

YOUTH CRIMINAL BEHAVIOR IN THE MOVING TO OPPORTUNITY EXPERIMENT*

Jeffrey R. Kling
Jens Ludwig
Lawrence F. Katz

February 2004

The Moving to Opportunity (MTO) demonstration assigned housing vouchers via random lottery to low-income public housing residents in five cities. We use the exogenous variation in residential locations generated by the MTO demonstration to estimate the effects of neighborhoods on youth crime and delinquency. We find that the offer to relocate to lower-poverty areas reduces the incidence of arrests among female youth for violent crimes and property crimes, and increases self-reported problem behaviors and property crime arrests for male youth -- relative to a control group. Female and male youth move through MTO into similar types of neighborhoods, so the gender difference in MTO treatment effects seems to reflect differences in responses to similar neighborhoods. Within-family analyses similarly show that brothers and sisters respond differentially to the same new neighborhood environments with more adverse effects for males. Males show some short-term improvements in delinquent behaviors from moves to lower-poverty areas, but these effects are reversed and gender differences in MTO treatment effects become pronounced by 3 to 4 years after random assignment.

JEL classifications: H43, I18, J23.

* Support for this research was provided by grants from the National Science Foundation to the National Bureau of Economic Research (SBE-9876337 and BCS-0091854) and the National Consortium on Violence Research (SBR-9513040), as well as by the U.S. Department of Housing and Urban Development, the National Institute of Child Health and Development (R01-HD40404), the National Institute of Mental Health (R01-HD40444), the Robert Wood Johnson Foundation, the Russell Sage Foundation, the Smith Richardson Foundation, the MacArthur Foundation, the W.T. Grant Foundation, the Spencer Foundation. Additional support was provided by grants to Princeton University from the Robert Wood Johnson Foundation and from NICHD (5P30-HD32030 for the Office of Population Research), by the Princeton Industrial Relations Section, the Bendheim-Thomas Center for Research on Child Wellbeing, the Princeton Center for Health and Wellbeing, the National Bureau of Economic Research, and a Brookings Institution fellowship supported by the Andrew W. Mellon foundation. We are grateful to Todd Richardson and Mark Shroder at HUD, to Judie Feins, Barbara Goodson, Robin Jacob, Stephen Kennedy, and Larry Orr of Abt Associates, to our collaborators Jeanne Brooks-Gunn, Alessandra Del Conte Dickovick, Greg Duncan, Tama Leventhal, Jeffrey Liebman, Meghan McNally, Lisa Sanbonmatsu, Justin Treloar and Eric Younger, and to seminar participants at Berkeley and numerous colleagues for valuable suggestions. Any findings or conclusions expressed are those of the authors.

I. INTRODUCTION

This paper examines the causal effects of a residential mobility experiment on youth criminal behavior. Crime rates vary dramatically across countries, states, cities and -- most relevant to this study -- neighborhoods [Glaeser, Sacerdote and Scheinkman 1996; Sampson, Raudenbush and Earls, 1997]. Understanding whether this variation in criminal behavior reflects the causal effects of social context, or instead simply reflects how high-risk people are sorted across areas, is relevant for government decisions that influence sorting across neighborhoods and schools, as well as for the design of other criminal justice and social policy interventions. This question is also of interest because of the substantial cost that crime imposes on American society – on the order of \$1 trillion per year by one recent estimate [Anderson 1999a].

A large and growing theoretical literature offers several explanations for how social interactions and context may affect criminal behavior [Jencks and Mayer 1990; Cook and Goss 1996; Sampson, Raudenbush and Earls 1997; Glaeser, Sacerdote and Scheinkman 2003]. Most theories suggest that the monetary and non-monetary returns to crime will be greater in communities where crime is more prevalent, and where affluent adults active in maintaining social order are scarcer. The net benefits of crime may also vary across neighborhoods because of variation in the quality or quantity of local schooling opportunities, police, and other institutions that have been shown to influence criminal behavior.¹ Some theories suggest that moves to less disadvantaged communities may have little effect on crime, if for example teens simply rejoin the same types of peer groups as in their old neighborhoods. Such moves could even increase criminal behavior if youth feel resentful towards or are discriminated against by their new, more affluent peers.

¹ Levitt [1997, 2002] provides evidence for the effects of police resources on crime. Lochner and Moretti [2001] assess the causal effects of educational attainment on criminal behavior.

Empirical claims for “neighborhood effects” on crime date back to at least the 1940’s [Shaw and McKay, 1942]. Most of the available empirical evidence relates the behavior of individuals to the characteristics of the neighborhoods in which they or their families have selected to live, and suggests that disadvantaged neighborhoods are “criminogenic.”² One recent review argues that of the outcomes studied in the neighborhood effects literature, the “strongest evidence links neighborhood processes to crime” [Sampson, Morenoff and Gannon-Rowley 2002]. Yet drawing causal inferences from these findings is complicated by the possibility of unmeasured individual- or family-level attributes that influence both criminal activity and neighborhood selection. The outcomes of poor teens from the Bronx may provide a poor counterfactual for poor youth whose families have managed to move to Scarsdale, New York.

In this paper we overcome this basic identification problem by examining the effects of neighborhood mobility on youth crime using data from the Moving to Opportunity (MTO) randomized housing-mobility experiment. Sponsored by the U.S. Department of Housing and Urban Development (HUD), MTO has been in operation since 1994 in five cities: Baltimore, Boston, Chicago, Los Angeles, and New York. Eligibility for the program was restricted to low-income families with children in these five cities, living within public or Section 8 project-based housing in selected high-poverty census tracts.³ The roughly 4,600 families who volunteered for the program from 1994 to 1997 were randomly assigned into one of the following three groups: experimental, Section 8, and control. The “experimental” group was offered the opportunity to relocate using a housing voucher that could only be used to lease a unit in census tracts with

² A different empirical approach is taken by Glaeser, Sacerdote and Scheinkman [1996], who show that observed variation in crime rates exceeds what can be predicted by “fundamental” factors. This excess variation is attributed to the effects of social interactions.

³ Section 8 project-based housing might be thought of as essentially privately-operated public housing [Olsen 2003]. The U.S. Department of Housing and Urban Development contracts with private providers to develop and manage housing projects that include units reserved for low-income families.

1990 poverty rates of 10 percent or less.⁴ Movers through MTO were required to stay in these tracts for at least one year. Experimental group families were also provided with mobility assistance and in some cases other counseling services as well. Families assigned to the “Section 8” group were offered housing vouchers with no constraints under the MTO program design on where the vouchers could be redeemed. Families assigned to the “control” group were offered no services under MTO, but did not lose access to social services to which they were otherwise entitled such as public housing.

Because of random assignment, MTO yields three comparable groups of families living in very different kinds of neighborhoods during the post-program period. Previous studies that use the exogenous variation in neighborhoods induced by MTO within individual demonstration sites on balance yield evidence consistent with the view that moving to less distressed communities reduces anti-social behavior by youth, at least in the short run (1 to 3 years from random assignment).⁵ The present paper improves upon early MTO research in several ways. We measure youth criminal behavior using both survey and official arrest data for youth across all five MTO sites. Data from surveys that we conducted in 2002 are available for youth who were in their peak offending ages (15-20) at the time of the survey. We also use the official arrest data to examine the impact of MTO on an expanded youth sample (15-25 at the end of 2001) that has been of peak offending age at any point during the post-program period. Unlike earlier studies that focus only on short-term outcomes, we have information on youth behavior

⁴ Housing vouchers provide families with subsidies to live in private-market housing. The subsidy amount is typically defined as the difference between 30 percent of the household’s income and the HUD-defined Fair Market Rent, which equals either the 40th or 45th percentile of the local area rent distribution.

⁵ In the Boston site, boys in the experimental and Section 8 groups exhibit about one-third fewer problem behaviors compared to controls in the short run [Katz, Kling and Liebman 2001]. For the Baltimore site, official arrest data suggest that teens in both treatment groups are less likely than controls to be arrested for violent crimes. These short-run impacts are large for both boys and girls, but not statistically significant when disaggregated by gender [Ludwig, Duncan and Hirschfield 2001]. Short-term survey data from the New York site reveals no statistically significant differences across groups in teen delinquency or substance use [Leventhal and Brooks-Gunn 2003].

from 4 to 7 years following random assignment. Finally, we are also able to examine effects on a variety of neighborhood and youth characteristics that the major current theories predict should mediate neighborhood effects on youth crime.

We find that the offer of a housing voucher to relocate to a lower-poverty area ultimately reduces the chance that females will be arrested for a violent crime, with effects for both treatment groups that are equal to nearly half of the arrest rate for the control group. We also find some evidence that the experimental treatment may substantially reduce the chances that females are arrested for property offenses as well. On the other hand for males, assignment to the experimental group increases self-reported problem behaviors and lifetime arrests by around one-fifth of the control group's mean, with the difference in arrests driven largely by property offenses. The results do not appear to be driven by different responses by males and females to the stress and disruption of moving *per se*, in part because in the first few years after random assignment experimental males experience fewer violent-crime arrests compared to controls. Some of the increase in property-crime arrests for males could be explained by an increase in the probability of arrest in lower-poverty communities, although our data are somewhat limited on this point. The gender difference in effects -- also found in recent MTO research on education, substance use, mental health, and physical health [Kling and Liebman 2004] -- seems to reflect differences in how males and females respond to similar neighborhoods. We find that boys and girls in the same randomly assigned treatment groups move into similar types of neighborhoods, and within families, brothers and sisters respond differentially to the same mobility patterns.

Overall, we find little effect on total arrest rates of moving out of high poverty neighborhoods because the reductions in arrests for violent and other crimes among females are largely offset by the increase in property-crime arrests among males. But from a social welfare

perspective we may not wish to give violent and property crimes equal weight, given that the former impose much greater costs to society than do the latter. Our findings suggest that the MTO experimental intervention leads to substantial reductions in the costs of crime by youth in the experimental group, although the standard errors for these point estimates are quite large.⁶

The remainder of the paper is organized as follows. Section II reviews the conceptual framework behind our analysis. Section III presents our econometric approach. Section IV describes our data. Our main findings on youth crime and delinquency outcomes are presented in section V. We explore neighborhood context and a variety of other potential mediating factors in section VI in an attempt to understand the mechanisms driving our results. Section VII examines net effects of MTO on the social costs of crime. Section VIII concludes.

II. CONCEPTUAL FRAMEWORK

Jencks and Mayer [1990] discuss three classes of models that predict moving out of high poverty neighborhoods should reduce participants' involvement in crime – epidemic models, collective socialization models, and institutional models – as well as a relative deprivation model predicting increases in youth crime. Epidemic models emphasize the tendency for “like to beget like.”⁷ Collective socialization arguments focus on the influence of local adults as role models for the returns to pro-social behaviors such as schooling or work [Wilson 1987; Ludwig 1999],

⁶ We recognize that experimental group youth in new neighborhoods could potentially affect criminal behavior by non-MTO youth living there, but the MTO experimental design does not permit informative inferences to be made about such spillover effects.

⁷ If law enforcement resources within an area are fixed, at least in the short run, then increased criminal behavior by one's neighbors depresses the chances that any given crime results in arrest [Sah 1991]. This type of model explains why we may observe quite different equilibrium crime rates across otherwise similar communities. A similar outcome may result if teens assume that their neighbors or peers have superior information about the benefits and costs of engaging in crime and interpret criminal activity by local residents as signaling the existence of net benefits to crime, leading to an “information cascade” [Cook and Goss 1996]. Epidemic models predict that if youth move to neighborhoods with lower crime as well as lower poverty rates, then criminal activity among movers should be reduced as a result.

or as enforcers of pro-social norms.⁸ Institutional models focus on the possibility that the quality of local schools, police and other institutions may be positively related to neighborhood affluence.⁹ Relative deprivation models focus on the psychological reaction of people to being surrounded by higher-achieving, more affluent peers. Some teens may feel resentful or anxious as a result. The heightened competition for grades and jobs within affluent communities could produce similar feelings, or could spur at least some youth to work even harder.

Because parents appear to provide boys more freedom than girls to socialize within their neighborhoods [Botcher 2001], the epidemic and collective socialization theories predict that boys may benefit more than girls from residential relocation. The institutional and relative deprivation theories do not yield clear predictions about gender differences in treatment effects.

The possibility that youth may sort themselves into the same type of social group in their old and new neighborhoods is also discussed by Jencks and Mayer [1990]. This scenario predicts little or no net effect on youth behavior from MTO moves. In principle, residential relocation could also lead youth to change the type of peer group with which the youth associate, with either “upward” or “downward” clique mobility. Akerlof and Kranton [2000] describe a model of peer sorting that depends in part on the match between a teen’s characteristics or behaviors and those idealized by a given group.¹⁰

⁸ Residents may be less likely to free ride on the efforts of their neighbors to maintain social control in communities characterized by shared values and high levels of trust – what Sampson, Raudenbush and Earls [1997] term “collective efficacy,” which they show is strongly correlated with neighborhood crime rates.

⁹ In principle predicting the effect of MTO on local policing is complicated by the fact that some police departments use targeted patrol programs that devote extra resources to high-risk areas. In practice, prior qualitative research suggests that policing is less effective in disadvantaged communities [Anderson 1998].

¹⁰ Living in a new neighborhood may present youth with more pro-social peer groups from which to choose, thereby enhancing the chances of a match. Downward clique mobility may occur if a teen’s pro-social assets are less valuable in their new environment. For example, teens that are academically successful in their old schools could be assigned to remedial classes in more competitive schools, which could make admission to an academic clique difficult. While achievement is a personal attribute that is hard to change, the technology required to gain acceptance into a community’s delinquent crowd is always readily available to all teens.

This peer sorting process may work differently for males and females because the set of attributes that generate social standing appear to differ by race and gender, and could in principle differ along class lines as well. For

Even if residential relocation does not change the type of peer group that teens experience, the residential relocation could still change behavior if the net returns to pro- or anti-social behavior vary across communities. The epidemic, collective socialization and institutional models predict teens in anti-social peer groups to be more anti-social in distressed neighborhoods, and those in pro-social peer groups to be more pro-social in advantaged areas.¹¹

Neighborhood effects on the quality of the teen's home environment could also affect youth outcomes. Family income could be increased through proximity to employment opportunities or improved access to neighbors who can provide useful job contacts [Wilson 1987]. Improved mental and physical health or family management practices among parents after residential relocation could also benefit youth [Furstenberg et al. 1999]. Teen behavior could also be affected by a generic moving effect, independent of any changes in specific neighborhood attributes. In section VI we present evidence from the MTO demonstration that bears on the relevance of these various models for understanding youth criminal behavior.

III. ANALYTICAL METHODS

Most previous non-experimental studies of neighborhood effects use micro-data to relate variation in criminal activity or other behaviors to variation in census tract or zip code attributes,

example, Ferguson [2001] shows that humor is more important for popularity among boys than girls. If tastes in humor differ more sharply across class lines than do tastes in "cool clothes," the most important criteria for popularity among girls, the social assets of teens who make MTO moves may depreciate more for boys than girls. Ferguson also finds that "being tough" is much more important for social success for black than white males, and is more important for males than females for both blacks and whites. If a similar differential holds across class lines, MTO boys who have developed "toughness capital" to gain status among all teens in their old neighborhoods may find that in the new neighborhood this attribute serves to impress primarily the area's delinquent group.

¹¹ The fieldwork of Elijah Anderson [1998, 1999b] raises the possibility of the reverse program impact. According to Anderson, the social interactions in distressed urban communities – particularly for males – are governed by "street values" that emphasize constant challenges to prove toughness, as well as displays of wealth (for example by wearing designer clothes). In this case teens in delinquent peer groups may find affluent areas to be a target-rich environment full of compliant youth who give up valuable loot with little resistance – thereby increasing the benefits and reducing the costs of "street" behavior. This scenario suggests that a teen's initial social group may affect their treatment response.

conditioning on observable individual and family characteristics. Bias may result from unmeasured variables that are associated with both neighborhood sorting and youth behavior. Predicting the magnitude or even direction of this bias is difficult. Many social scientists believe that conditional on observed family characteristics, the more effective or motivated parents will be the ones who wind up in more rather than less advantaged communities. Yet the short-term results from the Boston and Baltimore MTO sites suggest that among parents assigned to the experimental or Section 8 groups, those whose children are at relatively greater risk for problem or criminal behavior are the ones who are most likely to relocate through the program [Katz, Kling and Liebman 2001; Ludwig, Duncan and Hirschfield 2001].

MTO helps overcome the basic identification problem confronting previous studies of neighborhood effects by randomly assigning families into groups that are offered different types of mobility assistance. As we demonstrate below, random assignment generates pronounced differences across groups in neighborhood environments. With random assignment, the average outcomes of youth in the control group provide us with unbiased estimates for the counterfactual outcomes that youth assigned to the experimental and Section 8 groups would have experienced in the absence of MTO's mobility interventions. Differences across groups can be attributed to differences in mobility and neighborhood characteristics rather than to other pre-existing differences among families. Since MTO changes a wide variety of neighborhood characteristics simultaneously, with only two treatment groups our ability to isolate the causal effects of changes in particular neighborhood measures is limited.

The most straightforward way to exploit the exogenous variation in neighborhood characteristics generated by MTO is to simply compare average outcomes of youth assigned to different MTO groups, known as the *intent-to-treat (ITT) effect*. Assuming only that random

assignment is in fact random, the ITT estimates identify the causal effect of offering families the services made available through the experimental or Section 8 treatments. More formally, let Y represent some outcome of interest, such as the proportion of youth who engage in delinquent or criminal activity, and let $Z=1$ represent assignment to a treatment group, where $Z=0$ represents assignment to the control group. The ITT effect is given by:

$$(1) \quad \text{ITT} = E[Y \mid Z=1] - E[Y \mid Z=0]$$

In practice we calculate the ITT effect via ordinary least squares. We condition on a set of (pre-random assignment) baseline characteristics (X) that help predict post-program outcomes, including site, survey measures of the socio-demographic characteristics of household members, and survey reports about youth experiences in school such as expulsions or enrollment in gifted and talented classes; the complete list of covariates is given in Appendix Table A1. In models where the outcome of interest comes from official arrest data, we also condition on a set of indicators for the number of pre-program arrests for violent, property, drug or other offenses. Because the distribution of pre-program characteristics should be balanced across treatment groups with random assignment, conditioning on these variables serves mainly to improve the precision with which we estimate the treatment effect μ_1 , in equation (2).

$$(2) \quad Y_i = \mu_0 + Z_i\mu_1 + X_i\mu_2 + \varepsilon_i ,$$

where (i) indexes individuals. We estimate this model using pooled data from all three MTO groups with Z consisting of two separate indicators for assignment to the experimental and Section 8 groups. The difference in average outcomes between the treatment and control group is represented by the elements of μ_1 . Standard errors are adjusted for the presence of youth from the same family. Our ITT and all other estimates reported are computed using sample weights.¹²

¹² The weights we use to analyze survey-reported outcomes have three components, described in detail in Orr et al. [2003], Appendix B. In particular, the survey procedure attempted to contact a subsample of difficult to locate

As a benchmark against which to assess the magnitude of our estimated ITT effects, we also present the *adjusted control mean (ACM)*. The ACM represents the average outcome of the control group, adjusted for chance differences in the distribution of the pre-program characteristics across MTO treatment groups. Taking the case of a single treatment group for simplicity of exposition, the ACM is calculated as the average outcome Y for the entire MTO youth sample less the ITT effect times the proportion of the sample assigned to the treatment group, as in equation (3).

$$(3) \quad \text{ACM} = E[Y] - (P[Z=1] \times \mu_1)$$

To examine whether treatment effects vary by gender, we estimate a modified version of (2) that includes interactions between indicators for treatment group and gender, denoted by the indicator G . The difference in average outcomes between the females in the treatment and control groups is represented by β_1 and for males the difference is represented by β_2 .

$$(4) \quad Y_i = \beta_0 + G_i Z_i \beta_1 + (1-G_i) Z_i \beta_2 + G_i \beta_3 + X_i \beta_4 + v_i$$

Also of interest is whether MTO's effects change over time as youth spend more time in their new neighborhoods. We explore this possibility using a panel of all post-randomization person-quarters for MTO youth, with quarter since random assignment indexed by (t) , to examine how across-group differences in the average number of arrests per quarter change in periods after random assignment. The regression model includes a set of indicators for time

cases. Sub-sample members receive greater weight since, in addition to themselves, they represent individuals whom we did not attempt to contact during the sub-sampling phase. Youth in the survey from large families receive greater weight since we randomly sampled two children per household so these youth are representative of a larger fraction of the study population. Another reason weights are used is that the ratio of individuals randomly assigned to treatment groups was changed during the course of the demonstration to adjust in response to differences between projected and actual use of offered vouchers, and weighting avoids potential confounding of treatment group with calendar time effects. Individuals within treatment groups are weighted by their inverse probability of assignment to the group to account for changes in the random assignment ratios. Models that focus on official arrest outcomes use only this last weighting component.

since random assignment (R_{it} based on calendar quarters) and a set of indicators for calendar quarter (Q_{it}) to capture residual variation in youth offending rates during the 1990's.¹³

$$(5) \quad Y_{it} = \varphi_0 + Z_i\varphi_1 + X_i\varphi_4 + R_{it}\varphi_5 + Q_{it}\varphi_6 + \psi_{it}$$

We estimate this model separately for time periods such as 1-2 years after random assignment, 3-4 years after random assignment, etc. The coefficient φ_1 represents the average difference between the treatment and control groups during a particular time period.

Generalizing from our ITT estimates is complicated by the possibility of heterogeneity across individuals in their response to mobility programs and variation across such programs in take-up rates. We can address the second issue in part by presenting separate estimates for the *effects of treatment on the treated (TOT)*. In our application, the “treatment” is defined as relocation through the MTO program. As we demonstrate below, the MTO control group experiences non-trivial mobility over our study period, although none of these families are counted as having been “treated” because these moves do not necessarily have the same timing or nature as those induced by the MTO program. Put differently, the TOT estimate seeks to identify the effect of moving through the MTO program on the “compliers” compared to the counterfactual mobility trajectory that these families would have experienced otherwise.

The TOT impact can be calculated as the difference in average outcomes across treatment groups (the ITT effect from above) divided by the difference in treatment take-up rates [Bloom 1984]. We use two-stage least squares to estimate the effects of treatment take-up D , with treatment group assignment Z as the instrumental variable for D .

$$(6) \quad D_i = \alpha_0 + Z_i\alpha_1 + X_i\alpha_2 + \zeta_i$$

$$(7) \quad Y_i = \delta_0 + D_i\delta_1 + X_i\delta_2 + v_i$$

¹³ The indexing for calendar-quarter indicators reflects the fact that the date of random assignment varies across the sample. For example, the first post-randomization quarter falls in a different calendar quarter for different youth.

The TOT estimate will be an unbiased estimate of the effects of treatment on the treated if random assignment is truly random, and if assignment to the treatment group has no effect on those who do not move through MTO. This second assumption may not be literally true, since the counseling services and search assistance offered to treatment families may influence later mobility patterns or other youth behaviors even among families that do not relocate through MTO. The disappointment of searching but failing to find an apartment may also affect non-movers in the treatment groups. If the effects of treatment-group assignment are substantially smaller for those who do not move through MTO compared to those who do, our TOT estimates will approximate the effects of MTO moves on those who move through the MTO program.

To assess the magnitude of the TOT, for both the experimental and Section 8 treatments we also calculate the average outcomes of the would-be “compliers” [Angrist, Imbens and Rubin 1996] in the control group, who would have relocated through MTO had they been assigned to one of these treatment groups. Although we do not know which of the control families would have been compliers if they had been assigned to a treatment group, the MTO data nonetheless identify the *control complier mean (CCM)* as in equation (8) [Katz, Kling and Liebman 2001]:

$$(8) \quad CCM = E[Y | Z=1, D=1] - \delta_1$$

Finally, the rich survey data collected from MTO adults and youth provide us with an opportunity to learn more about how the program shapes the behavioral responses reported below. Some of the behavioral models reviewed in Section II yield similar predictions for how MTO may impact youth crime, but the models differ in their predictions about what types of factors should mediate these program impacts.

In what follows, we also report ITT effects on a fairly rich set of potential mediators. We recognize that evidence for across-group differences in our mediating factors is not *proof* that

one behavioral model or another is responsible for differences in youths' anti-social behavior. Many of the mediating variables that we examine below could be either cause or consequence of the program's impacts on youth crime. But in using the experimental design we ensure that variation in both candidate mediators and youth outcomes are driven ultimately by variation in neighborhood context, rather than by unmeasured teen or family characteristics. Evidence that a given mediating factor does not change as a result of MTO is taken as evidence against that factor's importance in explaining our results. For mediators that do change with MTO, we cannot rule such factors out as candidate explanations for program effects on youth crime.

Some mediators are reported by the parent (such as neighborhood safety), and not reported separately for each youth. We model these reports as household averages of treatment effects that may differ by gender, and estimate effects on these mediators using an aggregated regression model. Let j index households, and let \bar{G} and \bar{X} denote the household averages of G and X , as in equation (9).

$$(9) \quad Y_j = \pi_0 + \bar{G}_j Z_j \pi_1 + (1 - \bar{G}_j) Z_j \pi_2 + \bar{G}_j \pi_3 + \bar{X}_j \pi_4 + \eta_j$$

The weights for this regression are the household sum of the weights for use for parental reports on youth, which incorporate the probability that the youth was sampled if there were more than two children in the household. The identification for effects that differ by gender comes from the fact that household gender composition varies, with many households only having one youth present. Estimating the effects in a joint model allows the treatment effect for females to be estimated conditional on having the effect for males held constant and vice versa; otherwise estimates of the effect for one gender might be driven by the presence of the other gender in multi-sibling households.

IV. DATA

Our data on youth delinquency and criminal behavior are derived from two main sources: survey data and administrative arrest records. Information on potential mediating processes that could lead to these outcomes comes from our surveys as well as administrative data on local-area crime rates. Our data sources are described in detail in Appendix A.

During 2002 surveys were completed with 1807 youth ages 15-20 from the MTO households, with an effective response rate of 88% (90% for females and 86% for males). The surveys were generally conducted in-person and captured, in addition to self-reported arrests, multiple delinquent and anti-social behaviors beyond those that result in an arrest. Interviews conducted with an adult (usually the youth's mother) from the MTO household provide additional information about the household and the youth.

Administrative data on official arrest histories of the MTO youth enable us to examine involvement with different types of criminal activity, and explore the time pattern in program impacts. MTO youth were linked to adult and juvenile arrest records using information such as name, race, sex, date of birth, and SSN. In addition to obtaining records for the five MTO sites, we also obtained adult and in some cases juvenile arrest records from other states to which MTO participants had moved. Although some youth moved to states from which we did not obtain administrative data, we do have complete arrest histories for 94% of youth ages 15-20 and the response rate is very similar across MTO groups. These arrest histories include information on the date of all arrests, each criminal charge for which the individual was arrested, and typically information on dispositions as well. Our analyses use arrests through 2001 and exclude motor vehicle violations. An important policy question that we cannot answer is how MTO affects how the costs of crime are distributed across communities. In principle all of the crime committed by

experimental and Section 8 youth could be confined to the baseline neighborhoods, if, for example, youth only engage in such behaviors when visiting their old friends. The official arrest histories available to us are silent on this issue because they contain almost no information about where the criminal event occurs.¹⁴

The families of the youth in our main survey sample enrolled in the MTO demonstration from 1994 to 1997, when the youth were 8 -16 years old. At the time of enrollment, the head of household completed a baseline survey which included information about the family as well as some specific information about each child. Descriptive statistics for the baseline characteristics of youth with whom we completed a survey are shown in the first panel of Table I. Overall about two-thirds of MTO participants are black, with the program populations in Chicago and Baltimore almost entirely black and an even mix between black and Hispanic in the other sites. MTO households are quite poor, with around three-quarters having been on welfare at baseline. One quarter of household heads had their first child before the age of 18, and only a little more than half of all heads had a GED or high school diploma. Around three-quarters of households report gangs and drugs as the first or second most important reason they enrolled in the MTO program, while around one-half report access to better schools as one of their top two reasons.

The first three columns of Table I show that of the 30 comparisons of mean baseline characteristics between the treatment and control groups, none of the treatment - control differences is significant at the 5 percent level, and five are statistically significant at the 10 percent level.¹⁵ Columns 4-6 of Table I reveal almost no statistically significant differences

¹⁴ Some administrative arrest histories provide information about the location of the police station at which the arrestee is first processed. These data are not very informative in cities where police beats cover fairly large geographic areas. Also some arrestees may be first processed in beats other than those in which they were arrested.

¹⁵ In analysis of this same MTO youth sample separately by gender, Kling and Liebman [2004] find some evidence of differences in baseline characteristics due to chance (even if the effective response rate to the survey had been 100%) and some that may be due to attrition.

across groups for youth for whom administrative arrest data are available. This balance in baseline characteristics is true for official arrest as well as survey data: the second to last row of the table shows that according to official arrest histories, around 4 percent of youth in each of the MTO groups had been arrested at least once prior to random assignment.

In analysis not shown in the table, annual arrest rates begin to increase noticeably around age 13 or 14, and peak between the ages of 18 and 20 among young adults in the control group (ages 22-25 at the end of 2001). Nearly 40 percent of these young adults in the control group had been arrested at least once in their lives. The proportion ever arrested is more than 2.5 times higher for males than females (53 versus 19 percent). The “criminal careers” of the MTO control group appear to follow a trajectory that is similar to what has been found for other urban samples [Tracy, Wolfgang and Figlio, 1990].

Eligibility for the MTO program was limited to families in public housing or Section 8 project-based housing located in some of the most disadvantaged census tracts in the five MTO cities and, for that matter, in the country as a whole. Of the families with youth in our survey sample (15-20 at the end of 2001), 44 percent of those in the experimental group and 57 percent of those in the Section 8 group complied with treatment (that is, relocated through MTO). These moves lead to substantial differences across treatment groups in neighborhood attributes, although these differences narrow somewhat over time due to the subsequent mobility of the treatment and control families.

Table II shows that one year after random assignment, control group families continue to live in neighborhoods (census tracts) where around half of the residents are poor and around 90 percent are racial or ethnic minorities. Nearly two-thirds of families in these neighborhoods were headed by a single female. Table II also shows that families assigned to the experimental

group are 36 times more likely than those in the control group to live in a very low-poverty tract (<10 percent poverty rate), and almost four times as likely to live in tracts with 2000 poverty rates of between 10 and 20 percent.¹⁶ Assignment to the experimental group also increased the proportion of neighbors who are “affluent” – that is, have a college degree, or work in a managerial or professional occupation – by around 60 and 30 percent, respectively. The MTO moves made by families in the Section 8 group are on average less dramatic than those for experimental families. But the addresses of Section 8 families one year after random assignment are still in census tracts with average 2000 poverty rates that are nearly one-quarter lower than those of the control group.

Given the changes in tract poverty rates induced by MTO, it is surprising that the program engenders so little residential integration with respect to race. One year after random assignment, only one-quarter of experimental compliers and one-tenth of Section 8 compliers live in majority-white census tracts. The average family in all three MTO groups lives in a census tract where the large majority of residents are also minorities.

Another notable feature of the MTO program is that over time the average neighborhood characteristics of families across treatment groups begin to converge, which can be seen by comparing columns 1-5 with columns 6-10. All three treatment groups experience a decline in the proportion of families living in the highest-poverty tracts, which may be caused in part by the demolition of some of the baseline public housing projects under HUD’s Hope VI program. Part of this convergence also seems to come from the movement of control-group families into neighborhoods with moderate poverty levels (between 10 and 30 percent). As a result, the mean

¹⁶ Note that while experimental movers were required to move to tracts with 1990 poverty rates less than 10 percent, not all of their initial tracts have 2000 poverty rates below 10 percent because of increases in destination tract poverty rates from 1990 to 2000.

difference in tract poverty rates between the control and experimental groups declines from 15 to 11 percentage points (nearly 30 percent) from years one to four.

V. MTO EFFECTS ON YOUTH DELINQUENCY AND CRIME

To preview the findings in this section, our analysis suggests that moving to lower poverty neighborhoods leads to better behavior among females, particularly with respect to violent crime, but on balance leads to worse behavior for males. Compared to males in the control group, those in the experimental group have higher rates of problem behavior and property-crime arrests, although fewer arrests for violent crimes in the short run.

Our main findings from the survey data (for youths aged 15 to 20 at the end of 2001) are shown in Table III. Since we use this same table format to present many of the results that follow, we describe the table structure in detail here. The first column presents the adjusted control mean (ACM), which is the average outcome value for the control group holding the distribution of baseline characteristics described in the previous section constant across groups. The first row of the table presents results for an index measuring the fraction of eleven self-reported behavior problems (BPI).¹⁷ The average score for youth in the control group is .339. The experimental ITT estimate in the second column shows that assignment to the experimental group increases this index by 2 percentage points when boys and girls are pooled together. This pooled result masks a reduction in behavior problems relative to controls for girls, and a statistically significant increase relative to controls for boys. The third column shows that the increase is equal to 16 percentage points among boys who moved through the experimental

¹⁷ The behavior problems index uses items from the National Longitudinal Survey of Youth 1997 (NLSY97) and is defined as the fraction of 11 problems that youth report to be “often” or “sometimes” true of themselves: has difficulty concentrating; cheats or lies; teases others; is disobedient at home; has difficulty getting along with other children; has trouble sitting still; has a hot temper; would rather be alone; hangs around other children who get into trouble; is disobedient at school; and has trouble getting along with teachers.

group. Note that the TOT effect is equal to 2.5 times the ITT effect, consistent with the fact that around 40 percent of male youth in the experimental group comply with the MTO treatment ($2.5 = 1/.4$). The average behavioral problem fraction among control group families who would have complied with the experimental treatment had they been assigned to that group is shown in the fourth column. The effect of complying with the experimental treatment on the compliers (TOT=.160) for males is equal to nearly 60 percent of the control complier mean (CCM=.270).

In the upper panel of Table III, we do not observe any statistically significant impacts for BPI scores for girls in either treatment group, or for Section 8 boys. Table III also shows no differences across MTO groups for either boys or girls in our delinquency index.¹⁸ Detailed results for the individual components of the behavior problem and delinquency indices are presented in Appendix Table A2.

Key outcome measures available in both the survey and the administrative data indicate whether each youth has ever been arrested. This formulation has a pragmatic motivation, because survey respondents are likely to have trouble determining which arrests occurred before rather than after random assignment. Criminologists have documented the tendency of respondents to err on the side of reporting events that in fact occur just outside of a survey question's recall period, known as "telescoping." Our focus on lifetime arrests should yield results that are almost identical to properly-reported post-randomization arrests, because with random assignment the distribution of pre-program arrests should be equivalent across MTO groups. With our administrative data analysis we can also explicitly condition on each teen's pre-program arrest history.

¹⁸ The delinquency index uses items from the National Longitudinal Survey of Youth 1997 (NLSY97) and captures more serious anti-social behaviors. The index consists of the fraction of nine delinquent behaviors in which the teen has ever engaged: carrying a hand gun; belonging to a gang; damaging property; stealing something worth less than \$50; stealing something worth more than \$50; some other property crime; attacking someone with the intention of hurting them; selling drugs; or being arrested.

For the survey-based measure of whether the teen has ever been arrested in Table III, none of our impact estimates are statistically significant. In contrast, when we focus instead on administrative arrest data in Table IV, we find that females in the Section 8 group are about one-quarter less likely to have ever been arrested than those in the control group ($p < .05$). The difference in the share ever arrested for experimental versus control females is about half as large as the Section 8-control difference, and not statistically significant. The administrative data also reveal an adverse treatment response among boys: males in the experimental group are about 13 percent more likely than those in the control group to have ever been arrested ($p < .10$). The Section 8-control difference for males is of about the same magnitude as the experimental-control difference, although not statistically significant. Given the effects in opposite directions on arrests for males and females, estimates from the pooled sample of males and females together imply essentially no program effect on whether teens have ever been arrested.

We believe that part of the reason that we do not see statistically significant between-group differences in the survey data is that MTO youth appear to under-report anti-social behavior to our interviewers. Direct evidence for under-reporting with our MTO survey measure of “ever arrested” comes from a comparison with the official arrest data for these same youths. The control mean for our survey measure equals about two-thirds the figure recorded by official data. For females, the survey estimate is about one-half the official figure, and for males the former is about three-quarters the latter. We also find that the average delinquency score for both MTO males and females are about one-half the self-reported values for demographically similar respondents in the National Longitudinal Survey of Youth 1997 (NLSY97), with MTO reports for involvement with hard drugs, gangs, guns and violence in particular that appear to be

suspiciously low. Uniform under-reporting by youth in all three MTO groups can only explain part of the difference between the results from the survey versus official arrest data.¹⁹

Table IV also shows that focusing on the total number of lifetime arrests rather than whether the teen was ever arrested – that is, capturing treatment effects on both the intensive as well as extensive margins of criminal activity – produces more pronounced differences between the experimental and control groups.²⁰ For comparison with the survey data, we show results for ages 15-20. We also show results for our preferred sample for analysis of administrative arrest data, which is age 15-25 at the end of 2001. This age grouping captures most MTO youth who were in their peak-offending years at any point between random assignment and the end of our 2001 data analysis period and increases the sample size by nearly 50 percent. For females the number of lifetime arrests is about 33 percent lower for the experimental than control group, and the difference is statistically significant in both age groupings.

Table V extends this analysis by examining more detailed crime categories for youth ages 15-25 at the end of 2001. We find an effect on violent crimes (mainly assaults) for females in both treatment groups, and also that the experimental group females are much less likely than controls to be arrested for property crimes, and drug offenses ($p < .10$). We find the reduction in property-crime arrests among experimental group females, occurring primarily in burglary, breaking and entering and trespassing. In contrast, we find an increase in property-crime arrests for treatment group males, driven mostly by larceny. About two-thirds of the experimental-control difference in drug arrests for females is for drug dealing. In analysis not shown in the

¹⁹ A data-generating model with a constant propensity to under-report arrests, orthogonal to treatment-group assignment, could explain the entire difference between the survey and administrative-data point estimates for the experimental treatment's impact on females. But such a model could explain less than one-tenth of the difference in point estimates for the experimental-control contrast for males, and only around one-quarter and one-half of the difference in the Section 8-only point estimates for females and males, respectively.

²⁰ Previous research has found race and class differences in criminal offending may be driven by differences in high-frequency offending [Elliott and Ageton 1980], suggesting that this dimension might also be important for studying neighborhood effects.

table, we find that the program impacts are not substantially different for those who were in their early versus late adolescent years at the time of random assignment, and that interactions of treatment effects with age are not significant.²¹

Table VI shows the dynamics of treatment impacts on arrest rates over time for youth ages 15-25. During the first two years following random assignment, males in the experimental group have significantly lower rates of violent-crime arrests than those in the control group, but this effect appears to dissipate over time. The experimental-control difference in non-violent arrests for males becomes more positive over time, with significantly higher arrest rates in the third and fourth years after random assignment. In analysis not shown in the table, a smaller sample for which we have data five to six years after random assignment shows that the magnitude of the effects on non-violent arrests decreases in absolute value and becomes statistically insignificant for males (and also for females). While this analysis is organized around time since random assignment, we note that observations longer after random assignment also reflect later average calendar time -- so these results are not a pure effect of exposure to treatment.

The results shown in Table VI also suggest a way to reconcile our findings with the short-term results reported for Baltimore and Boston, which suggested substantial reductions for males in problem or violent criminal behavior [Katz, Kling and Liebman, 2001, Ludwig, Duncan and Hirschfield, 2001]. One concern with the earlier short-term findings is that they may simply have reflected idiosyncratic effects unique to those two demonstration sites. But when we use the same age group as in Ludwig, Duncan and Hirschfield [2001], we find that the MTO

²¹ When the analysis of lifetime arrests by type of arrest is limited to the sample of youth aged 15-20 in 2001, we find quite similar results to those in Table V for the full 15-25 age group. The main differences are that the property crime effect for experimental group females is negative but insignificant and the effects for both treatment groups of males on arrests for "other" crimes are positive and significant for the sample restricted to youth aged 15-20.

treatments reduce violent-crime arrests for males through the first two post-program years in every site but New York. A detailed discussion of the relationship between these earlier results for Baltimore and those reported in this paper is given in Appendix B. Although consistent survey data at different points in time after random assignment are not available for the entire sample, the same sample of youth from the Boston site were administered questions about behavior problems in 1997 and 2002. These data suggest that while the MTO experimental treatment reduces problem behavior among males in 1997, five years later the experimental-control difference in behavior problems is positive and no longer statistically significant. In sum, the lack of pronounced, persistent reductions in behavior problems or violent-crime arrests for males in our study appears to be due to changes over time in treatment effects for boys, rather than differences across sites in treatment impacts.

Gender differences in mobility patterns are unlikely to be an explanation for gender differences in treatment effects. One way to see this is to compare the experiences of brothers and sisters within the same household, since sibling pairs typically experienced the same moves. Table VII reports experimental and Section 8 ITT effects for MTO youth 15-25, where the sample is limited to one sibling of the opposite gender per family (selecting the eldest of each gender among multiple siblings). Boys assigned to the experimental group appear to experience different treatment effects on arrests for property offenses compared to the average program effect on their sisters. By construction, the average neighborhood and family characteristics are the same for boys and girls in this sample. Since this panel is balanced by construction, a family fixed effect model with a gender-interacted treatment indicator recovers the same difference in female versus male treatment effects shown in Table VII.

One possible simple story about these gender differences is that youth with a prior history of problems experienced more adverse treatment effects, and there were more boys at baseline with a prior history of problems. Table VIII shows separate treatment effects for males and females who prior to MTO have or have not exhibited problem behavior, defined as whether the teens had been arrested, expelled, provided with services for a behavior problem, or had their parents called to school for some type of problem.²² Around 45 percent of males in our core youth sample and 25 percent of females have some problem behavior during the pre-program period under this definition. For gender differences in pre-program problem behavior to explain gender differences in responses to MTO, teens with pre-program problems (who are disproportionately male) would need to react negatively to the experimental condition, while those with clean prior histories (disproportionately female) would need to react negatively to assignment to the experimental group. Yet the results in Table VIII do not support this hypothesis.

VI. EXPLORATION OF MEDIATING PROCESSES

In this section we explore possible reasons why moving through the MTO program appears to reduce anti-social, particularly violent behavior among females but increase problem behavior along a number of dimensions for males. One candidate explanation is that youth respond negatively to residential moves and associated social dislocation, and males in the MTO treatment groups are more likely to be in households that move than females. The top panel of Table IX shows that treatment take-up rates are similar by gender. The number of post-

²² Since only around 4 percent of youth have pre-program arrests, the use of this variable alone to define youth sub-groups yields uninformative estimates. Table VIII is calculated using our preferred administrative-data sample further restricted to those under 18 at enrollment, for whom baseline survey data are available on our other indicators of pre-program problem behavior.

randomization moves is significantly higher for females in both treatment groups compared to controls, while for males the effects are less than half as large in magnitude.²³ As we had hypothesized that moving itself would be associated with adverse outcomes, and our findings are that females tended to experience beneficial outcomes, we interpret the relatively greater mobility for females as being unsupportive of the idea that the experience of moving itself is responsible for the gender differences in outcomes that we observe. An alternative explanation is that males exhibit more pronounced short-term negative reactions to moving than do females. But the temporal pattern of MTO treatment effects discussed above does not seem consistent with a generic moving effect, since this explanation is at odds with short-run reduction in violent-crime arrests among experimental males and, at least in the Boston site, in problem behavior as well. Put differently, our results generally seem to be more consistent with behavioral responses to changes in specific neighborhood characteristics than to moving *per se*.

For epidemic, collective socialization, or institutional models to explain the difference in treatment effects by gender, MTO would need to move families with adolescent sons into “worse” neighborhoods than those of the control group, while families with daughters move through MTO into “better” neighborhoods. This does not seem to be the case, as seen in Tables IX and X. For most of the mediating factors that are central to these three models, males and females in the two treatment groups experience generally similar changes in neighborhood characteristics. Treatment families also feel safer than do the control families. Interestingly,

²³ These data are from administrative data on addresses collected from credit bureaus, National Change of Address, housing authorities, and in-person tracking for this study. We suspect these data have some upward bias in the number of moves in the treatment groups relative to the control group, since the treatment groups also include an extra source reporting initial program moves from program files (restricted by design to the treatment groups). Counting addresses only if they were also identified by a source other than program files -- so that the same address sources were used for both treatment and control groups and thereby subjecting program moves to the same imperfect tracking process as other types of moves -- decreased the number of treatment group moves by about .06 - .10. These common source results are more consistent with results from household-level self-reported moves; there were about 1.2-1.3 moves in all groups, and the effects were not significantly different between the groups (except 1.5 moves for Section 8 group females).

MTO winds up improving neighborhood safety for families despite the fact that since the program began, the baseline neighborhoods have become less crime-ridden compared to other places. From 1994 to 2001, the decline in FBI index offenses was proportionately about twice as large in the baseline areas as in the nation.²⁴ The MTO treatment does appear to produce somewhat larger improvements for females compared to males in terms of local property-crime rates²⁵, “collective efficacy,” defined as the willingness of neighbors to intervene against youth problem behavior [Sampson, Raudenbush and Earls 1997], neighborhood problems with social disorder and household victimization rates. But MTO does not lead to *deterioration* in any of these measures for males.

Of particular interest are the survey reports by parents about the quality of local policing, measured in Table X as the fraction of parents who report that police in the neighborhood do not come when called. The parents of both male and female youth in the treatment groups are much less likely than those in the control group to report that the neighborhood has a problem with police not coming when called. If parent reports about the quality of local policing are positively related to the probability that a crime results in arrest, then treatment effects on the probability of arrest will have two conflicting impacts on treatment-control group differences in arrest rates. On one hand, more and better policing may deter criminal behavior [Levitt, 1997, 2002], thereby leading to fewer arrests among the treatment than control groups. On the other hand, setting deterrence aside, the mechanical relationship between the probability of arrest (P), criminal

²⁴ In the United States the rate of FBI index offenses reported to the police declined by 23 percent from 1994 to 2001, with reductions of 29 and 22 percent for violent and property index crimes [FBI 1995, 2001]. For the MTO baseline neighborhoods of our main analytic sample, youth 15-20 at the end of 2001, the violent index rate declined by 42 percent over this period while the property index rate declined by 36 percent. The source of the crime drop during the 1990's appears to be driven by changes in imprisonment, police spending, the market for crack cocaine, and a reduction in unwanted births [Levitt 2003].

²⁵ For addresses in the Baltimore, Boston, Chicago, Los Angeles, and New York, these data are measured at the level of the local police beat, and other addresses are linked to FBI Uniform Crime Report data by the place or county code -- as described in detail in Appendix A.

behavior (C) and arrests (A), with $P \times C = A$, would lead the treatment groups to have higher arrest rates than controls even if there are no differences across groups in criminal behavior. This latter mechanical relationship leads our analysis of arrest data to overstate any treatment effects that serve to increase youth crime and understate treatment effects that reduce youth crime. The survey reports in Table X may suggest that such bias may be slightly larger for girls than boys.

Another important way in which policing could vary across neighborhoods is with respect to the probability that youth are subject to false arrests. It is plausible that the probability of false arrest could be greater for low-income youth living in relatively more affluent communities, and that this across-neighborhood differential could be particularly problematic for males given that they account for the overwhelming share of all criminal activity. Previous studies suggest that false arrests may be disproportionately associated with criminal charges that pit the word of the police against that of the arrestee, such as resisting arrest or disturbing the peace [Ogletree et al 1995]. If this is true, false arrests would be more likely to explain positive treatment-control differences for our “other” crime category (which includes resisting arrest and disorderly conduct) than for our property crime category, where most of the increase in offending by treatment-group males is concentrated. We can further explore the false arrest hypothesis by replicating our analyses using data on only those arrests that result in an adjudication of guilt or delinquency, which generally yields a pattern of coefficients that have similar signs to those calculated using all arrests. In results not shown in the table, we find that the differences in convictions across groups are almost never statistically significant, perhaps because only around one out of ten arrestees are adjudicated guilty or delinquent.²⁶

²⁶ The proportion of arrests resulting in such a ruling is even lower in the Chicago and Los Angeles MTO sites because of some idiosyncratic features of the juvenile justice data systems in those states and how they capture and report adjudication information for juvenile arrests. The difference in arrest/conviction ratios between Chicago and LA with the other sites is somewhat less pronounced for violent crimes than for other crime categories. In any case,

The relative deprivation model suggests that while some youth thrive in an environment with more affluent and higher-achieving peers, others may suffer from feelings of reduced status or efficacy and disengage from pro-social institutions. The results presented in Table X are consistent with the notion that on balance, treatment-group females thrive in their new environments in the sense that they experience heightened expectations for college completion, and generally are more engaged with school. Kling and Liebman [2004] also find that females in the treatment groups experienced improved mental health. In contrast, the sign of effects for males is to withdraw effort from school, although most of these results are not significant.

Table XI shows that MTO moves also lead to very different types of changes in peer or clique associations for boys versus girls. Girls in the experimental group are more likely than those in the control group to have friends who are involved in school activities. The MTO treatments for boys do not generate detectable changes in this measure of pro-social peers, and, in fact, both treatments for boys are associated with an increase in having friends who use drugs.

Models that emphasize the role of peer-group sorting give reason to suspect that the effects of MTO may vary according to the teen's initial peer-group affiliations. While it is possible that the gender difference in MTO treatment effects could be explained by differences in average initial social conditions, the results previously discussed from Table VIII suggested that the crime findings reported in the previous section do not lend themselves to a simple explanation along these lines. Table VIII suggested that in proportional terms, the experimental treatment's effects of increasing property-crime arrests for males and reducing violent-crime arrests for females are both largest for youth without prior histories of anti-social behavior.

when we drop data from Chicago and Los Angeles MTO youth and replicate both our "all arrest" and "conviction" analyses, the pattern of coefficients are generally similar to one another, although the evidence of a negative treatment effect on violent-crime arrests is attenuated.

Finally, the bottom panel of Table XI shows that MTO appears to have different effects by gender on youths' home environments. Unlike boys, females in the experimental ($p < .10$) and Section 8 groups seem to enjoy an increase in time with their fathers compared to females in the control group. Other MTO research finds that, on balance, both experimental and Section 8 assignment appears to improve the mental health of the parents of female youth, but treatment assignment has negative effects on the mental health of the parents of boys, and that neither of the MTO treatments have much of an impact on the most tangible of household resources – income [Orr et al 2003].

Our findings leave open the question of what exactly drives the impact of neighborhood effects on youth crime. Detailed data on mediators help narrow the set of candidate explanations, although many of our mediating factors could easily be consequences rather than causes of neighborhood effects on youth crime. What is clear is that males and females in MTO appear to be reacting to similar neighborhoods in quite different ways. We offer some speculative thoughts about what types of stories might be consistent with our pattern of results:

1. The social assets of males may depreciate more with MTO moves than those of females. The result may be that males wind up moving to more rather than less delinquent peer groups, while females experience the opposite effect on their peer associations. Delinquent peer groups could also be less violent in affluent neighborhoods compared to high-poverty areas.

2. The parents of daughters may relax after making MTO moves, knowing that their new neighborhood environments are safer than their old communities. Parents of males may instead worry about how their sons will fit in or whether they will be discriminated against or wind up in trouble in their new as well as old neighborhoods, and sons may either be influenced by or react negatively against this parental anxiety.

3. Work on the “code of the streets” by Anderson [1998, 1999b] suggests that females and particularly males may be subject to fewer physical challenges in low-poverty areas compared to the baseline communities, and so are required to stand up for themselves and fight for respect less often. It could be that males but not females in turn take advantage of their “softer” peers in the new neighborhoods to eventually become predators rather than prey.

4. While school quality seems to change little as a result of MTO moves, it could be that females react to their more affluent schoolmates by trying harder in school, while males react with resentment, stealing from their classmates and disengaging from academics.

VII. MTO EFFECTS ON THE SOCIAL COSTS OF CRIME

From a policy perspective, should MTO be considered a “success” or a “failure” with respect to the program’s ability to reduce crime by youth in participating families? Focusing on the net change in the overall arrest rate across groups leads to a somewhat negative answer to this question, because the findings for boys and girls are generally offsetting: while girls in both treatment groups experience pronounced declines in violent-crime arrests compared to those in the control group, boys in the experimental group experience sizable increases in problem behaviors and property-crime arrests compared to the control group. But distributional considerations aside, society is not indifferent towards the replacement of very damaging violent crimes with less costly property offenses.²⁷

In this section we consider the net effect the overall social costs of crime committed by youth in MTO. This exercise is complicated by the difficulties of assigning dollar costs to

²⁷ A more complete social welfare analysis of costs of crime would also incorporate estimates of any externalities of MTO youth on other youth in their neighborhoods. Identification of such externalities would be best addressed by a different research design than MTO, since the very small numbers of experimental group program movers in any single neighborhood and the selection processes involved in neighborhood location and peer association would make identification of these externalities based on the MTO experience extremely difficult.

criminal activities, which are not typically bought and sold in markets at measurable prices. To measure the cost of crime associated with each youth, we use a cost index to attach a dollar value to the primary offense for which a youth was arrested, and then we sum these values over the youth's lifetime arrests through the end of 2001. We use what we believe are the best published estimates for the costs of crime from Miller, Cohen and Wiersema [1996], hereafter MCW.²⁸ Table XII presents estimated intent-to-treat effects of MTO on our four main cost measures: (1) the default cost index that uses estimates from MCW (and imputes missing values using New York state law); (2) a modified index that trims the cost of murder to equal twice that of rape; (3) a version of the index that sets the costs of drug offenses to zero; and (4) an index that both trims the cost of murder and sets the cost of drug crimes to zero.

For the experimental group, the point estimates for every cost measure and sample suggest very large reductions in the social costs of youth criminal behavior compared to the control group. For boys and girls pooled together, the experimental-control difference in crime

²⁸ Use of these estimates raises several issues for our analysis. The first issue is that MCW only provide cost estimates for selected violent and property crimes, not for the full menu of offenses committed by MTO program participants. For those offenses for which published cost estimates are not available, we impute costs using published figures for offenses that have the same standing in New York state criminal law. For example, resisting arrest and vandalism are both Class-A Misdemeanors in New York for which cost figures are not reported in MCW. These offenses are assigned the cost estimated by MCW for larceny (\$440), which is also a Class-A Misdemeanor.

This procedure for imputing missing crime costs may overstate the costs of criminal activity among MTO participants because New York State law treats drug cases quite severely. Whether drug offenses are as costly as our default imputation procedure implies is open to debate. Previous research suggests that the supply of labor to illegal drug markets may be quite elastic [Moore 1990; Levitt and Venkatesh 2000], which suggests that the effects of a given individual's involvement with drug distribution on drug prices or use may be modest. Similarly, many of the costs of drug use itself may be internal to the user. On the other hand, drug use and distribution could lead to other types of criminal activity, but these will be captured directly by our estimates for MTO's effects on other criminal offenses. To examine the sensitivity of our estimates to how we treat the social costs of drug crimes, we also replicate our analysis setting these to zero.

A third complication arises from the fact that the costs of crime will be driven in large part by the value assigned to murder. MCW estimate a cost per murder equal to \$4.6 million, around 34 times the value for the second-most-costly offense, rape. The high cost of murder may drive our standard errors as well as point estimates, because previous research suggests that the difference between a homicide and an aggravated assault (with social costs of around \$11,000) is often just a matter of bad luck [Zimring, 1968, Cook, 1991]. If true, there will be substantial variation in our social cost measure that is unrelated to any behavioral effects related to MTO treatment assignment or our other baseline covariates. As a check on how sensitive our results are to the treatment of murder, we replicate our analyses under different values for the costs of murder, with a lower bound set equal to twice the cost of rape.

costs range from 14 to 31 percent of the costs imposed by control-group youth. While these are sizable program impacts, the standard errors around our point estimates are also large and so the effects are not statistically significant. However, for females the estimated program impacts on the social costs are typically larger in proportional terms than those for the pooled sample, and are statistically significant under some sets of the assumptions in Table XII. In analysis using equation (5), not shown in the table, we find that that most of the experimental treatment's effect on the costs per quarter from youth crime is concentrated during the first few years following random assignment, particularly when we limit the degree to which murder drives the overall cost index.²⁹

It could be argued that our estimates are either too conservative, in the sense that we are focusing only on the cost of those crimes that happen to result in arrest, or alternatively overstate the costs of criminal behavior by including arrests to MTO youth for which they were not adjudicated guilty or delinquent – and so may not have even committed. To address the concern that our estimates may be too conservative, we replicate our analysis multiplying each arrest by the estimated crime-to-arrest ratio for that offense type in national data. This adjustment yields results that are qualitatively similar to those in Table XII. Not surprisingly, the conviction-only point estimates are generally smaller in absolute value than those shown in the table.³⁰

²⁹ This first-year difference in the costs of crime between the experimental and control groups is statistically significant at the 5 percent level with our larger sample of youth and lower-bound cost estimate (trimmed murder cost, with drug costs set to \$0). Otherwise the t-statistics on this difference tend to range from around 1.3 to 1.7. We also find that, in contrast to the experimental group, any difference in crime costs between Section 8 and control youth appears to be driven in large part by what happens in year 6 (the final observed year). This year six Section 8-control difference is statistically significant at the 5 percent level for the first three of our four cost measures, and nearly significant at the 10 percent level with our final, lower-bound cost index.

³⁰ These adjustments are imperfect. For major crimes we can compare the number of arrests reported to the FBI as part of the Uniform Crime Reporting system with annual prevalence estimates derived from the National Crime Victimization Survey. For some other crime categories we are forced to rely on approximations that are likely to be quite noisy. For example, for disorderly conduct and drunkenness, we assign these two offenses a ratio defined as the sum of annual arrests for both offenses divided by survey-based estimates for the incidence of “drunkenness,” defined in national surveys as having had five or more drinks at one time, which yields a crime-to-arrest ratio of 38. In the case of crimes for which no published prevalence estimate can be derived at all, we assign such offenses the

VIII. CONCLUSION

Common wisdom within much of social science holds that residence within a high-crime, disorganized and disadvantaged urban community increases the propensity of youth to engage in crime. Yet this belief rests almost entirely on empirical evidence that may confound the causal effects of neighborhood context with those of unmeasured characteristics that are related to how families sort themselves across neighborhoods.

Using exogenous variation in neighborhood characteristics generated by the MTO randomized mobility experiment, we find that the relationship between neighborhood context and youth crime appears more complicated than common wisdom would suggest. The offer to move to neighborhoods with lower rates of poverty and crime produces reductions in criminal behavior (especially as seen in violent-crime arrests) for female youths and increases in property crime arrests and behavior problems for male youths.

Large reductions in the number of lifetime violent crime and property crime arrests were found for females in the experimental group relative to the control group. Assignment to the experimental group also appears to have a more pronounced effect on the behavior of males, but in the direction of increasing rather than decreasing anti-social behavior, at least along some dimensions. Males in the experimental group have scores on our behavior problem index that are about 20 percent higher than those for males in the control group, and are also arrested for property offenses 30 percent more often than controls.

crime/arrest ratio of disorderly conduct and drunkenness. To examine the sensitivity of our findings to our adjustment procedure, we replicate the analysis using a ceiling on crime-to-arrest ratios of 15.5, which is the highest ratio that we can directly estimate for any crime category (larceny) for which we have both NCVS and UCR arrest data. Capping the crime/arrest ratios at 15.5 does not appear to materially affect our results.

The gender differences in results do not appear to be driven by gender differences in the effects of residential moves *per se*, in part because the MTO control group also experiences substantial mobility and because the gender differences do not exhibit themselves near the time of the move but instead manifest themselves later in the youth's developmental trajectory. Female and male youth in MTO move into similar types of neighborhoods, so the gender difference in MTO effects seems to reflect differences in responses to similar neighborhoods. This interpretation is consistent with similar gender differences in treatment effects of more adverse effects for males in within-sibling comparisons.

MTO does appear to reduce violent crime and problem behaviors among males in the short term, but these effects dissipate over time and for problem behaviors even reverse. The difference between our medium-term estimates presented here and the short-term effects reported earlier for Baltimore and Boston, which show improvements in violent and problem behavior for males [Katz, Kling and Liebman, 2001, Ludwig, Duncan and Hirschfield, 2001], apparently stems from changes over time in MTO's effects rather than program impacts that are idiosyncratic to those two sites.

The main threats to internal validity with our estimates come from the possibility of self-reporting bias with our survey data and from possible variation across areas in the probability of arrest that may confound interpretation of results from official arrest data. Comparing the lifetime prevalence of arrest in the survey and administrative data does provide some support for the view that youth under-report anti-social behavior. Whether this misreporting varies systematically across treatment groups is difficult to determine. However for misreporting to explain our survey findings for problem behaviors and mediating variables, the treatment-control differences in misreporting tendencies would need to be exactly opposite for females and males.

The MTO survey data on the responsiveness of local police and previous qualitative research suggests that the probability of arrest conditional on committing a crime may be higher for the treatment than control groups. If this is true, our estimates from official arrest data will overstate the effect of the MTO mobility treatment to increase youth offending, and understate any reductions in offending among treatment relative to control youth.

Regarding external validity, we note that MTO is a relatively small program serving families who have volunteered to participate. If the effects of mobility programs are heterogeneous across families, the estimates reported here may not reflect the impacts of moving other low-income populations. Our findings are most relevant for understanding the impact of other moderately-sized, voluntary mobility programs. The average effects may be quite different for involuntary mobility programs such as HUD's Hope VI, which demolishes public housing projects and relocates residents. Even large-scale voluntary programs may produce effects that differ from those of MTO if they serve to re-concentrate poverty, which may lead to "endogenous" or "general equilibrium" effects [Manski, 1993, Heckman, 2001].

What do these results tell us about the nature of neighborhood effects on youth crime? For both males and females who entered MTO at ages from 8 to 21 years, moves through the MTO program change neighborhoods in ways that the epidemic, collective socialization and institutional models predict should reduce youth crime. But the estimated MTO treatment effects of increased problem behavior and property crime among male youths over the medium term suggest that the standard versions of these models do not provide a complete explanation. Put differently, we cannot reject the possibility that these moves affect youth behavior in the ways that the epidemic, collective socialization and institutional models predict -- but the effects of these processes do not dominate for males, at least over the medium run for some measures of

criminal or problem behavior. We also note that it is still too early to learn about the long-run effects on criminal behavior of the MTO treatments on the younger MTO children (those under age 8 at random assignment).

What do our results imply for public policy? We caution that our cost estimates are limited somewhat by the difficulty of reliably measuring the costs of crime, and the standard errors around our very large point estimates are also often very large. With these caveats in mind, because violent crime imposes substantially higher costs on society than do property offenses, we find that on net that increases in property crimes are more than offset by reductions in violent crime in our estimates of the aggregate social costs of crime committed by MTO youth.

Princeton University and NBER
Georgetown University
Harvard University and NBER

REFERENCES

- Akerlof, George A. and Rachel E. Kranton, "Economics and Identity," *Quarterly Journal of Economics*, 115 (2000), 715-754.
- Anderson, David, "The Aggregate Burden of Crime," *Journal of Law and Economics*, 42 (1999a), 611-642.
- Anderson, Elijah, "The Social Ecology of Youth Violence," in M. Tonry and H. Moore, eds., *Youth Violence: Crime and Justice, A Review of Research*, volume 24 (Chicago: University of Chicago Press, 1998), 65-104.
- Anderson, Elijah, *Code of the Street*, (W.W. Norton, 1999b).
- Angrist, Joshua A., Guido W. Imbens, and Donald B. Rubin, "Identification of Causal Effects Using Instrumental Variables," *Journal of the American Statistical Association*, 91 (1996), 444-472.
- Bloom, Howard, "Accounting for No-shows in Experimental Evaluation Designs," *Evaluation Review*, 8 (1984), 225-46.
- Bottcher, Jean, "Social practices of gender: How gender relates to delinquency in the everyday lives of high-risk youths," *Criminology*, 39 (2001), 893-931.
- Cook, Philip J., "The Technology of Personal Violence," in M. Tonry, ed., *Crime and Justice: An Annual Review of Research* (Chicago: University of Chicago Press, 1991), 1-71.
- Cook, Philip J. and Kristin A. Goss, "A Selective Review of the Social-Contagion Literature," Working paper, Sanford Institute of Policy Studies, Duke University, 1996.
- Dumanovsky, Tamara, Jeffrey Fagan, and Philip Thompson, "The Neighborhood Context of Crime in New York City's Public Housing Projects," Working Paper, Columbia University School of Public Health, 1999.
- Dunworth, Terence and Aaron Saiger, *Drugs and Crime in Public Housing: A Three-City Analysis* (Washington, DC: National Institute of Justice, 1994).
- Elliott, Delbert S., and Suzanne S. Ageton. "Reconciling Race and Class Differences in Self-Reported and Official Estimates of Delinquency." *American Sociological Review*, 45 (1980): 95-110.
- Ferguson, Ronald F., "A Diagnostic Analysis of Black-White GPA Disparities in Shaker Heights, Ohio," *Brookings Papers on Education Policy*, 2001, 347-414.
- Federal Bureau of Investigation, *Crime in the United States* (Washington, DC: FBI, 1995).
- Federal Bureau of Investigation, *Crime in the United States* (Washington, DC: FBI, 2001).
- Furstenberg, Frank. F. et al., editors, *Managing to Make It: Urban Families and Adolescent Success* (Chicago: University of Chicago Press, 1999).
- Glaeser, Edward L., Bruce Sacerdote, and Jose A. Scheinkman, "Crime and Social Interactions." *Quarterly Journal of Economics*, 111 (1996), 507-548.
- Glaeser, Edward L., Bruce Sacerdote, and Jose A. Scheinkman, "The Social Multiplier," *Journal of the European Economic Association*, 1 (2003), 345-353.
- Heckman, James J., "Accounting for Heterogeneity, Diversity and General Equilibrium in Evaluating Social Programs," *Economic Journal*, 111 (2001), 654-699.
- Jencks, Christopher and Susan E. Mayer, "The Social Consequences of Growing Up in a Poor Neighborhood," in L. Lynn and M. McGeary, eds., *Inner-City Poverty in the United States* (Washington, DC: National Academy of Sciences, 1990).

- Katz, Lawrence F., Jeffrey R. Kling, and Jeffrey B. Liebman, "Moving to Opportunity in Boston: Early Results of a Randomized Mobility Experiment," *Quarterly Journal of Economics*, 116 (2001), 607-654.
- Klein, Malcolm W., *The American Street Gang* (NY: Oxford University Press, 1995).
- Kling, Jeffrey R. and Jeffrey B. Liebman, "Causal Effects on Youth Wellbeing of Moving Out of High Poverty Neighborhoods in the Moving to Opportunity Experiment," Working Paper, Princeton University, 2004.
- Laub, John H., "Ecological Considerations in Victim Reporting to the Police," *Journal of Criminal Justice*, 9 (1981), 419-430.
- Leventhal, Tama and Jeanne Brooks-Gunn, "New York City Site Findings: The Early Impacts of Moving to Opportunity on Children and Youth," in J. Goering and J. Feins, eds., *Choosing a Better Life: Evaluating the Moving to Opportunity Social Experiment* (Washington, DC: Urban Institute Press, 2003), 213-244.
- Levitt, Steven D., "Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime," *American Economic Review*, 87 (1997), 270-290.
- Levitt, Steven D., "Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime: A Reply," *American Economic Review*, 92 (2002), 1244-1250.
- Levitt, Steven D., "Understanding Why Crime Fell in the 1990s: Four Factors that Explain the Decline and Six That Do Not," Working Paper, University of Chicago, 2003.
- Levitt, Steven D. and Sudhir Alladi Venkatesh, "An Economic Analysis of a Drug-Selling Gang's Finances," *Quarterly Journal of Economics*, 115 (2000), 755-790.
- Lochner, Lance and Enrico Moretti, "The Effect of Education on Crime: Evidence from Prison Inmates, Arrests, and Self-Reports," Cambridge, MA: NBER Working Paper No. 8605, 2001.
- Ludwig, Jens, "Information and Inner-City Educational Attainment," *Economics of Education Review*, 18 (1999), 17-30.
- Ludwig, Jens, Greg J. Duncan and Paul Hirschfield, "Urban Poverty and Juvenile Crime: Evidence from a Randomized Housing-Mobility Experiment," *Quarterly Journal of Economics*, 116 (2001), 655-680.
- Maguire, Kathleen, and Anne L. Pastore, *Bureau of Justice Statistics Sourcebook of Criminal Justice Statistics--1998* (Washington, DC: Government Printing Office, 1999).
- Maltz, Michael D., "Bridging Gaps in Police Crime Data, NCJ 176365," Washington, DC: Bureau of Justice Statistics, 1999.
- Manski, Charles F., "Identification of Endogenous Social Effects: The Reflection Problem," *Review of Economic Studies*, 60 (1993), 531-542.
- Miller, Ted R., Mark A. Cohen and Brian Wiersema, *Victim Costs and Consequences: A New Look* (Washington, DC: National Institute of Justice, NCJ 155282), 1996.
- Moore, Mark H., "Supply Reduction and Drug Law Enforcement," in M. Tonry and J.Q. Wilson, eds., *Crime and Justice: An Annual Review of Research*, vol. 13 (Chicago: University of Chicago Press, 1990), 109-158.
- Ogletree, Charles J., Mary Prosser, Abbe Smith, and William Talley, *Beyond the Rodney King Story* (Boston, MA: Northeastern University Press, 1995).
- Olsen, Edgar O., "Housing Programs for Low-Income Households," in R. Moffitt, ed., *Means-Tested Transfer Programs in the Untied States* (Chicago: University of Chicago Press and NBER, 2003).

- Orr, Larry et al., *Moving to Opportunity: Interim Impacts Evaluation* (Washington, DC: U.S. Department of Housing and Urban Development, Office of Policy Development and Research, 2003).
- Sah, Raaj K., "Social Osmosis and Patterns of Crime," *Journal of Political Economy*, 99 (1991), 1272-1295.
- Sampson, Robert J., Jeffrey D. Morenoff, and Thomas Gannon-Rowley, "Assessing 'Neighborhood Effects': Social Processes and New Directions in Research," *Annual Review of Sociology*, 28 (2002), 443-478.
- Sampson, Robert J., Stephen W. Raudenbush, and Felton Earls, "Neighborhoods and Violent Crime: A Multilevel Study of Collective Efficacy," *Science*, 277 (1997), 918-924.
- Shaw, Clifford and Henry McKay, *Juvenile Delinquency and Urban Areas* (Chicago: University of Chicago Press, 1942).
- Tracy, Paul E., Marvin E. Wolfgang and Robert M. Figlio, *Delinquency Careers in Two Birth Cohorts* (New York: Plenum Press, 1990).
- Wilson, William Julius, *The Truly Disadvantaged* (Chicago: University of Chicago Press, 1987).
- Zimring, Franklin E., "Is Gun Control Likely to Reduce Violent Killings?," *University of Chicago Law Review*, 35 (1968), 21-37.

APPENDIX A. DATA SOURCES

This appendix provides details about our survey data, local area crime rate data, and administrative data on arrests -- giving an overview and details for each site.

Survey data

Baseline survey data was collected for each household randomly assigned during 1994-97. Data from these baseline surveys is the basis for the covariates listed in Appendix Table A1. Address histories for each sample member were also tracked through program operations, credit bureaus, National Change of Address, housing authorities, and in-person tracking for this study.

In collaboration with HUD and Abt Associates, during 2002 our research team collected survey data from one adult and two randomly selected children in each MTO household. Of particular interest for the present paper are survey items covering child behavior, although the surveys covered a wide range of topics including housing, neighborhoods, health, child behavior, education, social interactions, employment and public assistance receipt [Orr et al 2003].

In this paper we focus on surveys with about 1,800 youth who were ages 15-20 at the end of 2001. Data come from interviews with an adult in their household (usually the mother), and from the youth themselves. The data were collected in two phases. In our main phase, we attempted to collect data from 10,931 children ages 5-20 and from adults from the 4248 households randomly assigned to MTO as of December 31, 1997. This data collection effort extended from December 2001 - July 2002, and we completed data collection with 78 percent of the sample. We refer to this as our Initial Response Rate (IRR). Among all individuals without completed surveys at that time, we drew a 3-in-10 subsample. The purpose of the subsampling was to concentrate our remaining resources on finding hard-to-locate families in a way that would minimize the potential for non-response bias in our analyses. Between July 2002 and September 2002, we completed surveys with 49 percent of the subsample. We refer to this as our Subsample Response Rate (SRR). Since the subsample members are representative of all nonrespondents from the initial phase, we combine them to report an overall Effective Response Rate; $ERR = IRR + (1 - IRR) * SRR$. For the study overall, ERR is therefore about 89%.

The ERR for our youth sample specifically is around 88%, and is slightly higher for females (90%) than males (86%). The difference in youth response rates are quite similar across MTO groups, equal to 87% for the experimental and control groups and 90% for the Section 8 group. Interviewers were not informed about the random assignment group of the respondent, though in some cases they may have been able to infer it. Response rates did vary somewhat across sites, ranging from 83% in Boston to 95% in Chicago. Because the number of youth respondents at each site is relatively modest, in our analysis we focus on the pooled sample of youth across all five sites.

Local-area crime rate data

To measure how MTO impacts participants' exposure to crime, we obtained local-area crime and population data for the years 1994 through 2001. We focus on those FBI Part I Index offenses for which consistent data are available across areas: murder, rape, robbery, and aggravated assault, the violent index offenses, as well as the property offenses of burglary, motor vehicle theft, and larceny. All MTO addresses located within the five original demonstration cities were geo-coded and assigned the crime rate of the police "beat" in which that address was

located.³¹ The resolution provided by this beat-level data varies across cities: Baltimore has 9 police beats, while Boston has 11, Chicago 279, Los Angeles 18, and New York City 76.³² Addresses that could not be geo-coded are assigned the city's overall crime rate.

Addresses located outside of the five original MTO cities are assigned either place- or county-level crime data, depending on whether the municipality in which the address is located is patrolled by a local or a county law enforcement agency. These crime figures come from the FBI's Uniform Crime Reporting system, which is subject to a number of well-known problems such as non-reporting or incomplete reporting. Our results for MTO's impact on local-area crime rates do not appear to be sensitive to how we handle these reporting problems.³³

In the end, we have local-area criminal justice data for nearly 47,000 of the 48,751 MTO address spells for the years 1997-2001, with figures that run a bit lower for 1994-6 because of missing crime data for two of Boston's police districts in those years. Fully 77% of addresses are matched to beat-level data and 10% to city-level data in the 5 MTO cities; an additional 7% of addresses are matched to place-level data outside of these cities, and around 2% are matched to county-level data outside the MTO cities. In the analysis presented in this paper we use these data to calculate the average local-area crime rate that each MTO participant experienced during the post-randomization period through June, 2001.

Our original intent was to use these local-area criminal justice data to also measure variation across areas in the probability of arrest. But local police beats appear to report the number of arrests made within their jurisdiction, rather than the number of crimes that occur within the beat that result in arrest. Many crimes (particularly property crimes) appear to be committed in affluent areas by people who live, and are subsequently arrested, in high-poverty neighborhoods. As a result, comparing arrest-to-crime ratios across beats will over-state the probability of arrest in high-crime, high-poverty areas and under-state the chance of arrest in affluent areas.

Administrative arrest history data overview

One of our main approaches for measuring youth criminal involvement matches data on MTO youth with official arrest histories. These arrest histories include information on the date of all arrests, each criminal charge for which the individual was arrested, and typically information on dispositions as well. Unfortunately these arrest histories do not provide information about the location of the crime, or confederates who may also have been arrested for the same offense.

Adult arrest histories maintained by state criminal justice agencies are intended to capture every arrest that someone has experienced within that state since at least the age of majority. Our main analytic sample for the arrest data consists of either participants of the same age as our survey sample (15-20 at the end of 2001), or an expanded sample of youth 15-25 at the end of 2001 who have spent at least part of their highest-risk years during the post-program period. Given the age of majority in the MTO states (18 in Maryland, 17 in Massachusetts, 18 in

³¹ Some cities call the operational divisions of their cities "districts" or "areas" instead of "beats," although for convenience in what we follows we refer to all of these geographic areas as beats.

³² For Baltimore we are missing beat-level offense data for 1994 and 1995, so we estimate these beat-level offense counts assuming that the annual percentage change observed between 1996 and 1997 is similar to what Baltimore experienced in 1994-6. We use a similar procedure to estimate beat-level 2002 data for Chicago and New York.

³³ Our default procedure is to impute missing data using the FBI's standard procedure, which is subject to a number of problems [Maltz 1999]. We replicate the analysis using only crime data for jurisdictions that report complete data and obtain similar results.

California, 17 in Illinois, and 16 in New York), most people in both of our analytic samples will be legally classified as adults for at least a few years during our observation period. Adult histories were provided by state criminal justice agencies in each of the five demonstration sites.

Juvenile arrest histories provide similar information to what is available from the adult data, but capture arrests to those under the age of majority.³⁴ For Massachusetts, Illinois and California, the same state criminal justice agencies that provided us with adult arrest histories also reported juvenile histories. For Baltimore, juvenile arrest histories were obtained from the Maryland Department of Juvenile Justice. For New York, juvenile arrest histories were provided by the New York City Department of Probation, which should capture all juvenile arrests that occur within the city but will miss arrests that occur in other parts of the state. 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.

We attempted to access arrest histories for MTO participants who have moved out of the five original MTO states, and were more successful in accessing adult than juvenile data for other states. In this paper we exclude from our sample those youths who had spent any of the post-randomization period in a jurisdiction from which we were unable to obtain either juvenile or adult criminal histories, a group that comprises 5.5 percent of the total sample of MTO youth who are 15-20 on 12/31/01. The proportion of youth excluded is very similar across groups, equal to 5.9 percent for the experimental group, 4.7 percent for the Section 8 group, and 5.7 percent for the control group. These across-group differences are also not statistically significant when we examine boys and girls separately, or focus on youth who are living in a jurisdiction for which arrest data were not available at the end of our observation period, rather than at any time during the post-randomization period.

We focus on criminal offenses committed through December, 2001, which is the earliest end date among our administrative arrest histories. These arrest data provide us with an average of 5.1 years of post-program data for our sample of MTO youth (min=3.5 years, max=6.7 years).

The arrest data from every site except New York State record information at the level of the criminal charge rather than the arrest, and record every criminal charge associated with each arrest.³⁵ The statewide New York adult arrest histories in contrast report only the most serious criminal charge per arrest, where severity is defined by New York state law (with class A felonies at the top of the list and "violations" at the bottom). In the analysis reported below, we initially use New York State law as a guide to select the most serious charge per arrest with all of our official arrest data.³⁶ One concern with this procedure is that New York has been known for

³⁴ In some cases young people arrested for very serious offenses may be waived from the juvenile to the adult criminal justice systems. Such arrests would then show up in the individual's adult history.

³⁵ For example, someone who runs across a crowded city street to start a fight and is then caught by the police with a marijuana cigarette in his pocket would have three criminal charges recorded for this arrest: jaywalking, possession of marijuana, and assault (or aggravated assault if either a weapon was used or the victim was injured).

³⁶ One complication with using the New York hierarchy is that the criminal histories available from the other MTO states do not always provide us with enough detail to determine where within the New York categories the criminal charge would fall. For example in the Massachusetts criminal histories, some criminal charges are simply reported as "assaults," without detail regarding whether a weapon or injury was involved, or whether the target of the assault was a police officer – details that may substantially affect the severity of the offense. We address this problem by conducting our analysis in two ways that should bracket the actual distribution of offenses. In our first approach we assume that in cases where relevant details of the crime are limited the crimes are of the more-severe variety, while in the second approach we assume they are of the less-severe variety. Both approaches yield very similar results. A related complication is that New York State's system for classifying the severity of drug offenses is based on a

having unusually severe drug laws, and so selecting the most serious charge per arrest on the basis of New York law may lead us to choose drug offenses over crimes that some might deem more serious. To address this concern, we replicate our analysis selecting FBI index offenses above drug offenses in all cases, and then rank all other offenses using data from a survey that asks respondents to assess the seriousness of different crimes (Wolfgang et al., 1985). In practice these different systems for choosing the most serious charge per arrest appear to yield quite similar results, in part because the majority of arrests involve only one criminal charge, and even for multiple-charge arrests, many of the charges are for similar types of offenses.³⁷

The detailed information available with these arrest histories enable us to focus on program impacts on different types of criminal offenses. In addition to examining impacts on overall arrests, we explore program effects on four separate types of crime: violent offenses (about 30% of all arrests), property offenses (28%), drug offenses (22%), and other crimes (20%).³⁸ Nearly three-quarters of all violent-crime arrests in the MTO data are for assaults, which includes both aggravated assaults (involving either a weapon or injury to the victim) and simple assaults, while around one-quarter are for robbery (where the perpetrator uses or threatens force against the victim), 5 percent are for rape or other sexual assaults, and around 1 percent for homicide. About half of the arrests in our property-crime category are for larceny (thefts that do not involve contact between the perpetrator and the victim), while around 45 percent are for burglary, breaking and entering or trespassing,³⁹ and 6 percent are for motor vehicle theft. Drug arrests are split about 60/40 between possession and dealing, respectively, although in practice the distinction between the two charges is often simply a matter of the quantity of drugs in the arrestee's possession at the time. About two-thirds of the arrests in our "other" crime category are accounted for by disorderly conduct, vandalism or weapons violations such as carrying a gun in public illegally.⁴⁰

combination of both the type and quantity of drugs. Because most of the criminal histories that we have for other MTO sites do not provide information about quantity, we rank drug offenses based solely on the type of drug.

³⁷ For example in the Massachusetts adult arrest histories, 60% of all arrests involve only one criminal charge, 25% have two charges, 10% have three charges, 4% have four charges, and around 3% have five or more. (This pattern is qualitatively similar for the arrest history files from other sites). For those Massachusetts adult arrests that involve at least one drug charge, 41% have only one charge, 34% have two charges, 12% have three charges, and 7% have four charges. For many of the drug arrests where there is more than one criminal charge, often the other charges are related to drug offenses (so that, for example, one arrest might involve both cocaine and marijuana possession as well as cocaine distribution and possession of drug paraphernalia).

³⁸ Note that while national data from the FBI's Uniform Crime Reporting system indicate that the number of property-crime arrests is nearly three times that for violent crime (see for example Maguire and Pastore [1999], p. 338), the MTO sample is arrested about as often for violent as for property crimes. The most likely explanation for this discrepancy between the MTO and the national data is that the rate of violent crimes reported to the police is higher in high-poverty public housing communities than in surrounding neighborhoods, the reported rate of property offenses is lower in public housing [Dunworth and Saiger 1994; Dumanovsky, Fagan and Thompson 1999]. The rate at which property offending is reported to the police could be lower in public housing than other neighborhoods because the "loot" available to steal in public housing is not very valuable, or because of differences in victim reporting to the police across communities. Previous research from the National Crime Victimization Survey suggests that victims are less likely to report less-serious crimes to the police in cities versus suburbs [Laub 1981].

³⁹ We include trespassing in the property-crime category because the difference between breaking and entering and trespassing may often be simply a matter of whether the night watchman caught the suspect after hopping over the warehouse fence or after climbing through the warehouse window.

⁴⁰ We do not include motor vehicle violations in our measures of criminal activity in part because only some states include such violations in their arrest databases, and because MTO may have an uninteresting mechanical relationship with traffic offenses by increasing the likelihood that MTO participants drive.

Administrative arrest history data details by site

This section provides additional details about the administrative arrest data available for MTO participants in each of the five demonstration sites. We pay particular attention to three issues: the process through which we match information on MTO participants to official arrest databases; the availability of juvenile arrest records, which are typically more difficult to access than adult arrest records; and the degree to which we can identify how arrests are disposed of in court. We believe that our official arrest records should capture most juvenile arrests to MTO program participants. Information on the disposition of arrests, and in particular juvenile arrests, appears to be more complete for the Baltimore, Boston and New York sites than in the Chicago and Los Angeles sites.

Matching MTO participants to official arrest histories will inevitably produce both false positives and false negatives. Criminal justice agencies typically prefer to link individuals with prior arrest histories using biometric markers such as fingerprints, because the same characteristics that lead people to become involved with crime also make them unreliable reporters of individual identifying information such as name, date of birth and social security number. Since fingerprint and other biometric measures are not available to us, we have no choice but to match on these standard identifiers. While the matching procedures and resulting match rates appear to vary across the five states that host demonstration sites, we do not expect this to generate much within-site variation in match rates across MTO groups because families in each of the three groups spend the overwhelming majority of their time within these five states. In particular the proportion of post-randomization days spent in these five states is 97.5% for the experimental group, 98.5% for the Section 8 group, and 98.6% for the control group. As a result, we expect the distribution of criminal justice agencies that conduct the arrest-data matching for us to be quite similar across the three treatment groups.

Regarding dates of coverage, our analysis focuses on arrests through December 31, 2001. The Maryland adult arrest histories capture arrest event through July 2002. The Maryland juvenile arrest histories capture arrest events through January 2002. The Massachusetts adult criminal histories capture arrest events through December 2001, while the juvenile data capture arrests through March 2003. The Illinois State Police data capture arrest activity in Illinois through December 2002. The California Department of Justice data capture arrests through April 2002. The New York City Department of Probation data capture arrests through August 2002. Details about these sources of data follow for each MTO site.

Baltimore. For adult arrest histories, the Maryland Department of Public Safety and Correctional Services (DPSCS) matched MTO program participants against their agency's criminal history data base electronically using name, race, sex, date of birth and social security number. In cases where the MTO participant has a social security number, the DPSCS attempts to find an exact match on this identifier using an automated match procedure. If there is no SSN for the participant or the automated search does not find an exact match on SSN, the DPSCS looks for an exact match on the last name and the submitted portion of the first name. If a name match is found and race, sex and/or date of birth is found they are used as limiting criteria. Race and sex must match exactly except that "U" (unknown) matches anything. If a date of birth is found then month and day must match exactly, but year will match plus or minus two years.

For juvenile arrest histories, the Maryland Department of Juvenile Justice (MD DJJ) maintains criminal histories that capture all arrests to people under the age of 18 in Maryland that were referred to the state's juvenile justice system. A MD DJJ staff member manually searched

the DJJ arrest database by date of birth and name to identify offense histories for MTO juveniles who had ever lived in Maryland. Between our earlier work on the Baltimore site (Ludwig, Duncan and Hirschfield, 2001) and the present study, we have asked the DJJ to conduct three separate matches of MTO participants to their juvenile justice database. The similarity in match outcomes across the three attempts provides us with some reassurance about the quality of their matching process. The DJJ also provides quite detailed information about how each of these juvenile arrests is disposed.

Boston For both adult and juvenile arrest histories, the Massachusetts Criminal History Systems Board (CHSB) matched information on MTO program participants with their administrative data base using names (with Soundex coding) and dates of birth as the main matching variables. Because birth date errors are common in the CHSB system, programmers matched to birth dates that were plus or minus one year but exact match on month and day, or if year and month matched but the day was off, and if the year was a match and the month and day were transposed. Because Massachusetts cannot require arrestees to report social security numbers, these are available for only around 40 percent of the CHSB's adult arrest records; where they are available they are used as an additional matching variable. Parents' name and SSN are also in some cases used as additional verifying variables for cases where there is not an exact match on the other criteria.

The CHSB provided us with a data file that contained a total of all candidate matches. We eliminate some matches because of differences in gender between CHSB arrestees and matched MTO participants, cases where the dates of birth reported for the two matched individuals are more than 20 years apart, or cases where the ethnicity was different in a way that police would detect if the arrestee tried to misreport this characteristic at the time of arrest. The CHSB data appear to capture most juvenile arrests within the state of Massachusetts, and all relevant disposition information for both adult and juvenile arrests.

Chicago Data on both adult and juvenile arrest histories for the Chicago MTO site were provided by the Illinois State Police (ISP). The ISP conducted an automated match between their arrest-history data base and information on MTO program participants using name (last name converted to a Soundex code, plus first initial), date of birth (where the day and month of birth were required to match exactly, while the year was identified as a match if it was within five years of that reported on the MTO file), sex, and race. The ISP provided us with the criminal histories for all candidate matches that were identified through this process.

These ISP data capture all adult arrests within the state of Illinois, as well as a subset of juvenile arrests. Before 1990, arrestees over the age of 13 who committed a violent felony were automatically treated like adults, and as a result these offenses were automatically reported to the ISP. After 1990, anyone over the age of 10 who was arrested for a felony was reported to ISP, while anyone over the age of 10 who was arrested for a misdemeanor "could" be reported to ISP. The result of this change is that the number of juvenile arrests reported to the ISP has increased substantially over time. We believe that in practice, the ISP data capture most juvenile arrests within the state. This judgment comes in part from examining arrest rates and trends in the ISP data around the time state residents are legally classified as adults in the criminal justice system (age 17 in Illinois), and comparing these arrest trajectories to what we observe in the arrest histories from our other MTO sites.⁴¹

⁴¹ The total number of arrests for any crime per 100 Chicago MTO participants (with age at the time of arrest in parentheses) equal: 7.8 (12), 11.2 (13), 23.1 (14), 27.2 (15), 40.0 (16), 65 (17) and 62.9 (18). The total number of arrests per teen is thus increasing by an average of around 50% per year through age 16, the last year in which

Unfortunately the ISP data appear to provide incomplete information on how juvenile arrests are disposed. Unlike with arrest information, data on how arrests are disposed is provided to the ISP by each county's juvenile court system. The juvenile court system for Cook County, where the majority of Chicago MTO participants live, report to the ISP dispositions only for juvenile arrests for forcible felonies.⁴²

Los Angeles The California Department of Justice (CA DOJ) matched information on MTO participants with adult and juvenile arrest histories using a two-step process. In the first step, an automated search is conducted to identify candidate matches using last name, first initial, date of birth and gender. These matches will include any aliases that the subject has used that are stored in the CA DOJ data base. If more than one candidate match is identified per MTO participant, the DOJ searches for a match on social security number. During this second step, if the MTO participant's social security number is tied to multiple candidate records then the case is considered "too many to identify" and none of these candidate cases are counted as matched.⁴³

In principle the CA DOJ data capture only a sub-set of all juvenile arrests, although in practice we believe that most juvenile arrests are included in their data system. As with the ISP data described above, this assessment comes from examining changes in arrest trajectories around the age at which individuals are legally classified as adults rather than juveniles.⁴⁴

New York Adult criminal histories were provided by the New York State Division of Criminal Justice Services (DCJS), which matched histories for MTO participants using a two-step process. During the first (automated) step, the DCJS uses a probabilistic matching process to identify up to 15 candidate matches per MTO participant using social security numbers and similar-sounding names (identified using Soundex software). During the second step of the matching process, an intern (in our case a graduate student in criminology from SUNY-Albany) manually determined whether any of the candidates identified for a MTO program participant should be counted as a match using names (and aliases), social security numbers, dates of birth, sex, race and ethnicity. The criteria that interns are asked to follow generally err on the side of avoiding questionable matches.⁴⁵ Because arrestees in New York State may accumulate multiple CJS identifying numbers, the DCJS matched MTO participants with all NY CJS identifying

Illinois residents are classified as juveniles. If we assume that arrests would continue to increase by 50% from age 16 to 17, then the observed arrests for 16-year-old Chicago MTO participants would have led us to predict something on the order of 60 arrests for Chicago participants at the age of 17, which is quite close to the observed number (65 arrests) for 17-year-olds (adults) for whom we should have information on all arrests.

⁴² Personal communication, Jens Ludwig with Debby Plain, ISP, 10/17/03.

⁴³ Personal communication with Vicki Sands, CA DOJ, Vicki.Sands@doj.ca.gov, 11/12/02.

⁴⁴ If the DOJ's data were missing a substantial proportion of juvenile arrests, we would expect to observe a surge in arrests per capita between the last age at which people are treated as juveniles and the first year they are processed as adults by the criminal justice system. Yet the arrest rates per 100 are quite similar at these ages (39.6 at age 17 versus 38.3 at age 18). Of course if age 17 were the peak offending year and arrest rates actually declined dramatically thereafter, it could be that the arrest rates at ages 17 and 18 in California would appear quite similar even if the DOJ data were missing a substantial proportion of juvenile arrests. Yet the arrest rates in California at age 19 and 20 are even higher than those at ages 17 and 18, which would seem to rule out the notion that 17 is the peak year followed by dramatic declines in offending thereafter.

⁴⁵ The manual search kept matches if one of the following conditions was met: (1) exact match on name and date of birth; (2) exact match on name and SSN; (3) exact match on DOB and SSN; (4) exact match on name or DOB or SSN, and "very close" match on both of the other two identifiers. "Exact match" for these purposes includes phonetic matches and nick-name matches. In cases where there is some ambiguity about whether any of the conditions (1) through (4) are met, sex and race/ethnicity are used as additional matching factors. (Personal communication with Steve Greenstein, November 12, 2002).

numbers that were judged to be sufficiently similar under the criteria described here.⁴⁶ This match process appears to be fairly reliable in the sense that two separate match requests yield very similar criminal histories for those MTO participants included in both requests. These statewide records will capture all arrests through December 2001 to people age 16 or older, or those younger than 16 who are processed as adults.

Information about arrests to juveniles (under 16) also come from the New York City Department of Probation's data system, which captures all juvenile arrests that are processed by the New York City Family Court. Some juvenile arrests for serious offenses are initially sent to Supreme Court rather than Family Court, at which point the judge makes a determination about whether to send the case to the adult criminal justice system or instead send the case to be processed by Family Court.

As best we can judge, the combination of DCJS and NYC Probation data appear to capture most arrests to those under age 16, in the sense that the proportional year-to-year change in arrest rates around age 16 do not appear to be out of line with changes we observe at other ages.⁴⁷ A different potential concern with the NYC Probation data is that a substantial share of the arrests are for "statutory" offenses such as truancy, runaway and incorrigible or ungovernable behavior, which would not be classified as criminal behavior were they committed by an adult. Yet even when we focus on violent, property or drug crime arrest rates, the data for New York City participants does not look out of line with other sites.⁴⁸

⁴⁶ Individuals may accumulate multiple NY CJS numbers (NYSID's) because criminal event histories that are disposed in the arrestee's favor are sealed. When the individual is arrested again, these sealed histories may not be called up in the state's data system and so a new NYSID may be assigned to that person. When the DCJS matched information on MTO participants against their data file, they matched against both sealed and unsealed histories. Note that individuals should not have multiple NYSIDs associated with unsealed cases, because NYSIDs are created for these arrests on the basis of biometrics (fingerprints). (Personal communication with Steve Greenstein, November 12, 2002).

⁴⁷ From ages 12 to 13 the arrest rate doubles (3.2 to 6.8 per 100), doubles again from 13 to 14 (6.8 to 11.4), nearly triples from 14 to 15 (11.4 to 32), and then increases by around two-thirds from 15 to 16, the first year at which New Yorkers are treated as adults for all arrests (32 to 51.1). We focus on the control group because most of these families stay within New York City, and as a result we do not confound the coverage rate of the NYC Department of Probation for juvenile arrest cases with mobility outside of NYC (and thus outside of the NYC Department of Probation's jurisdiction).

⁴⁸ For example, the arrest rates per 100 teens at age 15 equal: for violent crime, 8.4 (NYC), 10.0 (LA), 10.4 (Chicago), 16.7 (Baltimore); for property crime, 6.6 (NYC), 18.2 (LA), 5.1 (Chicago), 23.4 (Baltimore); for drug cases, 10.3 (NYC), 3.6 (LA), 9.1 (Chicago), and 13.1 (Baltimore). Note that with the exception of drug arrests, Baltimore always has much higher arrest rates than those observed in other sites.

APPENDIX B. EXTENDING PREVIOUS RESEARCH

Previous research by Ludwig, Duncan and Hirschfield [2001], hereafter LDH, reported large experimental and Section 8 treatment effects on violent-crime arrests for MTO participants in the Baltimore site, using juvenile arrest data on participants 11-15 at random assignment through their first three to four years in the program. This appendix explores the long-term outcomes for this Baltimore sample, and generates similar estimates for the same ages using data from all five MTO demonstration sites.

There are several differences in econometric specification between the main results used in this paper and those reported in LDH. The LDH estimates come from a person-quarter panel that includes arrest data through March 1999, obtained from Maryland's juvenile justice system. Because information is available only for arrests before age 18 in these juvenile justice histories, person-quarters after age 18 are dropped from the panel. The analytic sample consists of the 336 MTO youth who were 11 to 15 years of age at the time of random assignment, as determined by the date of birth information available with the original baseline data.⁴⁹ Estimates for treatment effects by gender in LDH come from applying OLS to separate samples of boys and girls, rather than using a pooled sample with gender-treatment interactions as in the present paper. Another difference with the present paper is that the model used by LDH includes a somewhat more limited set of baseline conditioning variables.⁵⁰

The first panel of Appendix Table B1 on the left reports the findings for MTO boys and girls in Baltimore from LDH, Table IV. These results suggest very large reductions in violent-crime arrests for males and females in both treatment groups compared to controls. However, disaggregating the already-small Baltimore sample by gender leads to large standard errors, so that only the treatment effect for experimental males is statistically significant. The results also suggest a proportionally large (although not quite statistically significant) increase in property-crime arrests for experimental males compared to controls. The results for all five sites are given in columns 7-12 of the upper panel, using the same econometric specifications and age-based sample construction rules as in LDH.

The second panel of Appendix Table B1 replicates these results for both the Baltimore and full five-site sample through March, 1999, but now uses the newly available adult arrest histories to retain in the sample person-quarters where the participant is 18 years of age or older. This panel also uses the regression model specification as in the present paper, with the expanded set of baseline conditioning variables and gender effects identified by pooling boys and girls and including gender-treatment interaction terms.

Columns 1-6 of the second panel show that for Baltimore, adding adult arrest data and modifying the regression model has the effect of reducing the absolute value of the point estimates and standard errors for the estimated MTO effect on violent crime. However these

⁴⁹ Ongoing checks and revisions to the MTO data by Abt Associates have since led to some changes in the date of birth variable for some program participants. Two youth classified in LDH as 11-15 at random assignment are no longer so classified with the revised data in Baltimore, while 25 participants are now identified as being in this age range, for an updated sample of 359.

⁵⁰ LDH control for pre-program arrests as well as race, household head gender and age, number of children with the home at baseline, whether the household head has a high school degree or GED, whether the household receives AFDC at baseline, whether anyone in the home had been the victim of a crime during the six months prior to the baseline survey, and indicators for the primary reason the family volunteered for the program. This regression specification does not include among other things the baseline survey reports of the child's performance in school and problem behavior that are included in the models that generate our results in the present paper.

point estimates are still quite large measured as a proportion of the control group's mean. Columns 7-12 of the second panel of Table B1 shows that this pattern of MTO effects on violent-crime arrests for males and females is not limited to the Baltimore site. When we re-estimate this model using data from all five sites, we find that assignment to the experimental rather than control groups reduces violent-crime arrests by nearly one-half percent for females and nearly one-third for males ($p < .10$ in both cases). While the Baltimore data find no evidence of treatment effects on other types of crimes for females, using data from all five sites suggests that experimental-group assignment reduces the likelihood of arrest for property and other crimes as well for females. Interestingly, the Baltimore finding of an increase in property-crime arrests for experimental boys relative to controls also holds in the full five-site sample ($p < .10$).

The third panel of Appendix Table B1 uses the same regression setup as in the second panel, but now we extend our observation period through the end of 2001 as in the present paper. For comparability, the sample is still limited to those ages 11-15 at baseline, as opposed to our preferred sample in this paper of those ages 15-25 in 2001. For females in Baltimore (in the first three columns), extending the sample period leads to negative point estimates for every crime category for both treatment groups. Those point estimates that were already negative using only data through March, 1999 are now even larger in absolute value. In contrast, extending the sample period generally has the effect of attenuating the estimated treatment effects for males. For the full sample from all five sites, extending the sample period attenuates the estimated experimental effect on violent-crime arrests for both girls and boys. On the other hand, the higher rate of property-crime arrests for experimental boys relative to controls becomes if anything slightly larger with the extended sample period and remains statistically significant.

TABLE I
Baseline Characteristics of Survey and Administrative Data Samples, Ages 15-20

Baseline Characteristic	Survey Sample			Administrative Arrest Data Sample		
	Experimental	Section 8	Control	Experimental	Section 8	Control
<u>Household characteristics</u>						
Head educational attainment:						
GED	.16	.17	.17	.17	.20	.21
High school	.39	.40	.37	.38	.36	.34
Head was teen (<18) parent	.26	.25	.25	.27	.26	.28
Household on AFDC	.76	.74	.75	.74	.75	.75
Primary/secondary reason for enrolling in MTO:						
Crime, drugs	.79	.74	.78	.78	.74	.77
Better schools	.50	.58	.51	.50	.56	.52
<u>Youth characteristics</u>						
Male	.49	.48	.49	.50	.52	.50
Age in years on 12/31/01	17.7	17.7	17.6	17.9	17.8	17.8
African-American	.66	.65	.63	.63	.63	.65
Hispanic	.32	.32	.31	.33	.32	.32
Behavior problems	.12	.12	.07	.11	.12	.11
Expelled from school	.16	.15	.11	.13	.14	.12
In gifted programs	.16	.16	.22	.16	.18	.19
Learning problems	.21	.19	.21	.21	.18	.21
Ever arrested	.035	.042	.034	.044	.041	.042
N	749	510	548	1233	903	943

NOTES: Sample is ages 15-20 on 12/31/01. Survey randomly selected up to two children per household. Administrative sample uses all youth. Data are from MTO baseline survey, except for ever arrested which is from administrative records. Behavior/Learning problems = gone to a special class or school or gotten special help in school for behavior/learning problems in two years prior to baseline.

TABLE II
Neighborhood Characteristics 1 and 4 Years after Random Assignment, Ages 15-20

Census tract characteristics	1 Year After Random Assignment					4 Years After Random Assignment				
	Experimental		Section 8		Control	Experimental		Section 8		Control
	All	Move	All	Move	All	All	Move	All	Move	All
	1	2	3	4	5	6	7	8	9	10
Distribution of tract poverty rate:										
0-10 %%	.16	.33	.02	.04	.00	.11	.24	.04	.05	.01
10-20 %	.21	.49	.18	.28	.06	.24	.45	.22	.26	.09
20-30 %	.12	.11	.16	.25	.05	.14	.16	.18	.25	.14
30-40 %	.13	.01	.25	.26	.18	.19	.09	.24	.25	.20
40-50 %	.15	.05	.20	.13	.32	.14	.04	.19	.13	.27
50-60 %	.11	.01	.09	.02	.13	.09	.02	.08	.04	.14
60% or more	.12	.00	.11	.02	.24	.08	.00	.04	.02	.15
Avg tract poverty rate	.33	.15	.37	.28	.48	.31	.18	.33	.28	.42
Avg tract % minority	.83	.72	.89	.86	.90	.85	.77	.86	.85	.90
% tract single mother families w/children	.50	.32	.55	.48	.62	.50	.35	.51	.47	.57
% tract college degree	.20	.29	.15	.17	.12	.19	.25	.16	.17	.13
% tract in managerial/professional occupations	.26	.32	.22	.23	.21	.25	.29	.23	.23	.21
N	743	335	509	272	541	747	337	510	273	544

NOTES: Move = youth in households moving through MTO. Sample is ages 15-20 on 12/31/01. Census tract characteristics calculated using 2000 Census data.

TABLE III
Effects on Problem Behavior, Delinquency and Arrest Outcomes, Survey Data, Ages 15-20

		Experimental versus Control				Section 8 versus Control		
		Adjusted control mean	Intent-to-treat difference	Treatment-on-treated difference	Control complier mean	Intent-to-treat difference	Treatment-on-treated difference	Control complier mean
		1	2	3	4	5	6	7
11 item behavior problem index [SR]	All (N= 1795)	.339	.021 (.017)	.048 (.039)	.330	.010 (.019)	.017 (.034)	.348
	Females (N=926)	.340	-.019 (.023)	-.039 (.050)	.374	-.009 (.024)	-.015 (.042)	.364
	Males (N=869)	.338	.064* (.025)	.160* (.062)	.270	.031 (.028)	.060 (.053)	.323
9 item delinquency index [SR]	All (N= 1797)	.105	-.003 (.011)	-.007 (.024)	.111	.004 (.013)	.007 (.023)	.112
	Females (N=926)	.071	-.008 (.011)	-.016 (.023)	.076	-.005 (.012)	-.008 (.021)	.074
	Males (N=871)	.140	.002 (.018)	.005 (.044)	.151	.013 (.022)	.025 (.041)	.158
Ever arrested [SR]	All (N= 1790)	.211	-.002 (.025)	-.004 (.056)	.222	.006 (.029)	.011 (.051)	.236
	Females (N=926)	.127	-.015 (.028)	-.032 (.060)	.130	-.012 (.032)	-.020 (.054)	.149
	Males (N=864)	.301	.013 (.041)	.032 (.100)	.327	.026 (.049)	.050 (.091)	.341

Notes: SR = self-report. Adjusted control mean (ACM) from equation (3). Intent-to-treat (ITT) from equation (4). Treatment-on-treated (TOT) from equation (7) estimated by two stage least squares with treatment group assignment dummy variables as the instruments for the treatment take-up dummy variables. Control complier mean (CCM) from equation (8). Estimates use covariates in Table A1 and weights described in section III. Standard errors in parentheses, adjusted for household clustering. * = statistically significant at the 5 percent level. Results for the individual components of the behavior problem and delinquency indices are presented in Appendix Table A2.

TABLE IV
Effects on Arrest Outcomes, Administrative Data, Ages 15-20 and 15-25

		Experimental versus Control				Section 8 versus Control		
		Adjusted control mean	Intent-to-treat difference	Treatment-on-treated difference	Control complier mean	Intent-to-treat difference	Treatment-on-treated difference	Control complier mean
		1	2	3	4	5	6	7
Ever arrested, ages 15-20 [ADMIN]	All (N=3,079)	.318	.011 (.019)	.025 (.044)	.291	-.005 (.022)	-.010 (.038)	.298
	Females (N=1,541)	.250	-.029 (.025)	-.067 (.059)	.267	-.059* (.027)	-.100* (.047)	.281
	Males (N=1,538)	.386	.053 (.028)	.120 (.063)	.314	.047 (.032)	.083 (.056)	.381
# lifetime arrests, ages 15-20 [ADMIN]	All (N=3,079)	.963	.043 (.086)	.098 (.196)	.788	.060 (.100)	.102 (.173)	.921
	Females (N=1,541)	.561	-.186* (.078)	-.430* (.186)	.763	-.139 (.093)	-.239 (.162)	.704
	Males (N=1,538)	1.367	.279 (.150)	.637 (.339)	.811	.258 (.174)	.455 (.305)	1.107
# lifetime arrests, ages 15-25 [ADMIN]	All (N=4473)	1.382	-.035 (.085)	-.083 (.204)	1.361	.032 (.096)	.059 (.171)	1.404
	Females (N=2252)	.655	-.225* (.071)	-.545* (.176)	.973	-.012 (.089)	-.024 (.160)	.662
	Males (N=2221)	2.126	.160 (.150)	.391 (.364)	1.792	.076 (.170)	.138 (.306)	1.858

Notes: ADMIN = administrative records. Adjusted control mean (ACM) from equation (3). Intent-to-treat (ITT) from equation (4). Treatment-on-treated (TOT) from equation (7) estimated by two stage least squares with treatment group assignment dummy variables as the instruments for the treatment take-up dummy variables. Control complier mean (CCM) from equation (8). Estimates use covariates in Table A1 and weights described in section III. Standard errors in parentheses, adjusted for household clustering. * = statistically significant at the 5 percent level.

TABLE V
Effects on Lifetime Arrests for Detailed Crime Categories, Ages 15-25

		Experimental versus Control				Section 8 versus Control		
		Adjusted control mean	Intent-to-treat difference	Treatment-on-treated difference	Control complier mean	Intent-to-treat difference	Treatment-on-treated difference	Control complier mean
		1	2	3	4	5	6	7
# lifetime violent arrests [ADMIN]	All	.413	-.061* (.031)	-.147* (.074)	.524	-.027 (.038)	-.048 (.068)	.500
	Females	.250	-.077* (.031)	-.185* (.076)	.360	-.079* (.036)	-.143* (.065)	.284
	Males	.579	-.045 (.051)	-.107 (.123)	.700	.024 (.062)	.046 (.113)	.518
# lifetime property arrests [ADMIN]	All	.338	.045 (.031)	.108 (.075)	.241	.051 (.037)	.091 (.065)	.330
	Females	.185	-.057* (.026)	-.140* (.065)	.274	.031 (.039)	.053 (.070)	.222
	Males	.495	.150* (.055)	.363* (.136)	.215	.072 (.059)	.127 (.106)	.472
# lifetime drug arrests [ADMIN]	All	.355	-.007 (.040)	-.017 (.096)	.304	-.018 (.041)	-.033 (.073)	.321
	Females	.100	-.060 (.034)	-.143 (.082)	.179	.019 (.040)	.035 (.072)	.065
	Males	.616	.047 (.071)	.112 (.171)	.441	-.055 (.075)	-.100 (.135)	.605
# lifetime arrests, other crimes [ADMIN]	All	.277	-.012 (.026)	-.028 (.063)	.293	.027 (.030)	.048 (.054)	.254
	Females	.120	-.032 (.020)	-.077 (.049)	.160	.018 (.024)	.031 (.042)	.092
	Males	.436	.009 (.046)	.023 (.111)	.436	.036 (.054)	.065 (.098)	.263

Notes: ADMIN = administrative records. Adjusted control mean (ACM) from equation (3). Intent-to-treat (ITT) from equation (4). Treatment-on-treated (TOT) from equation (7). Control complier mean (CCM) from equation (8). Estimates use covariates in Table A1 and weights described in section III. Standard errors in parentheses, adjusted for household clustering. "Other crimes" category includes weapons offenses, threats, receipt of stolen property, vandalism, arson, disorderly conduct, resisting arrest, prostitution, and alcohol violations. * = statistically significant at the 5 percent level. Sample size is 4475: 2252 females and 2221 males.

TABLE VI
Effects on Arrests Per Year by Time Since Random Assignment, Ages 15-25

	Females			Males		
	ACM 1	Exp ITT 2	S8 ITT 3	ACM 4	Exp ITT 5	S8 ITT 6
<u>All arrests [ADMIN]</u>						
1-2 years since RA	.0723	-.0241 (.0125)	-.0086 (.0145)	.2450	-.0132 (.0255)	.0011 (.0271)
3-4 years since RA	.0925	-.0295 (.0165)	.0147 (.0202)	.3238	.0479 (.0346)	.0188 (.0357)
1-4 years since RA	.0827	-.0272* (.0120)	.0028 (.0146)	.2845	.0173 (.0252)	.0099 (.0259)
<u>Violent arrests [ADMIN]</u>						
1-2 years since RA	.0280	-.0066 (.0071)	-.0121 (.0076)	.0780	-.0263* (.0118)	-.0137 (.0122)
3-4 years since RA	.0337	-.0071 (.0082)	-.0048 (.0094)	.0807	-.0099 (.0123)	.0110 (.0146)
1-4 years since RA	.0308	-.0068 (.0060)	-.0084 (.0069)	.0794	-.0181 (.0093)	-.0013 (.0106)
<u>Non-violent arrests [ADMIN]</u>						
1-2 years since RA	.0443	-.0174 (.0094)	.0035 (.0115)	.1670	.0131 (.0209)	.0148 (.0224)
3-4 years since RA	.0589	-.0225 (.0133)	.0195 (.0166)	.2431	.0578* (.0295)	.0078 (.0297)
1-4 years since RA	.0519	-.0205* (.0095)	.0112 (.0116)	.2051	.0354 (.0212)	.0112 (.0213)

NOTES: ADMIN = administrative records. Adjusted control mean (ACM) from equation (3). Intent-to-treat (ITT) from equation (5). Exp = Experimental; S8 = Section 8. RA = Date of random assignment. Estimates use covariates in Table A1 and weights described in section III. Standard errors in parentheses, adjusted for household clustering. * = statistically significant at the 5 percent level. Sample is ages 15-25 on 12/31/01. Sample size is 2252 females and 2221 males.

TABLE VII
Effects on Lifetime Arrests for Siblings, Ages 15-25

	Females			Males		
	ACM 1	Exp ITT 2	S8 ITT 3	ACM 4	Exp ITT 5	S8 ITT 6
<u>Siblings 15-25 [ADMIN]</u>						
# arrests, all crimes	.515	-.008 (.112)	.084 (.155)	2.377	.338 (.273)	-.183 (.247)
# violent arrests	.202	-.029 (.042)	-.086* (.042)	.647	-.051 (.089)	-.050 (.092)
# property arrests	.149	.007 (.047)	.059 (.068)	.530	.219* (.095)	.055 (.083)
# drug arrests	.059	.008 (.049)	.073 (.071)	.706	.116 (.134)	-.199 (.112)
# other arrests	.105	.006 (.035)	.038 (.045)	.494	.055 (.080)	.010 (.088)

NOTES: ADMIN = administrative records. ACM = Adjusted control mean from equation (3). ITT = Intent-to-treat from equation (4). Exp = Experimental; S8 = Section 8. Estimates use covariates in Table A1 and weights described in section III. Standard errors in parentheses, adjusted for household clustering. * = statistically significant at the 5 percent level. Sample restricted to youth with at least one sibling of opposite gender in same age range. In cases where a MTO family contains more than one son or daughter, we select the oldest son or daughter, so that each family contributes a single son-daughter pair. Sample size is 1,570.

TABLE VIII
Effects on Lifetime Arrests for Ages 15-25, by Prior History of Anti-Social Behavior

	Females			Males		
	ACM 1	Exp ITT 2	S8 ITT 3	ACM 4	Exp ITT 5	S8 ITT 6
<u>All crimes</u> [ADMIN]						
Prior history	1.144	-.112 (.180)	.368 (.240)	3.386	.056 (.272)	.091 (.303)
No prior history	.477	-.216* (.080)	-.167 (.089)	1.058	.192 (.164)	.102 (.200)
<u>Violent</u> [ADMIN]						
Prior history	.453	-.070 (.090)	-.022 (.107)	.955	-.086 (.097)	.074 (.115)
No prior history	.183	-.071* (.035)	-.111* (.038)	.259	.004 (.055)	.016 (.073)
<u>Property</u> [ADMIN]						
Prior history	.364	-.088 (.074)	.083 (.106)	.748	.154 (.102)	.087 (.111)
No prior history	.124	-.041 (.027)	0 (.038)	.240	.152* (.060)	.096 (.068)
<u>Drugs</u> [ADMIN]						
Prior history	.080	.031 (.068)	.244* (.100)	.954	.028 (.128)	-.163 (.133)
No prior history	.097	-.069 (.041)	-.054 (.039)	.355	.013 (.079)	-.006 (.090)
<u>Other</u> [ADMIN]						
Prior history	.224	-.027 (.053)	.049 (.061)	.683	-.098 (.080)	.052 (.108)
No prior history	.065	-.044* (.020)	-.003 (.021)	.201	.012 (.047)	-.017 (.051)

NOTES: ADMIN = administrative records. Adjusted control mean (ACM) from equation (3). Intent-to-treat (ITT) from equation (4). Exp = Experimental; S8 = Section 8. Estimates use covariates in Table A1 and weights described in section III. Standard errors in parentheses, adjusted for household clustering. * = statistically significant at the 5 percent level. Prior history of anti-social behavior is defined as whether the teens had been arrested, expelled, provided with services for a behavior problem, or had their parents called to school for some type of problem. Sample restricted to youth 15-25 at end of 2001 who were under 18 at the time of enrolling in MTO, for whom therefore pre-program problem behavior information is available from the baseline surveys. Among males, 1008 have prior history while 1172 do not; for females 585 have a prior history and 1619 do not.

TABLE IX
MTO Effects on Mediating Processes for Collective Socialization, Ages 15-20

	Females				Males			
	ACM 1	Exp ITT 2	S8 ITT 3	N 4	ACM 5	Exp ITT 6	S8 ITT 7	N 8
<u>General mobility outcomes</u>								
MTO take-up rate [ADDRESS]	0	.461* (.033)	.595* (.038)	928	0	.415* (.032)	.542* (.039)	879
# moves since RA [ADDRESS]	1.005	.496* (.090)	.573* (.100)	927	1.162	.193 (.104)	.223* (.114)	878
Child lives in or visits baseline neighborhood [SR]	.704	-.143* (.044)	-.140* (.046)	893	.708	-.060 (.045)	-.086 (.052)	851
Adult encountered discrimination/bias in housing search [PR]	.067	.037 (.028)	.058 (.038)	782	.110	.048 (.035)	.102* (.045)	807
Adult describes housing as in good or excellent condition [PR]	.523	.094 (.048)	.020 (.053)	783	.495	.074 (.050)	.085 (.054)	803
Adult is somewhat or very satisfied w/neighborhood [PR]	.445	.197* (.050)	.208* (.053)	784	.494	.099 (.051)	.048 (.055)	807
<u>Census tract characteristics</u>								
% poor [ADDRESS]	.454	-.129* (.014)	-.106* (.013)	927	.451	-.112* (.015)	-.094* (.015)	878
% minority [ADDRESS]	.907	-.080* (.015)	-.021 (.014)	927	.898	-.058* (.018)	-.033 (.022)	878
% female household heads [ADDRESS]	.614	-.116* (.014)	-.078* (.013)	926	.603	-.096* (.015)	-.086* (.016)	878
% adults in professional/managerial occupations [ADDRESS]	.227	.057* (.008)	.024* (.009)	926	.239	.035* (.008)	.003 (.009)	878
<u>Collective socialization</u>								
Neighbors would do something about youth graffiti [PR]	.506	.179* (.049)	.110* (.055)	749	.583	.051 (.050)	-.024 (.057)	768
Neighbors would do something about youth skipping school [PR]	.370	.125* (.053)	.027 (.056)	727	.379	.079 (.050)	.045 (.057)	752
Neighborhood problems index [PR]	.549	-.174* (.037)	-.127* (.040)	784	.513	-.115* (.038)	-.080* (.039)	808

NOTES: ADMIN = administrative records; ADDRESS = address history from tracking file; PR = parental report about the household; PRY = parental report about the youth; SR = self-report. ACM = Adjusted control mean from equation (3). ITT = Intent-to-treat from equation (4) for all rows except for [PR], where estimation is from equation (9). Exp = Experimental; S8 = Section 8. Estimates use covariates in Table A1 and weights described in section III. Standard errors in parentheses, adjusted for household clustering. For estimates based on [PR], the number of observations in columns 4 and 8 are the total number of parental reports multiplied by the fraction of sample youth ages 15-20 in those households who are female and male, respectively. * = statistically significant at the 5 percent level. Census characteristics averaged over all post-randomization tracts through 2001, weighted by residential duration. Neighborhood problems include trash, graffiti, public drinking, abandoned buildings, people hanging out, and police not responding.

TABLE X
MTO Effects on Mediating Processes for Epidemic, Institutional and Relative Deprivation Models, Ages 15-20

	Females				Males			
	ACM 1	Exp ITT 2	S8 ITT 3	N 4	ACM 5	Exp ITT 6	S8 ITT 7	N 8
<u>Epidemic theory</u>								
Violent index crimes/10K [ADDRESS]	.265	-.42* (.11)	-.27* (.12)	924	.254	-.35* (.11)	-.29* (.11)	875
Property index crimes/10K [ADDRESS]	.615	-.83 (.57)	-.57 (.57)	925	.547	-.29 (.26)	-.25 (.23)	875
Feel safe in neighborhood, day [PR]	.717	.156* (.040)	.171* (.040)	780	.758	.084* (.041)	.071 (.044)	806
Feel safe in neighborhood, night [PR]	.576	.171* (.046)	.078 (.052)	774	.487	.184* (.050)	.148* (.052)	794
Household victimized in last 6 months [PR]	.266	-.092* (.042)	-.094* (.043)	791	.241	-.021 (.044)	-.048 (.045)	812
<u>Institutions</u>								
Attended youth activities at church [SR]	.388	-.011 (.047)	-.032 (.051)	909	.309	-.036 (.045)	.017 (.051)	866
School test percentile [SCHOOL]	.176	.023 (.017)	-.017 (.018)	458	.154	.028 (.020)	.010 (.020)	439
Pupils/teachers [SCHOOL]	17.78	.56 (.33)	-.59 (.42)	618	17.24	.72 (.42)	.11 (.46)	600
School % white [SCHOOL]	.087	.055* (.015)	.022 (.016)	757	.094	.032 (.020)	.055 (.031)	716
Problem in neighborhood w/police not coming when called [PR]	.373	-.165* (.047)	-.102 (.055)	740	.356	-.117* (.047)	-.066 (.050)	764
<u>Relative Deprivation Model</u>								
Believes chances high or very high will complete college [SR]	.550	.096* (.045)	.043 (.050)	917	.442	-.044 (.046)	-.049 (.049)	861
% school days absent [SR]	.076	-.020* (.009)	-.015 (.009)	848	.058	.021* (.009)	.011 (.010)	801
Works very hard on schoolwork, ages 15-18 [SR]	.502	.052 (.056)	.001 (.060)	612	.451	-.103 (.056)	.013 (.066)	599
Gifted or advanced class in last 2 years, ages 15-18 [PRY]	.078	.055 (.032)	.004 (.034)	563	.128	-.017 (.039)	-.025 (.041)	562
Ever took advanced math class [SR]	.833	.009 (.033)	.003 (.037)	925	.823	-.078* (.038)	-.021 (.038)	876
Participated in after-school sports activity [SR]	.035	.047* (.024)	.002 (.018)	883	.133	.003 (.032)	.031 (.038)	824
Ever repeated grade [PRY]	.213	.082* (.039)	-.012 (.041)	844	.345	-.037 (.045)	-.084 (.047)	823
Enrolled in school [SR]	.638	.065 (.038)	.054 (.039)	928	.619	.031 (.038)	.012 (.040)	878

NOTES: ADMIN = administrative records; ADDRESS = address history from tracking file; PR = parental report about the household; PRY = parental report about the youth; SCHOOL = parental report of schools attended linked to school characteristics; SR = self-report. ACM = Adjusted control mean from equation (3). ITT = Intent-to-treat from equation (4) for all rows except for [PR], where estimation is from equation (9). Exp = Experimental; S8 = Section 8. Estimates use covariates in Table A1 and weights described in section III. Standard errors in parentheses, adjusted for household clustering. For estimates based on [PR], the number of observations in columns 4 and 8 are the total number of parental reports multiplied by the fraction of sample youth ages 15-20 in those households who are female and male, respectively. * = statistically significant at the 5 percent level. School characteristics averaged over all post-randomization schools and local-area crime rates averaged over all post-randomization neighborhoods.

TABLE XI
Effects on Mediating Processes for Clique and Household Resource Models, Ages 15-20

	Females				Males			
	ACM 1	Exp ITT 2	S8 ITT 3	N 4	ACM 5	Exp ITT 6	S8 ITT 7	N 8
<u>Cliques</u>								
Saw people sell/use drugs [SR]	.437	-.114* (.048)	-.105* (.052)	878	.462	-.065 (.048)	-.028 (.056)	821
Gangs in neighborhood [SR]	.521	-.017 (.049)	.003 (.049)	870	.564	.003 (.046)	.035 (.054)	828
Friends use drugs [SR]	.298	.007 (.043)	.029 (.046)	893	.334	.118* (.048)	.134* (.053)	805
Friends involved in school activities [SR]	.609	.092* (.046)	.036 (.049)	897	.686	-.007 (.044)	-.011 (.049)	832
Friends carry weapons [SR]	.095	.007 (.026)	.027 (.031)	906	.161	.037 (.040)	-.052 (.039)	825
Child has 1+ close friend [PRY]	.894	.043 (.028)	-.006 (.035)	721	.918	.010 (.029)	.037 (.031)	676
Child has 5+ friends [SR]	.372	.060 (.044)	.048 (.047)	926	.513	.024 (.049)	.072 (.053)	867
<u>Household resources and parenting</u>								
Primary caregiver is very supportive [SR]	.669	.031 (.044)	.008 (.049)	924	.838	-.039 (.036)	-.035 (.038)	874
Parental monitoring index, ages 15-18 [SR]	.347	.042 (.041)	-.029 (.044)	616	.234	.019 (.040)	.077 (.044)	599
Saw father at least once per week during past year [SR]	.239	.075 (.040)	.094* (.044)	913	.343	-.024 (.044)	.010 (.049)	865
Adult stops to chat w/neighbor at least once a week [PR]	.521	-.038 (.052)	-.117* (.058)	786	.499	-.035 (.053)	.032 (.057)	811
Adult attends church at least once per month [PR]	.460	-.089 (.050)	-.044 (.055)	784	.463	-.033 (.050)	.007 (.055)	810
Adult has no friends in neighborhood [PR]	.675	-.008 (.047)	-.002 (.052)	788	.584	.002 (.050)	-.061 (.055)	810

NOTES: PR = parental report about the household; PRY = parental report about the youth; SR = self-report. ACM = Adjusted control mean from equation (3). ITT = Intent-to-treat from equation (4) for all rows except for [PR], where estimation is from equation (9). Exp = Experimental; S8 = Section 8. Estimates use covariates in Table A1 and weights described in section III. Standard errors in parentheses, adjusted for household clustering. For estimates based on [PR], the number of observations in columns 4 and 8 are the total number of parental reports multiplied by the fraction of sample youth ages 15-20 in those households who are female and male, respectively. * = statistically significant at the 5 percent level.

TABLE XII
Effects on Social Costs of Crime in Dollars

Sample and cost measure	All youth			Females			Males		
	ACM	Exp ITT	S8 ITT	ACM	Exp ITT	S8 ITT	ACM	Exp ITT	S8 ITT
<u>Youth 15-20</u>									
Cost index	22,476	-6,349 (8,786)	381 (11,179)	12,622	-11,427 (6,607)	-10,208 (7,294)	32,373	-1,140 (16,253)	10,747 (20,609)
Trim murder	10,376	-1,684 (1,436)	-1,672 (1,599)	4,988	-2,235* (1,118)	-2,571* (1,212)	15,805	-1,119 (2,505)	-789 (2,905)
Drug costs=\$0	19,535	-6,024 (8,553)	1,067 (11,037)	11,436	-10,562 (6,504)	-9,595 (7,225)	27,662	-1,368 (15,974)	11,493 (20,532)
Trim murder & Drug costs=\$0	7,435	-1,360 (1,203)	-985 (1,399)	3,802	-1,370 (812)	-1,958* (969)	11,094	-1,348 (2,174)	-43 (2,639)
<u>Youth 15-25</u>									
Cost index	60,911	-12,762 (16,309)	-16,799 (16,213)	18,442	-3,451 (17,413)	-15,399 (9,028)	104,023	-22,235 (28,394)	-18,165 (29,809)
Trim murder	15,626	-2,170 (1,517)	-1,789 (3,658)	6,148	-2,528 (1,363)	-1,941 (1,272)	25,278	-1,807 (2,646)	-1,640 (2,861)
Drug costs=\$0	56,209	-12,372 (16,208)	-16,331 (16,113)	17,096	-2,498 (17,405)	-15,647 (8,887)	95,901	-22,422 (28,223)	-16,995 (29,686)
Trim murder and Drug costs=\$0	10,924	-1,781 (1,288)	-1,321 (1,392)	4,802	-1,575 (1,200)	-2,188* (1,003)	17,156	-1,993 (2,255)	-470 (2,538)

NOTES: ACM = Adjusted control mean from equation (3). ITT = Intent-to-treat from equation (4). Exp = Experimental; S8 = Section 8. Estimates use covariates in Table A1 and weights described in section III. Standard errors in parentheses, adjusted for household clustering. * = statistically significant at the 5 percent level. Social costs (inflation adjusted to 2000) based on valuations of crime categories, as discussed in section VII.

APPENDIX TABLE A1
Baseline Covariates Used in Regression Adjustment

Male adult	Household member was victim of crime during past six months
Adult 19-29	Household member disabled
Adult 30-39	Adult moved 3+ times in 5 years
Adult 40-49	Adult has no friends in neighborhood
Adult Hispanic	Adult has no family in neighborhood
Adult African-American	Adult lived in neighborhood 5+ years
Adult other non-white	Adult previously applied to Section 8
Adult never married	Adult gave getting away from gangs/drugs as primary or secondary reason for moving
Adult working	
Adult was teen parent	Adult gave better schools as primary or secondary reason for moving
Adult in school	Adult very dissatisfied with neighborhood
Adult graduated from high school	Adult reports streets near home very unsafe at night
Adult obtained GED	Adult stops to chat with neighbor at least once a week
Core household size equals 2	Adult very likely to tell neighbor if saw neighbor's kid in trouble
Core household size equals 3	Adult very sure would find apartment in other area
Core household size equals 4	Male child
No teens in core household	Child requires special medicine/equipment
Receiving AFDC/TANF	Hard for child to get to school or play because of health problem
Adult has car that runs	Child got help for learning problem (2 years prior to baseline)
Baltimore	Child got help for behavior or emotional problem (2 years prior to baseline)
Boston	Child expelled from school (2 years prior to baseline)
Chicago	School asked to talk about problems child had in school (2 years prior to baseline)
Los Angeles	Child went to special class for gifted or did advanced work
	Set of child age indicators: Child Age = X years as of May 31, 2001

NOTES: All baseline covariates are binary indicators. Regressions with arrests as an outcome also include nine indicators for arrests prior to random assignment: violent crime arrests (1, 2, or 3+); property crime arrests (1, 2, or 3+); and other crime arrests (1, 2, or 3+). Regressions using the age 15-25 sample also include additional indicators (for youth at least age 18 at baseline) for whether the youth at baseline was working, was in school, had graduated from high school, had obtained a GED at baseline, or had never been married.

APPENDIX TABLE A2
Effects for Behavior Problem and Delinquency Index Components, Ages 15-20

	Females				Males			
	ACM 1	Exp ITT 2	S8 ITT 3	N 4	ACM 5	Exp ITT 6	S8 ITT 7	N 8
<u>Behavior Problems</u>								
Difficulty concentrating [SR]	.541	-.043 (.047)	-.010 (.050)	924	.505	.002 (.046)	-.041 (.052)	871
Cheats [SR]	.331	.003 (.042)	.051 (.048)	922	.397	.132* (.048)	.063 (.053)	868
Teases others [SR]	.285	.031 (.042)	.007 (.045)	925	.345	.093 (.048)	.069 (.056)	868
Disobeys parents [SR]	.318	.003 (.041)	.058 (.047)	921	.263	.129* (.044)	.092 (.051)	866
Trouble sit still [SR]	.357	-.028 (.041)	-.023 (.046)	925	.323	.018 (.044)	.064 (.049)	868
Has a hot temper [SR]	.504	-.018 (.047)	-.053 (.050)	925	.452	.084 (.048)	.037 (.052)	868
Rather be alone [SR]	.531	-.100* (.047)	-.098 (.051)	927	.366	-.023 (.044)	.012 (.050)	869
Hangs around w/kids in trouble [SR]	.220	-.036 (.037)	.005 (.040)	926	.280	.136* (.043)	.015 (.044)	869
Disobeys at school [SR]	.173	-.031 (.034)	-.025 (.037)	918	.224	.073 (.041)	.024 (.044)	849
Not get along w/other kids [SR]	.245	-.013 (.039)	.006 (.041)	924	.256	.013 (.040)	-.018 (.044)	865
Not get along w/teachers [SR]	.230	.033 (.038)	-.008 (.042)	921	.295	.046 (.042)	.012 (.046)	857
<u>Delinquency</u>								
Ever carried gun [SR]	.014	-.006 (.010)	.006 (.012)	926	.074	.003 (.027)	.007 (.027)	868
Ever in a gang [SR]	.024	-.013 (.010)	-.012 (.012)	925	.086	-.017 (.027)	-.007 (.028)	867
Ever damaged property [SR]	.080	.014 (.028)	.004 (.028)	926	.165	.007 (.036)	-.006 (.041)	873
Ever stole < \$50 [SR]	.122	.016 (.030)	.014 (.034)	926	.216	.004 (.038)	.023 (.044)	869
Ever stole > \$50 [SR]	.031	-.004 (.014)	.027 (.022)	926	.080	-.009 (.021)	-.009 (.026)	871
Ever committed other property offense [SR]	.018	-.006 (.010)	.012 (.015)	926	.069	-.024 (.021)	-.003 (.025)	870
Ever attacked someone [SR]	.199	-.046 (.034)	-.070 (.036)	925	.182	.038 (.039)	.070 (.047)	868
Ever sold drugs [SR]	.026	-.013 (.012)	.006 (.016)	924	.083	.008 (.028)	.025 (.031)	868
Ever arrested [SR]	.127	-.015 (.028)	-.012 (.032)	926	.301	.013 (.041)	.026 (.049)	864

NOTES: SR = self-report. ACM = Adjusted control mean from equation (3). ITT = Intent-to-treat from equation (4). Exp = Experimental; S8 = Section 8. Estimates use covariates in Table A1 and weights described in section III. Standard errors in parentheses, adjusted for household clustering. * = statistically significant at the 5 percent level.

APPENDIX TABLE B1
Effects on Probability of Arrest per Quarter for Baltimore and All 5 sites, Ages 11-15 at Baseline

Sample/Outcome	Baltimore Site						All 5 Sites					
	Females			Males			Females			Males		
	ACM	Exp ITT	S8 ITT	ACM	Exp ITT	S8 ITT	ACM	Exp ITT	S8 ITT	ACM	Exp ITT	S8 ITT
	1	2	3	4	5	6	7	8	9	10	11	12
Juvenile arrests <=3/99 [ADMIN]; model from Ludwig et al. [2001]												
Violent	.0184	-.0067 (.0073)	-.0043 (.0086)	.0431	-.0294* (.0141)	-.0192 (.0116)	.0088	-.0033 (.0024)	-.0033 (.0027)	.0229	-.0069 (.0043)	-.0036 (.0048)
Property	.0092	.0104 (.0063)	.0162 (.0114)	.0335	.0267 (.0174)	-.0053 (.0121)	.0059	-.0010 (.0017)	.0017 (.0026)	.0183	.0040 (.0038)	-.0044 (.0037)
Other	.0118	-.0054 (.0053)	.0043 (.0065)	.0590	-.0186 (.0255)	-.0307 (.0203)	.0065	-.0034 (.0019)	.0010 (.0022)	.0257	.0028 (.0050)	.0061 (.0057)
Juvenile and adult arrests <=3/99 [ADMIN]; model as in present paper												
Violent	.0162	-.0031 (.0060)	-.0017 (.0076)	.0440	-.0133 (.0106)	-.0112 (.0107)	.0094	-.0044 (.0024)	-.0035 (.0030)	.0227	-.0066 (.0038)	-.0019 (.0043)
Property	.0114	-.0009 (.0058)	.0019 (.0091)	.0350	.0260* (.0128)	-.0127 (.0113)	.0080	-.0039 (.0021)	-.0006 (.0029)	.0169	.0067 (.0035)	-.0012 (.0033)
Other	.0071	.0053 (.0085)	.0127 (.0094)	.0770	-.0004 (.0183)	-.0289 (.0160)	.0069	-.0035 (.0020)	.0031 (.0027)	.0288	.0025 (.0049)	.0033 (.0054)
Juvenile and adult arrest data <=12/01 [ADMIN]; model as in present paper												
Violent	.0163	-.0062 (.0050)	-.0067 (.0063)	.0345	-.0055 (.0081)	-.0078 (.0079)	.0097	-.0022 (.0019)	-.0037 (.0024)	.0235	-.0045 (.0031)	.0009 (.0036)
Property	.0115	-.0026 (.0044)	-.0017 (.0064)	.029	.0148 (.0087)	-.0129 (.0075)	.0077	-.0021 (.0016)	.0008 (.0024)	.0183	.0087* (.0029)	.0030 (.0031)
Other	.0179	-.0071 (.0116)	-.0059 (.0128)	.0794	-.0001 (.0167)	-.0269 (.0151)	.0093	-.0036 (.0026)	-.0015 (.0028)	.0398	.0032 (.0049)	.0011 (.0056)

NOTES: ADMIN = administrative records. ACM = Adjusted control mean from equation (3), except for first panel where it is the unadjusted control mean.. ITT = Intent-to-treat as described in Appendix B. Exp = Experimental; S8 = Section 8. Robust standard errors in parentheses, adjusted for the clustering of MTO participants within families and the panel structure of our person-quarter data. * = statistically significant at the 5 percent level. Results come from regressing the number of arrests per person per quarter against indicators for experimental or Section 8 assignment and a set of baseline controls. The first panel contains estimates from separate regressions for males and females, conditioning on family socio-demographics and each participant's pre-program arrest history, dropping person-quarters after age 18 -- as in Ludwig et al [2001]. The second and third panels keep person-quarters after 18, use an expanded set of conditioning variables that include baseline survey measures of pre-program school experiences, and estimate gender effects using a sample that pools males and females together and includes treatment-gender interactions and quarter indicators as in equation (5).