LONG-TERM EFFECTS OF THE MOVING TO OPPORTUNITY RESIDENTIAL-MOBILITY EXPERIMENT ON CRIME AND DELINQUENCY

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ABSTRACT

Objectives: Using data from a randomized experiment, to examine whether moving youth out of areas of concentrated poverty, where a disproportionate amount of crime occurs, prevents involvement in crime.

Methods: We draw on new administrative data from the U.S. Department of Housing and Urban Development's Moving to Opportunity (MTO) experiment. MTO families were randomized into an *experimental group* offered a housing voucher that could only be used to move to a low-poverty neighborhood, a *Section 8 housing group* offered a standard housing voucher, and a *control group*. This paper focuses on MTO youth ages 15-25 in 2001 (n=4,643) and analyzes intention to treat effects on neighborhood characteristics and criminal behavior (number of violent- and property- crime arrests) through 10 years after randomization.

Results: We find the offer of a housing voucher generates large improvements in neighborhood conditions that attenuate over time and initially generates substantial reductions in violent-crime arrests and sizable increases in property-crime arrests for experimental group males. The crime effects attenuate over time along with differences in neighborhood conditions.

Conclusions: Our findings suggest that criminal behavior is more strongly related to current neighborhood conditions (situational neighborhood effects) than to past neighborhood conditions (developmental neighborhood effects). The MTO design makes it difficult to determine which specific neighborhood characteristics are most important for criminal behavior. Our administrative data analyses could be affected by differences across areas in the likelihood that a crime results in an arrest.

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INTRODUCTION

One of the most striking features of crime in America is its disproportionate concentration in disadvantaged, racially segregated communities. For example, in 2008 the homicide rate in Hyde Park, the economically and racially mixed neighborhood that is home to the University of Chicago, was 3 per 100,000. In adjacent Washington Park, where over half of residents are poor and 98 percent are African-American, the homicide rate was nearly 20 times as high. In its explanation of the riots of 1967, the Kerner Commission cited the destructive role of “the black ghettos where segregation and poverty converge on the young to destroy opportunity and enforce failure. Crime, drug addiction, dependency on welfare, and bitterness and resentment against society in general and white society in particular are the result” (Kerner et al., 1988, p.10). Neighborhood environments potentially can affect criminal behavior through several mechanisms, including peer groups and social interactions (Cook and Goss, 1996; Gaviria and Raphael, 2001; Glaeser, Sacerdote, and Scheinkman, 1996) as well as the quality of local public goods such as schools and police (Becker and Murphy, 2000).

A large body of non-experimental empirical research documents that youth and adults living in more disadvantaged, disordered neighborhoods are at elevated risk of engaging in crime even after statistical regression adjustment is made to account for individual-level observable socio-demographic characteristics and other risk factors. In a recent review of the “neighborhood effects” literature, Robert Sampson, Jeffrey Morenoff and Thomas Gannon-Rowley concluded that the evidence linking neighborhood processes to crime is stronger than evidence linking neighborhood processes to other outcomes such as health (2002). This non-experimental research raises the question of whether moving people out of areas of concentrated poverty can prevent them from becoming involved in criminal activity.

This question is difficult to answer with non-experimental methods because most families have some degree of choice over where they live and with whom they associate. There necessarily remains much uncertainty about the degree to which non-experimental studies are able to isolate the causal effects of neighborhood environments themselves on criminal behavior from the impacts of hard-to-measure individual or family attributes associated with both residential selection and criminal propensities. Experimental methods that generate exogenous variation in neighborhood environments can provide more-plausibly causal estimates of the impacts of neighborhoods on criminal behavior (for a summary of this methodological literature see Cook, Shadish, and Wong (2008).

In this paper we discuss what is known about the long-term effects of moving from a very distressed to a less-distressed neighborhood, drawing on new data from the U.S. Department of Housing and Urban Development's (HUD) Moving to Opportunity (MTO) randomized residential mobility experiment. A randomized lottery provided families—living in high-poverty public housing at five sites (Baltimore, Boston, Chicago, Los Angeles, and New York)—with the opportunity to move to lower-poverty neighborhoods with a housing voucher. We utilize administrative and survey data collected as part of the "long-term" study of MTO families measuring outcomes 10-15 years after baseline. Our paper builds on the "interim" MTO study by Kling, Ludwig, and Katz (2005), which focused on MTO youth – the age group at highest risk for crime involvement – and examined outcomes of those who were ages 15-25 at 4-7 years after baseline to answer three key questions: (1) Does moving youth out of areas of concentrated poverty prevent involvement in crime? (2) Do these neighborhoods effects vary over time? and (3) Do the effects differ by gender?

That study found that the offer of an MTO housing voucher reduced lifetime violent-crime arrests by 32% and property-crime arrests by 33% for females in the experimental group relative to their control group counterparts. In contrast, for males in the experimental group, the offer of a voucher increased lifetime property-crime arrests by 32% relative to controls and the associated reduction in lifetime violent-crime arrests was not statistically significant; however, in the first two years after random assignment, violent-crime arrests for males offered an experimental voucher were statistically significantly lower (by 34%) than for controls. On net across types of crime, females in the experimental group experienced a significantly lower (by 37%) number of arrests relative to controls while the slightly higher number of arrests for experimental group males relative to controls was not statistically significant. Using interim MTO data, Ludwig and Kling (2007) found that racial segregation appears to be the most important neighborhood characteristic in predicting youth violence involvement, more so than a neighborhood's rate of poverty or violent crime, which were generally not statistically significant predictors.

We present new results demonstrating that there is an attenuation over time of these MTO effects on male criminal behavior for the same cohort of youth examined by Kling, Ludwig, and Katz (2005). We examine new data on these youth covering 10 years after the time of random assignment.[1] Fade-out over the longer-term is apparent for MTO's interim adverse effects on property offending by male youth and for beneficial, protective effects in reducing youth

[1] Other studies of data from the long-term MTO follow-up find beneficial effects on adult physical health, specifically extreme obesity and diabetes (Ludwig et al., 2011) and on adult subjective well-being (Ludwig et al., 2012). See Sanbonmatsu et al. (2011) and Ludwig et al. (2013) for summaries of long-term MTO findings.

violence involvement. We present evidence suggesting that this fade out of MTO crime impacts for males appears to be partly explained by the attenuation of MTO treatment impacts on neighborhood conditions over time.

The dynamic pattern of MTO crime impacts seems more concordant with a behavioral model emphasizing contemporaneous rather than past neighborhood conditions in driving crime, or what Sampson (2012) calls "situational" rather than "developmental" neighborhood effects. The situational neighborhood effects hypothesis suggests that the offending behavior even of adolescents who have been exposed for many years to distressed neighborhood environments may respond to changes in community contexts.[2] Our results are consistent with some of the previous situational neighborhood effects research suggesting that even modest changes to the "in the moment" decision-making environment can substantially affect criminal behavior. Other examples include work by Ronald Clarke and others on 'situational crime prevention' (Clarke, 1995; Homel and Clarke, 1997; Cornish and Clarke, 2003), the discussion by Zimring (2011) of the determinants of New York City's crime drop, and the recent cognitive behavioral therapy study by Heller et al. (2013).

The next section briefly reviews the candidate mechanisms through which neighborhood environments might influence crime and violence. The third section provides a very brief review of previous non-experimental studies. Section four discusses the MTO experiment, section five

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[2] Consider a model in which criminal behavior in time period T, Y_T, is potentially affected by someone's entire accumulated history of exposure to different neighborhood conditions: $Y_T=f(X_T, X_{T-1}, \ldots, X_0)$. Criminal behavior could be affected by neighborhood conditions in time T, X_T ("situational neighborhood effects") and/or by neighborhood conditions in some previous period, X_{T-K} ("developmental neighborhood effects").

discusses our data sources and analytic methods, section six presents our main empirical findings, and the final section discusses the implication of our results for public policy and other attempts to carry out long-term follow-ups of randomized social experiments.

CANDIDATE MECHANISMS

Many theories of crime suggest that people engage in crime because they lack impulse control, future orientation or other non-academic factors (what researchers variously call "non-cognitive" or social-cognitive skills), or because people have poor earnings prospects in the legal labor market (perhaps due to low levels of either academic or non-academic skills) and so view criminal behavior as a superior alternative (Gottfredson and Hirschi, 1990; LaGrange and Silverman, 1999). These theories imply that the accumulated exposure of young people to neighborhood environments is important in affecting risk of criminal involvement by changing people's development of academic or non-academic skills.

Neighborhood environments may shape the developmental environments in which children grow up partly by affecting the quality of local public schools, which previous research suggests varies dramatically across areas (Rivkin, Hanushek, and Kain, 2005). Neighborhood social conditions may also affect the developmental environments children experience if for example neighborhood adults or peers generate human capital externalities (Borjas, 1995), or if local adults act as role models that change the incentives that young people perceive for investing in human capital (Wilson, 1987).

MTO could have effects on risky or criminal behavior mediated through MTO's impacts on household environments important for child development. Parental unemployment, substance use, poor mental health, exposure to community violence, and inadequate housing may be risk factors for child maltreatment. Each of these risk factors could be affected by MTO. Previous research also suggests that neighborhood social conditions (such as crime and violence) may change the way that parents monitor and supervise their children (Furstenberg et al., 1999).

These "developmental neighborhood effects" hypotheses imply that MTO children who are relatively younger at baseline (and so will experience more exposure to new neighborhood environments at any given follow-up point) should be more affected by MTO moves than those who are older at baseline. This is particularly true if there are developmentally "sensitive" or "critical" periods (Knudsen et al., 2006; Shonkoff and Phillips, 2000; Shonkoff et al., 2012). Sensitive periods are hypothesized to be ages in which development of certain skills or developmental processes are particularly susceptible to environmental influences, which in our case is the social environment of the neighborhood, but are not necessarily the only times in which those skills or processes can be modified. Critical periods are thought to be ages at which some skills or developmental processes are shaped, after which they are largely fixed.

Other neighborhood effect theories implicitly argue instead that contemporaneous neighborhood environments should be most important for affecting criminal behavior by young people. For example, urban planners and criminologists have been concerned with the possibility that some building designs contribute to crime, for example, through the construction of enclosed stairwells in public housing buildings that are difficult for local residents to monitor. Neighborhoods may also vary in the willingness of local adults to monitor public space and enforce shared values as in the "collective efficacy" model of Sampson, Raudenbush, and Earls (1997) (see also Coleman, 1988).

It is possible that high-crime neighborhoods might have policing of lower quality or quantity compared with more affluent areas. Whether or not such variation in policing exists depends in part on the degree to which policymakers choose to allocate additional police resources to the highest-crime neighborhoods (Sherman, 2002), as well as on the intensity of patrol activities in different areas, the degree to which community members are willing to work with police to solve cases, and how police use their discretion to make arrests. If youth move to neighborhoods with more or more intensive policing, MTO could increase the likelihood of a youth being arrested for any given level of actual criminal activity. Previous research raises the possibility that minorities may be at particularly elevated risk for being arrested, even after controlling for frequency of involvement with criminal behavior and other risk factors such as neighborhood type, family income, and educational history (Huizinga et al., 2007).

A large literature suggests that contemporaneous peer influences should affect decisions about engaging in crime, or what Jencks and Mayer (1990) call "epidemic models," which emphasize the power of peers to spread behaviors. Such contagion effects can arise from learning criminal behavior from peers, pure preference externalities (individuals enjoy imitating their peers), stigma effects and social norms (the negative signal from criminal behavior declines when more people do them), and physical externalities (for example, higher rates of crime reduce the chances of getting arrested because of congestion effects in law enforcement); see Kleiman (1993), Cook and Goss (1996), Glaeser and Scheinkman (1999), Brock and Durlauf (2001), Manski (2000), and Moffitt (2001). Some epidemic models predict peer influences on youth criminal behavior that vary with the prevalence of peer criminal behavior within a community, potentially leading to nonlinearities in peer effects or "tipping points."

Although most behavioral models predict that MTO moves should reduce youth involvement with risky or criminal behavior, Jencks and Mayer's discussion implies that other outcomes are possible. *Competition models* emphasize the competition between neighbors for scarce resources like grades or jobs. Failure in the competition for pro-social rewards may lead youth to compete instead for anti-social rewards, which could elevate risk for criminal involvement. *Relative deprivation* models focus on negative psychological impacts from experiencing a decline in one's relative material or social standing (Luttmer, 2005). These theories about the potential adverse effects of moving into a less distressed, disadvantaged neighborhood also seem to emphasize contemporaneous over cumulative or lagged neighborhood conditions experienced by young people.

Kling, Ludwig, and Katz (2005) discuss possible explanations for the gender differences they observe for MTO's impacts on criminal offending. Male and female youth might move to different neighborhoods. Minority males might face greater discrimination than females in their new neighborhoods or they might sort into higher risk peer groups. Males might adopt a more confrontation strategy in adapting to their new environments whereas females might turn to adults for support. Lastly, males may be more likely than females to take advantage of their comparative advantage in their new neighborhood to commit property offenses. In their view, the comparative advantage in property offenses is the explanation that seems most likely.

PREVIOUS NON-EXPERIMENTAL FINDINGS

The non-experimental empirical literature reveals mixed results on the importance of these theoretical neighborhood mechanisms in affecting risky and criminal behaviors. Case and Katz (1991) found strong relationships between one's own risky and delinquent behaviors and that of one's peers for illegal drug use, alcohol use, and criminal offending in the Boston Youth Survey. However, Esbensen and Huizinga (1990) found that the level of disorganization of the neighborhood was not associated with neighborhood-level prevalence or frequency of drug use.

Studies of a sample of young black women in Chicago found some relationship between pregnancy risk and low neighborhood socioeconomic status (Hogan and Kitagawa, 1985) and evidence that this risk was related to lower contraceptive use (Hogan, Astone, and Kitagawa, 1985). The proportion of managerial workers in a census tract has been shown to be related to teen childbearing (Brooks-Gunn et al., 1993; Crane, 1991), but Case and Katz did not find direct evidence of peer influences on out-of-wedlock childbearing.

The non-experimental research provides stronger support for an association between neighborhood attributes and involvement with crime or violence. An influential non-experimental study on this question is Sampson, Raudenbush, and Earls' (1997) analysis of data from the Project on Human Development in Chicago Neighborhoods (PHDCN). Their analysis found one of the best predictors for involvement in violence is a neighborhood's degree of informal social control combined with social cohesion and trust—what they term "collective efficacy."

Collective efficacy is found to have a robust association with violence even after controlling for a rich set of individual-level characteristics, and seems to mediate the effects of other neighborhood attributes such as socioeconomic composition (Sampson and Raudenbush, 1999; Morenoff, Sampson, and Raudenbush, 2001; and Sampson, Morenoff, and Raudenbush, 2005). Similar patterns of results have been reported in multiple studies including Hirschfield and Bowers (1997), Warner and Rountree (1997), Bellair (2000), Wikström and Loeber (2000), Beyers et al. (2001), and Simons et al. (2004). Sampson, Morenoff, and Gannon-Rowley (2002) provide an excellent survey of such studies. A more recent observational study drawing on PHDCN data presents suggestive evidence of a non-linear relationship between youth violence involvement and exposure to violent peers, where the effect of exposure to additional violent peers declines at higher levels of peer violence (Zimmerman and Messner, 2010).

THE MOVING TO OPPORTUNITY EXPERIMENT

The MTO experiment was designed to test the impact of moving families living in public housing projects in the most disadvantaged neighborhoods in American cities into neighborhoods with much lower poverty rates using private housing vouchers. Eligibility was limited by design to families living at baseline in public housing units operated by local government housing authorities. In 1994 HUD began randomly assigning eligible low-income families with young children who volunteered to participate in MTO into three different groups:

- The *experimental group* (or low-poverty voucher group) was offered a housing voucher that could only be used in neighborhoods where the poverty rate was 10 percent or less according to the 1990 census.[3] This group was also provided counseling to help locate an appropriate unit and neighborhood.

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[3] Olsen (2003, pp. 365-441) provides an excellent review of the housing voucher program, which provides families with a subsidy to live in private-market housing. The maximum voucher subsidy is determined by the Fair Market Rent (FMR), which is a function of family size, the gender mix of adults and children in the home, and the local rent distribution. For a family of four, the FMR is between 40 and 50 percent of the local metropolitan area private-market rent distribution. For example, the FMR for a two-bedroom apartment in the Chicago area was equal to $699 in 1994, $732 in 1997, and $762 in 2000. Families are expected to pay 30 percent of their income (adjusted by family size, childcare expenses and medical expenses) towards their rent. Note that in the United States, housing assistance is not an entitlement, so housing voucher (and other housing) programs usually have long wait lists. Olsen estimates that only around 28 percent of income-eligible families in the U.S. receive any housing assistance.

- Families assigned to the *"Section 8" housing group* (or traditional voucher group) were offered standard housing vouchers that could be used for any unit that met basic standards, but were not restricted geographically.
- The *control group* did not receive any special MTO funding, but could receive any of the regularly available social services for which they would have been eligible regardless of the experiment.

In total, 4600 families representing about one-quarter of the universe of eligible families in the target public housing projects signed up for MTO between 1994 and 1998 (Goering et al., 1999, Table 5). Although all families assigned to the experimental and Section 8 groups were offered housing vouchers, only around 47 percent in the experimental group and 63 percent in the Section 8 group used the voucher to move. Despite less than complete compliance with the MTO treatment, the random assignment of MTO families into treatment and control groups alleviates concerns about selection effects (see further discussion below). Furthermore, these utilization rates are consistent with rates observed in studies of other housing voucher programs (Olsen, 2003; Rubinowitz and Rosenbaum, 2000). Some families did not use their voucher because they could not find an affordable unit within the time limits of the program. Finding affordable housing may have been particularly challenging for families in the experimental group because they were restricted to searching in low-poverty census tracts. The tight housing markets of some of the MTO cities during this time period also contributed to the difficulty of finding suitable units.

A baseline survey that HUD administered to all families when they applied for MTO provides baseline characteristics of the adults who were assigned to the three different groups. Averaged across the five MTO study sites, about two-thirds of all the participants were African-

American and about one-third were Hispanic. However, in two of the sites, Chicago and Baltimore, the participating families were overwhelmingly African-American, while the other three demonstration sites (Boston, Los Angeles, and New York City) participants were more evenly mixed between African-American and Hispanic.

Concerns about crime and safety play an important role in motivating families to participate in MTO. Around 40 percent reported living with someone who had been victimized in the last six months. Three-quarters of the respondents reported a desire to get away from drugs and gangs; half reported they wished to find better schools for their children as their primary or secondary reason for moving.

Table 1 presents the baseline characteristics of the sample of youth that is the focus of this paper – those who were ages 15-25 at the end of 2001 and ages 17-28 as of the end of the follow-up period for this paper, or ten years after random assignment (N=4,643). As detailed below, these youth were not eligible for the youth survey that was part of the MTO long-term (10-15) evaluation because they were over age 20 as of December 2007 – the point at which eligibility for the survey sample was determined. In other published work on the long-term evaluation, this group is known as "grown children". This is essentially the same cohort studied using data from the interim MTO evaluation (4-7 year follow-up) by Kling, Ludwig, and Katz (2005), although there are a few differences in the two samples. However, we have added youth whose families were randomized in 1998, the last year of MTO recruitment. In addition, we have excluded a small number of youth who were 18 or older at baseline, and we have decided to include youth even if they moved to other jurisdictions at some point after random assignment. As expected, due to the random assignment of families to the treatment and control groups, baseline characteristics of youth generally do not differ by treatment status. An omnibus F-test fails to reject the null hypothesis that the full set of baseline characteristics controlled for in our analysis (listed in Appendix Table 1) is the same for youth in the experimental group and the control group (*P*=0.74) or the Section 8 group and controls (*P*=0.43).

The distribution of behavioral outcomes is quite different for boys versus girls. At baseline, parents were more likely to report behavioral and emotional issues for boys than for girls. Nearly 20% of boys (compared with some 10% of girls) had recently been suspended or expelled from school. Behavioral/emotional, learning, and physical health problems were all about twice as prevalent among boys as girls. Violent crime arrests were somewhat higher for experimental and Section 8 boys than for controls, but pre-random assignment differences could occur by chance. We also control for pre-random assignment arrests in our analysis, although as mentioned above, there is no overall pattern of pre-randomization differences between the control group and either the experimental or Section 8 group. Because previous MTO studies have found that boys and girls also respond differently to MTO-assisted moves into less distressed neighborhoods (Kling, Ludwig, and Katz, 2005; Kling, Liebman, and Katz, 2007), we follow previous MTO work and analyze the data separately by gender.

DATA AND METHODS

This section discusses the data sources analyzed in the present paper, which come from longitudinal arrest records and in-person parent surveys collected for the long-term MTO evaluation to measure outcomes through 10-15 years after the time of random assignment. Given the randomized experimental design, our analysis methods are straightforward. We begin by presenting “intention to treat” (ITT) estimates, which represent the effects of being *offered* the chance to move through MTO. Since not all families offered a MTO voucher actually moved through the program, the ITT will understate the effects of actually moving through MTO. Accordingly, we also present estimates that use the randomized treatment group as an instrument for actually moving with an MTO voucher to estimate the effects of moving for those families who actually moved—the so-called "treatment on the treated" (TOT) estimates.

Data Sources and Measures

Our data on criminal behavior are derived from administrative arrest records. Information on potential mediating processes comes from constructing an address history for each youth and linking these addresses to census tract data.

Arrest Data. We use administrative arrest records to construct measures of the number of times that a youth was arrested between the MTO random assignment and the end of the tenth year (fortieth quarter) after random assignment.[4] Arrest records include information on the date of all arrests, each criminal charge for which the individual was arrested, and typically information on dispositions as well.

Because studies of criminal behavior often find that interventions can have qualitatively different impacts on different types of offenses, we analyze MTO impacts on total arrests as well as arrests for specific types of crimes, with a focus on violent and property offenses. As in most samples our violent offense category is dominated by assault (simple or aggravated) and to a

[4] Another difference between the present study and Kling , Ludwig, and Katz (2005) is that the interim study counted by quarter from the specific date of random assignment whereas in our study we set the quarter of random assignment to "quarter 0" and the next calendar quarter to "quarter 1".

lesser extent robbery (where the perpetrator uses or threatens force) and a much smaller number of arrests for the more serious crimes such as murder, rape, or kidnapping. Larceny or thefts that do not involve contact with the victim were the most common type of property offense with other types including burglary, breaking and entering or trespassing, and motor vehicle theft. Total arrests includes both violent and property crime as well as other types of crime such as drug offenses, disorderly conduct, vandalism, and weapons violations (such as carrying a gun illegally). If a youth was charged with more than one offense on the date of the arrest, we characterize the arrest using the most serious charge. We select the most serious charge based on New York State criminal law classification because New York provides us with information on only the most serious criminal charge per arrest. Thus applying New York's criteria to other states (where we have all charges associated with each arrest) helps improve data consistency across states. We obtain results similar to those reported below using other methods for selecting the most serious charge per arrest, in part because the majority of arrests involve charges for only a single criminal offense. Arrests that occurred prior to randomization are controlled for in the statistical analysis (that is, used as explanatory variables), but were not included in the outcome measures.

We attempted to match youth to both adult and juvenile arrest records using information such as name, social security number, birth date, sex, and race. We obtained individual-level adult and juvenile records the states of California, Illinois, Maryland, and Massachusetts; de-identified adult data from New York State (where de-identified records were randomly assigned to respondents with the same treatment status, gender, and year of birth); juvenile data from New York City Department of Probation, which should capture juvenile arrests that occur within the city; and adult or juvenile records from 8 additional states in which participants have lived. New York's criminal justice system classifies arrestees as "adults" at a very young age (16), so a substantial proportion of all teen arrests will be included in the adult arrest histories for this state. The detailed information available with these arrest histories enables us to focus on program impacts for different types of criminal offenses.

As a sensitivity analysis we relax the assumption that the effect on arrests is the same across youth who entered the program at different ages, given a growing body of research in developmental psychology and related fields that raises the possibility that developmental plasticity may decline with age. We re-estimate these impacts interacting baseline age with treatment status and gender.

Neighborhood characteristics. For each youth we constructed an address history using information from MTO program operations, the U.S. Postal Service's National Change of Address system, local housing authorities, HUD administrative records, in-person tracking of youth at the interim follow-up, and from interviews conducted with their parent (or other adult in the family). Youth analyzed in this paper were ages 21 to 30 at the time of the final evaluation (2008-10) and were not part of the interview sample. Although we did not interview these youth, we did ask the adult respondent whether the youth still lived with them and, if not, for their current address. For about 70% of the youth, we interviewed their parent or another adult member of the household during the MTO final evaluation. Consistent with our initial concerns about the difficulty of locating these youth for interview, three quarters of the adults reported that the youth no longer lived with them and half could not (or declined to) provide a current address for the youth. And in fact we are missing a long-term survey address for even more of these youth (about 70%) because we do not have proxy report of addresses for youth in families where the adult was not interviewed. In some instances, we also obtained address updates at final for these youth through HUD administrative records and mailings to the families.

We built on the algorithm that Abt Associates developed for the interim MTO study to determine which addresses were reliable and to stitch together the addresses into a continuous address history that approximates when and how long the youth lived at each address (Orr et al., 2003). Creating a continuous address history required certain assumptions, namely that the youth remained at their last known address through the end of the follow-up period. Therefore, we assume that youth for whom we were unable to obtain an address update as part of the long-term survey (and for whom no HUD administrative address updates were available) continued living at their address from the interim survey.

The neighborhood unit that we analyze in this paper is the census tract, so we geocoded the addresses in the history and then linked them to census tract characteristics for that time period interpolated from the 1990 and 2000 U.S. decennial censuses and the 2005–09 five-year averages from the American Community Survey. Census tracts are geographic areas defined by the U.S. Census Bureau that typically contain 2,500 to 8,000 residents, with boundaries that were originally drawn to be "homogenous with respect to population characteristics, economic status, and living conditions" (U.S. Census Bureau, 2000). We use census tract poverty rates (the fraction of persons in the tract living below the poverty threshold) as our primary measure of neighborhood characteristics because the MTO program was explicitly designed to change the poverty rate of program participants' neighborhoods. Given the strong correlation of poverty with other measures of neighborhood socioeconomic composition, the poverty rate can be viewed as a proxy for the bundle of neighborhood characteristics that are changing. In addition to poverty, we analyze the share of tract residents who are members of racial or ethnic minority groups, the share of adults who are college-educated, the share of families headed by single females, the shared of the civilian population who are employed, and average household income (in 2009 dollars). For each two-year period after random assignment we present estimates of treatment effects on neighborhood share poor and share minority to analyze the dynamics of the impact of MTO on neighborhood conditions.[5] Families enrolled in MTO from 1994 to 1998, and therefore the calendar period represented by each two-year period after random assignment varies across families.

Although this paper focuses on census tract as the neighborhood unit, an advantage of MTO over non-experimental neighborhood effect studies is that we do not need to clearly specify the neighborhood unit because the randomized MTO intervention generates differences in the neighborhood conditions experienced by MTO families assigned to the treatment and control groups for almost all of the candidate geographic areas definitions that have been used in the previous literature (e.g. block, block group, tract, ZIP code). Consider, for example, if the key driver of criminal behavior is the poverty rate in a youth's apartment building, and not in the rest of the census tract. In a non-experimental study, if we include the wrong neighborhood characteristic as an independent variable in a regression model predicting criminal behavior (or another outcome), we might erroneously conclude that neighborhood effects do not matter. Random assignment assures that our estimates of the impacts of the MTO program (see next section) do not depend on correctly defining the neighborhood unit. Nonetheless, the lack of

[5] We also present estimates of treatment effects on a broader range of neighborhood characteristics averaged (using duration weights) across all addresses between random assignment and May 31, 2008, just prior to the start of the MTO long-term survey fielding period.

clarity about the correct definition of neighborhood does limit our ability to estimate the effect of a unit change in a given neighborhood condition on an outcome of interest (e.g. criminal behavior).

Since this is a special issue of the journal devoted to long-term follow-ups of participants in randomized experiments, it is worth mentioning the challenge of successfully tracking low-income families over such an extended period (10-15 years after baseline). To carry out the surveys our research team subcontracted with the Survey Research Center (SRC) at the University of Michigan. The success of the long-term surveys was due in large part to the tremendous skill of SRC, which also employed two-stage sampling (Groves et al., 2004) to increase the efficiency of the data collection effort and managed to achieve an 88-90% effective response rate (ERR) for MTO youth and adults that was quite similar across randomized MTO groups. Our ERR calculation weights up cases from the second phase of fielding in which a subsample of hard-to-reach cases were selected for additional outreach efforts (see Gebler et al. (2012) for further details on SRC's participant tracking and recruitment efforts and special end-game strategies). This success derived from HUD's foresight in supporting the tracking of MTO families over the entire study period, including periodically supporting active tracking (canvassing) efforts. Such tracking activities proved to be an astute investment that could be a model for other sponsors of social experiments.

We do not have self-reports for these youth from the MTO 10-15 year evaluation because they were not part of our sample frame. One challenge with long-term follow-up studies is how to optimally allocate finite data-collection resources across program participants. Even with the generous support of a long list of funders (see acknowledgements below), we were forced to choose which two of the following three study populations we would survey: (a) parents in the

MTO households; (b) the youth who were the focus of the interim (4-7 year) follow-up who were now mostly grown children living apart from their parents; or (c) those participants who were young children at the time of the interim study but by the time of the long-term follow-up had entered their peak offending ages. We focused on samples (a) and (c) because it allowed us to economize on survey costs as the younger youth were more likely to be still living at home and because it allowed us to for the first time measure a number of key outcomes for youth who were under age six at the time their families enrolled in the program. These youth were of particular interest because they experienced very large neighborhood changes during the most developmentally sensitive time period. The tradeoff, however, in focusing on this group was to lose observing the same cohort of youth over the two consecutive follow-ups. However, to mitigate this loss, we did collect administrative data such as the arrest histories and we also asked the adult interviewed in the household about the older youth.

We test the sensitivity of our estimates of impacts on neighborhood characteristics to the types of address information available for the youth. We re-estimate our model using only those individuals for whom we have fairly complete address information because their parent provided us with an update on them during the final evaluation. We also look at whether the results differ if we limit our analyses to those youth who were still living with their parent. Lastly, we re-estimate the data using only the addresses of the youth up through age eighteen and assuming the youth's addresses remained unchanged beyond age 18. This approach is similar to the overall approach of creating a continuous address history from data that includes gaps by definition (described above). In some cases the address at age 18 will in fact be the same as the interim survey address, meaning for some youth their last known address was their address at ages as young as 15.

Analytical methods

The random assignment of families to different treatment conditions allows us to identify the causal effects of being offered a housing voucher by comparing the average outcomes of youth assigned to each treatment group (experimental or Section 8) with youth assigned to the control group.[6] We focus our analyses on the intent to treat (ITT) effect or the effect of the offer of services through MTO for the entire treatment group, which consists of both families who took up the treatment (i.e. used the voucher) and those who did not. Let Y represent an outcome of interest. We estimate a model using data from all three MTO groups with two separate indicators for assignment to the experimental and Section 8 groups: Z_{exp} and Z_{s8}. We calculate the ITT effects of assignment to the experimental and Section 8 groups as the two elements of π_1 in equation (1) using ordinary least squares, conditioning on a set of (pre-random assignment) baseline characteristics (X), where *i* indexes individuals.

$$(1)\ Y_i = Z_{exp,i}\pi_{10} + Z_{s8,i}\pi_{11} + X_i\beta_1 + \varepsilon_{1i}$$

The control group is the omitted category in the model, and the treatment group indicators, Z_{exp} and Z_{s8}, represent the average difference between the control and treatment groups. Baseline characteristics include site, socio-demographic characteristics about the household and youth,

[6] The offer of a housing voucher in MTO is the chance to move to a new neighborhood characterized by a range of different socio-demographic, physical, and other features. As discussed below, MTO is less informative about the causal effects of particular elements within the bundle of neighborhood characteristics that change via MTO moves, but it does provide very strong grounds for inference about the causal effects of changing that bundle of neighborhood features.

and youth experiences in school such as expulsions or enrollment in gifted and talented classes, and in our models of arrests we also include indicators for the number of arrests prior to random assignment for violent, property, and drug or other offenses. We show the complete list of covariates in Appendix Table 1. In our analyses we apply sampling weights (individuals within treatment groups are weighted by their inverse probability of assignment to the group to account for changes in the random assignment ratios) and cluster the data to adjust the standard errors for the presence of multiple youth from the same family. Significance levels are reported using two-tailed hypothesis tests.

Because this is a special issue dedicated to understanding how the effects of social experiments evolve over time, we present the results of estimating equation (1) separately for different two-year windows following baseline (that is, 1-2 years after randomization, 3-4 years after randomization, etc., up to 9-10 years after randomization). For completeness we also present results that show outcomes averaged over the entire 1-10 year post-randomization period.

To examine whether treatment effects vary by gender, we modify equation (1) to include interactions between the treatment indicators and male and female gender:

$$(2)\ Y_i = Male*Z_{exp,i}\pi_{20} + Female*Z_{exp,i}\pi_{21} + Male*Z_{s8,i}\pi_{22} + Female*Z_{s8,i}\pi_{23} + X_i\beta_2 + \varepsilon_{2i}$$

An indicator for male is also included as an element of X. In equation (2), the effect of the experimental group treatment for males is represented by π_{20} and for females is represented by π_{21}. We also present estimates of the difference between the estimated effects for males and females ($\pi_{20} - \pi_{21}$).

Note that while the ITT estimate will understate the effects of actually moving through MTO, since around 42 percent of the families of youth assigned to the experimental group and

58 percent of those assigned to the Section 8 group relocated with an MTO voucher, the ITT still constitutes an unbiased estimate of the effects of *offering* families the chance to move through MTO. Because the ITT estimator compares the average outcome of the control group with the average outcome of *all* families assigned to one of our two treatment groups, regardless of whether the family assigned to the treatment group relocates through MTO or not, the ITT estimate is not susceptible to concerns about "selection bias" that plague non-experimental estimation approaches. Random assignment ensures that, absent the MTO intervention, control and treatment group families would have had the same outcomes, on average, and that any post-baseline differences between the two groups can be attributed to the intervention itself. Thus, the ITT estimate reflects the effects of *being offered* an MTO voucher. The intervention in MTO is the opportunity to change neighborhoods, and so the ITT estimate will be non-zero if the effect of changing neighborhood conditions on the risk of involvement in crime, violence, or other behaviors is not zero. Ludwig and colleagues (2008) discuss these issues in greater detail.

Two other potential issues that warrant discussion are differences between "compliers" – those who moved as a result of the program – and "non-compliers" – those who did not make program moves – as well as whether merely being given the option of moving could have affected families. Previous MTO research has shown that compliers and non-compliers differ on a number of baseline characteristics (Ludwig & Kling, 2007; Shroder, 2002). These correlates of the motivation to take up the MTO offer do not bias our experimental estimates of the effects of the offer of a housing voucher (that is, do not affect internal validity) since the randomized design of MTO should ensure that both the treatment and control groups included highly motivated families. In other words, the ITT estimates are not biased because they compare the outcomes of *all* members experimental group members (both compliers and non-compliers) with *all* members of the control group (both those who would and would not have complied had they received the offer). Differences between compliers and noncompliers do, however, affect the generalizability (external validity) of our TOT estimates: we cannot assume that the impacts we observe for the treatment compliers would generalize to the non-compliers if relocating had been mandatory.

Another complicating issue is whether being offered a voucher affected families even if they did not use the voucher to move. Some non-complier families searched for but were unable to find housing in the type of low-poverty areas to which the experimental voucher required them to move and the search experience could potentially have benefited them later (outside the context of the MTO voucher). Conversely, failure to find housing could have disheartened these families and discouraged them from future housing searches.

We believe that any effect on non-compliers is likely to be modest relative to the effects of actually using a voucher to move. We can also see empirically that very few of the control group families and very few of the non-compliers in the experimental group relocate to the types of neighborhoods to which the experimental group compliers initially move. On average, the neighborhoods that MTO treatment and control groups were living in at the time they enrolled in MTO had poverty rates of over 50% (Ludwig, 2012). One year after random assignment, the experimental group compliers were living in neighborhoods with average poverty rates of 15%, whereas the control group and experimental non-compliers were still living in neighborhoods where about 50% of residents are poor.

If we are willing to impose the additional assumption that experimental or Section 8 treatment assignment only affects the outcomes of families that actually relocate through MTO, then one can also estimate the effects of MTO moves on those who actually moved using an

MTO program housing voucher (the "treatment on the treated," or TOT, effect). The basic idea behind the TOT is that we observed outcomes for both the treatment group compliers and non-compliers as well as for the entire control group. If we assume that the "would-be non-compliers" in the control group (those who would not have moved had they been given a chance) are similar to the treatment group noncompliers, then we can back out an estimate of outcomes for the "would-be compliers" in the control group and compare them with the observed outcomes for the actual compliers in the treatment group. The TOT effect is therefore the difference in outcomes for people who moved in conjunction with the program and those who would have moved had they been given a chance.

We calculate the TOT effect with two-stage least squares by using indicators for random assignment to treatment as instrumental variables for actually moving through MTO (see Angrist, Imbens, and Rubin (1996)). In a model without additional baseline covariates this would be equivalent to dividing the ITT point estimate and its standard error by the share of families assigned to the treatment group that move through MTO (Bloom, 1984). This approach provides further evidence for why the TOT is not biased by the fact that families who comply with treatment may be different from non-compliers. As argued above, the ITT estimate is not biased by differences between compliers and non-compliers. Moreover, our TOT estimate is basically the ratio of two ITT effects: the ITT effect on the behavioral outcome of interest (involvement in crime or violence) divided by the ITT effect on moving with an MTO voucher. Given the MTO compliance rates reported above, the TOT effects for the experimental and Section 8 groups will be about 2.4 and 1.7 times the ITT effects for these groups, respectively.

Because both the ITT and TOT effects are important when evaluating the MTO experiment, we include discussion of both effects below. MTO was a voluntary program—

eligible families volunteered to be entered into the lottery for assignment to treatment and control groups, and families randomized into the two treatment groups chose to move using their MTO vouchers or to remain in their baseline housing arrangement. Any future housing policy that might be informed by the MTO results would presumably also be voluntarily, so the compliance rate and the ITT results tell us about the effects of a voluntary housing assistance program. However, when evaluating "neighborhood effects" it is important to analyze the effects on families who actually moved to low-poverty areas, which is why we also include the TOT effects. However, because the TOT effects can be roughly calculated using the ITT effects as described above, only the ITT effects are presented in the tables.

We also estimate the relationship between male violent-crime arrests and neighborhood share minority (M). Using ordinary least squares to estimate this relationship could yield biased estimates because of a possible correlation of share minority with unmeasured individual characteristics that influence both neighborhood selection and criminal behavior. Instead, we use a modified version of the instrumental variables (IV) approach of Kling, Liebman, and Katz (2007) to estimate the relationship. In our IV models, we use interactions of MTO random assignment with indicators for the five 2-year time periods following randomization as instrumental variables to generate predicted values of share minority that are then substituted into the second stage (equation (3) below), which also includes a set of baseline characteristics (X) for greater precision:

(3) $Y_i = M_i\pi_{10} + X_i\beta_1 + \varepsilon_{1i}$

IV estimation essentially fits a dose-response model to determine whether the time periods and treatment groups in which male youth experienced larger impacts on share minority also experienced larger impacts on violent-crime arrests. This approach assumes that the only pathway through which the instruments affect criminal behavior is through neighborhood minority composition. Because MTO changed many neighborhood attributes and socio-demographic characteristics such as share poor and share minority are highly correlated, we cannot completely isolate the effects of one neighborhood attribute. We illustrate our IV model results by showing a regression line fitted to the average values of share minority and violent-crime arrests (relative to the overall means for the time period) for the 15 data points corresponding to our instruments created by interacting the three treatment groups with 5 time periods.

RESULTS

Using data collected through 10-15 years after random assignment, we find that MTO generates large differences in average neighborhood conditions across randomly-assigned groups that shrink over time. We also show that in the years immediately following random assignment MTO generates substantial reductions in violent-crime arrests but sizable increases in property-crime arrests, primarily among males. These MTO effects on crime generally seem to attenuate over time as the MTO effect on neighborhood conditions attenuates. We also find that among the youth who are 15-25 at the end of 2001, there is little evidence that those who were relatively younger at baseline benefit more from MTO than those who were older at baseline. Taken together these findings suggest that what Sampson (2012) calls “situational” neighborhood effects may be relatively more important than “developmental” neighborhood effects.

Neighborhood Characteristics

As reported in prior publications using data from the MTO long-term evaluation (see for example Ludwig et al. (2011, 2012) and Sanbonmatsu et al. (2011)), Panel A of Table 2 demonstrates that both the experimental and Section 8 groups were on average living in neighborhoods that were better off economically through 10 years after random assignment. Although the size of the differences decreased over time, the differences remain statistically significant ten years post-randomization. Another general trend is that the experimental-control group differences tend to be larger in magnitude than the Section 8-control group differences.

Initial differences between treatment and control groups were large: The experimental group ITT for neighborhood poverty for females (-12.2 percentage points) in the first two years of the post-random assignment period represents a nearly 25% decrease from the control mean of almost 50%, while the Section 8 group ITT (-10.3 percentage points) represents roughly a 20% decrease. These effects are equivalent to a 0.81 standard deviation decrease in poverty for females in the experimental group relative to the control group and a 0.68 standard deviation decrease for Section 8 group females. Another way to think about these effect sizes is within the context of the national tract poverty distribution. One to two years after random assignment, control group females were living in census tracts that were 2.8 standard deviations above the national mean in the Census 2000 data. The experimental treatment reduced tract poverty by nearly 1 full standard deviation relative to the Census 2000 mean while the Section 8 treatment reduction was 0.84 standard deviations. Furthermore, because not all families in the experimental and Section 8 groups used their MTO vouchers to move, the TOT effects are even more substantial: experimental group females in families who moved through MTO experienced a

28.3-percentage point (1.88 standard deviation) drop in neighborhood poverty, while the poverty reduction was 17.3 percentage points (1.15 standard deviations) for Section 8 group females in families who moved through MTO. MTO's effects on neighborhood conditions were generally similar for male and female youth. Formal tests for the null hypothesis that the ITT effects for males and females are equivalent are presented in the rightmost column of Table 2. Few differences are significant.

While MTO was explicitly designed to help families move to lower-poverty neighborhoods, given the strong correlation in different measures of neighborhood socioeconomic composition, MTO helped families move to neighborhoods that differed on a wide range of other indicators of economic disadvantage as well (see Appendix Table 2). For example, over the course of 10-15 year period between random assignment and May 2008, female youth in the experimental group lived in neighborhoods that on average had a higher share of college-educated neighbors (0.044 ITT, 0.155 control mean), and mean income in experimental group neighborhoods was 30% higher than in the control group neighborhoods (for the Section 8 group, mean income was about 20% higher).

MTO generated large and persistent differences in neighborhood characteristics over the course of the 10-year follow-up period that we examine here. However, differences between the treatment and control groups decreased over time, and while still statistically significant, by 9-10 years after random assignment the differences were much more modest. A sizable share of the convergence over time across groups in average neighborhood conditions occurs because of improvements over time in the neighborhoods experienced by the control group. For example, from 1-2 years after baseline to the period 9-10 years after baseline the experimental ITT on neighborhood poverty for males declined by about 4 percentage points (from 11.2 to 7.1). Over this period the control group's average poverty rate declined by 13 percentage points (from about 49% in years 1-2 to about 36% in years 9-10). This implies that, while the treatment group neighborhoods were on average becoming less poor over time, after the initial wave of MTO moves occurred, the rate at which neighborhood poverty declined was even faster for the control group than for the treatment groups.

Although neighborhood race and poverty composition tend to be strongly correlated in the cross section in observational data, MTO wound up having only modest effects on neighborhood racial composition for program participants (in proportional terms). Even 1-2 years after random assignment, when the experimental ITT for females on tract share minority was 8.7 percentage points, the average treatment group youth was still living in neighborhoods that were overwhelmingly populated by other minorities (with more than four of five residents being from a minority race or ethnic group). Furthermore, the difference between Section 8 and control group youth on share minority was marginally significant for males only in years 1-2 and not significant for males or females in later years.

One slight complication to the analysis presented above stems from the difficulty of tracking young people over time as they age and move away from their parents, who originally applied to MTO and so form the core of our follow-up study sample. For cost reasons we focused on surveying the heads of the original MTO households and the participants who were youth (ages 10-20) at the time of the long-term follow up, and we therefore rely on a variety of imperfect data sources to track addresses for those who were ages 15-25 at the time of the interim (4-7) year follow up but grown children (ages 21-30) at the time of the long-term study. We examined three alternative, restricted samples based on the nature of the address history data available, and we generally find that the statistically significant treatment differences are robust to the alternative specifications and that the overall pattern of results for neighborhood outcomes does not change dramatically (see Appendix Table 3).[7]

Criminal Behavior

Panels C and D of Table 2 also show that, while MTO had few statistically significant effects on youth arrests averaged over the entire follow-up period, there are signs of sizable effects on arrest rates concentrated among males during the first two to four years following random assignment. Over time as the MTO effects on neighborhood conditions attenuate, these MTO impacts on arrests also seem to attenuate at least for experimental group males.

One feature of Table 2 that is common to studies of "street crime" but is nonetheless worthy of comment is the substantial gender difference in arrest rates. Among male youth who were 15-25 at the end of 2001, fully 61% were arrested at least once in the 10 years after random assignment compared with only 30% of females (not shown). Scaling up to the full 10-year

[7] The experimental group impacts from the models limited to (a) the youth for whom we have fairly complete address information via address updates from the adult long-term survey interview (the proxy address sample) and (b) the youth who were still living with the adult as of the long-term survey are very similar to those for the main sample. But the Section 8 results are less robust across specifications, particularly in later years. However, because for cost reasons we sought interviews with only a random two-thirds of adults from Section 8 households, these restricted analysis samples are rather small, leading to power concerns. Furthermore, the results from the specification where we extrapolate from the youth's address at age 18 demonstrate the importance of tracking the youth over time because the effects of MTO on neighborhood characteristics, especially for females, appear stronger in these models than they do in the unadjusted results and the two other alternative specifications. Because address at 18 is more likely to have been the home of the household head, it appears that the youth were leaving home and moving to somewhat worse neighborhoods than where their parents lived.

follow-up period, the control group mean for the number of arrests for all crime types among males across groups was almost 4 arrests per person (including 0.79 violent- and 0.83 property-crime arrests). Neither experimental nor Section 8 group females committed fewer crimes than their control group counterparts, but arrest rates among the females were very low overall. Again scaling up the results to the full 10-year follow-up period, the control group mean for the number of arrests for all crime types among females was about 0.8, and the arrest rate per year was no higher than 0.1 in any of the five 2-year periods.

Table 2 shows that, during the period right after randomization, MTO generated sizable reductions in violent-crime arrests among experimental group males. Violent-crime arrests among experimental group males were almost a third lower than among control group males in years 1-2 and 3-4. Since fewer than half of the families of these youth who were assigned to the experimental group moved with a MTO voucher, the TOT effect is more than twice as large as the ITT effect here. These very large reductions in violent-crime arrests were no longer statistically significant by years 5-6. There was no statistically significant effect of the Section 8 treatment on violent-crime arrests for male youth. (These results are qualitatively similar if we restrict the analysis sample to male youth ages 15-20 at the end of 2001).

Property-crime arrests did not differ between the experimental and control group males with the exception of years 3-4 post-RA when experimental males were arrested *more* often than control group males. Although only marginally significant ($p<.10$), this result is large in proportional terms: the ITT effect is nearly one-third of the control mean.[8] The Section 8

[8] The results presented here differ slightly from those presented in Kling, Ludwig, and Katz (2005) because we resubmitted the identifying information for these youth to the criminal justice agencies to match again from scratch,

treatment seems to reduce property-crime arrests for male youth in the first two post-baseline years (by 42%) and in the last two post-baseline years (by 29%, p<.10).

DISCUSSION

This paper presents a long-term (10-year) follow-up of the effects of the MTO residential mobility experiment on youth who were ages 15-25 at the end of 2001 (the group studied by Kling, Ludwig, and Katz (2005) about 4-7 years after random assignment). We find large and statistically significant reductions in violent-crime arrest rates among male youth assigned to the experimental rather than control group, concentrated during the first four years following random assignment, and adverse effects on property-crime arrests, which are also concentrated during the first few years after random assignment.

Given the significant effects on Section 8 male property-crime arrests at the beginning and end of the follow-up period, a natural question is why there weren't effects in the intervening years. The standard errors around our estimates do not let us rule out the possibility that there were some modest sustained impacts on arrests over time for these youth. Additionally, the experimental-control ITT for property-crime arrests in years 9-10 is not quite statistically significant (p=0.11) but is also large as a fraction of the control mean—about 25%.

Why do the MTO impacts on arrest rates for male youth in the experimental group – particularly the beneficial effects on of violent-crime arrests —attenuate over time? One

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and the matching procedures used by the agencies changed slightly from when these identifiers were submitted for matching for the interim (4-7 year) MTO study. We rely on the data we received back for the long-term MTO study match so that we can consistently examine how MTO impacts on arrests evolve over time.

candidate explanation for the changes over time in MTO's effects on arrests among experimental group males is that they are driven by the size of MTO's effect on contemporaneous neighborhood conditions, and that the attenuation of these effects on arrests over time is due to the attenuation of MTO's effects on neighborhood environments. Figure 1 shows the pattern implied by the results presented in Table 2 for how MTO's effect on violent-crime arrests for males changes in response to the effect on neighborhood minority concentration. There appears to be a relationship between the size of the neighborhood "dose" and the size of the arrest "response", with a pattern suggesting that generally larger reductions in violent-crime arrests occur during periods when the MTO effect on neighborhood conditions (in this case, census tract share minority) is larger.

Another hypothesis that a criminologist might consider stems from the well-known age-crime curve: Perhaps this cohort of youth is simply "aging out" of their crime-prone years, so that our study sample is no longer engaged in criminal activity towards the end of our 10-year follow-up period (the average age of the youth in our sample 10 years after random assignment is almost 23). Perhaps over time there is just no crime to be prevented through changing neighborhoods? Yet this explanation does not square well with the data presented in Table 2, which shows that the control means for violent- and property-crime arrests are *higher* in the later years of the follow-up period.

A different candidate explanation is that crime rates themselves were changing over the course of the study period, since MTO began enrolling families in 1994 – not long after the start of the sizable drop in crime rates observed over the 1990s nationwide (see for example Levitt (2004) for a discussion and candidate explanations). However, because MTO was structured as a random assignment experiment, secular changes that affected all places equally should have similar effects on the offending patterns of youth assigned to control and treatment conditions.

Because we rely on administrative records on arrests to MTO youth, rather than directly measure youth criminal behavior, our analysis could be affected if the likelihood that a crime results in arrest varies systematically between more and less disadvantaged neighborhoods. The interim follow-up survey in MTO asked household heads about the likelihood that police respond to 911 calls, with a pattern of responses that suggests police may be more responsive in lower-poverty areas. If the probability of arrest is indeed higher in lower-poverty areas, our analysis would understate any protective MTO effects to prevent crime and overstate any adverse effects of MTO moves on criminal behavior. That is, our analysis may understate the beneficial effects on violent crime and overstate adverse effects on property crime. However, it is not entirely clear why this pattern should change over time, and so it is not clear that differential enforcement can explain the evolution of MTO effects on arrests over time. It could be the case that low-income, minority youth as in MTO learn over time to better "blend in" or interact with law enforcement in low-poverty areas. But this explanation would predict that the beneficial MTO effect in reducing violent-crime arrests in the few years right after random assignment should actually increase rather than decrease (in absolute value) over time.

Perhaps a more plausible explanation for the adverse effect of MTO on property-crime arrests is that there is simply more valuable loot to steal in less racially and economically segregated areas compared with the more disadvantaged communities in which the control group families reside. This hypothesis also provides an explanation for why the MTO effect on property offending declines over time – because the treatment-control difference in neighborhood conditions converges over the 10-year study period. Another possibility is that fade-out represents acclimation to the new neighborhoods. For example, males moving to new neighborhoods may initially act out and steal items, but this behavior may stop once they become accustomed to the neighborhood and form social connections.

More generally our pattern of findings seems to be more consistent with the relative importance of "situational" neighborhood effects that depend on the environments in which decisions to engage in crime or not are made, rather than to "developmental" neighborhood effects that influence the quality of the developmental environments children experience growing up. The developmental neighborhood hypothesis has the additional testable prediction that MTO youth who are relatively younger at baseline should be more responsive to MTO moves than those older at baseline, because they experience larger neighborhood changes during a more developmentally "plastic" life stage. To test this hypothesis, we interact a linear term for baseline age with treatment assignment among our sample of youth 15-25 as of the end of 2001 and find no pattern of statistical significance (i.e. the effect of MTO on the criminal behavior of younger youth does not differ from the effect on older youth).

The findings described above also fit the pattern of previous studies, including studies from the MTO interim evaluation, where treatment group females fare better (or at least are no worse off) than their control counterparts and males are arrested less for violent crimes but are actually arrested more for property crimes. Kling, Ludwig, and Katz (2005) examined variation by gender in three candidate explanations when reviewing the gender differences in the interim MTO results: mobility patterns, discrimination, and adaptability. They largely ruled out the first two explanations because neither varied systematically by gender. Our results are consistent: Table 2 shows that (a) the impacts on neighborhood characteristics by gender are quite similar, especially earlier in the post-randomization period when effects on criminal behavior are strongest, and (b) the effects on neighborhood racial composition, while statistically significant, were more modest than the effects on neighborhood economic composition and even experimental group youth who moved with a voucher lived in high-minority neighborhoods. Kling, Ludwig, and Katz find some evidence that MTO youth differ in their adaptability to new neighborhoods, however, and point to a comparative advantage in property offending that affects males more than females due to lower achievement test scores and higher school absence rates, less adult supervision, and more friends who have engaged in risky and delinquent behavior.

Furthermore, the findings presented here are consistent with the idea that neighborhood conditions may have an important influence on the violent criminal behavior of youth in particular and also with the idea that contemporaneous rather than lagged neighborhood conditions (particularly minority composition), i.e. situation neighborhood effects, may be the most important determinant of the neighborhood's influence on violent behavior. Our analysis does not isolate the specific mechanisms through which neighborhood conditions might affect youth violence, but the exercise of tracking youth over the long term that we present here begins to narrow down the range of mechanisms that are consistent with the data. Our findings are also consistent with the optimistic view that even the criminal behavior of adolescents who have been exposed to distressed neighborhood conditions for extended periods may be sensitive to changes in the offending environment. The young people growing up in our nation's distressed urban neighborhoods, even those who have already reached their peak offending ages, appear to be affected in important ways by their current situation.

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Exhibit List

Main Exhibits

Appendix Tables

Table 1. Baseline Characteristics of the Youth Sample (1994-98)

	Females			Males		
	Experimental	Section 8	Control	Experimental	Section 8	Control
Age as of December 31, 2007						
21	0.137	0.144	0.154	0.129	0.148	0.134
22	0.127	0.157	0.132	0.137	0.120 *	0.153
23	0.129	0.121	0.119	0.132	0.130	0.136
24-26	0.345	0.339	0.369	0.340	0.376 *	0.331
27-31	0.261	0.240	0.226	0.261	0.225	0.246
Other characteristics						
Gifted student or did advanced coursework	0.153	0.179	0.163	0.142	0.144	0.173
Suspended or expelled from school in past two years	0.103	0.094	0.087	0.207	0.183	0.189
School called about behavior in past two years	0.218	0.199	0.197	0.377	0.342	0.372
Behavioral or emotional problems	0.052 *	0.058 **	0.034	0.124	0.119	0.115
Learning problems	0.122	0.110	0.117	0.252	0.219	0.244
Health problems that limited activity	0.045	0.053	0.057	0.096 *	0.085	0.067
Health problems that required special medicine or equipment	0.054	0.053	0.053	0.106	0.138 **	0.091
Ever Arrested before Random Assignment						
Any crime	0.031	0.023 **	0.042	0.103	0.094	0.088
Violent crime	0.013	0.011	0.022	0.052 *	0.056 **	0.032
Property crime	0.011	0.009	0.016	0.046	0.035	0.039
Site						
Baltimore	0.168	0.141	0.141	0.149	0.149	0.141
Boston	0.177	0.187	0.210	0.159	0.190	0.181
Chicago	0.204	0.202	0.193	0.209	0.211	0.200
Los Angeles	0.203	0.241	0.239	0.231	0.226	0.232
New York	0.248	0.229	0.216	0.253	0.224	0.245
Sample Size	930	646	701	957	690	719

Table 1. (continued)

Notes : All values represent shares. Values were calculated using sample weights to account for changes in random assignment ratios across randomization cohorts. Missing values were imputed based on gender, age, randomization site, and whether randomized through 1997 or in 1998. *** Significant at the 1 percent level on an independent group t-test of the difference between the control group and the experimental group or the Section 8 group. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Source and Sample : All measures except the arrest measures come from the MTO baseline survey. The baseline head completed the baseline survey, providing information on both the household and its individual members. The arrest measures come from individual criminal justice system arrest data: adult and juvenile data from California, Illinois, Maryland, and Massachusetts; de-identified adult data from New York State; juvenile data from New York City; and adult or juvenile records from 8 additional states in which participants have lived. The sample is core household members ages 15-25 as of December 31, 2001 and under age 18 at baseline (N=4,643).

Measures : Age as of December 31, 2007 determined eligibility for the long-term survey and provides a rough estimate of youth age as of the end of the 10-year post-random assignment window analyzed for the paper. Violent-crime arrests involve charges of force or threat of force including homicide, rape, robbery, assault, kidnapping, and weapons charges. Property-crime arrests involve taking money or property and include burglary, motor vehicle theft, larceny, trespassing, and receiving stolen property. Any crime arrests include violent- and property-crime arrests as well as arrests for any other charge, including drug crimes (possession or distribution), disorderly conduct, and moving violations.

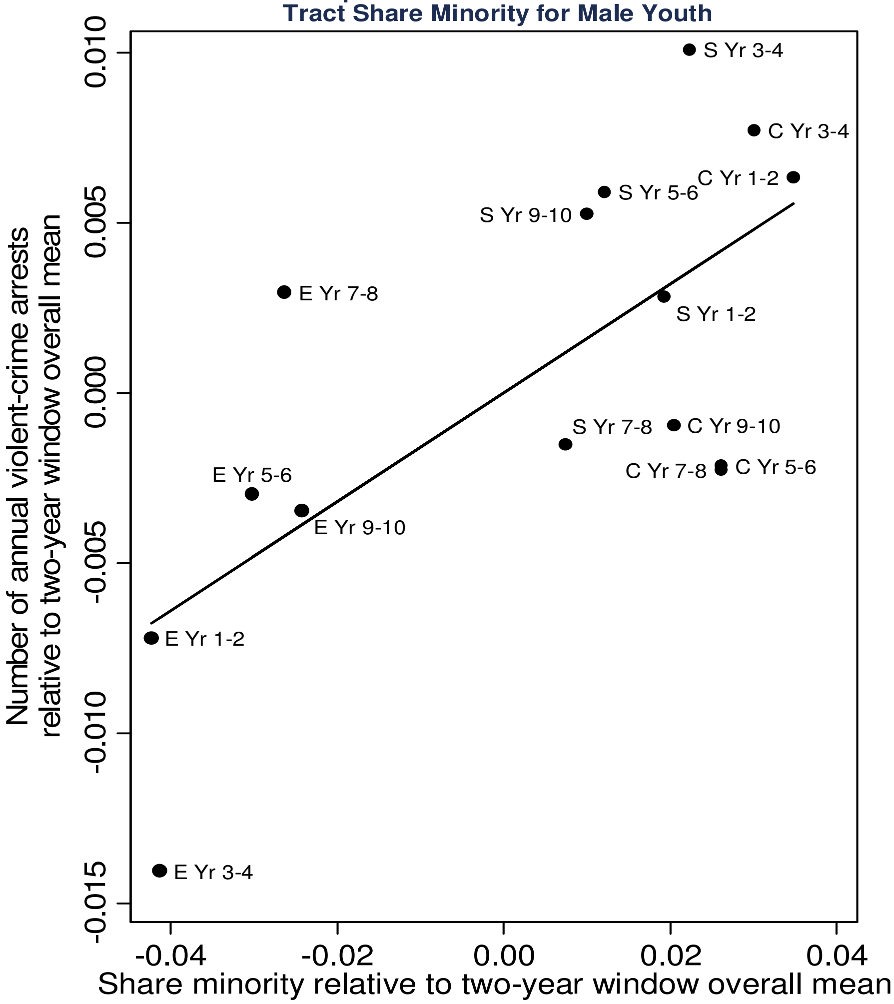
Table 2. Effects on Neighborhood Conditions and Number of Arrests by Year Since Random Assignment

	Females			Males			Male-Female Difference	
		Intent to Treat Effect			Intent to Treat Effect		Intent to Treat Effect	
	CM	E-C	S-C	CM	E-C	S-C	E-C	S-C
A. Neighborhood Share Poor								
1-2 years since RA	0.489	-0.122 ***	-0.103 ***	0.487	-0.112 ***	-0.092 ***	0.010	0.011
		(0.009)	(0.009)		(0.009)	(0.009)	(0.010)	(0.010)
3-4 years since RA	0.439	-0.120 ***	-0.094 ***	0.437	-0.117 ***	-0.091 ***	0.003	0.003
		(0.010)	(0.010)		(0.011)	(0.010)	(0.012)	(0.012)
5-6 years since RA	0.400	-0.088 ***	-0.071 ***	0.406	-0.100 ***	-0.078 ***	-0.012	-0.007
		(0.010)	(0.010)		(0.010)	(0.010)	(0.012)	(0.012)
7-8 years since RA	0.372	-0.065 ***	-0.055 ***	0.378	-0.081 ***	-0.069 ***	-0.016	-0.014
		(0.009)	(0.009)		(0.010)	(0.010)	(0.012)	(0.012)
9-10 years since RA	0.350	-0.048 ***	-0.042 ***	0.361	-0.071 ***	-0.060 ***	-0.023 *	-0.017
		(0.009)	(0.009)		(0.010)	(0.010)	(0.012)	(0.012)
B. Neighborhood Share Minority								
1-2 years since RA	0.899	-0.087 ***	-0.012	0.903	-0.079 ***	-0.018 *	0.008	-0.006
		(0.012)	(0.011)		(0.012)	(0.011)	(0.013)	(0.012)
3-4 years since RA	0.893	-0.080 ***	-0.014	0.893	-0.073 ***	-0.011	0.007	0.003
		(0.012)	(0.011)		(0.013)	(0.012)	(0.014)	(0.014)
5-6 years since RA	0.874	-0.040 ***	-0.001	0.889	-0.058 ***	-0.016	-0.018	-0.015
		(0.012)	(0.012)		(0.012)	(0.012)	(0.015)	(0.015)
7-8 years since RA	0.866	-0.038 ***	0.004	0.882	-0.054 ***	-0.020	-0.016	-0.025
		(0.012)	(0.012)		(0.012)	(0.014)	(0.015)	(0.017)
9-10 years since RA	0.859	-0.038 ***	-0.007	0.869	-0.045 ***	-0.011	-0.008	-0.004
		(0.012)	(0.012)		(0.013)	(0.015)	(0.016)	(0.018)
C. Number of Annual Violent-Crime Arrests								
1-2 years since RA	0.0127	0.0093	0.0044	0.0596	-0.0197 *	-0.0082	-0.0290 **	-0.0126
		(0.0058)	(0.0063)		(0.0111)	(0.0121)	(0.0129)	(0.0137)
3-4 years since RA	0.0249	0.0027	-0.0019	0.0776	-0.0231 **	0.0003	-0.0259 *	0.0022
		(0.0072)	(0.0071)		(0.0114)	(0.0141)	(0.0133)	(0.0149)
5-6 years since RA	0.0304	-0.0053	-0.0088	0.0880	0.0002	0.0135	0.0056	0.0223
		(0.0075)	(0.0085)		(0.0135)	(0.0166)	(0.0156)	(0.0180)
7-8 years since RA	0.0274	-0.0051	-0.0097	0.0911	0.0056	-0.0014	0.0107	0.0082
		(0.0077)	(0.0081)		(0.0138)	(0.0148)	(0.0153)	(0.0167)
9-10 years since RA	0.0243	-0.0090	-0.0051	0.0811	0.0003	0.0084	0.0093	0.0135
		(0.0073)	(0.0078)		(0.0117)	(0.0137)	(0.0138)	(0.0157)
1-10 years since RA	0.0239	-0.0015	-0.0042	0.0795	-0.0073	0.0025	-0.0059	0.0067
		(0.0039)	(0.0043)		(0.0071)	(0.0085)	(0.0081)	(0.0093)
D. Number of Annual Property-Crime Arrests								
1-2 years since RA	0.0193	-0.0070	-0.0035	0.0522	-0.0081	-0.0221 **	-0.0011	-0.0186 *
		(0.0050)	(0.0067)		(0.0095)	(0.0090)	(0.0105)	(0.0111)
3-4 years since RA	0.0299	-0.0106	-0.0048	0.0728	0.0223 *	0.0135	0.0329 **	0.0183
		(0.0076)	(0.0098)		(0.0134)	(0.0146)	(0.0156)	(0.0173)
5-6 years since RA	0.0292	-0.0054	0.0079	0.0883	0.0172	0.0153	0.0226	0.0074
		(0.0087)	(0.0101)		(0.0148)	(0.0185)	(0.0163)	(0.0201)
7-8 years since RA	0.0282	0.0007	0.0000	0.0853	0.0129	-0.0013	0.0121	-0.0013
		(0.0075)	(0.0086)		(0.0141)	(0.0150)	(0.0161)	(0.0172)
9-10 years since RA	0.0266	-0.0048	-0.0012	0.1166	-0.0286	-0.0342 *	-0.0238	-0.0330
		(0.0068)	(0.0097)		(0.0180)	(0.0183)	(0.0184)	(0.0203)
1-10 years since RA	0.0266	-0.0054	-0.0003	0.0831	0.0031	-0.0058	0.0085	-0.0055
		(0.0042)	(0.0060)		(0.0082)	(0.0088)	(0.0088)	(0.0104)

Table 2. (continued)

Notes : E−C, experimental − control; S − C, Section 8 − control; CM, control mean. The estimates are from an ordinary least squares regression of each outcome on treatment indicators (experimental and Section 8 effects were estimated in one model) and the baseline covariates listed in Appendix Table 1. Impacts by gender were estimated as an interaction with treatment status. Robust standard errors adjusted for household clustering are in parentheses. *** Treatment-control difference is significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.
Source and Sample : Census tract characteristics are interpolated data from the 1990 and 2000 decennial censuses as well as the 2005-09 American Community Survey. Arrest measures are from individual criminal justice system data: adult and juvenile data from California, Illinois, Maryland, and Massachusetts; de-identified adult data from New York State; juvenile data from New York City; and adult or juvenile records from 8 additional states in which participants have lived. The sample for the neighborhood measures is core household members ages 15-25 as of December 31, 2001 and under age 18 at baseline (N=4643) and the sample for the arrest measures is the limited to the subset of those youth for whom arrest data are avaialble (N=4641).
Measures : The arrest measures are based on quarterly arrest data rescaled to represent the number of arrests per year. Violent-crime arrests involve charges of force or threat of force including homicide, rape, robbery, assault, kidnapping, and weapons charges. Property-crime arrests involve taking money or property and include burglary, motor vehicle theft, larceny, trespassing, and receiving stolen property. Total arrests include violent- and property-crime arrests as well as arrests for any other charges, including drug possession and distribution, disorderly conduct, and moving violations.

Figure 1. Instrumental Variable Estimation of the Relationship between Violent-Crime Arrests and Tract Share Minority for Male Youth



Notes: The y-axis is the number of arrests per year for violent crimes. The x-axis is share minority (the fraction of census tract residents who are members of racial or ethnic minority groups), which is linearly interpolated from the 1990 and 2000 decennial census and 2005-09 American Community Survey and is weighted by the time respondents lived at all of their addresses during each two-year window in the follow-up period (through 10 years after random assignment). The points represent the two-year follow-up window (e.g. Yr 1-2 = post-random assignemt years 1 and 2) and treatment group (E = experimental group, S = Section 8 group, C = control group). The line through the data points is equivalent to a two-stage least-squares estimate of the relationship between the annual number of arrests and the mediator, using interactions of two-year follow-up window and treatment group as instruments for the mediator (conditional on two-year follow-up window main effects). The size of each point is proportional to the sum of the weights for that group and, correspondingly, to the weight that the point receives in the two-stage least squares regression. Our instrumental variable (IV) estimation, controlling for the covariates listed in Appendix Table 1, shows that a 1 percentage point decrease in share minority is associated with a 0.165 decrease in the number of violent-crime arrests per year (SE=0.102, P=0.108). (The slope of the graph presented here differs very slightly from the IV coefficient listed above because the individual-level covariates could not be fully collasped to the treatment group-time period level; regression residuals were used instead).

Source and Sample: Individual criminal justice system arrest data: adult and juvenile data from California, Illinois, Maryland, and Massachusetts; de-identified adult data from New York State; juvenile data from New York City; and adult or juvenile records from 8 additional states in which participants have lived. The sample is male core household members ages 15-25 as of December 31, 2001 and under age 18 at baseline for whom arrest data are available (N=2364).

Appendix Table 1. Full Set of Baseline Characteristics of the Youth Sample Controlled for in the Analysis (1994-98)

	Females			Males		
	Experimental	**Section 8**	**Control**	**Experimental**	**Section 8**	**Control**
Youth Characteristics						
Age as of December 31, 2007						
21	0.137	0.144	0.154	0.129	0.148	0.134
22	0.127	0.157	0.132	0.137	0.120 *	0.153
23	0.129	0.121	0.119	0.132	0.130	0.136
24-26	0.345	0.339	0.369	0.340	0.376 *	0.331
Other youth characteristics						
Gifted student or did advanced coursework	0.153	0.179	0.163	0.142	0.144	0.173
Suspended or expelled from school in past two years	0.103	0.094	0.087	0.207	0.183	0.189
School called about behavior in past two years	0.218	0.199	0.197	0.377	0.342	0.372
Behavioral or emotional problems	0.052 *	0.058 **	0.034	0.124	0.119	0.115
Learning problems	0.122	0.110	0.117	0.252	0.219	0.244
Health problems that limited activity	0.045	0.053	0.057	0.096 *	0.085	0.067
Health problems that required special medicine or equipment	0.054	0.053	0.053	0.106	0.138 **	0.091
Pre-Random Assignment Arrests						
Violent Crime						
1 arrest	0.009	0.009	0.014	0.036 *	0.040 **	0.019
2 arrests	0.001 **	0.001 **	0.006	0.008	0.009	0.004
3 or more arrests	0.002	0.001	0.001	0.005	0.002	0.001
Property Crime						
1 arrest	0.007	0.006	0.015	0.025	0.021	0.022
2 arrests	0.003	0.002	0.001	0.007	0.004	0.010
3 or more arrests	0.000	0.000	0.000	0.008	0.001	0.002
Drug and Other Crimes						
1 arrest	0.015	0.009	0.012	0.023	0.019 **	0.037
2 arrests	0.002	0.002	0.004	0.013	0.011	0.010
3 or more arrests	0.000	0.002	0.000	0.005	0.005	0.003

Appendix Table 1. (continued)

	Females			Males		
	Experimental	**Section 8**	**Control**	**Experimental**	**Section 8**	**Control**
Adult Characteristics						
Gender						
Male	0.020	0.031	0.027	0.028	0.035	0.040
Race and ethnicity						
Black (any ethnicity)	0.643	0.602	0.640	0.606	0.616	0.626
Other race (non-black/white, any ethnicity)	0.289	0.291	0.267	0.301	0.287	0.293
Hispanic ethnicity (any race)	0.297	0.326	0.310	0.335	0.327	0.329
Age as of December 31, 2007						
<=35	0.003	0.005	0.006	0.005	0.005	0.005
36-40	0.075	0.107 *	0.074	0.068 **	0.070 *	0.102
41-45	0.279	0.278	0.298	0.299	0.311	0.298
46-50	0.291	0.281	0.292	0.289	0.287	0.303
Other demographic characteristics						
Never married	0.516	0.505	0.528	0.499	0.513	0.540
Parent before age 18	0.264	0.272	0.280	0.275	0.265	0.284
Working	0.286	0.237	0.250	0.270	0.249	0.260
Enrolled in school	0.121	0.164	0.155	0.120	0.164	0.129
High school diploma	0.388 ***	0.317	0.293	0.316	0.319	0.315
Certificate of General Educational Development (GED)	0.150	0.181	0.184	0.173	0.166	0.187
Household Characteristics						
Receiving Aid to Families with Dependent Children (AFDC)	0.736	0.775	0.749	0.745	0.725	0.735
Own car	0.185	0.203	0.185	0.218	0.185	0.185
Disabled household member	0.189	0.140	0.156	0.171	0.160	0.157
No teens in household	0.240	0.292	0.265	0.246 *	0.281	0.294
Household size						
Two	0.086	0.078	0.102	0.084	0.086	0.063
Three	0.240	0.188	0.215	0.195	0.199	0.224
Four or more	0.255	0.274	0.230	0.256	0.242	0.241

Appendix Table 1. (continued)

	Females			Males		
	Experimental	Section 8	Control	Experimental	Section 8	Control
Neighborhood characteristics						
Household member was crime victim in last six months	0.458	0.398	0.421	0.473	0.449	0.464
Streets unsafe at night	0.508	0.446 **	0.518	0.516	0.510	0.519
Very dissatisfied with neighborhood	0.462	0.409	0.455	0.469	0.459	0.457
Lived in neighborhood 5+ years	0.648	0.710	0.659	0.669	0.695	0.660
Moved more than 3 times in past 5 years	0.068	0.071	0.095	0.073	0.076	0.080
No family in neighborhood	0.617	0.614	0.657	0.640	0.655	0.670
No friends in neighborhood	0.416	0.334 **	0.404	0.404	0.391	0.418
Chatted with neighbors at least once per week	0.520	0.476	0.522	0.497	0.465	0.509
Very likely to tell neighbor about child getting into trouble	0.603	0.535	0.583	0.554	0.545	0.598
Confident about finding a new apartment	0.471 **	0.481 **	0.405	0.440	0.420	0.435
Had Section 8 voucher before	0.494	0.382 ***	0.472	0.450	0.420	0.472
Primary or secondary reason for wanting to move						
To get away from gangs and drugs	0.803	0.734	0.777	0.773	0.768	0.784
Better schools for children	0.468	0.539	0.488	0.532	0.555	0.500
Site						
Baltimore	0.168	0.141	0.141	0.149	0.149	0.141
Boston	0.177	0.187	0.210	0.159	0.190	0.181
Chicago	0.204	0.202	0.193	0.209	0.211	0.200
Los Angeles	0.203	0.241	0.239	0.231	0.226	0.232
New York	0.248	0.229	0.216	0.253	0.224	0.245
Sample Size	930	646	701	957	690	719

Appendix Table 1. (continued)

Notes : All values represent shares. Values were calculated using sample weights to account for changes in random assignment ratios across randomization cohorts. Missing values for youth characteristics were imputed based on gender, age, randomization site, and whether randomized through 1997 or in 1998. Missing values for adult- and household-level characteristics were imputed based on randomization site and whether randomized through 1997 or in 1998. If a covariate was missing for more than 5% of cases, the value of the covariate was set to zero and a dummy variable indicating that the covariate had a missing value was included in the analysis. Regression analysis of arrest records included as control variables all covariates in this table as well as gender and missing flags for the suspension/expulsion, gifted student, learning problems, activity-limiting health problems, and the adult education level measures; the analysis of neighborhood characteristics excluded the pre-random assignment arrest covariates. *** Significant at the 1 percent level on an independent group t-test of the difference between the control group and the experimental group or the Section 8 group. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Source and Sample : All measures except the arrest measures come from the MTO baseline survey. The arrest measures come from individual criminal justice system arrest data: adult and juvenile data from California, Illinois, Maryland, and Massachusetts; de-identified adult data from New York State; juvenile data from New York City; and adult or juvenile records from 8 additional states in which participants have lived. The sample is core household members ages 15-25 as of December 31, 2001 and under age 18 at baseline (N=4,643).

Measures : Violent-crime arrests involve charges of force or threat of force including homicide, rape, robbery, assault, kidnapping, and weapons charges. Property-crime arrests involve taking money or property and include burglary, motor vehicle theft, larceny, trespassing, and receiving stolen property. Drug and other crime arrests cover any charges not classified as violent or property and include drug possession and distribution, disorderly conduct, and moving violations.

Appendix Table 2. Duration-Weighted Effects on Neighborhood Conditions

	Females			Males			Male-Female Difference	
		Intent to Treat Effect			Intent to Treat Effect		Intent to Treat Effect	
	CM	E-C	S-C	CM	E-C	S-C	E-C	S-C
Share poor	0.400	-0.081 ***	-0.068 ***	0.405	-0.093 ***	-0.075 ***	-0.013	-0.007
		(0.008)	(0.008)		(0.008)	(0.008)	(0.010)	(0.010)
Share minority	0.875	-0.053 ***	-0.009	0.883	-0.060 ***	-0.017	-0.007	-0.007
		(0.009)	(0.010)		(0.011)	(0.011)	(0.012)	(0.013)
Share college-educated	0.155	0.044 ***	0.021 ***	0.156	0.039 ***	0.019 ***	-0.005	-0.003
		(0.005)	(0.005)		(0.006)	(0.005)	(0.006)	(0.007)
Share single female-headed families	0.534	-0.064 ***	-0.054 ***	0.538	-0.072 ***	-0.061 ***	-0.008	-0.007
		(0.008)	(0.008)		(0.008)	(0.009)	(0.010)	(0.010)
Share employed	0.809	0.033 ***	0.032 ***	0.806	0.034 ***	0.032 ***	0.001	0.000
		(0.004)	(0.004)		(0.004)	(0.004)	(0.005)	(0.005)
Average household income ($2009)	$27,455	8250 ***	5418 ***	$27,454	9256 ***	5910 ***	1005.3	491.7
		(770.1)	(742.2)		(848.9)	(782.2)	(962.0)	(931.3)

Notes : E−C, experimental − control; S − C, Section 8 − control; CM, control mean. The estimates are from an ordinary least squares regression of each outcome on treatment indicators (experimental and Section 8 effects were estimated in one model) and the baseline covariates listed in Appendix Table 1. Impacts by gender were estimated as an interaction with treatment status. Robust standard errors adjusted for household clustering are in parentheses. *** Treatment-control difference is significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.
Source and Sample : Census tract characteristics are interpolated data from the 1990 and 2000 decennial censuses as well as the 2005-09 American Community Survey. The sample is core household members ages 15-25 as of December 31, 2001 and under age 18 at baseline (N=4643).
Measures : All measures are weighted by the time respondents lived at all of their addresses from random assignment through May 31, 2008 (just prior to the beginning of the long-term survey fielding period).

Appendix Table 3. Effects on Neighborhood Conditions by Year Since Random Assignment, Sensitivity Analyses by Youth Gender

	Control Mean				Experimental-Control Intent to Treat Effect				Section 8-Control Intent to Treat Effect			
	All	Proxy Address	Living w/ Adult	Through Age 18	All	Proxy Address	Living w/ Adult	Through Age 18	All	Proxy Address	Living w/ Adult	Through Age 18
A. Females												
Sample size	701	290	140	701	930	379	189	930	646	157	81	646
Share poor												
1-2 years since RA	0.489	0.493	0.478	0.491	-0.122 ***	-0.130 ***	-0.122 ***	-0.110 ***	-0.103 ***	-0.078 ***	-0.075 ***	-0.085 ***
					(0.009)	(0.014)	(0.017)	(0.009)	(0.009)	(0.016)	(0.021)	(0.009)
3-4 years since RA	0.439	0.440	0.435	0.448	-0.120 ***	-0.123 ***	-0.128 ***	-0.111 ***	-0.094 ***	-0.075 ***	-0.068 ***	-0.076 ***
					(0.010)	(0.015)	(0.019)	(0.009)	(0.010)	(0.019)	(0.022)	(0.010)
5-6 years since RA	0.400	0.400	0.396	0.421	-0.088 ***	-0.088 ***	-0.093 ***	-0.094 ***	-0.071 ***	-0.058 ***	-0.049 **	-0.062 ***
					(0.010)	(0.015)	(0.018)	(0.009)	(0.010)	(0.017)	(0.021)	(0.009)
7-8 years since RA	0.372	0.369	0.372	0.406	-0.065 ***	-0.057 ***	-0.071 ***	-0.088 ***	-0.055 ***	-0.036 **	-0.034	-0.057 ***
					(0.009)	(0.014)	(0.018)	(0.009)	(0.009)	(0.015)	(0.022)	(0.009)
9-10 years since RA	0.350	0.345	0.345	0.392	-0.048 ***	-0.039 ***	-0.052 ***	-0.083 ***	-0.042 ***	-0.018	-0.021	-0.055 ***
					(0.009)	(0.014)	(0.018)	(0.009)	(0.009)	(0.016)	(0.022)	(0.009)
Share minority												
1-2 years since RA	0.899	0.894	0.888	0.899	-0.087 ***	-0.083 ***	-0.065 ***	-0.080 ***	-0.012	0.009	-0.009	-0.005
					(0.012)	(0.017)	(0.023)	(0.012)	(0.011)	(0.019)	(0.026)	(0.010)
3-4 years since RA	0.893	0.887	0.891	0.897	-0.080 ***	-0.076 ***	-0.068 ***	-0.078 ***	-0.014	0.006	0.002	-0.003
					(0.012)	(0.018)	(0.022)	(0.011)	(0.011)	(0.022)	(0.023)	(0.011)
5-6 years since RA	0.874	0.874	0.881	0.887	-0.040 ***	-0.041 **	-0.041 **	-0.048 ***	-0.001	0.018	0.006	0.009
					(0.012)	(0.017)	(0.020)	(0.011)	(0.012)	(0.021)	(0.025)	(0.011)
7-8 years since RA	0.866	0.866	0.874	0.888	-0.038 ***	-0.034 *	-0.034	-0.043 ***	0.004	0.040 **	0.029	0.005
					(0.012)	(0.018)	(0.022)	(0.011)	(0.012)	(0.019)	(0.025)	(0.010)
9-10 years since RA	0.859	0.869	0.872	0.884	-0.038 ***	-0.038 **	-0.030	-0.038 ***	-0.007	0.030	0.021	0.004
					(0.012)	(0.017)	(0.024)	(0.011)	(0.012)	(0.019)	(0.026)	(0.011)

Appendix Table 3. (continued)

	Control Mean				Experimental-Control Intent to Treat Effect				Section 8-Control Intent to Treat Effect			
	All	Proxy Address	Living w/ Adult	Through Age 18	All	Proxy Address	Living w/ Adult	Through Age 18	All	Proxy Address	Living w/ Adult	Through Age 18
B. Males												
Sample size	719	198	153	719	957	284	193	957	690	144	100	690
Share poor												
1-2 years since RA	0.487	0.489	0.487	0.492	-0.112 ***	-0.114 ***	-0.106 ***	-0.093 ***	-0.092 ***	-0.068 ***	-0.062 ***	-0.077 ***
					(0.009)	(0.015)	(0.018)	(0.009)	(0.009)	(0.017)	(0.019)	(0.008)
3-4 years since RA	0.437	0.440	0.436	0.453	-0.117 ***	-0.116 ***	-0.114 ***	-0.102 ***	-0.091 ***	-0.063 ***	-0.061 ***	-0.077 ***
					(0.011)	(0.018)	(0.021)	(0.009)	(0.010)	(0.020)	(0.022)	(0.009)
5-6 years since RA	0.406	0.405	0.397	0.429	-0.100 ***	-0.094 ***	-0.087 ***	-0.091 ***	-0.078 ***	-0.047 **	-0.043 **	-0.068 ***
					(0.010)	(0.017)	(0.020)	(0.009)	(0.010)	(0.020)	(0.022)	(0.009)
7-8 years since RA	0.378	0.376	0.361	0.407	-0.081 ***	-0.070 ***	-0.054 ***	-0.079 ***	-0.069 ***	-0.047 **	-0.033	-0.060 ***
					(0.010)	(0.017)	(0.019)	(0.009)	(0.010)	(0.019)	(0.022)	(0.009)
9-10 years since RA	0.361	0.351	0.341	0.394	-0.071 ***	-0.062 ***	-0.054 ***	-0.076 ***	-0.060 ***	-0.032 *	-0.018	-0.057 ***
					(0.010)	(0.017)	(0.019)	(0.010)	(0.010)	(0.019)	(0.021)	(0.009)
Share minority												
1-2 years since RA	0.903	0.916	0.918	0.906	-0.079 ***	-0.085 ***	-0.079 ***	-0.066 ***	-0.018 *	-0.033	-0.034	-0.015
					(0.012)	(0.019)	(0.023)	(0.011)	(0.011)	(0.020)	(0.023)	(0.011)
3-4 years since RA	0.893	0.905	0.908	0.900	-0.073 ***	-0.078 ***	-0.071 ***	-0.066 ***	-0.011	-0.024	-0.021	-0.009
					(0.013)	(0.022)	(0.025)	(0.012)	(0.012)	(0.023)	(0.026)	(0.011)
5-6 years since RA	0.889	0.894	0.894	0.897	-0.058 ***	-0.049 **	-0.051 **	-0.055 ***	-0.016	-0.018	-0.008	-0.006
					(0.012)	(0.021)	(0.025)	(0.011)	(0.012)	(0.024)	(0.026)	(0.011)
7-8 years since RA	0.882	0.893	0.887	0.893	-0.054 ***	-0.050 **	-0.040	-0.051 ***	-0.020	-0.022	0.009	-0.005
					(0.012)	(0.021)	(0.025)	(0.011)	(0.014)	(0.024)	(0.027)	(0.011)
9-10 years since RA	0.869	0.878	0.877	0.892	-0.045 ***	-0.040 *	-0.036	-0.049 ***	-0.011	-0.011	0.022	-0.007
					(0.013)	(0.022)	(0.026)	(0.011)	(0.015)	(0.025)	(0.027)	(0.011)

Notes : Estimates are the Intent to Treat effect sizes from an ordinary least squares regression of each outcome on treatment indicators (experimental and Section 8 effects were estimated in one model) and the baseline covariates listed in Appendix Table 1. Impacts by gender were estimated as an interaction with treatment status. Robust standard errors adjusted for household clustering are in parentheses. *** Treatment-control difference is significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Source and Sample : Census tract characteristics are interpolated data from the 1990 and 2000 decennial censuses as well as the 2005-09 American Community Survey. The sample in the columns labeled All and Through Age 18 is core household members ages 15-25 as of December 31, 2001 and under age 18 at baseline (N=2277 females, N=2366 males). The Proxy Address columns are limited to the subset of those youth for whom we have fairly complete address information via address updates from the adult long-term survey interview (N=826 females, N=626 males) and the Living with Adult columns are further limited to the subset who were still living with the adult as of the long-term survey interview (N=410 females, N=446 males). The results in the Through Age 18 columns reflect only the addresses of the youth up through age 18 and thus assume the addresses remained unchanged from age 18 through the end of the follow-up period (10 years after random assignment).