0.000474)	(0.00242)
Violent arrests		0.001	0.0000167	0.0000220	0.000112
			(0.000146)	(0.000146)	(0.000744)
Drug arrests		0.0017	0.000143	0.000112	0.000574
			(0.000217)	(0.000215)	(0.00110)
Financial arrests		0.0011	0.000121	0.000108	0.000549
			(0.000181)	(0.000180)	(0.000921)
Property arrests		0.008	0.00000698	-0.0000103	-0.0000525
			(0.000172)	(0.000171)	(0.000874)
Observations	353178		353178	353178	353178
Individuals	19621		19621	19621	19621

Notes: Each cell represents a separate regression. Unit of observation is a person-quarter. Column 1 contains results where the dependent variable is an indicator for post lease-up. The pre-lottery means (PLM), mean of quarterly probability of arrest in the crime category from the year 2006, are shown in column 2. Intent-To-Treat effects are shown in columns 3 and 4. Two-stage least squares estimates are shown in column 5. The dependent variables in columns 3 to 5 are dummy variables indicating an arrest in the person-quarter for the particular category of offense. Controls include age at the time of the lottery, number of bedrooms and a dummy indicating an arrest in the crime category in the 5 years prior to the lottery. Robust standard errors, clustered at the individual level, are presented in parentheses.
Significance: * 10% level; ** 5% level; *** 1% level

Table A3: Test of identification - leaser sample

Dependent variables	Observations	Independent variables: Lottery number/1000	Voucher service quarter
Arrested in 5 years prior to lottery	4510	0.0003	0.0003
		(0.0006)	(0.0013)
Violent offense in 5 years prior	4510	0.0000	-0.0002
		(0.0003)	(0.0006)
Drug offense in 5 years prior	4510	0.0005	0.0009
		(0.0003)	(0.0006)
Financial offense in 5 years prior	4510	-0.0001	-0.0004
		(0.0003)	(0.0006)
Number of arrests in 5 years prior	4510	0.0008	0.0016
		(0.0009)	(0.0018)
Number of violent arrests in 5 years prior	4510	0.0002	0.0001
		(0.0003)	(0.0006)
Number of drug arrests in 5 years prior	4510	0.0005	0.0011
		(0.0004)	(0.0008)
Number of financial arrests in 5 years prior	4510	0.0001	0.0002
		(0.0003)	(0.0007)
Arrested between 1990 and 2006	4510	0.0003	0.0005
		(0.0009)	(0.0018)
Age	4510	0.0109	0.0405
		(0.0312)	(0.0638)
Number of bedrooms	4510	0.0046**	0.0088**
		(0.0021)	(0.0043)
Male	3844	-0.0005	-0.0012
		(0.0007)	(0.0015)
Female	3844	-0.0003	0.0001
		(0.0009)	(0.0018)
Black	2612	0.0004	0.0009
		(0.0007)	(0.0015)
White	2612	-0.0001	0
		(0.0005)	(0.0011)
Other race	2612	-0.0004	-0.0009
		(0.0005)	(0.001)
Homeless at the time of admission	2612	-0.0001	0.000
		(0.0001)	(0.0002)
Percent black in Census Tract	3633	0.0008	0.0024*
		(0.0007)	(0.0014)
Median Rent in Census Tract	3629	-0.701*	-0.878
		(0.413)	(0.848)
Poverty rate in Census Tract	3632	-0.0004	-0.0009
		(0.0003)	(0.0006)
Unemployment rate in Census Tract	3633	0.0272*	0.0723**
		(0.0139)	(0.0283)
Median Household Income in Census Tract	3632	21.51	50.57
		(31.48)	(63.97)
Crimes per 1k population	2939	0.15**	0.409***
		(0.0653)	(0.136)
Violent Crimes per 1k population	2939	0.0195**	0.0539***
		(0.0086)	(0.0179)
Property Crimes per 1k population	2939	0.0436	0.111*
		(0.0291)	(0.0604)

Notes: Each cell represents a separate regression, estimating equation 4 with the listed covariate as the dependent variable. Unit of observation is an individual. Column 2 shows the coefficients of lottery number scaled down by 1000 and column 3 shows coefficients of the quarter in which the voucher is serviced (where Q1:2007 is indexed to one). Robust standard errors are presented in parentheses.
Significance: * 10% level; ** 5% level; *** 1% level

Table A4: Effect of vouchers on crime - By crime type for leaser sample

	First stage (with controls)	PLM	ITT (without controls)	ITT (with controls)	2SLS (with controls)
Voucher use	0.866***				
	(0.00376)				
All arrests		0.0055	0.000431	0.000395	0.000456
			(0.00100)	(0.000990)	(0.00114)
Violent arrests		0.0007	0.000701*	0.000690*	0.000796*
			(0.000361)	(0.000359)	(0.000414)
Drug arrests		0.0012	0.000168	0.000215	0.000248
			(0.000379)	(0.000376)	(0.000434)
Financial arrests		0.0007	0.000189	0.000155	0.000179
			(0.000442)	(0.000439)	(0.000506)
Property arrests		0.005	0.000110	0.000102	0.000118
			(0.000394)	(0.000392)	(0.000452)
Observations	81180		81180	81180	81180
Individuals	4510		4510	4510	4510

Notes: Each cell of each column represents a separate regression. Unit of observation is a person-quarter. Column 1 contains results where the dependent variable is an indicator for post lease-up. The pre-lottery means (PLM), mean of quarterly probability of arrest in the crime category from the year 2006, are shown in column 2. Intent-To-Treat effects are shown in columns 3 and 4. Two-stage least squares estimates are shown in column 5. The dependent variables in columns 3 to 5 are dummy variables indicating an arrest in the person-quarter for the particular category of offense. Controls include age at the time of the lottery, number of bedrooms and a dummy indicating an arrest in the crime category in the 5 years prior to the lottery. Robust standard errors, clustered at the individual level, are presented in parentheses.
Significance: * 10% level; ** 5% level; *** 1% level

Table A5: Effect of vouchers on crime - By crime type for non-leaser sample

	ITT	ITT (with interaction)
All arrests		
Post Voucher Service	0.000201	0.000246
	(0.000539)	(0.00054)
Ineligible		0.00391**
		(0.00165)
PVS*Ineligbile		-0.00294
		(0.00268)
Violent arrests		
Post Voucher Service	-0.000167	-0.000132
	(0.00016)	(0.00016)
Ineligible		0.00100*
		(0.00056)
PVS*Ineligbile		-0.00129*
		(0.00074)
Drug arrests		
Post Voucher Service	0.0000775	0.000161
	(0.00026)	(0.00026)
Ineligible		0.000777
		(0.00085)
PVS*Ineligbile		-0.00224*
		(0.00120)
Financial arrests		
Post Voucher Service	0.0000954	-0.00000759
	(0.00019)	(0.00019)
Ineligible		-0.0000552
		(0.00044)
PVS*Ineligbile		0.0023
		(0.00148)
Obervations	271998	271998
Individuals	15,111	15,111

Notes: The dependent variables are dummy variables indicating an arrest in the person-quarter for the particular category of offense. Controls include age at the time of the lottery, number of bedrooms and a dummy indicating an arrest in the crime category in the 5 years prior to the lottery. Robust standard errors, clustered at the individual level, are presented in parentheses.
Significance: * 10% level; ** 5% level; *** 1% level

Table A6: Test for attrition - Voter registration and voting in 2012

	(1)	(2)
Panel A	Registered	Voted
Lottery Number/1000	-0.000209	-0.000604
	(0.000506)	(0.000444)
Panel B	Registered	Voted
Quarter of Voucher Service	-0.000311	-0.00135
	(0.00105)	(0.000922)
Observations	19621	19621

Notes: Each cell represents a separate regression estimating the ITT models, according to equation 4 with indicators for being registered and having voted in 2012 as the dependent variables in columns 1 and 2, respectively. Unit of observation is an individual. Panel A shows the coefficients for lottery number scaled down by 1000 and Panel B shows coefficients for the voucher service quarter. Robust standard errors are presented in parentheses.
Significance: * 10% level; ** 5% level; *** 1% level

Table A7: Comparison of application and voucher use addresses for recipients across past arrest status

	Past Arrest			No Past History			Difference
	Pre-Lottery	Voucher-Use	Difference	Pre-Lottery	Voucher-Use	Difference	(Past-No Past)
Census Tract Characteristics							
Percent male	48.40	48.39	-0.01	48.07	47.91	-0.16	0.15
Percent white	27.80	32.16	4.36	31.37	34.93	3.56	0.8
Percent black	57.76	50.66	-7.1	53.15	47.27	-5.88	-1.22
Percent over 18 years	71.49	69.40	-2.09	70.28	69.39	-0.89	-1.2 ***
Median age	32.94	31.03	-1.91	31.62	30.73	-0.89	-1.02 **
Median rent	762.39	802.59	40.2	768.45	808.22	39.77	0.43
Unemployment rate	12.55	11.30	-1.25	12.17	10.96	-1.21	-0.04
Median household income	32599.15	34893.94	2294.79	33449.23	36199.00	2749.77	-454.98
Median family income	37579.15	38961.05	1381.9	37850.59	39838.74	1988.15	-606.25
Percent poverty	29.07	28.17	-0.9	29.16	26.75	-2.41	1.51 *
Observations	333	333		1360	1360		1693
Police Division Characteristics (Annual rates per 1000 population)							
Crime rate	136.73	133.79	-2.94	134.34	133.23	-1.11	-1.83
Murder rate	0.17	0.16	-0.01	0.16	0.16	0	-0.01
Violent crime rate	13.75	13.32	-0.43	13.37	13.13	-0.24	-0.19
Propety crime rate	57.72	57.53	-0.19	57.98	58.24	0.26	-0.45
Observations	244	244		853	853		1097

Notes: Statistics are shown for voucher recipients for whom both pre and post-lottery addresses were available and geocodable. Crime rates at the police division level are from 2000 to 2005.
Significance: * 10% level; ** 5% level; *** 1% level

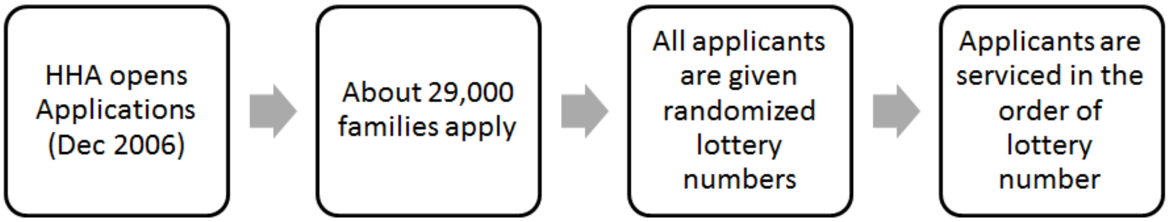
Table A8: Comparison of application and voucher use addresses for recipients across gender

	Male			Female			Difference
	Pre-Lottery	Voucher-Use	Difference	Pre-Lottery	Voucher-Use	Difference	(Male-Female)
Census Tract Characteristics							
Percent male	48.49	48.56	0.07	48.05	47.88	-0.17	0.24
Percent white	31.75	35.14	3.39	30.58	34.13	3.55	-0.16
Percent black	51.72	47.08	-4.64	54.26	48.25	-6.01	1.37
Percent over 18 years	70.07	69.71	-0.36	70.55	69.22	-1.33	0.97 *
Median age	31.77	30.45	-1.32	31.89	30.72	-1.17	-0.15
Median rent	762.62	765.76	3.14	767.38	806.30	38.92	-35.78 **
Unemployment rate	11.80	11.28	-0.52	12.23	10.98	-1.25	0.73 +
Median household income	32518.32	32930.90	412.58	33326.18	35747.65	2421.47	-2008.89 +
Median family income	36747.64	36809.11	61.47	37775.84	39382.04	1606.2	-1544.73
Percent poverty	30.34	30.25	-0.09	28.99	27.28	-1.71	1.62
Observations	135	135		1207	1207		1342
Police Division Characteristics (Annual rates per 1000 population)							
Crime rate	131.34	131.25	-0.09	135.55	133.96	-1.59	1.5
Murder rate	0.15	0.15	0	0.16	0.16	0	0
Violent crime rate	12.91	12.76	-0.15	13.54	13.25	-0.29	0.14
Propety crime rate	57.48	58.22	0.74	58.17	58.32	0.15	0.59
Observations	110	110		794	794		904

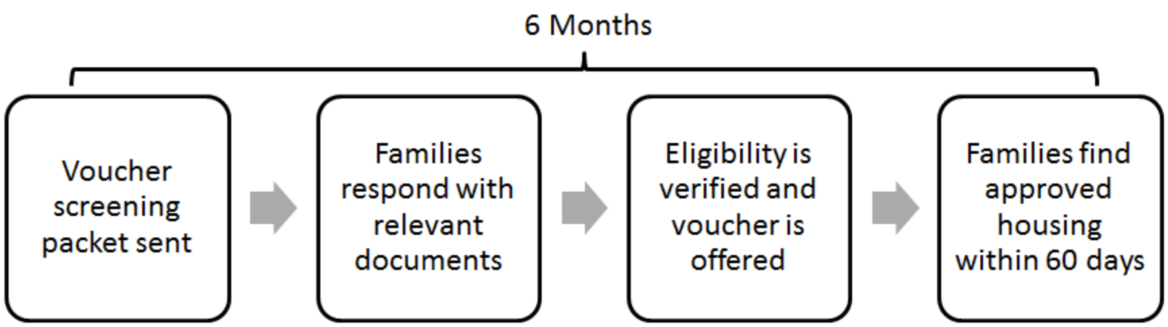
Notes: Statistics are shown for voucher recipients for whom both pre and post-lottery addresses were available and geocodable. Crime rates at the police division level are from 2000 to 2005.
Significance: + 15% level; * 10% level; ** 5% level; *** 1% level

Figure A1: Lottery and voucher service processes

(a) Lottery Process

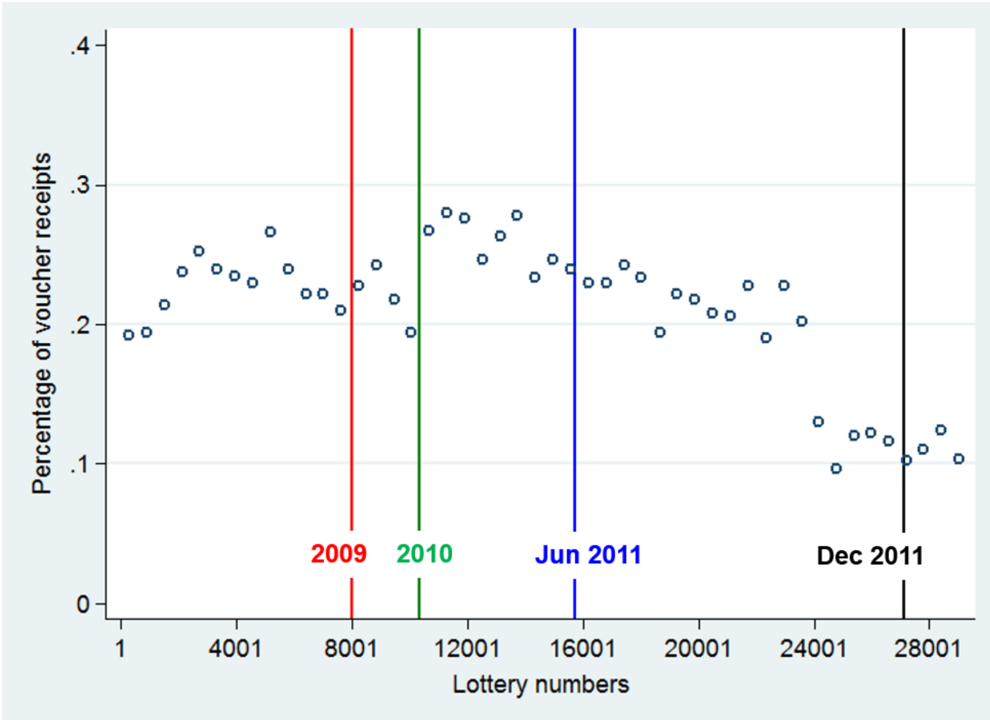


(b) Voucher Service Process



Notes: Voucher service process began in 2007 right after the lottery, but very few vouchers were serviced. The bulk of vouchers were serviced starting from 2009. The last of the vouchers were serviced in 2012.

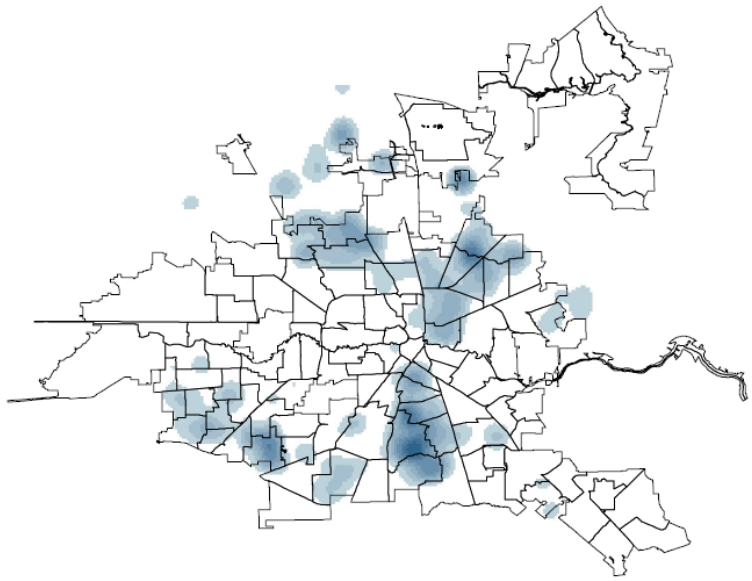
Figure A2: Take-up Across Lottery Numbers



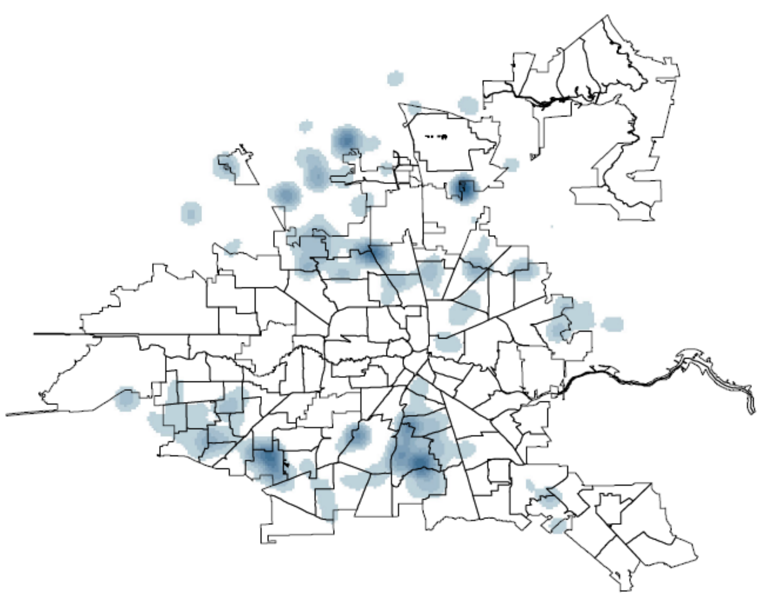
Notes: Each marker represents the percentage of lease-up within bins of about 500 applicants. The vertical lines indicate the timing of voucher service

Figure A3: Heatmaps of pre-lottery and voucher use addresses

(a) Distribution of Pre-Lottery Addresses



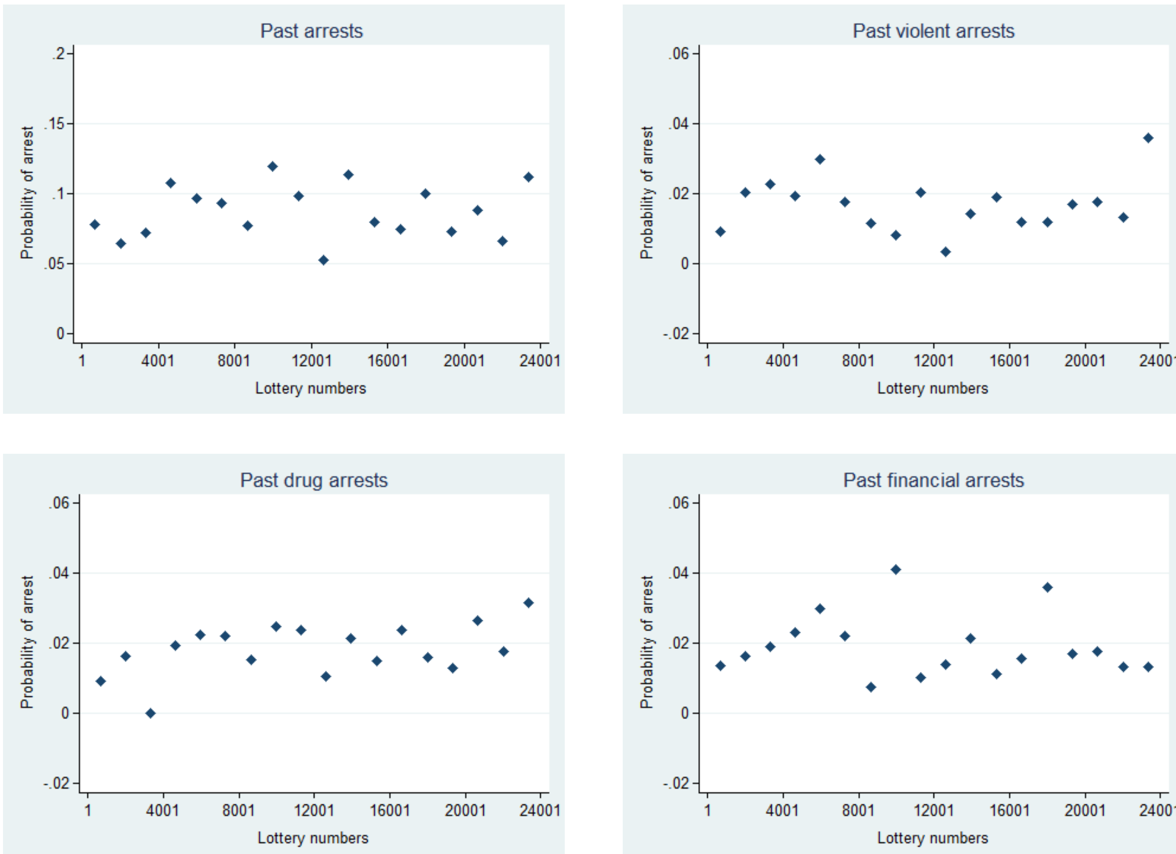
(b) Distribution of Voucher Use Addresses



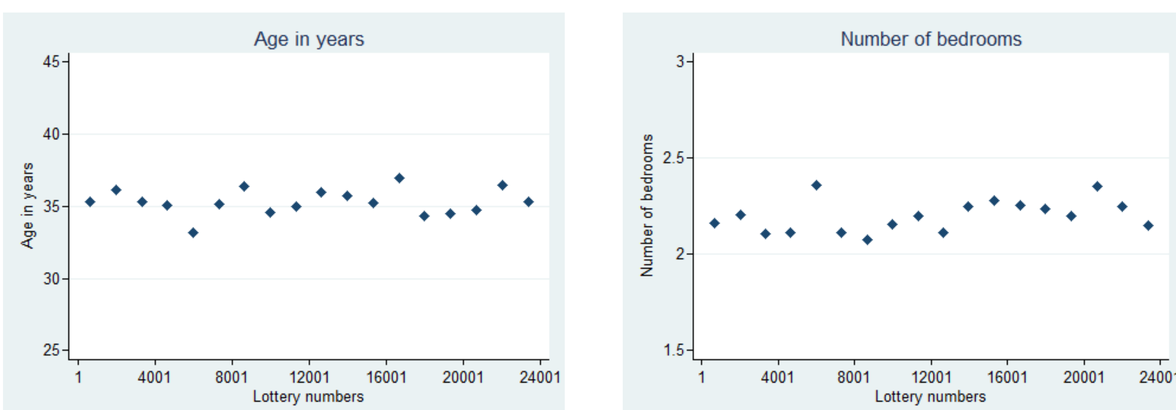
Notes: The heat maps are created in ArcMap using a point density operation that creates a grid over the map and then counts the number of address points within each grid cell. The outline indicates the boundaries of the police beats of the Houston Police Department.

Figure A4: Test of identification - Distribution of pre-lottery characteristics among leasers

(a) Criminal history

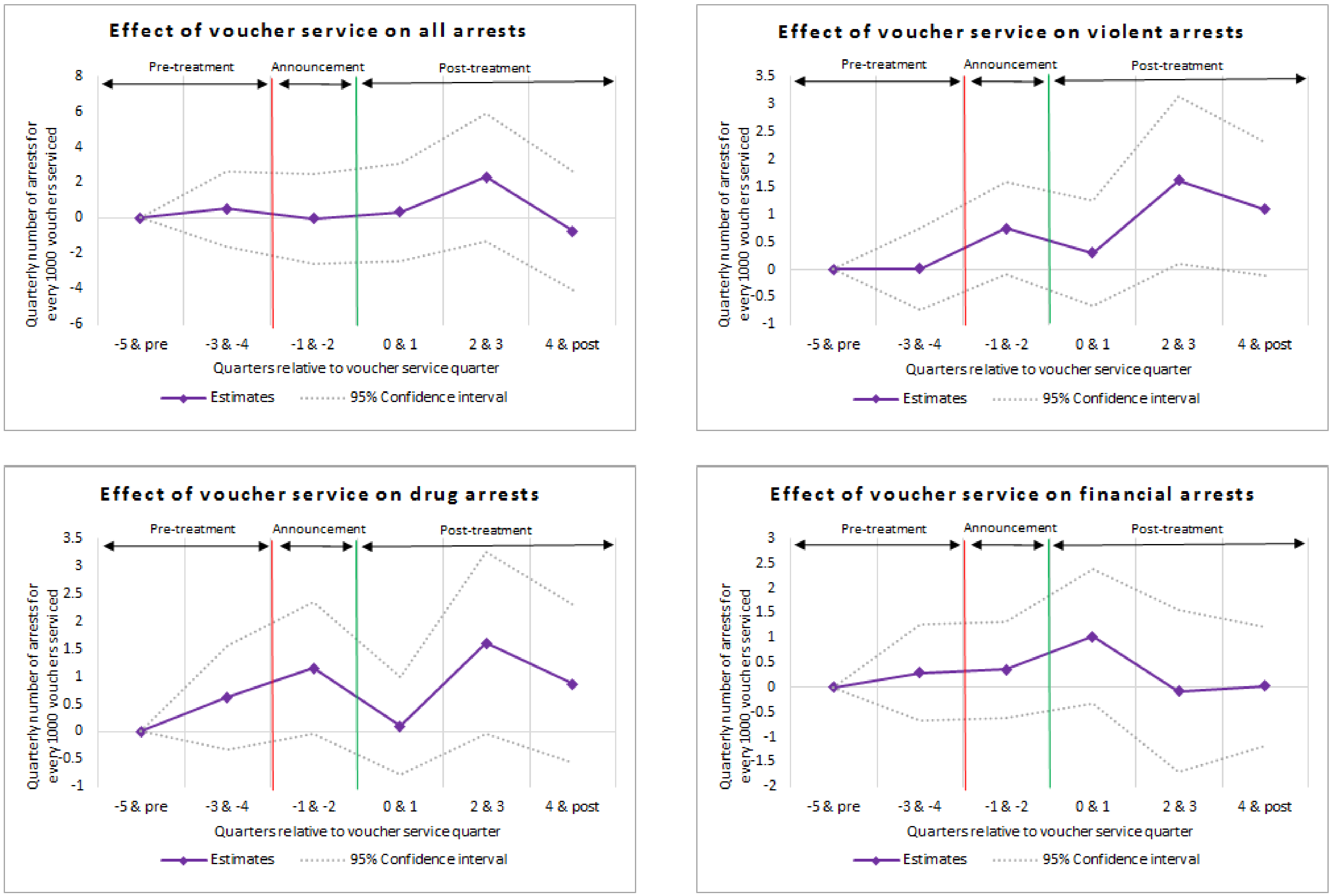


(b) Demographics



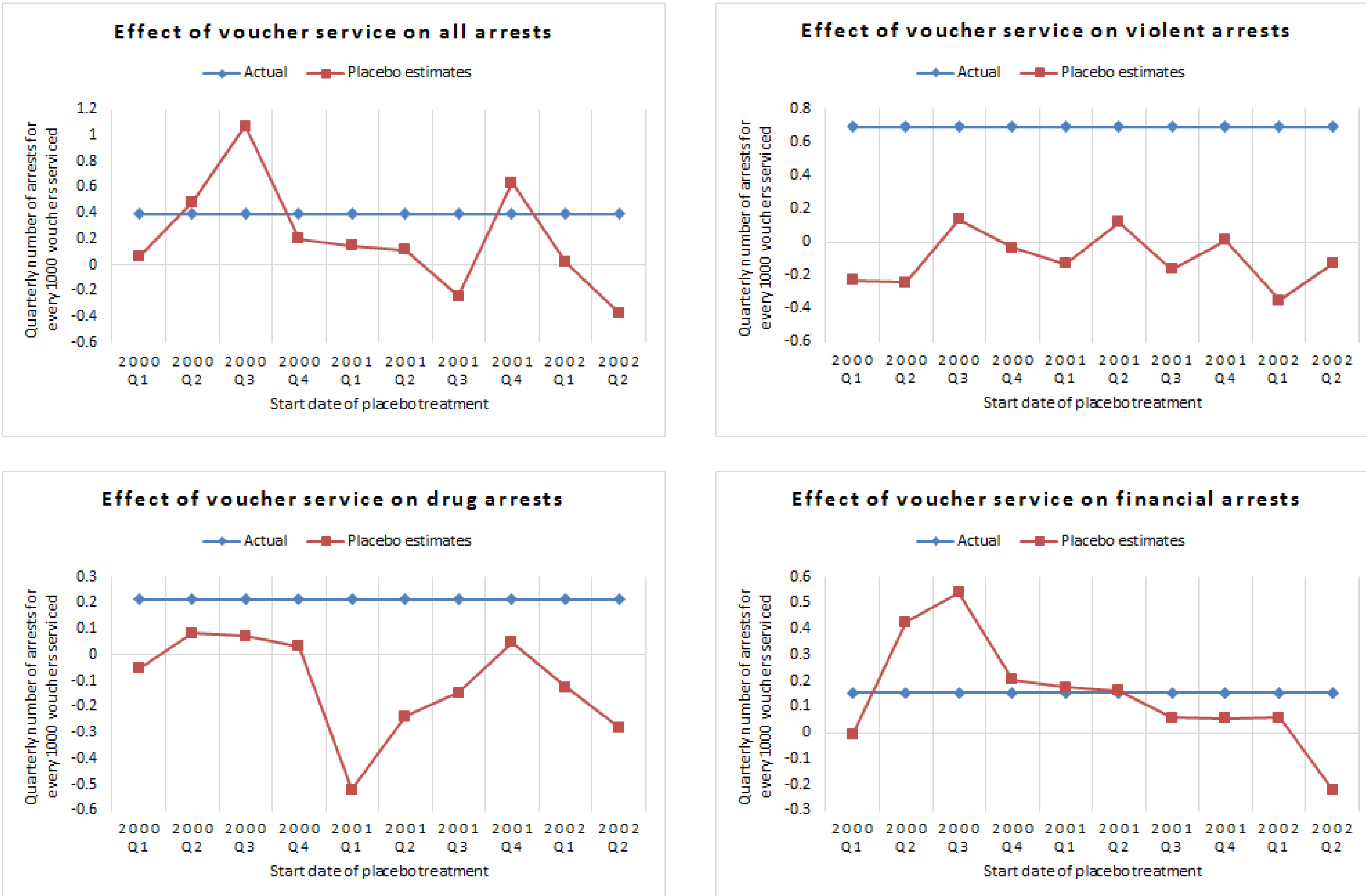
Notes: Each marker represents the local average of the variable within lottery number bins of about 250 leasers. Criminal history variables represent the probability of arrest in the crime category between 2002 and 2006 (5 years prior to the lottery).

Figure A5: Event study - Effect of voucher service on crime



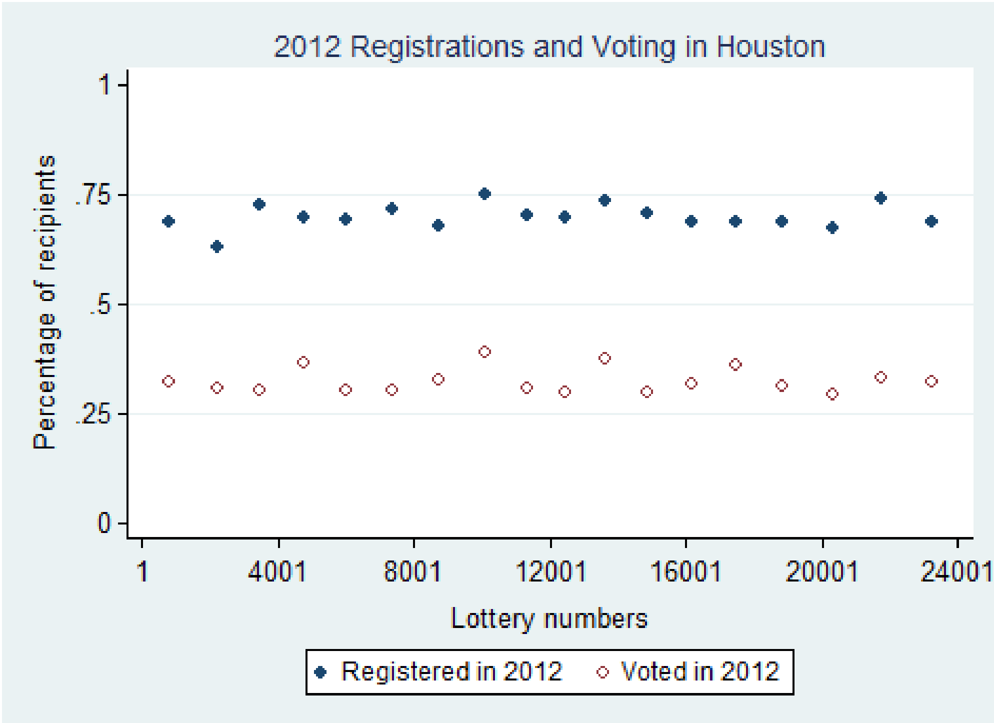
Notes: This figure plots the divergence between the treated and yet to be treated leasers before and after voucher service (intention to treat). The estimates were generated from the ITT model with the treatment variable split up by time since voucher service. The red and green vertical lines indicate the beginning of the voucher service process and enrollment into the program respectively. The points to the left of the red line show the divergence in the pre-treatment period. The point in between the red and green lines represents the announcement effect, and the points to the right of the green line represent the effect of voucher service.

Figure A6: Placebo treatment test - Effect of voucher service on crime



Notes: We conducted 10 placebo treatment tests by applying treatment with different start dates in the pre-treatment period. The estimates from these tests of the effect of voucher service (ITT) on arrests in different crime categories are presented by the red lines (square markers). As a comparison, the actual estimated effect is shown by the blue lines (diamond markers).

Figure A7: Test for attrition - Voter registration and voting in 2012 among leasers



Notes: Each marker represents the local percentage of recipients that were registered to vote and that voted in Houston in 2012 within bins of about 250 individuals.