URBAN POVERTY AND JUVENILE CRIME: EVIDENCE FROM A RANDOMIZED HOUSING-MOBILITY EXPERIMENT*

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This paper uses data from a randomized housing-mobility experiment to study the effects of relocating families from high- to low-poverty neighborhoods on juvenile crime. Outcome measures come from juvenile arrest records taken from government administrative data. Our findings seem to suggest that providing families with the opportunity to move to lower-poverty neighborhoods reduces violent criminal behavior by teens.

I. INTRODUCTION

Crime has profound effects on the quality of life in the United States, imposing social costs on the order of $1 trillion per year [Anderson 1999]. Of particular concern is the possibility that the volume of crime in America may be related in part to the spatial concentration of low-income families in high-poverty, high-crime urban neighborhoods. Criminal activity may be "contagious" in high-crime areas because the social penalties for committing crime or the probability of arrest may be lower than in other neighborhoods [Sah 1991; Cook and Goss 1996; Sampson, Raudenbush, and Earls 1997; Schrag and Scotchmer 1997], as may be the costs of acquiring an important "input" for crime—confederates [Reiss 1988]. Neighborhood poverty may also affect the ac-

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tual or perceived returns to schooling and work by affecting access to quality schools, jobs, and role models [Wilson 1987; Ludwig 1999], which may depress the opportunity costs of crime. While the existence of neighborhood or peer effects receives some support from findings that the variation in crime rates across cities exceeds what is predicted by measurable city characteristics [Glaeser, Sacerdote, and Scheinkman 1996], more definitive evidence is currently not available.

Studies using individual-level data have produced mixed findings on whether census tract, ZIP code, or peer-group characteristics are correlated with teen problem behavior [Jencks and Mayer 1990; Brooks-Gunn, Duncan, and Aber 1997a; Brooks-Gunn, Duncan, and Aber 1997b; Ellen and Turner 1997; Matsueda and Anderson 1998]. Interpretation of these findings is complicated by the fact that families typically have some degree of choice over where they live and with whom they associate. As a result, correlations between individual behaviors and neighborhood or peer characteristics may reflect in part or whole the effect of unmeasured variables associated with residential or peer-group selection [Evans, Oates, and Schwab 1992; Manski 1993; Moffitt 1998]. Only one previous study focuses on crime and attempts to control directly for the selection problem (using instrumental-variables methods), and finds some evidence that peer behaviors influence self-reported juvenile crime [Case and Katz 1991].[1]

The present paper examines the effects of neighborhoods on juvenile criminal activity using data generated by a randomized housing-mobility experiment. Since 1994, the U. S. Department of Housing and Urban Development's (HUD) Moving to Opportunity (MTO) experiment has assigned a total of 638 families from high-poverty Baltimore neighborhoods into three different "treatment groups:" *Experimental group* families receive housing subsidies, counseling, and search assistance to move to private-market housing in low-poverty census tracts (poverty rates under 10 percent); *Section 8-only comparison group* families receive private-market housing subsidies with no program constraints on

1. Other studies that address the selection problem using fixed-effects or instrumental-variables methods focus on other outcomes such as teen pregnancy or high school dropout, with mixed results [Evans, Oates, and Schwab 1992; Aaronson 1998; Plotnick and Hoffman 1999]. Sacerdote [2001] exploits variation in peer groups generated by the random assignment of freshman roommates at Dartmouth and finds evidence of peer effects on academic effort, GPA, and fraternity membership.

relocation choices; and a *Control group* receives no special assistance under MTO. The randomized experimental design of MTO thus breaks the link between family residential preferences and adolescent outcomes, and helps us overcome the endogenous-membership problem found with previous studies.

Our outcome measures come from juvenile arrest records obtained from the Maryland Department of Juvenile Justice (DJJ), which are not subject to the self-reporting problems associated with survey studies of criminal offending[2] and are less susceptible to problems of sample attrition. The drawback is that arrest records reflect the combined behaviors of juveniles and local criminal justice systems. As we discuss below, this may lead us to slightly understate any reductions in crime caused by MTO, or overstate any increases in offending. Analysis of the MTO data suggests large reductions in arrests for violent crimes among experimental and Section 8-only teens relative to controls, although there may also be some increase in property-crime arrests for teens in the experimental group. We hasten to note that because participation in the MTO program is voluntary, our estimates of the effects of relocation may be different from the effects of relocating a randomly selected group of families from poor areas.

The next section describes the MTO experiment in greater detail. The third section discusses our conceptual framework, the fourth section discusses the data, and the fifth section presents the key results. The final section discusses the implications of our findings.

II. The Moving to Opportunity Demonstration

The MTO demonstration is based in five cities: Baltimore, Boston, Chicago, Los Angeles, and New York. The present paper uses data from the Baltimore site, where eligibility was restricted to low-income families with children who lived in public housing in one of the five poorest census tracts in the city. The average poverty rate in these tracts in 1990 was 67 percent [Goering,

2. See, for example, Donohue and Siegelman [1998] for a discussion. The most serious concern is that misreporting may vary across MTO treatment groups, since previous research in criminology finds that misreporting patterns are correlated with individual-level sociodemographic characteristics (and thus perhaps with neighborhood characteristics as well) [Hindelang, Hirschi, and Weis 1981].

Carnevale, and Teodoro 1996], with a crime rate more than three times that of the state as a whole (194 versus 61 per 1000 residents) [Maryland State Police 1997].[3]

The program was publicized in the baseline tracts by the Housing Authority of Baltimore (HAB) and a local nonprofit, the Community Assistance Network (CAN). Families who volunteered for the program were added to the MTO waiting list. Families were drawn off the MTO waiting list over time on the basis of a random lottery, and then randomized into one of the three MTO treatment groups. Both types of randomization were conducted by Abt Associates.

Families in the experimental and Section 8-only groups were offered Section 8 housing vouchers or certificates, which provide subsidies to lease private-market housing. As part of the program's design, the Section 8 subsidies provided to the experimental group can only be redeemed for housing in census tracts with 1990 poverty rates less than 10 percent. Families in both groups had up to 180 days to identify a suitable rental unit and sign a lease.

The experimental group also received housing-search assistance and life-skills counseling from CAN. Relocators in both the experimental and Section 8-only groups were required to sign leases for one year. Those who wished to move again before the initial lease expiration date were not eligible for a new Section 8 subsidy, although families could move thereafter with no restrictions. CAN contacted experimental families twice following relocation; otherwise, postprogram monitoring was limited [Goering, Carnevale, and Teodoro 1996].

III. Conceptual Framework

The potential effects of MTO on juvenile crime can be highlighted using the reduced-form equation (1) from Moffitt [1998]. The "supply" of criminal offenses by teen (i) in neighborhood (n) in period (t), Y_{int}, is a function of teen (i)'s individual and family characteristics, X_{int}, the characteristics and criminal involvement of others in the neighborhood ($X\text{-}_{int}$ and $Y\text{-}_{int}$), and unmea-

3. These are FBI Uniform Crime Index offense rates, an index which consists of criminal homicide, forcible rape, robbery, assault, breaking or entering, larceny-theft, motor vehicle theft, and arson [Maryland State Police 1997]. Our calculations are likely to understate the actual crime rate in the MTO baseline neighborhoods because we only have crime information at the police district level in Baltimore City, and MTO families presumably live within the higher-crime parts of these districts.

sured variables specific to the neighborhood (ϵ_{nt}) and teen (ϵ_{int}). Identifying β_2 and β_3 in equation (1) using nonexperimental data is complicated by possible correlation between neighborhood characteristics (X_{-int} and Y_{-int}) and unmeasured individual-level variables (ϵ_{int}):

$$(1) \qquad Y_{int} = \beta_0 + \beta_1' X_{int} + \beta_2' X_{-int} + \beta_3' Y_{-int} + \epsilon_{int} + \epsilon_{int}.$$

The MTO experiment overcomes this problem by randomly assigning families into different mobility treatment groups. Since assignments to treatment groups but not relocation outcomes are random, we initially focus on comparing the mean outcomes of families according to their treatment-group assignment. This "intent-to-treat" (ITT) effect is calculated by estimating equation (2), where Z indicates whether families are assigned to the experimental ($Z = 1$) or control group ($Z = 0$). (The analysis is identical for the effects of the Section 8-only treatment.) The regression is estimated using a panel of person-quarter observations for MTO teens where (t) indexes quarters since random assignment ($t > 0$). The use of panel data allows us to control for common trends in crime over time by including indicators for quarter since randomization (δ_t) and calendar quarter ($\lambda_{t'}$), thus improving the precision of our estimates. We also control for a vector of preprogram characteristics (X_{in}) to adjust for chance differences in these variables across groups, and estimate robust standard errors that account for the panel structure of the data and the presence of multiple teens from the same family:

$$(2) \qquad Y_{int} = \alpha_0 + \alpha_1 Z_{in} + \alpha_2' X_{in} + \delta_t + \lambda_{t'} + \upsilon_{int}.$$

Also of interest are the "effects of treatment on the treated" (TOT), which can be recovered if assignment to the experimental or Section 8-only groups have no effect on families who do not relocate through MTO ("noncompliers").[4] This assumption seems

4. The TOT estimate also assumes that the proportion of families who would comply with each treatment is equivalent across treatment groups, which should be met because of random assignment, and that none of the families in the control group receive either the experimental or Section 8-only treatments. This second assumption is met under our definition of the experimental and Section 8-only treatments as "relocation to Section 8-subsidized private-market housing through the MTO program." Control families who relocate on their own into private-market housing are different from those who receive the Section 8-only and experimental treatments because their private-market housing is not subsidized. Some control families may have received something close to the Section 8-only group's treatment through HUD's Hope VI program, which funded the demolition of two of the baseline public housing buildings, although the timing of the Hope VI

reasonable since the only other service provided to the experimental group is the CAN counseling, and even intensive youth counseling programs appear to have modest if any effects on delinquency [Donohue and Siegelman 1998]. The TOT effect (equation (3)) compares the outcomes of experimental and control group families who *would* comply with the experimental treatment, known as "potential compliers" ($C = 1$). Actual compliance with the MTO experimental treatment (indicated by $D = 1$) is only observed among families who are assigned to the experimental group:

$$TOT = E[Y|Z = 1,C = 1] - E[Y|Z = 0,C = 1]. \tag{3}$$

The TOT effect is estimated by applying two-stage least squares to equation (4) using Z as an instrument for D, which in large samples will converge to the ITT effect divided by the probability of compliance with the assigned treatment [Bloom 1984]. The mean outcome of control teens who would have complied with the experimental group, the "control complier mean" (CCM), is given by equation (5) [Katz, Kling, and Liebman, 2001]:

$$Y_{int} = \gamma_0 + \gamma_1 D_{in} + \gamma_2 X_{in} + \delta_t + \lambda_{t'} + \eta_{in} \tag{4}$$

$$\begin{aligned} CCM &= E[Y|Z = 0,C = 1] \\ &= E[Y|Z = 1,C = 1] - TOT. \end{aligned} \tag{5}$$

Since MTO simultaneously changes all of the neighborhood characteristics of the treatment-group compliers (X_{-int},Y_{-int}, and ϵ_{nt}), we cannot identify the specific mechanisms through which neighborhoods affect juvenile crime (β_2 and β_3 in equation (1)). On net we expect MTO to reduce teen involvement in violent crime, since the program moves families to more affluent, lower-crime neighborhoods. The likely effect of MTO on property crime is ambiguous, since more affluent neighborhoods may also provide more lucrative opportunities for theft.[5]

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and MTO moves were different. Moreover, none of the control families moved through Hope VI received the additional life-skills counseling and search assistance or the relocation restrictions imposed on experimental-treatment families.

5. The standard economic model of crime suggests that the material gains from crime ("loot") will affect the probability of property offending [Ehrlich 1996]. The possibility of more valuable loot in low-poverty areas should be less of an issue for violent offending, since robbery is the only violent crime that is motivated by material gain and accounts for only 16 percent of juvenile violent-crime arrests in 1995 [Maguire and Pastore 1997]. Consistent with our prediction that neighborhood affluence may contribute to increases in property offending, previous re-

The final complication is that our outcome measure reflects arrests rather than criminal activity. Since the probability of arrest is less than one, our analyses of arrest data provide lower-bound estimates for the effects of MTO on criminal offending that are proportional to the probability of arrest. A more troublesome possibility is that the probability of arrest for MTO teens may be higher in more affluent areas; a suitably large increase in this probability could cause experimental-group arrest rates to *increase* relative to controls even if criminal offending *decreases.* Maryland data suggest that the probability of arrest for crimes reported to the police (the "clearance rate") is similar across areas.[6] But because victims are somewhat less likely to report less-serious crimes to the police in cities than in suburbs [Laub 1981],[7] our estimates may understate reductions in minor offenses among experimental teens and overstate any increases.

Differences in false arrests across areas will also bias our findings, although we assume that these constitute only a minority of arrests and thus are unlikely to substantially distort our estimates. We test this assumption by analyzing arrests by sepa-

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search suggests that high-poverty public-housing complexes typically have higher rates of violent and drug crimes than surrounding areas, but less property crime [Dunworth and Saiger 1994; Dumanovsky, Fagan, and Thompson 1999].

6. We estimate clearance rates for FBI "index crimes" using data at the police-district level for the addresses of MTO families as of December 1997. Index crimes include those that fall into our violent-crime category (aggravated assault, robbery and rape, but not simple assaults) as well as our property-crime category (larceny, burglary/breaking and entering, and motor vehicle theft). Estimated clearance rates in the postprogram neighborhoods equal 22.0, 22.9, and 23.8 percent for the experimental, Section 8-only, and control groups, respectively. Unfortunately, we cannot calculate separate clearance rates for violent versus property crimes at the police-district level.

7. Other research on victim reporting from the National Crime Victimization Survey (NCVS) finds that victim reporting rates are lower for teens than for adults, lower among high school dropouts than those with a high school degree or more, slightly lower for whites than nonwhites [Biderman and Lynch 1991; Levitt 1998], and bears no relationship to household income (unpublished tabulations provided to us by James Lynch). The net effects on differences in victim reporting in postprogram neighborhoods across MTO treatment groups are likely to be modest, since for the experimental and Section 8-only groups the effects on reporting from moving to areas with older and more educated residents is partially or wholly offset by the fact that more of these residents are white. The effects of differences in gang activity across neighborhoods on crime reporting are ambiguous [Akerlof and Yellen 1994]. It is possible that victim reporting could have increased in Baltimore City if police initiated an intensive policing effort such as the Boston Gun Project [Piehl, Kennedy, and Braga 2000]. While Baltimore did initiate a program to mobilize communities against gun violence and drugs starting in 1995 [OJJDP 1999], our conversations with John Tewey, formerly of the Baltimore City Police Department's Violent Crime Task Force, suggest that on the whole no systematic changes in police practices were undertaken in Baltimore during our study period.

rate crime categories to isolate arrests for disorderly conduct, resisting arrest, and assaulting a police officer, which previous case studies suggest are disproportionately associated with false arrests [Chevigny 1969; Ogletree et al. 1995]. Second, we produce qualitatively similar findings when we focus on convictions rather than arrests, which should be less susceptible to discrimination because courtroom behaviors are presumably more easily monitored than street-level police practices.[8]

IV. DATA

All families were required to complete a self-administered survey questionnaire designed by Abt in order to enroll in the MTO program. Abt also recorded the locations of the initial program moves by families in the experimental and Section 8-only groups, as well as follow-up addresses identified through passive and active tracking methods from July to December of 1997.[9]

Our key outcome measures come from official arrest histories maintained by the Maryland DJJ through March 1999, which include the charges for which juveniles (under 18) are arrested, the date and disposition of the arrest, and other information.[10]

8. We define "convictions" in two different ways with the DJJ data. Our narrow definition includes only those cases that are adjudicated and result in a juvenile court ruling of delinquency (around 20 percent of all arrests to the MTO population), while the more expansive definition also includes cases that are disposed of informally and result in probation or service referral (35 percent of arrests). The results are qualitatively similar using either definition, and many of the violent-arrest effects remain statistically significant at the 10 percent level. Consistent with our prediction that discriminatory police behavior in low-poverty areas will show up as arrests within our other-crime category, the experimental treatment has a larger negative effect on convictions than arrests for crimes in our "other" category discussed below (results available upon request).

9. These addresses come from passive tracking sources such as administrative records from the Baltimore-area housing agencies that administer the Section 8 subsidies to the experimental and Section 8-only groups, change-of-address registries, and credit bureaus, as well as from the results of a brief follow-up survey of MTO families. Surveys were conducted on the phone for as many families as possible; those who could not be reached by telephone were interviewed in person. The response rate to Abt's survey was 91 percent. The survey asked household heads about the current composition of the household, and about the new addresses of individuals who were listed as members of the household on the baseline survey but were no longer living in the home at the time of the follow-up survey. The survey data suggest that around 10 percent of children under 18 were living apart from the householder as of July 1998, a proportion that is surprisingly similar across MTO treatment groups (personal communication with Judie Feins and Debi Magri-McInnis, Abt Associates).

10. In principle, a population of DJJ referrals will differ from a population of juvenile arrests because some juveniles may be referred directly to the DJJ without having been arrested, and some juveniles may be arrested and referred directly to the adult justice system. Both events are rare in Maryland. For

These data provide us with an average of 3.7 years of postprogram criminal-offending information for MTO teens. A DJJ staff member manually searched the state's arrest database by date of birth and name to identify offense histories for the 1406 MTO participants born in 1977 or later. Our estimates will be proportional to the match rate produced by this process, which we believe is reasonably high for two reasons: first, separate matches conducted by the DJJ produced quite similar results;[11] second, the arrest rate implied for MTO teens is quite similar to what has been found in other studies for similar samples of teens.[12] Another source of attrition comes from moves out of state, although only three MTO families had done so by 1997.

A total of 279 MTO teens were arrested 998 times in the pre- and postprogram period for charges ranging from shoplifting to attempted murder. We classify these crimes into three categories (see Ludwig, Duncan, and Hirschfield [2000] for details): Violent crimes (292 arrests, of which 77 percent are assaults and 16 percent are robberies); Property crimes (354 arrests, of which 55 percent are larcenies/thefts, 25 percent are motor vehicle thefts, and 20 percent are burglaries); and "Other" crimes (352 arrests, 50 percent of which are drug offenses, and 19 percent of which are for disorderly conduct or resisting arrest).[13] As argued above, we

example, 95 percent of the referrals in our MTO data involved an arrest. In Maryland as a whole in 1996, only 5 percent of arrested juveniles were automatically charged as adults (personal communication with Denise Scherer, Maryland State Police). As a result, for convenience we refer to DJJ referrals as "juvenile arrests."

11. The DJJ conducted two matches for this project, once during February 1998 (for an earlier version of this paper) and again during March 1999. Of the 240 MTO teens arrested prior to February 1998, eleven were identified in the first matching attempt but not the second, and ten were identified in the second matching attempt but not the first. Presumably there are some teens who have DJJ criminal records but were not identified by either match because they give a false name or date of birth to the police upon arrest, their names are grossly misspelled in one of the data sets, or some other reason. We believe this group is likely to be small based on our comparisons of arrest rates for MTO males and those found with similar samples (see below).

12. We find that 81 percent of the MTO males born from 1977 to 1981 (who thus had turned eighteen by the end of our arrest data set's observation period) had been arrested at least once. By way of comparison, previous studies have found that between 42 and 64 percent of black males in other urban samples have been arrested by age eighteen [Blumstein et al. 1986]. The samples examined in previous studies are likely to be more advantaged than the MTO population and come from earlier cohorts of juveniles.

13. In cases where teens were charged with multiple offenses per incident, we define the arrest charge as the most serious offense for which the teen has been charged.

expect any effects of law enforcement discrimination to be most pronounced for arrests in this last category.

V. Empirical Results

This section begins with a brief description of the characteristics of the MTO population and their relocation outcomes. We then present our estimates for the program impacts on arrests.

A. Baseline Characteristics of the MTO Population

As shown in the top panel of Table I, nearly all of the MTO households are headed by unmarried African-American women, the large majority of whom were receiving AFDC at baseline.[14] Over three-quarters of household heads who volunteered for the program reported that escaping from gangs and drugs was the first or second most important reason for enrolling in MTO, which is not surprising given that around half of all families reported that someone in the household had been the victim of a crime during the previous six months.[15] The vectors of means for the baseline household characteristics are similar across groups.[16]

Table I also presents background information for the MTO teens "at risk" for criminal involvement during the postprogram period, which we initially define as the 336 teens who are at least eleven but less than sixteen years of age at the time of random assignment. We exclude children under eleven from our analytic sample because arrests of younger children are very rare [OJJDP 1996], and we exclude older children since we wish to focus on teens who are still under the jurisdiction of the juvenile justice system (under eighteen) during the postprogram period. The pre-

14. The number of families differs across the three treatment groups because the Abt randomization algorithm attached a higher probability of assignment to the experimental group. In the Baltimore MTO site, the weighting proportions for the experimental, Section 8-only, and control groups changed on February 1, 1996, from 8:3:5 to 3:8:5. This change could in principle affect our results if average criminality is different across MTO cohorts. To address this possibility, we weight all of our estimates so that the weighted proportion of families from each cohort is equal across MTO treatment groups, and by including a control variable for the time between when the family was randomized and the end of our observation period (March 1999).

15. While these victimization rates may be somewhat overstated because of "telescoping" and other reporting errors [Skogan 1981], they are nonetheless higher than the six-month victimization rate of 6 percent estimated for New York City public housing residents [Goering, Carnevale, and Teodoro 1996].

16. Multivariate analysis of variance (MANOVA) cannot reject the null hypothesis that the vectors of means presented in the top panel of Table I are equal across the three MTO treatment groups, with a probability value of 0.70.

TABLE I
BASELINE CHARACTERISTICS OF BALTIMORE MTO HOUSEHOLDS

	Total	Exp	Section 8-only	Control
Household characteristics				
Families (N)	638	252	188	198
African-American (%)	97.3	96.9	96.8	98.4
Female householder (%)	97.9	98.4	96.2*	99.0
Householder age	33.6	33.9	33.4	33.2
Number of children	2.72	2.67	2.98	2.55
Householder w/high school or GED (%)	57.9	59.4	60.8	53.3
AFDC at baseline (%)	81.3	80.3	83.5	80.4
During last 6 months, someone in HH had been victim of crime (%)	50.4	54.4*	49.9	45.7
Primary reasons for Enrolling in MTO (%):				
Better schools, job access	14.4	13.1	17.9	13.3
Avoid gangs, drugs	53.6	53.9	50.7	55.8
Better apartment	26.4	26.8	27.5	25.0
Other	5.4	6.4	3.8	5.8
Second most important reason (%):				
Better schools, job access	36.1	38.3	36.3	36.7
Avoid gangs, drugs	27.9	27.5	26.8	29.3
Better apartment	29.6	26.4	33.2	30.3
Other	6.5	7.8	3.7	7.4
Characteristics of teen analytic sample				
Teens (N)	336	148	92	96
Male (%)	46.7	43.8	52.0	45.8
Age (%) 11	19.1	19.7	15.7	21.9
12	21.5	21.4	23.4	19.8
13	21.7	19.7	22.7	24.0
14	19.5	22.0	19.4	15.6
15	18.1	17.3	18.8	18.8
Violent-crime arrests during preprogram period (% of teens)				
1	9.0	9.5	9.8	7.3
2	2.1	3.1	0.6	2.1
3 or more	1.2	1.0	1.6	1.0
Property-crime arrests during preprogram period (% of teens):				
1	5.8	5.7	4.5	7.3
2	1.1	2.6**	0.0	0.0
3 or more	1.4	1.6	1.6	1.0
Other-crime arrests during preprogram period (% of teens):				
1	6.1	10.2**	2.8	3.1
2	2.1	3.7	0.6	1.0
3 or more	1.3	0.5	2.8	1.0

Authors' weighted calculations from MTO baseline survey data (see text). Baseline survey response rate for MTO families equals 100 percent. ** = Difference with control group statistically significant at 5 percent. * = Difference with control group statistically significant at 10 percent.

program arrest rates for property and other offenses appear to be somewhat higher for the experimental than for other groups. We believe these differences are most likely due to random chance because discussions with Abt suggest that randomization was conducted properly, and because there are no systematic differences in the full set of baseline household variables for MTO families in the Baltimore (top panel, Table I) or Boston sites [Katz, Kling, and Liebman 2001].

B. Relocation Outcomes

Given the restrictions placed on the location decisions of experimental families, it is perhaps not surprising that a smaller proportion relocated through MTO relative to those assigned to the Section 8-only group (54 versus 73 percent).[17] The median program-mover in the experimental and Section 8-only groups relocated within nine and four months of random assignment, respectively.[18] While a larger proportion of Section 8-only families relocate through MTO, most of these families stay within Baltimore City (Table II), quite close to the baseline neighborhoods. In contrast, a larger share of experimental relocators move outside of the city, and those who stay in the city move further from the baseline areas than Section 8-only movers.

Most program-movers in the experimental group were still in very low-poverty census tracts (<10 percent poverty rate) as of December 1997, even though their initial leases had expired and they were free to relocate to other areas. In contrast, most Section 8-only families who relocated did not voluntarily move to very low-poverty neighborhoods, and less than 5 percent of control

17. Of the Section 8-only noncompliers, almost all formally requested a Section 8-subsidy but could not sign a lease before the subsidy offer expired. In contrast, only half of the experimental group noncompliers ran up against the Section 8 subsidy time limit. One-quarter of the experimental noncompliers did not successfully complete the mandatory CAN counseling program, and the remaining noncompliers never contacted CAN after being assigned to the experimental group.

18. While families had around 180 days from the time they were issued their Section 8 vouchers and certificates to relocate, the actual time between random assignment and MTO relocation may be greater than 180 days because of lags between randomization and the issuance of the rental subsidies. For the experimental-group relocators, one-quarter move within one-half year of random assignment, half move within nine months, three-quarters have moved within one year, and all of the families have moved within two years. For the Section 8-only group relocators, half moved within four months of random assignment, three-quarters had moved within six months, and all of the families had moved within the first year.

TABLE II
RELOCATION OUTCOMES FOR MTO FAMILIES

	Baseline (all families)	Experimental		Section 8-only		Control	
	1994–1995	Initial Post-program	As of 12/97	Initial Post-program	As of 12/97	Initial Post-program	As of 12/97
Distribution of MTO households							
Jurisdiction:							
Baltimore City	100.0	77.1	79.4	89.9	86.7	99.5	98.0
Anne Arundel County	0.0	0.8	2.0	0.0	0.5	0.0	0.0
Baltimore County	0.0	13.0	10.7	5.3	8.0	0.0	1.0
Harford County	0.0	0.4	0.4	0.0	0.0	0.0	0.0
Howard County	0.0	7.1	5.9	2.7	2.7	0.0	0.5
Montgomery County	0.0	0.4	0.4	0.0	0.0	0.0	0.0
Other	0.0	1.2	1.2	2.1	2.1	0.5	0.5
% Census tract poor:							
0–9.9	0.0	49.4	43.0	8.7	12.5	0.0	4.5
10–19.9	0.0	4.8	8.4	14.7	21.2	0.0	7.6
20–29.9	0.2	0.0	7.6	10.3	15.8	0.0	3.0
30–39.9	0.3	0.4	4.0	12.5	13.0	0.0	6.6
40–49.9	2.0	1.6	6.4	9.8	7.1	2.0	6.6
50–59.9	4.4	1.2	4.0	6.5	4.9	5.6	4.5
60–69.9	52.5	22.7	18.7	26.6	19.6	49.0	43.4
70–79.9	20.4	9.6	4.0	7.1	3.8	23.2	14.6
80 plus	20.1	10.4	4.0	3.8	2.2	20.2	9.1

"Initial" postprogram addresses reflect the initial moves made by experimental and Section 8-only families who relocate through MTO, and the baseline locations for remaining families. Neighborhood characteristics are calculated using 1990 Census data for all families assigned to each of the three MTO treatment groups (program-movers as well as nonmovers).

families had moved on their own to very low-poverty tracts by the end of 1997.

C. Effects of MTO on Juvenile Arrests

Figures I and II present the regression-adjusted number of arrests for violent and property crimes per 100 teens for each of the three MTO treatment groups by quarter. Our regression adjustment controls for random treatment group differences in the background variables shown in Table I.[19] As noted above,

19. The regression models also control for the amount of postprogram exposure time of MTO teens, defined as the time (in years) between the date of random assignment and March 1999, the last date for which we have arrest information, dummies for mother's employment status at baseline, and a set of detailed

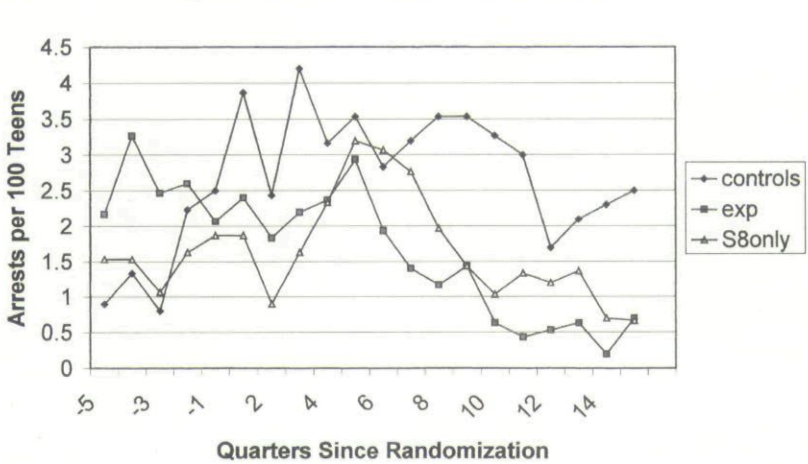

FIGURE I

Regression-Adjusted Violent-Crime Arrest Rates

Figures above show regression-adjusted arrests per quarter per 100 teens. Regression-adjustment controls for preprogram variables are shown in Table I; experimental and Section 8-only group figures are evaluated at the control group means for the explanatory variables. The regression-adjusted figures for preprogram quarters do not control for the preprogram arrests shown in Table I. Analytic sample consists of the 336 MTO teens who are ages $11 \leq \times < 16$ at random assignment; quarters following each teen's eighteenth birthday are excluded from the analysis. We present three-quarter moving averages of each treatment group's regression-adjusted to reduce the influence of random quarter-to-quarter variability.

these estimates are calculated using the cohort of MTO teens who are at least eleven but less than sixteen years old at random assignment. We exclude from our panel person-quarters for this cohort that follow the participant's eighteenth birthday. The figures suggest that starting four to six quarters after randomization, the experimental and Section 8-only groups experience a reduction in violent-crime arrests relative to controls. The figures also suggest that relative to controls the experimental teens may have somewhat higher rates of property-crime arrest, although this increase seems to be concentrated in the period shortly after families move.

preprogram victimization dummies (household was broken into, someone had purse/wallet stolen, someone was threatened, someone was beaten, and someone was shot or stabbed) instead of the overall victimization variable as in Table I. The regression-adjusted arrest rates for the prerandomization period in Figures I and II do not control for the preprogram arrest dummies shown in Table I. We evaluate each treatment group's predicted arrest rate at the mean value of the control group covariates. The figures present three-quarter moving averages for quarterly arrest rates.

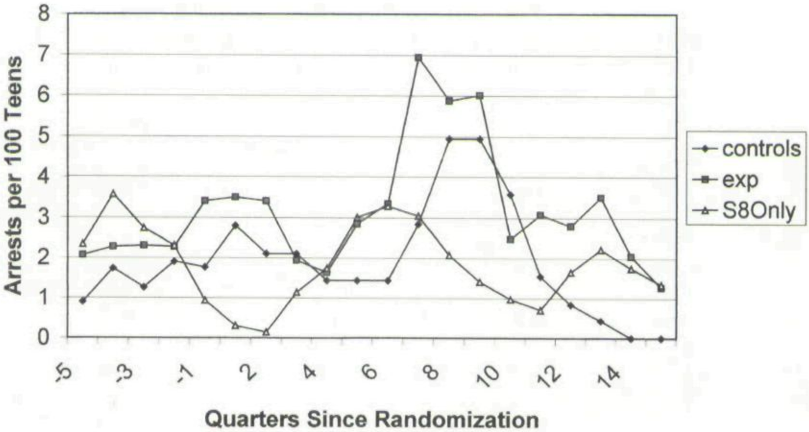

FIGURE II

Regression-Adjusted Property-Crime Arrest Rates

Figures above show regression-adjusted arrests per quarter per 100 teens. Regression-adjustment controls for preprogram variables are shown in Table I; experimental and Section 8-only group figures are evaluated at the control group means for the explanatory variables. The regression-adjusted figures for preprogram quarters do not control for the preprogram arrests shown in Table I. Analytic sample consists of the 336 MTO teens who are ages $11 \leq \times < 16$ at random assignment; quarters following each teen's eighteenth birthday are excluded from the analysis. We present three-quarter moving averages of each treatment group's regression-adjusted to reduce the influence of random quarter-to-quarter variability.

More formally, Table III shows that on average, 2.7 percent of control-group teens are arrested for a violent crime during each quarter of the postprogram period, with around three arrests per 100 teens per quarter (the "prevalence" and "incidence" of arrest, respectively). When we use our panel data set to estimate equation (2) controlling for the background variables from Table I as well as dummies for calendar quarter and quarters since randomization (bottom panel, Table III), we find that the prevalence and incidence of arrests for violent crimes for experimental teens during the postprogram period equal around one-half of the control-group averages ($p < .10$ and $p < .05$, respectively). A reduction in robbery arrests accounts for half of this difference, although robberies account for only 16 percent of all violent-crime arrests to the MTO sample as a whole.

Experimental teens may also experience an increase in property arrests, although this evidence is somewhat more ambiguous: while the differences in raw means are statistically significant at the 5 percent level (top panel of Table III), these

TABLE III
MTO INTENT-TO-TREAT EFFECTS ON POSTPROGRAM ARRESTS FOR JUVENILES AGES 11–16 AT RANDOM ASSIGNMENT

	Control group mean arrests		Experimental vs. control		Section 8-only vs. control	
	Prev.	Incid.	Prev.	Incid.	Prev.	Incid.
Full sample (N = 336)						
Unadjusted violent crime	0.027	3.0	−0.008	−1.0	−0.012	−1.4
			(0.007)	(0.8)	(0.008)	(0.8)*
Property crime	0.018	2.0	0.014	1.6	0.003	0.1
			(0.007)**	(0.8)**	(0.008)	(0.8)
Other crime	0.028	3.3	0.001	−0.1	−0.004	−0.8
			(0.009)	(1.1)	(0.009)	(1.0)
All crimes	0.068	8.3	0.006	0.6	−0.010	−2.1
			(0.015)	(2.1)	(0.017)	(2.0)
Full sample (N = 336)						
Regression-adjusted violent crime	0.027	3.0	−0.013	−1.6	−0.012	−1.4
			(0.007)*	(0.8)**	(0.008)	(0.8)*
Property crime	0.018	2.0	0.009	1.3	−0.003	−0.5
			(0.006)	(0.8)	(0.007)	(0.8)
Other crime	0.028	3.3	−0.005	−0.7	−0.009	−1.3
			(0.008)	(1.0)	(0.008)	(1.0)
All crimes	0.068	8.3	−0.009	−0.9	−0.022	−3.1
			(0.012)	(1.8)	(0.015)	(1.8)*

"Prevalence" refers to the proportion of teens who are arrested per quarter during the postprogram period, while "incidence" refers to arrests per 100 teens per quarter. Treatment effects are calculated by applying a linear probability model to a quarterly panel data set, controlling for the preprogram control variables shown in Table I. The intent-to-treat effects shown above are the coefficient estimates for variables indicating random assignment to the experimental or Section 8-only group. Huber-White robust standard errors (in parentheses) account for panel structure of data as well as the inclusion of multiple children from the same family in our sample. Analytic sample consists of the 336 MTO teens who are ages $11 \leq \times < 16$ at random assignment; quarters following each teen's eighteenth birthday are excluded from the analysis. ** = Statistically significant at 5 percent. * = Statistically significant at 10 percent.

differences are no longer statistically significant when we adjust for preprogram characteristics (bottom panel). Nearly three-quarters of the increase in property-crime arrests is accounted for by larceny-thefts, which account for only 55 percent of all property-crime arrests for the MTO sample as a whole.

We also find that the prevalence and incidence of violent-crime arrests for the Section 8-only group are around one-half the rate observed for the control group, although only the effect on the incidence of arrest is statistically significant (at the 10 percent level) even after regression-adjusting for preprogram characteristics. As with the experimental treatment, around half of the reduction in violent-crime arrests comes from reductions in robbery arrests.

The top panel of Table IV suggests that our findings are robust to decisions about how to control for random differences in preprogram arrests, since we obtain qualitatively similar results when we focus only on those teens with no preprogram arrests. Table IV also shows that the results are similar when we stratify the sample by gender, although the experimental-treatment effects for girls are smaller in both absolute and proportional terms than those for boys.

Finally, Table V presents estimates for the effects of treatment-on-the-treated. The TOT effects for both MTO treatments on the prevalence and incidence of violent-crime arrest equal one-half or more of the control-complier mean (calculated from equation (5) above);[20] the estimated TOT effect appears to be slightly larger for the experimental than Section 8-only treatment.

Table V also shows that the postprogram arrest rates for experimental and Section 8-only compliers are somewhat higher than those of the noncompliers (particularly for violent and property crimes).[21] We also find that *preprogram* arrests of teens have a positive effect on the family's probability of making an MTO move.[22] These results suggest that families with teens at above-average risk of criminal involvement were more likely to relocate through MTO. This finding runs counter to the commonly held view that families with "better" outcomes are more likely to self-select into more affluent areas [Evans, Oates, and Schwab

20. The estimated control-complier means are generally similar for the control families who would have complied with the experimental treatment compared with those who would have complied with the Section 8-only treatment. The observed differences in Table V are presumably due in part to sampling variability, and may also be due to differences in the set of families who choose to take up the different treatments, as well as whatever differences exist across neighborhoods in the probability that a criminal act results in an arrest (a problem that may be of particular concern for the less-serious property and "other" crimes because of differences in victim reporting of such crimes to the police).

21. Note that these results do *not* imply that MTO relocation increases arrest rates, even though the arrest rates are higher for experimental and Section 8-only compliers (Table V) than for the control group as a whole (Table III). The reason is that compliers are a self-selected subgroup of MTO families; as a result, their average outcomes can only be compared with those of the potential compliers within the control group (as is done with the TOT estimates), and not with those of all control teens.

22. We find that the number of preprogram arrests for other crimes has a positive and statistically significant correlation with the probability of making an MTO move for families assigned to the experimental group, while preprogram property-crime arrests have a positive and statistically significant correlation with the probability of an MTO move by families in the Section 8-only group. We also find that preprogram violent-crime arrests have a stronger effect on the probability of MTO moves for experimental than Section 8-only families, consistent with the findings in Table V that the postprogram CCM for violent crime is higher for the experimental than Section 8-only treatment.

TABLE IV

MTO INTENT-TO-TREAT EFFECTS ON POSTPROGRAM ARRESTS FOR JUVENILES AGES 11–16 AT RANDOM ASSIGNMENT, STRATIFIED BY PREPROGRAM ARRESTS AND GENDER

	Control group mean arrests		Experimental vs. control		Section 8-only vs. control	
	Prev.	Incid.	Prev.	Incid.	Prev.	Incid.
Teens with No preprogram arrests (N = 256)						
Regression-adjusted violent crime	0.022	2.2	−0.010	−1.0	−0.014	−1.4
			(0.007)	(0.7)	(0.008)*	(0.8)*
Property crime	0.011	1.2	0.008	0.9	−0.003	−0.4
			(0.006)	(0.7)	(0.006)	(0.6)
Other crime	0.019	2.2	−0.003	−0.6	−0.004	−0.6
			(0.008)	(0.9)	(0.008)	(0.9)
All crimes	0.047	5.6	−0.006	−0.7	−0.017	−2.5
			(0.011)	(1.6)	(0.013)	(1.5)
All males (N = 162)						
Regression-adjusted violent crime	0.038	4.3	−0.021	−2.9	−0.013	−1.9
			(0.012)*	(1.4)**	(0.010)	(1.2)
Property crime	0.029	3.3	0.014	2.7	−0.007	−0.5
			(0.013)	(1.7)	(0.010)	(1.2)
Other crime	0.048	5.9	−0.011	−1.9	−0.022	−3.1
			(0.020)	(2.5)	(0.017)	(2.0)
All crimes	0.105	13.6	−0.024	−2.1	−0.044	−5.5
			(0.025)	(4.0)	(0.025)*	(3.2)*
All females (N = 174)						
Regression-adjusted violent crime	0.018	1.8	−0.007	−0.7	−0.004	−0.4
			(0.007)	(0.7)	(0.009)	(0.9)
Property crime	0.009	0.9	0.010	1.0	0.016	1.6
			(0.006)	(0.6)	(0.011)	(1.1)
Other crime	0.012	1.2	−0.005	−0.5	0.004	0.4
			(0.005)	(0.5)	(0.006)	(0.6)
All crimes	0.037	3.9	0.001	−0.2	0.018	1.6
			(0.012)	(1.3)	(0.018)	(1.8)

"Prevalence" refers to the proportion of teens who are arrested per quarter during the postprogram period, while "incidence" refers to arrests per 100 teens per quarter. Treatment effects are calculated by applying a linear probability model to a quarterly panel data set, controlling for the preprogram control variables shown in Table I. The intent-to-treat effects shown above are the coefficient estimates for variables indicating random assignment to the experimental or Section 8-only group. Huber-White robust standard errors (in parentheses) account for panel structure of data as well as the inclusion of multiple children from the same family in our sample. Analytic sample consists of the 336 MTO teens who are ages $11 \le X < 16$ at random assignment; quarters following each teen's eighteenth birthday are excluded from the analysis. ** = Statistically significant at 5 percent. * = Statistically significant at 10 percent.

TABLE V
EFFECTS OF TREATMENT-ON-THE-TREATED FOR MTO JUVENILES AGES 11–16 AT RANDOM ASSIGNMENT

	EXPERIMENTAL TREATMENT			
	Experimental families: relocation status in postprogram period—			
	Move through MTO (1)	Not move through MTO (2)	Imputed arrest rate for controls who would have moved if assigned to exp. group (3)	Effects of treatment on the treated (1) − (3)
Postprogram				
Prevalence				
Violent crime	0.024	0.015	0.050	−0.026 (0.014)*
Property crime	0.034	0.030	0.022	0.012 (0.013)
Other crime	0.031	0.028	0.039	−0.008 (0.016)
All crimes	0.081	0.067	0.103	−0.022 (0.023)
Incidence				
Violent crime	2.5	1.6	5.7	−3.2 (1.5)**
Property crime	3.8	3.5	1.5	2.3 (1.7)
Other crime	3.2	3.3	4.3	−1.1 (2.1)
All crimes	9.5	8.4	11.4	−1.9 (3.5)
	SECTION 8-ONLY TREATMENT			
	Section 8-only families: Relocation status in postprogram period—			
	Move through MTO (1)	Not move through MTO (2)	Imputed arrest rate for controls who would have moved if assigned to S8 group (3)	Effects of treatment on the treated (1) − (3)
Postprogram				
Prevalence				
Violent crime	0.019	0.007	0.039	−0.020 (0.011)*
Property crime	0.026+	0.008	0.027	−0.001 (0.009)
Other crime	0.018	0.041	0.039	−0.021 (0.011)*
All crimes	0.061	0.052	0.096	−0.035 (0.021)*
Incidence				
Violent crime	1.9	0.7	4.3	−2.4 (1.2)**
Property crime	2.6+	0.8	3.0	−0.4 (1.0)
Other crime	1.8	4.4	4.5	−2.7 (1.3)**
All crimes	6.3	5.9	11.8	−5.5 (2.5)**

"Prevalence" refers to the proportion of teens who are arrested per quarter during the postprogram period, while "incidence" refers to arrests per 100 teens per quarter. Treatment effects are calculated by applying 2SLS to equation (4) in text, using each family's MTO treatment-group assignment as an instrument for the family's MTO relocation outcome. The 2SLS equations also control for the preprogram control variables shown in Table I. Huber-White robust standard errors (in parentheses) account for panel structure of data as well as the inclusion of multiple children from the same family in our sample. Analytic sample consists of the 336 MTO teens who are ages $11 \leq \times < 16$ at random assignment; quarters following each teen's eighteenth birthday are excluded from the analysis. ** = Statistically significant at 5 percent. * = Statistically significant at 10 percent. ++ = Difference between MTO program-movers and nonmovers is statistically significant at 5 percent. + = Difference between program-movers and nonmovers is statistically significant at 10 percent.

1992], and raises the possibility that nonexperimental estimates may *understate* neighborhood effects on adolescent behaviors.

D. Sensitivity Analysis

The advantage of the linear estimators that we use to regression-adjust our treatment-group comparisons is that they facilitate a straightforward calculation of the TOT effects using two-stage least squares. Yet for the analysis of the number of arrests (incidence) and other count data, a compound-Poisson regression model such as the negative-binomial will be more efficient [Grogger 1990; Cameron and Trivedi 1998]. When we replicate our analyses for the incidence of arrest using the negative binomial, the findings are qualitatively similar to those in Table III.

Our results are also generally robust to our decisions about which age cutoffs should be used in constructing the analytic sample. Adding teens age seventeen at randomization to our analytic sample increases the magnitude (in absolute value) of the experimental and Section 8-only effects on violent-crime arrests. Including children ages nine and ten at randomization reduces the experimental ITT effect on violent arrests (because young children commit very few violent crimes) and improves the precision of the estimated increase in property offending (because the relatively few crimes that are committed by young children tend to be property offenses) [Snyder and Sickmund 1999]. A similar attenuation in the estimated effect of MTO on violent offending results from using a "rolling cohort" approach, in which the sample includes every teen-quarter in which an MTO participant is at least eleven but less than eighteen years old, because fully 70 percent of the teen-quarters added to the sample come from those younger than eleven at randomization.

VI. Discussion

In this paper we present evidence suggesting that the offer to relocate families from high- to low-poverty neighborhoods reduces juvenile arrests for violent offenses by 30 to 50 percent of the arrest rate for controls. On the other hand, the offer to relocate to very low-poverty areas (less than 10 percent, as in the experimental treatment) may increase property-crime arrests, although this effect is no longer statistically significant once we control for random differences in preprogram characteristics across treatment groups. In any case, the overall pattern is consistent with previous findings from neighborhood-level data, which suggest that high-poverty areas have on average more violent crime but less property crime than low-poverty areas [Dunworth and Saiger 1994; Dumanovsky, Fagan, and Thompson 1999].

Do these findings represent actual behavioral changes among MTO teens caused by changes in neighborhood conditions? One alternative explanation is that differences in arrests across groups reflect differences in the behavior of local criminal justice systems. Some evidence against this hypothesis comes from the fact that the postprogram neighborhoods for the three MTO treatment groups have similar clearance rates. Nevertheless, there remains the possibility that MTO teens are subject to heightened scrutiny in low-poverty areas, and national victimization surveys suggest that victims are more likely to report less-serious crimes to the police in suburbs than cities. If the probability of arrest is higher for experimental than control teens, we will understate any reductions in offending and overstate any increases in crime. Put differently, we are more confident that changes in criminal offending are responsible for the observed reductions in violent arrests compared with the apparent increase in property arrests for experimental teens.

Another alternative explanation for our findings is that they may be artifacts of the slightly higher rates of preprogram offending for the experimental group, because of either a deterrent effect (if teens are "scared straight") or an incapacitation effect (when teens are incarcerated). We address this possibility in part by regression-adjusting for a detailed set of dummy variables capturing the number of preprogram arrests in each of our three crime categories. We also obtain similar results for Section 8-only teens, who have very similar preprogram arrest rates to the controls, and when we restrict our analysis to teens with no preprogram arrests.[23]

Our findings could in principle be due to a general "moving effect" that temporarily disrupts teen involvement in antisocial peer groups, rather than to the specific effects of moving to lower-poverty or lower-crime neighborhoods. The effects of lingering social ties are illustrated by our finding that of the 23 arrests of experimental teens whose families moved out of Baltimore

23. The "incapacitation" argument seems unlikely given that only around 10 percent of arrests result in a sentence to some type of formal detention setting; the mean, median and maximum sentence length equal 90, 70, and 333 days, respectively.

City, two-fifths occurred within the city.[24] Yet our findings that the TOT effects on violent crime are somewhat larger for the experimental than Section 8-only treatment is less consistent with a generic moving effect than with a "dose-response" effect (since experimental compliers move to lower-poverty neighborhoods than do Section 8-only compliers).[25]

Taken together, our findings suggest that moving MTO families from high- to low-poverty neighborhoods reduces juvenile involvement in violent crime. While there remains some uncertainty about the mechanisms underlying these effects, remarkably similar findings have been obtained for the Boston MTO site by Katz, Kling, and Liebman [2001]. Survey data from Boston suggest experimental and Section 8-only ITT effects on problem behavior on the order of 30 to 75 percent for boys six to fifteen years of age, and 15 to 75 percent for girls. Data from both the Baltimore and Boston MTO sites also show that families whose children are most likely to exhibit problem behaviors during the postprogram period are also more likely to relocate through MTO. This finding runs counter to the common assumption that families predisposed toward "better" outcomes are more likely to self-select into lower-poverty areas.

Generalizing from our findings is complicated by the fact that MTO participants are a self-selected group of public housing residents. Nevertheless, these results are at least suggestive that policies designed to change the spatial concentration of poverty in America may influence the overall volume of violent crime. While it is possible that property offending could increase among relocators, any increases in property offending among experimental-group MTO teens occur disproportionately among the least-serious property offenses (larceny-thefts), which on average impose $370 in costs to society per crime [Miller, Cohen, and Wiersema 1996]. On the other hand, reductions in violent crime occur disproportionately among robberies, which have social costs on the order of $8000 per crime.

Determining whether such policies are desirable from soci-

24. More detailed analysis of the location of these arrests is not possible because of the small number of arrests to suburban teens, and because Baltimore City police reporting districts are fairly large geographic areas.

25. This test is imperfect because the lack of a difference in TOT effects is consistent with either a generic moving effect, or nonlinearities in the effects of specific neighborhood characteristics on teen behavior. Moreover, a difference in TOT effects could be due to differences in the composition of the complier populations across the two treatments, rather than to a dose-response effect.

ety's perspective depends in part on the full range of impacts on the families who are relocated. Separate analyses of data from the Baltimore and Boston MTO sites reveal statistically significant treatment effects on welfare receipt, welfare-to-work transitions, criminal victimization, and the physical health of children and mental health of adults [Katz, Kling, and Liebman 2001; Ludwig, Duncan, and Pinkston 2000], although little is yet known about changes in other outcomes. Judging the desirability of housing-mobility programs also requires information about effects on the other residents of both host and baseline neighborhoods, about which almost nothing is currently known. Measuring MTO's impacts on a full range of outcomes over the longer run, and developing a better understanding of why neighborhoods affect juvenile crime and other problem behaviors, remain important goals for future research.

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