Preliminary
Please do not cite or quote

Is Crime Contagious?

Jens Ludwig
Georgetown University and NBER

Jeffrey Kling
Princeton University and NBER

May 2005

Support for this research was provided by grants from the National Science Foundation to the National Bureau of Economic Research (9876337 and 0091854) and the National Consortium on Violence Research (9513040), as well as by the U.S. Department of Housing and Urban Development, the National Institute of Child Health and Development and the National Institute of Mental Health (R01-HD40404 and 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-Thoman 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, Stephen Kennedy, and Larry Orr of Abt Associates, to our collaborators Jeanne Brooks-Gunn, Alessandra Del Conte Dickovick, Greg Duncan, Lawrence Katz, Tama Leventhal, Jeffrey Liebman, Meghan McNally, Lisa Sanbonmatsu, Justin Treloar and Eric Younger, and numerous colleagues for valuable suggestions. Any findings or conclusions expressed are those of the authors.

Is Crime Contagious?

Abstract

We test the hypothesis that criminal behavior is "contagious" – or susceptible to what economists term "endogenous effects" – by examining the extent to which lower local-area crime rates decrease arrest rates among individuals. Using data from the Moving to Opportunity (MTO) randomized housing-mobility experiment, in operation since 1994 in five U.S. cities, we exploit the fact that the effect of treatment group assignment yields different types of neighborhood changes across the five demonstration sites and use treatment-site interactions to instrument for measures of post-randomization neighborhood crime rates as well as neighborhood poverty or racial segregation in analysis of individual arrest outcomes. We find no evidence that violence is contagious; neighborhood racial segregation appears to be the most important explanation for across-neighborhood variation in arrests for violent crimes. Our only evidence for contagion comes with less serious crimes. Some estimates suggest an effect for males, but these results are imprecise. We also find evidence that young males are more likely to engage in property crimes when violent crimes are relatively more prevalent within the community. These findings are consistent with a "resource swamping" model in which increases in the prevalence of more serious crimes dilutes the police resources available for deterring less serious crimes.

Keywords: endogenous effects, social multiplier, arrests, social experiment.

JEL classifications: H43, I18, J23.

Corresponding author:
Jens Ludwig
Georgetown Public Policy Institute
Georgetown University
3520 Prospect Street, NW
Washington, DC 20007
(202) 687-4997
ludwigj@georgetown.edu

I. Introduction

Crime rates vary dramatically across countries, states, cities and, most relevant for the present paper, neighborhoods,[1] which represents what Glaeser, Sacerdote and Scheinkman (1996, p. 507) call "the most puzzling aspect of crime." 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 affect residential sorting, 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, 1999).

A large theoretical literature has developed within economics, sociology, criminology and social psychology to explain how and why social context may affect an individual's propensity to engage in criminal behavior. One possibility is that criminal behavior is "contagious," or susceptible to what economists term "endogenous effects" (Manski, 1993). That is, local prevalence of a given type of criminal behavior may change the individual's propensity to engage in that same behavior by affecting the social stigma associated with the act (preferences), perceived net returns to the behavior (information), or the actual probability of arrest (constraints) (Cook and Goss, 1996; Manski, 2000). An alternative possibility is that the individual's criminal behavior is affected by "contextual effects" -- that is, other attributes of neighborhood residents, including socio-demographic characteristics as in role model stories (Wilson, 1987) or the willingness of neighbors to become involved in local order maintenance, which Sampson, Raudenbush and Earls (1997) term "collective efficacy." A third possibility is "correlated effects" -- that is, the institutional or other characteristics of neighborhoods themselves may matter for criminal behavior (Jencks and Mayer, 1990). Determining whether any of these models – or selection – explain neighborhood variation in crime is important because only with contagion do policy interventions or other exogenous shocks become amplified through "social multipliers" (Glaeser, Sacerdote and Scheinkman, 1996, 2003).

[1] For example in Wilmette, Illinois, an affluent suburb just north of Chicago with a median home value of $441,000, the homicide rate in 2002 equaled 0 per 100,000 residents. In Chicago the homicide rate was equal to 23 per 100,000 for the city as a whole, and in some high-poverty neighborhoods on the South Side homicide rates are as high as 60 or 70 per 100,000 (Citywide homicide rates from city-data.com, while district-level homicide rates within Chicago come from the Chicago Police Department (2003). Similarly, data from the Chicago Police Department show that around 10% of police beats accounted for around 30% of all homicides.

Is crime actually contagious? Despite the large theoretical literature on this question, the available empirical evidence is currently quite limited. Glaeser, Scheinkman and Sacerdote (1996) document excess variation in crime rates across areas beyond what can be explained by variation in standard socio-demographic determinants of criminal behavior. Their results suggest that social interactions are more important for less-serious than more-serious crimes. While a number of empirical studies have claimed to produce more direct evidence for social multipliers for anti-social behavior, perhaps most famously Crane (1991), in the presence of endogenous residential sorting such studies may confound the causal effects of neighborhood environment with those of unmeasured individual or family characteristics associated with neighborhood selection. Of course even in the absence of the selection problem, such studies will have difficulty determining which of the models above are responsible for any observed neighborhood effects on criminal behavior.

In this paper we provide an empirical test of whether crime is contagious (that is, susceptible to endogenous effects) by drawing on data from the Moving to Opportunity (MTO) randomized housing-mobility experiment. The MTO demonstration, sponsored by the U.S. Department of Housing and Urban Development (HUD), 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.[2] From 1994 to 1997, 4,248 families were randomly assigned into one of three groups. 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 1990 poverty rates of 10 percent or less.[3] 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

[2] 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.

[3] 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. MTO vouchers required residence in these tracts for a minimum of one year for renewal of the subsidy. Experimental group families were also provided with mobility assistance and in some cases other counseling services as well.

comparable groups of families living in very different kinds of post-program neighborhoods. Kling, Ludwig, and Katz (2005; hereafter KLK) found the offer to relocate to lower-poverty areas reduces arrests among female youth for violent and property crimes, relative to a control group. For males the offer to relocate reduces arrests for violent crime, at least in the short run, but increases problem behaviors and property crime arrests. For the present analysis, we re-examine these data on youth and also incorporate outcome data on the arrests of adults, who in the general population are responsible for most of the crimes committed in the U.S. each year.

We use data from MTO to determine the degree to which variation across neighborhoods in criminal behavior is due to the prevalence of crime in the neighborhood, as in contagion models, or to some other feature of the neighborhood. Our analysis exploits the fact that random assignment to the two MTO treatment groups produced different types of neighborhood changes across the five demonstration sites. We use site-treatment interactions as instrumental variables for specific neighborhood attributes, which exploits the fact that differences across sites in the effect of treatment assignment on different neighborhood characteristics are not perfectly correlated. That is, treatment assignment in some sites has an unusually large effect on census tract poverty rates or percent minority while in other sites treatment assignment yields the most pronounced changes in neighborhood crime rates. As a result we can use treatment assignment interacted with site indicators as instruments for both a measure of neighborhood crime and some measure of neighborhood socio-demographic composition, such as economic or racial integration.

Our results are not consistent with the idea that contagion (or equivalently social multipliers or endogenous peer effects) provide a good explanation for across-neighborhood variation in crime rates. Like Glaeser, Sacerdote and Scheinkman (1996) we find no evidence of contagion for the most serious (violent) crimes.[4] When we use MTO assignment interacted with demonstration site as instruments we find no relationship between beat-level violent crime rates and the rate at which MTO participants are arrested for violent crime. This finding holds for our full sample of MTO youth and adults as well as for sub-groups defined by gender and age, and also holds when we simultaneously instrument for either census tract poverty rates or the fraction of tract residents who are racial or ethnic minorities. Our results suggest that neighborhood class

[4] While Glaeser, Sacerdote and Scheinkman (2003) find some evidence for a social multiplier even for homicides, these findings are at least still consistent with the general pattern of our own and Glaeser, Sacerdote and Scheinkman (1996), suggesting that such multipliers are smaller in magnitude for more serious compared to less serious crimes.

segregation may play a more important role in understanding variation across communities in violent crime than many people have believed.

We also find no evidence for contagion for less serious (property) offenses for the full MTO sample, although for young males our results are consistent with a particular type of contagion model, namely a specific version of the "resource swamping" model. In these models, police resources within an area are assumed to be fixed, at least in the short term, so that involvement in crime by others within the community reduces the average police resources available to investigate each crime, which in turn reduces the probability of arrest and so may thereby increase the individual's propensity to engage in crime through standard deterrence arguments. The remainder of the paper is organized as follows. The next section provides a very brief review of the theoretical literature on neighborhood effects, with an emphasis on the various contagion or endogenous-effects models that have been discussed in the literature. Section 3 describes our data. Section 4 discusses our empirical approach. Section 5 presents our results. Section 6 discusses the limitations of our analysis as well as policy implications.

II. Theory

Jencks and Mayer (1990) discuss three classes of models that predict moving out of high poverty neighborhoods should reduce participants' involvement in crime – epidemic (contagion) models, collective socialization models, and institutional models. These three models correspond to Manski's (1993) categories of endogenous effects, contextual effects and correlated effects.

Contagion models (or endogenous effects) emphasize the tendency of "like to beget like," and focus on the role of social interactions in affecting the returns to crime (see Sah, 1991, Glaeser, Sacerdote and Scheinkman, 1996, and Cook and Goss, 1996). One way this may occur is when the prevalence of some behavior affects an individual's preferences for engaging in that activity, as for example by changing the stigma associated with the behavior.[5] Changes in the prevalence of criminal behavior within the community may also change individual perceptions about the net returns to crime if people believe others in the community have superior information about the costs and benefits of living outside the law, as in "information cascade"

[5] For example in the 1980s, when "Madison" was the 539th most popular girl's name in the U.S., most parents who wished to avoid complicated or awkward introductions at the local playground chose more common names. Of course the non-comformists who named their daughters Madison in the 1980s presumably would have been less likely to do so if their children had been born instead in 2001, when Madison was the second-most-popular girl's name in the country (Postrel 2002).

models (Cook and Goss, 1996). A related model that emphasizes the role of information is "modeling," in which behavior by others causes individuals to consider activities that would otherwise not have occurred to him or her.[6] A third possibility is that the actual payoffs to crime change with its prevalence. For example if police resources within a given community are relatively fixed, at least in the short run, an increase in criminal behavior by one's neighbors depress the chance that the marginal criminal will be apprehended because the increase in crime reduces the police-hours available per crime ("resource swamping") (Cook and Goss, 1996, Schrag and Scotchmer, 1997).

Collective socialization arguments (or contextual effects) focus on the influence of local adults as role models for the returns to pro-social behaviors such as work (Wilson 1987, Ludwig, 1999) or as enforcers of pro-social norms. 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.

Institutional models (correlated effects) focus on variation across neighborhoods in the quality of local institutions rather than in the characteristics or behavior of neighborhood residents. The opportunity costs of criminal activity should be relatively greater in areas with better public schools (Lochner and Moretti, 2004) and with access to public transportation. The probability of punishment would also be expected to be higher in more affluent communities that can afford more or better policing (Levitt, 1997, 2002; Sherman, 2002).[7]

While the three types of models discussed above predict that moving from a high-crime, highly disadvantaged community to an area with less crime and poverty should reduce the propensity to engage in crime, the reverse effect is in principle also possible. The relative deprivation model discussed by Jencks and Mayer (1990) focuses on the psychological reaction of people to being surrounded by higher-achieving, more affluent peers. Some MTO adults may feel resentful or anxious as a result.[8] Moves from more to less disadvantaged communities

---

[6] For example after a "human barbeque" stunt on MTV's extreme-stunt show Jackass, newspapers were filled with stories of young men who voluntarily had themselves set on fire (and in some cases videotaped) by their friends; see for example San Antonio Express-News (2002) and Taylor (2002a, 2002b).

[7] 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.

[8] Some evidence to support this possibility comes from Costa and Kahn's (2001) finding that the decline in social capital produced outside of the home in the U.S. may be due to increased community heterogeneity, particularly with respect to income, which may be due to the preferences of people to socialize with others who are like them.

could also increase property crime because of the increased value of potential “loot” (Ehrlich, 1981, 1996; Cook, 1986). More generally the economic model of the “market” for crime suggests that MTO families may have a comparative advantage in their new, less disadvantaged neighborhood in criminal rather than pro-social behavior as a means of securing resources, respect or resolving disputes.

III. Data

Our analysis focuses on all adults who were part of MTO households at baseline, as well as baseline youth who were ages 15 to 25 at the end of 2001 (which is the analytic sample used in KLK). Because every applicant was required to fill out a baseline survey when they applied to MTO, we have basic socio-demographic information for everyone in our analytic sample, as well as baseline information about the household itself such as total income and welfare receipt. Our outcome measures come from two sources: administrative arrest records, which are available for all MTO adults and capture all arrests through the end of 2001; and follow-up surveys conducted in 2002, which are available primarily for a random sample of MTO youth and, by virtue of the sampling scheme, most MTO female adults.

Table 1 presents basic characteristics for male and female adults and youth. Almost all program participants are members of racial or ethnic minorities, and most households were receiving AFDC at baseline. About three-quarters of households reported that getting away from gangs and drugs was one of the two most important reasons for volunteering for MTO, while around half report that better schools was one of the top two reasons they signed up for the program. These baseline characteristics are balanced across MTO treatment groups, as we would expect with random assignment. However for a given MTO treatment group, note that the baseline characteristics for male adults in MTO differ somewhat from those of female adults or youth because of differences by city and race / ethnic group in the propensity of women to be married or cohabit with an adult male. In principle this could complicate our effort to examine the influence of specific neighborhood attributes using treatment-demonstration site interactions, although as we show below our results are not sensitive to the uneven distribution of adult males across demonstration cities.

For females assigned to the experimental group, the MTO “take-up” rates (use of the offered voucher, or compliance rate) were 48 percent for adults and 46 percent for youth. For male adults and youth, the take-up rates were 40 and 42 percent, respectively. Among females in the Section 8 group, the MTO relocation rates were 62 and 60 percent for adults and youth. The take-up rates for males were 53 percent for adults and 54 percent for youth. Leasing up through MTO is complicated in part because many private-market apartments are not affordable under HUD's voucher payment standards and some landlords may be reluctant to accept vouchers. Families also have a limited time (usually no more than half a year) to use their housing vouchers from the time that they are issued. And in the case of the experimental group, the requirement that families move to low poverty tracts constrains their housing choice set.

Follow-up surveys conducted during 2002 were completed by one adult per household from a total of 4,248 MTO households, as well as with 1,807 youth ages 15-20 from the MTO households. The adult surveys gave priority to interviewing the female head of household identified at baseline, then to interviewing the wife of the head of household at baseline, then to interviewing male household heads. In practice over 98% of completed surveys were with female adults. The overall effective response rate for the adult survey was 90%,[9] and was equal to 88% for the youth survey. For both adults and youth the survey response rates are quite similar across MTO treatment groups. The youth but not adult surveys include questions about risky and delinquent behavior, although both surveys capture a variety of other non-market behaviors that are relevant for understanding the potential mechanisms through which MTO affects adult crime.

Our main source of outcome data for the present study comes from administrative arrest records obtained from government criminal justice agencies. We attempted to match all MTO adults and youth to their official arrest histories using information such as name, race, sex, date of birth, and social security number. We successfully obtained arrest data from criminal justice agencies in the states of each of the five MTO sites – California, Illinois, Maryland, Massachusetts and New York – as well as from 15 other states to which MTO participants had moved. Overall, we have complete arrest histories for around 95% of MTO participants. As seen in the final row of Table 1, this "administrative data response rate" is quite similar across MTO groups. (We exclude from our sample the small share of observations for which we are missing arrest data).

[9] An initial interviewing phase from January to June of 2002 yielded an 80% response rate. At that point, we drew a 3-in-10 sub-sample of the remaining cases in order to concentrate our resources on interviewing these hard-to-find families, and interviewed 48% of this selected group. We calculate the effective response rate as 80 + (1 - .8)*48 = 89.6.

The administrative arrest histories include information on the date of all arrests, each criminal charge for which the individual was arrested, and in most cases information on the disposition of each charge as well. Because these are lifetime arrest histories we are able to construct measures of arrest experiences both before and after random assignment, and examine how neighborhood effects change with time since randomization. The detailed charge information in these records enables us to focus on different types of criminal activity. Of particular importance is the distinction between property and other offenses, given that variation across communities in the availability of "loot" should be more relevant for the former.[10]

Table 1 also shows that there are no statistically significant differences across MTO groups in the fraction of male or female adults or youth who have ever been arrested prior to random assignment. These results together with those presented here and elsewhere for other baseline characteristics and MTO samples suggest that random assignment was in fact random (Kling et al., 2004). Table 1 also shows that the fraction of adults who had ever been arrested prior to MTO random assignment is markedly higher for males than females.[11]

A sense for the "criminal careers" of males and females in the MTO control group are shown in Figure 1, using a "synthetic cohort" approach that compares arrest rates for people of different ages using data for 2000 and 2001. As with previous research (e.g., Tracy, Wolfgang and Figlio, 1990), we find that offending rates typically peak during late adolescence, and at any given age are higher for men than women.

IV. Empirical Methods

A key issue in the study of the impact of residential location on individual outcomes is the selection problem arising from the likely systematic sorting of individuals among neighborhoods on the basis of important (unobserved) determinants of socioeconomic outcomes. To identify the causal effect of residential location on an outcome of interest, we must compare people living in different locations who would have experienced the same outcome, at least on average, if they

[10] We exclude motor vehicle violations from the data in states where such offenses are included in arrest histories.

[11] As an aside, the pre-baseline arrest data also provide some evidence for positive assortative mating, consistent with what we would expect in an efficient marriage market (Becker, 1991): Among those women who are living with an adult male at baseline, those with a prior criminal record are more than twice as likely as other women to be living with a man who has a criminal record. Overall women with prior criminal records are less than half as likely as other women to have an adult male in the home at baseline, perhaps because many criminally-involved men are taken out of the marriage market by imprisonment or premature mortality.

had lived in the same location. Since people cannot be located in two places at once, this comparison necessarily involves a counterfactual that cannot be directly observed.

We use the random assignment of families to different treatment groups in MTO to examine how individual criminal behavior responds to changes in neighborhood crime rates and other characteristics. Our analysis builds on the approach of Liebman, Katz and Kling (2004; hereafter LKK), who developed a method for examining the effects of neighborhood attributes by exploiting variation across MTO sites in the effects of both the experimental and Section 8 treatments on neighborhood characteristics. With this approach a socio-economic measure of the local area (W) such as the Census tract poverty rate is viewed as a summary index for a bundle of neighborhood characteristics that are changed as a result of MTO. Interactions between treatment group assignments (Z) and site indicators (S) are used as instrumental variables to isolate the experimentally-induced variation in W across sites and groups, as in equation (1), where the main site effects are subsumed in a set of baseline characteristics (X).[12] All regressions use sample weights.[13] The second-stage estimates in equation (2) using $Z*S$ interactions as excluded instruments show how the effects on neighborhood characteristics in the MTO sample are related to treatment effects on outcomes (Y).

(1) $W = Z*S\pi_1 + X\beta_1 + \varepsilon_1$

(2) $Y = W\gamma_2 + X\beta_2 + \varepsilon_2$

Our analysis differs from LKK in two important respects. First, we focus on criminal behavior, which for a variety of theoretical reasons may be more "contagious" than the behaviors

[12] We control for a set of individual and household characteristics taken from the MTO baseline surveys in order to account for residual variation in our arrest outcome measures and so improve the precision of our key parameter estimates of interest. Excluding these baseline measures from our specification has little effect on our point estimates, but causes our standard errors to increase slightly. A full description of our baseline characteristics is provided in Kling, Ludwig and Katz (2005), Appendix Table 3.

[13] The weights have three components, described in detail in Orr et al. (2003), Appendix B. For administrative data analyses, the first two components are set to one since they are only relevant to surveys. First, survey subsample members receive greater weight since, in addition to themselves, they represent individuals whom we did not attempt to contact during the subsampling phase. Second, surveyed youth from large families receive greater weight since we randomly sampled two children per household implying that youth from large families are representative of a larger fraction of the study population. Third, all individuals in both survey and administrative data analyses are weighted by the inverse of their probability of assignment to their experimental group to account for changes in the random assignment ratios over time. The ratio of individuals randomly assigned to treatment groups was changed during the course of the demonstration to minimize the minimum detectable effects after take-up of the vouchers turned out to be much higher than had been projected. This last component of the weights is, therefore, necessary to prevent time or cohort effects from confounding the results. Our weights imply that each random assignment period is weighted in proportion to the number of people randomly assigned in that period.

examined by LKK such as employment or mental health.[14] Second, LKK focus on estimating the effects of neighborhood poverty rates and testing for nonlinear effects of neighborhood socioeconomic composition.[15] We extend this approach to also disentangle the effects of neighborhood crime rates as well as class and race composition. That is, we can use the 10 separate treatment-site interactions to instrument for multiple neighborhood measures simultaneously. As noted above, the literature on neighborhood effects suggests that each of these measures may have conceptually distinct effects on criminal behavior. The key empirical prediction from contagion models is that neighborhood crime rates should be positively related to individual criminal offending, even after conditioning on neighborhood poverty or race composition.

Note that because MTO engenders change in many neighborhood characteristics simultaneously, these instrumental variables estimates cannot be interpreted literally as the effects of changing a given neighborhood characteristic on youth behavior. We expect neighborhood crime rates to capture any contagion mechanisms that may operate on individual criminal behavior plus whatever other neighborhood attributes influence crime and are correlated with neighborhood crime rates. Our ability to simultaneously condition on neighborhood poverty or racial composition should help account for other criminogenic neighborhood attributes, particularly since, as we demonstrate below, neighborhood poverty rates appear to be highly correlated with a number of institutional and social factors that existing theories predict should influence criminal behavior.

V. Results

The logic of our analysis is as follows. First, we demonstrate that there were overall program effects of MTO voucher offers on both individual arrest outcomes and neighborhood (Census tract or police beat) variables that might lead to those outcomes. Second, we show that there are some neighborhood variables with effects on the outcomes when the effects of neighborhood variables are identified by site-by-treatment group interactions as excluded instruments,

[14] Some support for this possibility comes from the recent review of Sampson, Morenoff and Gannon-Rowley (2002), which argues that to date the strongest (non-experimental) evidence of neighborhood effects is for criminal behavior.
[15] LKK examine poverty rates as well as fraction college graduates, median income and share of households headed by a female. To explore non-linear effects, LKK use these site-by-treatment interactions to instrument for higher-order terms in poverty rates.

consistent with an increasing dose-response relationship of the neighborhood variable on the outcome in the MTO experiment. Third, we find that these effects hold for some neighborhood demographic variables after conditioning on a neighborhood measure of criminal behavior, such as in analysis of violent crime arrests when using tract percentage minority and neighborhood violent crime rates as endogenous right hand side variables and ten site-by-treatment indicators as instruments. Fourth, we show that the site-by-treatment group effects of beat violent crime on individual violent crime arrests and beat property crime on individual property crime arrests are negative for females and positive, relatively small in magnitude, and statistically insignificant for males. Fifth, for females we find this lack of a strong positive effect holds after conditioning on neighborhood demographics.

To summarize our findings for the full sample (pooling female and males together), if there had been a large endogenous social effect of neighborhood crime on individual arrests for crimes, we would have expected to have found large coefficients on beat crime variables.[16] The beat crime variables, however, were not positive on their own or after conditioning on neighborhood demographic characteristics when using site-by-treatment interactions as excluded instruments -- suggesting that an endogenous/contagion effect is not simply being offset by a third factor. We believe the lack of association between neighborhood crime and individual arrests is quite informative. Although not conclusive, we interpret the pattern of results as generally indicating that there are aspects of residential neighborhoods that affect crime, particularly neighborhood racial segregation, but that neighborhood crime itself is not a key aspect. That is, we little evidence that crime is contagious for our full sample. The remainder of this section describes our five main findings in detail.

MTO Effects on Neighborhood Environments and Arrests. 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. As shown in Table 2, the mean of duration-weighted averages of poverty rates since random assignment for the control group were greater than 40 percent for each gender and age group. Despite the fact that a large share of families in both the experimental and Section 8 groups do not move through MTO, Table 2 shows that assignment to either of these treatment

---

[16] Of course, such a finding would not have proved the existence of an endogenous social effect, since the beat crime variable could also have been serving as a proxy for an omitted contextual variable.

groups still produced significant changes in the average Census tract characteristics, where the addresses of MTO participants were obtained over time through a variety of active and passive tracking methods (Goering et al 1999). MTO had more pronounced effects on economic than racial residential integration. In principle neighborhood mobility under MTO could differ by gender and age if household composition affects mobility outcomes, but Table 2 shows that in general neighborhood characteristics within MTO treatment groups do not appear to vary much by gender or age.

Table 2 also shows average crime rates for the police beats in which MTO families resided since random assignment. These findings come from local-area crime and population data for the years 1994 through 2001, using the FBI Part I Index offenses for which consistent data are available across areas.[17] The crime types used to construct our neighborhood violent and property crime rates are the same as those used to define the violent and property arrest outcome measures for MTO participants.[18] 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.[19] 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.[20] As demonstrated in Table 2, the available resolution is sufficient to pick up the large effects of MTO voucher offers on crime rates measured at the beat level. Random assignment to

[17] 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. 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.

[18] The violent crime rate includes murder, rape, robbery, and aggravated assault. The property crime rate includes burglary, motor vehicle theft, and larceny. The only distinctions between the neighborhood crime rate measures and the individual arrest outcomes for MTO participants is that our arrest data do not allow us to distinguish between aggravated and simple assaults, and so we count arrests for all assaults as violent-crime arrests, and also do not allow us to distinguish between grand and petite larceny

[19] 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.

[20] 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. 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. 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. These figures 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.

the experimental or Section 8 group resulted in substantial reductions in beat violent crime rates relative to the control group. The effects of voucher offers on property crime rates were approximately the same absolute magnitude (over 30 crimes per 10,000 residents), but smaller relative to the larger base rate of property crimes than violent crimes in the control group beats and with larger p-values on the between group differences.[21]

The average number of arrests since random assignment -- for total (including non-violent non-property) arrests, arrests for violent crimes, and arrests for property crimes -- is also shown in Table 2. Notable differences between groups are in the reduction in violent crime arrests among female youth for the Section 8 group relative to controls, and the increase in property crime arrests among male youth in the experimental group relative to controls.[22]

Effects of Neighborhoods on Arrests of Individuals. Given that there were some program effects -- large effects of the voucher offers on local-area (tract and beat) characteristics and also effects on individual arrest outcomes for some groups -- we next examine the extent to which the pattern of effects across the MTO sites and treatment groups showed an association of effects on a local-area variable with effects on arrest outcomes of MTO individuals, using the instrumental variables method outlined in section IV. In a model where the only baseline covariates are site indicators, two stage least squares estimation of equation (2) with one endogenous variable reduces the data to fifteen group means (three randomly assigned groups at each of the five sites) normalized so that the overall mean for each site is zero, and then calculates the slope of the relationship between the normalized site-by-group means of the outcome and the normalized site-by-group means of the endogenous variable.[23] Although we use a larger set of covariates than just site indicators, they are approximately orthogonal to the treatment indicators conditional on site, and the same essential intuition holds. Assuming no other confounders, this method assesses the dose-response relationship, and estimates how the magnitude of the dose (such as the effect on beat violent crime rates of the experimental or Section 8 voucher at a particular site) is associated with the response (such as the effect on the total number of violent crime arrests for MTO individuals in the experimental or Section 8 voucher groups at that site).

[21] The differences between the both voucher groups and the control group had p-values less than five percent for female youth and adults, but not for males.
[22] Simple differences in means are the basis of the statistical tests reported in Table 2. Regression-adjustment of the intent-to-treat effects using baseline covariates increases the precision of these results, as reported in KLK.
[23] For a graphical representation of fifteen group means and associated slope relating tract poverty rates to female youth outcomes in MTO, see Liebman, Katz, and Kling (2004).

The results for tract percentage minority are given in panel A of Table 3. The first row shows results of estimation of equation (2) in which right-hand side endogenous variable (*W*) is tract percentage minority, and each column shows the estimates for the minority variable from a separate 2SLS estimation on the sample for that column. All endogenous variables in Table 3 are scaled in standard deviation units to facilitate comparison across panels, with the standard deviation values themselves given in the notes to the table. The first column pools all observations, including both genders and all age ranges. The coefficient shows that a one standard deviation unit decrease in tract percentage minority, which is equivalent to a change from 90 percent minority to 73 percent minority, is associated with a decrease of .067 violent crime arrests per person since random assignment -- which is a relative change of 33% from a benchmark such as the control mean. This effect is of large substantive magnitude and is statistically significant for the full sample and for subsamples of female youth and adults as shown in the second and fourth columns, although not for male youth. The results are of similar magnitude for males.

The finding that neighborhood racial composition has a strong relationship with violent crime arrests is robust to conditioning on changes in neighborhood poverty, violent-crime rates, or property-crime rates, also shown in panel A. Although we have ten excluded instruments, the neighborhood variables are somewhat collinear and accounting for two right hand side endogenous variables rather than one increases our standard errors by 15, 39, and 55 percent after conditioning on poverty, violent-crime, or property-crime respectively. We use no more than two endogenous variables in any of our instrumental variable estimations in order to avoid severe multicollinearity.

In contrast to the strong association with neighborhood racial composition, individual arrest outcomes do not have a consistent pattern of association with neighborhood poverty rates. Panel B of Table 3 shows large and significant effects only for female youth. Moreover, the coefficient falls dramatically when racial composition is also included as a second endogenous variable, indicating that racial composition is more potent explanatory factor.

Although there is evidence in panel A that some neighborhood conditions such as percentage minority are positively associated with individual arrest outcomes for violent crimes, we find no evidence for contagion in violent arrests among MTO participants, as shown in panel C of Table 3. Contagion models predict a positive relationship between neighborhood violent crime rates and the propensity of MTO participants to engage in violent crime. For the full sample, in the first row and first column of panel C, the results indicate that participants in sites where treatment assignment leads to relatively greater declines in beat-level violent crime rates do not experience relatively greater declines in their own violent-crime arrest rates. Our point estimate for the effect of local-area violent crime rates on violent arrests for the full MTO sample is small both absolutely and in relation to the standard error. Nor are the point estimates close to statistical significance for any of the sub-groups that we examine.

We find little evidence that violent crime is contagious for MTO families even after conditioning on census tract poverty rates or racial composition, with results shown in the remaining rows of panel C. One concern with the results presented in the first row of panel C is that MTO moves induce changes in other neighborhood attributes as well, which could confound our effort to isolate the effects of contagion on violent criminal behavior. For example, Table 2 showed that MTO led families to move to neighborhoods with fewer poor or minority families as well as lower crime rates. If program participants increase their involvement with violent crime in response to social isolation (being around fewer economically or demographically similar residents) then the results using a single endogenous variable would confound these negative reactions to MTO moves with any decline in violent crime that arises from now living in a neighborhood where violent crime is less prevalent. If anything, the results run counter to this hypothesis, as the point estimates for the effect of beat violent crime conditional on tract percentage minority are actually negative in sign (and statistically significant for females) as opposed to the positive prediction. Overall, the results for beat level violent crime show no evidence of substantial positive coefficients that would have been consistent with an important influence of contagion for violent-crime arrests.

To the extent that neighborhood demographic factors are associated with individual property crime arrests, the signs of the estimates in panels A and B of Table 4 indicate that a lower percentage minority or poverty is associated with more arrests -- which are consistent with the increase in property crime arrests for male youth seen in Table 2. We do find some evidence of a positive relationship between neighborhood violent crime rates (conditioning on tract percentage minority or poverty) and the rate at which young males are arrested for property crimes, as shown in panel C. Similarly, we find that male youth, in neighborhoods where violent and property crimes are relatively more common, are more likely to be arrested for drug offenses or other (non-violent, non-property, non-drug) offenses in results shown in the appendix.[24] Lower neighborhood property crime rates did lead to fewer individual property-crime arrests for males, with point estimates that are of large magnitude but that are imprecisely estimated as shown in panel D. The results in panel D provide little evidence for contagion in property-crime arrests for females or for the pooled sample as a whole.[25]

One potential concern with these results is that we may be confounding the effect of neighborhood characteristics on the probability of arrest (P) with neighborhood effects on actual criminal behavior (C), since we are relying on a measure of arrests (A) and the three factors have a mechanical relationship $A=P\times C$. If the probability of arrest is higher in low-crime, low-poverty neighborhoods, then our estimates would understate the effects of moving to a less distressed neighborhood on criminal behavior – that is, we might be understating any contagious processes at work among the MTO population. Some support for this concern comes from evidence that MTO household heads assigned to the experimental or Section 8 groups are less likely than controls to report that their neighborhoods have a problem with police not coming in response to 911 calls for service (Kling, Ludwig and Katz, 2005). This police-responsiveness measure is presumably positively correlated to some degree with the probability that a crime results in arrest.

The first row of Table 5, however, shows that our neighborhood poverty rate measure is much more strongly correlated with the survey-reported measure of police responsiveness than are any of our other neighborhood measures. Even after conditioning on neighborhood violent crime or percent minority, tract poverty rates have a statistically significant positive relationship with our proxy for policing quality while these other neighborhood characteristics do not. If our empirical test of contagion is confounded by policing intensity bias, we would expect the magnitude of our IV estimate for neighborhood violent- or property-crime rates on the violent or

---

[24] About half of the effect of beat-level crime rates on drug arrests is due to impacts on MTO young male drug dealing arrests, the other half for drug possession arrests for this group; the arrestee may or may not have been dealing with these possession arrests, since the distinction between the two charges is often a matter of the quantity of drugs in the individual's possession. We urge some caution in drawing inferences for drug and other arrests, however, since most of these are – unlike violent and property crimes – victimless (at least there is no immediate identifiable victim), and so arrest rates for such offenses across neighborhoods may be particularly susceptible to variation in law enforcement practices rather than the prevalence of actual criminal behavior.

[25] The results for the full sample do not fall within the range of estimates of the four subgroups shown in Table 4. This is driven by the inclusion of the adult male sample, which as shown in Table 1 has a much larger fraction of the sample in Boston and LA -- apparently because female adults in these sites are more likely to be married or cohabit than are women in the other sites. Pooled results for female adults, female youth, and male youth are close to a simple sample-size-weighted average of the subgroup estimates.

property crime arrest rates among MTO participants to increase (move to the right on the real number line, that is, show more evidence of contagion) when we condition on neighborhood poverty rates, which seems to be highly correlated with our measure of policing quality. Yet this does not appear to be the case for our full MTO sample or any sub-groups for violent crime (Table 4), or for any sample or sub-group for property crime excepting perhaps young males (Table 5). More generally neighborhood poverty rates also appear to be strongly correlated with most of the other institutional or social processes that theories of contextual or correlated effects predict should influence individual criminal behavior.

VI. Conclusion

Our results taken as a whole are not very consistent with the idea that contagion (or equivalently social multipliers or endogenous peer effects) provide a good explanation for across-neighborhood variation in crime rates. We find no evidence for contagion for violent crime. For property crimes we do not find strong evidence that "like begets like" (neighborhood property crimes affect property offending); for males our results are too imprecise to constitute strong evidence, although the point estimates are consistent with some contagion. Our results taken together suggest that the role of neighborhood class segregation may play a more important role in understanding variation across neighborhoods in violent crime than is currently thought.

We do find some evidence that is consistent with one specific contagion model – "resource swamping" – or at least a modified version of this model that may apply to at least young males. While violent crime by male youth is not responsive to the local violent crime rate, we do find that male youth are more likely to engage in property crime when local violent crime rates are higher.[26] The general pattern seems to be that in areas where more serious crimes are more common, young men may take advantage of the police emphasis on addressing more severe offenses by increasing their involvement with less serious types of criminal offenses. These results are consistent with previous findings that police resources affect criminal behavior (Levitt, 1997, 2002, Sherman, 2002, Cohen and Ludwig, 2003) and that juveniles are responsive just as are adults to the threat of punishment (Levitt, 1998). Under this interpretation, one implication is that spillovers may occur across crime categories, where changes in rates of more

[26] In results given in the appendix, male youth are more likely to be arrested for drug or other (non-drug, non-property, non-violent) crimes in areas where violent or property crime rates are relatively higher

serious crimes affect the prevalence of less serious offenses. Spillovers from increases in serious crimes could in principle be mitigated through commensurate increases in local police resources, without necessarily having to directly address other aspects of social interactions. Our findings are in some sense the reverse of the "broken windows" hypothesis offered by James Q. Wilson and George Kelling (1982), in which they argued that changes in less serious offenses change perceptions if disorder and law enforcement within a neighborhood and thereby affect the prevalence of more serious crime.[27]

Perhaps the most important concern with our findings is that with only 10 treatment-site interactions, we are limited in the number of neighborhood measures that can be instrumented for at once. However it is important to note that no previous study in the neighborhood effects literature has had a source of truly credible identifying information for a *single* measure of neighborhood environment, much less been able to instrument for multiple neighborhood attributes simultaneously. MTO's random-assignment experimental design provides us with one of the first opportunities to explore the relationship between different specific neighborhood measures and criminal behavior without the self-selection concern that plagues most previous studies on this topic.

We interpret the specific neighborhood measures in our study as proxies for the bundle of neighborhood attributes that may affect individual behavior. The instrumental variables analysis in Table 5 relating local-area characteristics to self-reports of neighborhood environment shows that tract poverty seems to be the best proxy for the constellation of neighborhood attributes that contextual effect and correlated effect theories predict should affect behavior, including measures of policing quality, neighborhood disorder, "collective efficacy," and access to employment opportunities. Yet conditional on tract poverty (or tract percent minority, for that matter), we find no evidence for "contagion" for violent crime – that is, we do not find a positive relationship between beat-level crime and the rate at which MTO participants are arrested for violent crime. For contagion to be at work for violent crime, our beat-level crime rates would need to be *negatively* correlated with other neighborhood-level "criminogenic" factors (those that increase people's predisposition to commit crime), which is rather counter-intuitive – criminogenic factors would need to be more scarce in higher-crime communities. And even here,

[27] While Kelling and Sousa (2001) and Corman and Mocan (2002) present evidence from New York City data that they claim provide empirical support for broken windows, Harcourt and Ludwig (2005) re-analyze these data and find little support for the hypothesis.

bias could only come from unmeasured neighborhood attributes that are both criminogenic and uncorrelated with tract poverty or percent minority, since our results are robust to instrumenting for these measures in the same specification.

A final question with our estimates has to do with generalizability, since MTO participants represent a self-selected set of public housing residents who volunteered to join the program. We might expect that the eligible public housing families who signed up for MTO would be those who expected to benefit the most from moving. Far and away the most important reason MTO that families signed up for the program is to escape from gangs and drugs. There are in short reasons to believe that if anything people participating in the MTO demonstration may be above-average in their behavioral sensitivity to changes in neighborhood environment.

In sum, our findings on the whole do not support the idea that contagion explains the variation across neighborhoods in violent-crime rates, nor with the idea that contagion is of first-order importance in explaining variation in less-serious crimes at least for females (as our results about property crime contagion for males are inconclusive). The finding that contagion might be somewhat more important for less-serious than more-serious crimes – that is, a "contagion gradient" – is consistent with the results reported by Glaeser, Sacerdote and Scheinkman (1996, 2003). To the extent to which other studies have claimed that contagion is in fact the main source of variation across neighborhoods in violent crimes the results would seem to be due instead to some combination of endogenous sorting (self selection) and unmeasured aspects of neighborhood racial segregation.

REFERENCES

Anderson, David (1999). "The Aggregate Burden of Crime," *Journal of Law and Economics*, 42, 611-642.

Becker, Gary S. (1991). *A Treatise on the Family*, Cambridge, MA: Harvard University Press.

Chicago Police Department (2003). *Annual Report, 2002: Year in Review*, Chicago: Chicago Police Department.

Cohen, Jacqueline and Jens Ludwig (2003). "Policing Crime Guns," in (Jens Ludwig and Philip J. Cook, eds.) *Evaluating Gun Policy*, Washington, DC: Brookings Institution Press, 217-250.

Cook, Philip J. (1986). "The Demand and Supply of Criminal Opportunities," in (M. Tonry and N. Morris, eds.) *Crime and Justice: An Annual Review of Research*, Chicago: University of Chicago Press, 1-27.

Cook, Philip J. and Kristin A. Goss, (1996). "A Selective Review of the Social-Contagion Literature," Working paper, Sanford Institute of Policy Studies, Duke University.

Corman, Hope and Naci Mocan (2002). "Carrots, Sticks and Broken Windows," Cambridge, MA: NBER Working Paper 9061.

Costa, Dora L., and Matthew Kahn (2003). "Understanding the Decline in American Social Capital 1952-1998," *Kyklos*, 56(1), 17-46.

Crane, Jonathan (1991). "The Epidemic Theory of Ghettos and Neighborhood Effects on Dropping Out and Teenage Childbearing," *American Journal of Sociology* 96, 1226-1259.

Ehrlich, Isaac, (1981). "On the Usefulness of Controlling Individuals: An Economic Analysis of Rehabilitation, Incapacitation and Deterrence," *American Economic Review*, 71, 307-322.

Ehrlich, Isaac, (1996). "Crime, Punishment and the Market for Offenses," *Journal of Economic Perspectives*, 10, 43-68.

Glaeser, Edward L., Bruce Sacerdote, and Jose A. Scheinkman, (1996). "Crime and Social Interactions." *Quarterly Journal of Economics*, 111, 507-548.

Glaeser, Edward L., Bruce Sacerdote, and Jose A. Scheinkman, (2003) "The Social Multiplier," *Journal of the European Economic Association*, 1, 345-353.

Goering, John, Joan Kraft, Judith Feins, Debra McInnis, Mary Joel Holin, and Huda Elhassan, (1999). *Moving to Opportunity for Fair Housing Demonstration Program: Current Status and Initial Findings*, Washington, DC: U.S. Department of Housing and Urban Development.

Harcourt, Bernard E. and Jens Ludwig (2005). "Broken Windows: New Evidence from New York City and a Five-City Social Experiment." Working Paper, University of Chicago Law School.

Jencks, Christopher and Susan E. Mayer, (1990). "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.

Kelling, George L. and William H. Sousa (2001). *Do Police Matter? An Analysis of New York City's Police Reforms*. New York: Manhattan Institute.

Kling, Jeffrey R., Jeffrey B. Liebman., Lawrence F. Katz., and Lisa Sanbonmatsu (2004). "Moving to Opportunity and Tranquility: Neighborhood Effects on Adult Economic Self-sufficiency and Health from a Randomized Housing Voucher Experiment." Princeton IRS Working Paper 481.

Kling, Jeffrey R., Jens Ludwig, and Lawrence F. Katz (2005). "Neighborhood Effects on Crime for Female and Male Youth: Evidence from a Randomized Housing Voucher Experiment," *Quarterly Journal of Economics,* 120(1), 87-130.

Levitt, Steven D. (1997). "Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime," *American Economic Review*, 87, 270-290.

Levitt, Steven D. (1998). "Juvenile Crime and Punishment." *Journal of Political Economy* 106, 1156-1185.

Levitt, Steven D. (2002). "Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime: A Reply," *American Economic Review*, 92, 1244-1250.

Liebman, Jeffrey B., Lawrence F. Katz., and Jeffrey R. Kling (2004). "Beyond Treatment Effects: Estimating the Relationship between Neighborhood Poverty and Individual Outcomes in the MTO Experiment," Princeton IRS Working Paper 493.

Lochner, Lance and Enrico Moretti (2004). "The Effect of Education on Crime: Evidence from Prison Inmates, Arrests, and Self-Reports," *American Economic Review*, 94, 155-189.

Ludwig, Jens, (1999). "Information and Inner-City Educational Attainment," *Economics of Education Review*, 18, 17-30.

Maltz, Michael D. (1999). "Bridging Gaps in Police Crime Data, NCJ 176365," Washington, DC: Bureau of Justice Statistics.

Manski, Charles F. (1993). "Identification of Endogenous Social Effects: The Reflection Problem." *Review of Economic Studies*, 60, 531-542.

Manski, Charles F. (2000). "Economic Analysis of Social Interactions," *Journal of Economic Perspectives*, 14(3), 115-136.

Olsen, Edgar O. (2003). "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.

Orr, Larry, Judith D. Feins, Robin Jacob, Erik Beecroft, Lisa Sanbonmatsu, Lawrence F. Katz, Jeffrey B. Liebman, and Jeffrey R. Kling (2003). *Moving to Opportunity: Interim Impacts Evaluation*, Washington, DC: U.S. Department of Housing and Urban Development, Office of Policy Development and Research.

Postrel, Virginia (2002). "Economic Scene: How can the Marketplace Gauge Fashion? Consider What to name the Baby," *New York Times*, May 23, C2.

Sah, Raaj K. (2001). "Social Osmosis and Patterns of Crime," *Journal of Political Economy*, 99, 1272-1295.

Sampson, Robert J., Stephen W. Raudenbush, and Felton Earls, (1997). "Neighborhoods and Violent Crime: A Multilevel Study of Collective Efficacy," *Science*, 277, 918-924.

Sampson, Robert J., Jeffrey D. Morenoff, and Thomas Gannon-Rowley (2002). "Assessing 'Neighborhood Effects': Social Processes and New Directions in Research," *Annual Review of Sociology*, 28, 443-478.

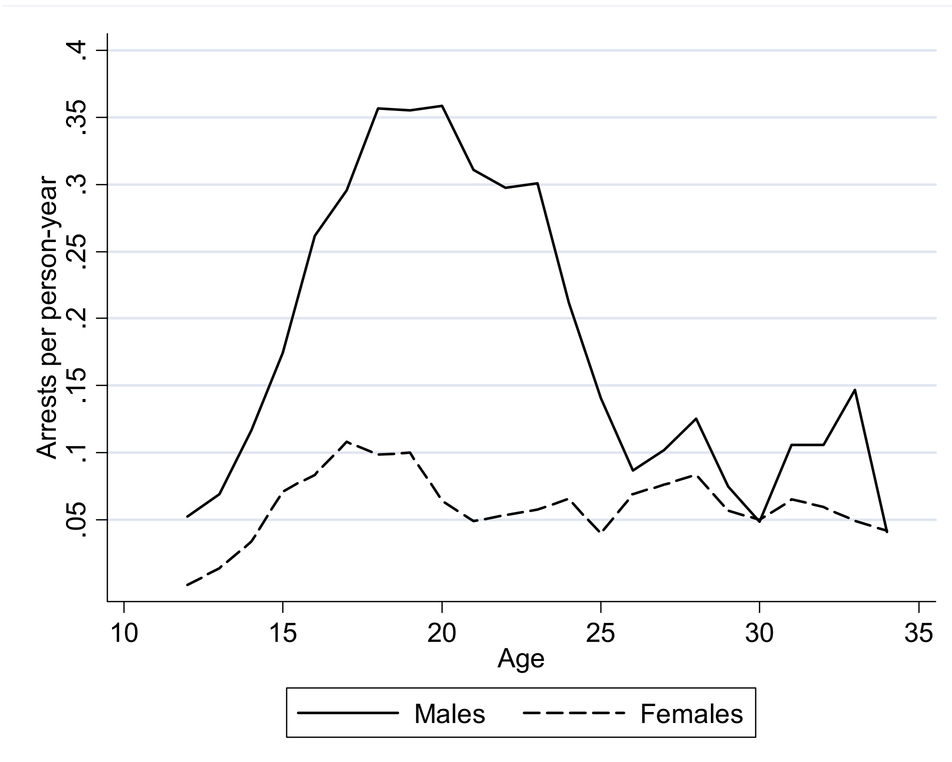
San Antonio Express-News (2002). "News Roundup," *San Antonio Express-News*, October 5, B2.

Schrag, Joel and Suzanne Scotchmer (1997) "The Self-Reinforcing Nature of Crime." *International Review of Law and Economics*. 17: 325-335.

Sherman, Lawrence W. (2002). "Fair and Effective Policing," in (James Q. Wilson and Joan Petersilia, eds.) *Crime: Public Policies for Crime Control*, Oakland, CA: Institute for Contemporary Studies Press, 383-412

Taylor, Marisa (2002). “Four Plead Guilty in Mess-Hall Mess,” *San Diego Union-Tribune*, May 11, B7.
Taylor, Marisa (2002). “Boy Seriously Hurt in Fire Stunt,” *The Guardian (London)*, January 9, 8.
Tracy, Paul E., Marvin E. Wolfgang and Robert M. Figlio (1990). *Delinquency Careers in Two Birth Cohorts*, New York: Plenum Press.
Wilson, James Q. and George Kelling (1982). “Broken Windows: The Police and Neighborhood Safety.” *The Atlantic Monthly*, March, 1-11.
Wilson, William Julius (1987). *The Truly Disadvantaged,* Chicago: University of Chicago Press.

Figure 1: Age-crime curves for MTO Control Group



Notes. Figure 1 presents 3-year moving averages of arrest rates by age, pooling data from 2000 and 2001. Arrest rates below the age of majority may be underestimated due to superior coverage for juvenile arrest records among those MTO participants who were youth at baseline rather than adults.

Table 1
Baseline Descriptive Statistics for MTO Adult and Youth Samples

	Females			Males		
	Experimental	Section 8	Control	Experimental	Section 8	Control
ADULTS						
Black	.650	.646	.657	.359	.364	.386
Hispanic	.294	.297	.298	.505	.494	.487
MTO site:						
Baltimore	.150	.162	.147	.039	.071	.051
Boston	.229	.223	.221	.211	.192	.287
Chicago	.209	.209	.210	.149	.128	.131
LA	.155	.149	.158	.304	.351	.345
NYC	.257	.257	.264	.297*	.259	.185
HH on AFDC at baseline	.739	.752	.756	.579	.586	.491
Moved because:						
Drugs, crime	.767	.755	.783	.739	.755	.764
Schools	.468	.521*	.465	.469	.577	.489
Age at end of 2001	39.0	39.4	39.1	43.0	43.4	44.8
Any pre-RA arrest	.258	.231	.260	.375	.423	.354
Missing admin arrest data	.038	.054	.035	.056	.048	.057
N	1,483	1,013	1,102	224	153	166
YOUTH						
Black	.647	.606	.640	.609	.605	.612
Hispanic	.296	.318	.304	.329	.333	.339
MTO site:						
Baltimore	.168	.138	.140	.151	.154	.139
Boston	.187	.192	.216	.166	.200	.189
Chicago	.210	.215	.203	.220	.209	.205
LA	.165	.185	.199	.195	.189	.196
NYC	.270	.271	.242	.269	.248	.270
HH on AFDC at baseline	.732	.744	.749	.743	.706	.727
Moved because:						
Drugs, crime	.807	.732	.782	.780	.760	.791
Schools	.460	.524	.483	.511	.549	.505
Age at end of 2001	19.1	18.9	18.9	19.0	18.9	19.0
Any pre-RA arrest	.062	.041	.048	.147	.122	.131
Missing admin arrest data	.057	.048	.055	.059	.063	.061
N	966	651	716	988	691	739

Notes: * = p-value <.05 on Experimental-Control or Section 8-Control difference.

Table 2
Mobility Outcomes by MTO group, Age Group and Gender

	Females			Males		
	Experimental	Section 8	Control	Experimental	Section 8	Control
ADULTS						
Tract poverty rate	.326*	.351*	.439	.329*	.339*	.417
0-20%	.363*	.212*	.110	.333*	.235*	.121
20-40%	.266	.409*	.292	.261	.407	.320
40% plus	.371*	.379*	.598	.406*	.359*	.559
Percent tract black	.532*	.537*	.566	.389	.454	.402
Percent tract minority	.816*	.868*	.890	.833*	.887	.883
Beat violent crime rate	224.3*	228.3*	264.0	171.9	185.0	194.4
Beat property crime rate	520.2*	522.9*	561.2	403.7	465.6	440.6
Arrests since RA:						
Violent	.088	.087	.078	.171	.205	.100
Property	.145	.081	.106	.192	.163	.093
Total	.315	.159	.272	.582	.748*	.378
YOUTH						
Tract poverty rate	.335*	.356*	.444	.338*	.358*	.448
0-20%	.329*	.215*	.104	.330*	.208*	.098
20-40%	.290	.399*	.290	.274	.403*	.282
60% plus	.382*	.386*	.606	.396*	.390*	.620
Percent tract black	.536	.527	.555	.524	.531	.542
Percent tract minority	.831*	.880	.899	.831*	.875*	.903
Beat violent crime rate	223.2*	228.2*	260.1	225.4*	231.0*	260.3
Beat property crime rate	531.9	518.2	574.9	535.4	540.6	547.0
Arrests since RA:						
Violent	.155	.126*	.210	.457	.467	.447
Property	.115	.192	.145	.555*	.454	.398
Total	.398	.476	.530	2.014	1.761	1.689

Notes: Tract data are based on duration-weighted averages of tract characteristics, interpolating between and extrapolating from 1990 and 2000 Censuses. Police beat rates are crimes per 10,000 residents in the beat. * = p-value <.05 on Exp-Control or S8-Control difference.

Table 3
IV Estimates for Violent-Crime Arrests Since Random Assignment

	Full sample	Female Youth	Male Youth	Female Adults	Male adults
A. Tract percentage minority					
Minority only	.067* [.033]	.114* [.036]	.006 [.057]	.064* [.029]	.031 [.061]
Minority | Tract poverty	.115* [.051]	.108* [.053]	.002 [.084]	.152* [.043]	.091 [.069]
Minority | Beat violent crime	.110* [.046]	.163* [.045]	.015 [.068]	.131* [.040]	.057 [.065]
Minority | Beat property crime	.058 [.038]	.106* [.039]	.016 [.061]	.056 [.033]	.033 [.064]
B. Tract percentage in poverty					
Poverty only	.008 [.020]	.082* [.031]	-.012 [.050]	-.015 [.020]	-.057 [.059]
Poverty | Tract minority	-.041 [.030]	.034 [.048]	.034 [.073]	-.106* [.029]	-.08 [.060]
Poverty | Beat violent crime	.037 [.034]	.174* [.035]	-.032 [.062]	.071* [.036]	-.102 [.071]
Poverty | Beat property crime	-.005 [.021]	.084* [.042]	-.020 [.050]	-.013 [.022]	-.074 [.057]
C. Beat violent crime rate					
Violent crime only	-.016 [.070]	-.031 [.077]	.046 [.070]	-.071 [.072]	.016 [.097]
Violent crime | Tract minority	-.137 [.095]	-.173 [.100]	-.054 [.091]	-.209* [.098]	-.103 [.116]
Violent crime | Tract poverty	-.111 [.124]	-.267* [.131]	-.078 [.114]	-.243 [.128]	-.077 [.146]
D. Beat property crime rate					
Property crime only	.137 [.134]	.035 [.119]	.165 [.108]	.037 [.122]	.128 [.129]
Property crime | Tract minority	.047 [.147]	-.044 [.129]	.066 [.120]	-.065 [.134]	.025 [.139]
Property crime | Tract poverty	.148 [.144]	-.020 [.129]	.123 [.117]	-.008 [.132]	.098 [.138]

Notes: Each cell in the table contains the coefficient [with standard errors] from a separate 2SLS estimate of equation (2), with each row label describing the one or two components of W in (2). E.g., in the first row W only contains traction fraction minority; in the second row, W contains tract percentage minority and tract percentage in poverty, and the coefficient reported is for minority. Samples vary by column. Endogenous variables are all expressed in standard deviation units relative to the standard deviation in the control group for that variable. The control group standard deviations are: 17% for tract minority, 14% for tract poverty, 185 for beat violent crime, and 525 for beat property crime.

Table 4
IV Estimates for Property Crime Arrests Since Random Assignment

	Full sample	Female Youth	Male Youth	Female Adults	Male adults
Endogenous variables in s.d. units					
Minority only	-.089* [.041]	-.013 [.037]	-.117* [.057]	-.112* [.044]	-.060 [.061]
Minority | Tract poverty	-.086 [.062]	.019 [.058]	-.103 [.084]	-.125* [.060]	-.044 [.068]
Minority | Beat violent crime	-.091 [.058]	-.027 [.049]	-.177* [.068]	-.123* [.059]	-.088 [.076]
Minority | Beat property crime	-.085 [.049]	-.039 [.049]	-.109 [.063]	-.141* [.052]	-.062 [.066]
B. Tract percentage in poverty					
Poverty only	-.039 [.023]	.008 [.030]	-.070 [.051]	-.054 [.029]	-.030 [.076]
Poverty | Tract minority	-.003 [.035]	-.045 [.046]	-.020 [.079]	.021 [.037]	-.031 [.084]
Poverty | Beat violent crime	-.051 [.038]	.004 [.046]	-.191* [.061]	-.044 [.047]	-.098 [.13]
Poverty | Beat property crime	-.034 [.025]	.009 [.032]	-.073 [.051]	-.048 [.033]	-.040 [.089]
C. Beat violent crime rate					
Violent crime only	-.086 [.085]	-.055 [.085]	.061 [.081]	-.092 [.083]	.011 [.128]
Violent crime | Tract minority	.014 [.113]	.038 [.112]	.191 [.102]	.027 [.111]	.126 [.154]
Violent crime | Tract poverty	.045 [.141]	.077 [.146]	.271* [.119]	.056 [.140]	.186 [.195]
D. Beat property crime rate					
Property crime only	-.136 [.127]	-.073 [.114]	.083 [.099]	-.090 [.121]	.042 [.170]
Property crime | Tract minority	-.003 [.149]	.079 [.138]	.211 [.120]	.046 [.146]	.190 [.192]
Property crime | Tract poverty	-.061 [.134]	-.021 [.132]	.134 [.111]	-.032 [.139]	.095 [.186]

Notes: Each cell in the table contains the coefficient [with standard errors] from a separate 2SLS estimate of equation (2), with each row label describing the one or two components of W in (2). E.g., in the first row W only contains traction fraction minority; in the second row, W contains tract percentage minority and tract percentage in poverty, and the coefficient reported is for minority. Samples vary by column. Endogenous variables are all expressed in standard deviation units relative to the standard deviation in the control group for that variable and sample. The control group standard deviations are: 17% for tract minority, 14% for tract poverty, 185 for beat violent crime, and 525 for beat property crime.

Table 5
IV Estimates for Neighborhood Institutions and Social Processes

LHS variables	RHS endogenous variables, in s.d. units							
	Poverty | Minority	Poverty | Violent crime	Minority | Poverty	Minority | Violent crime	Violent crime | Poverty	Violent crime | Minority	Violent crime | Property crime	Property crime | Violent crime
Problem w/ police not coming when called	.14*	.19*	.04	.11	-.14	.35*	.62*	-.13
	[.04]	[.06]	[.06]	[.07]	[.22]	[.17]	[.22]	[.21]
Neighborhood problems index	.26*	.41*	.02	.18	-.61	.53	1.20*	-.53
	[.08]	[.13]	[.13]	[.14]	[.48]	[.32]	[.47]	[.46]
Household victimized in previous 6 months	.07*	.07	-.03	.00	-.06	.17*	.22*	-.08
	[.03]	[.04]	[.05]	[.05]	[.15]	[.10]	[.12]	[.13]
Neighborhood "collective efficacy"	-.09*	-.14*	-.11	-.17*	.02	-.18	-.55*	.19
	[.04]	[.05]	[.06]	[.06]	[.17]	[.13]	[.20]	[.20]
Treated unfairly by police b/c of race	.05*	.05*	-.02	.00	-.06	.11	.08	.05
	[.02]	[.02]	[.03]	[.03]	[.10]	[.07]	[.07]	[.09]
Treated unfairly at school / work because of race	.05*	.06*	-.09*	-.05	-.23*	.06	.06	-.11
	[.02]	[.03]	[.03]	[.03]	[.10]	[.05]	[.07]	[.08]

Notes: Each cell in the table contains the coefficient [with standard errors] from a separate 2SLS estimate of equation (2). Left-hand side variables are described by the row labels, and were taken from household head survey responses in 2002. Each column label describes the two components of W used in estimation of (2). E.g., in the first column W includes tract percentage minority and tract percentage in poverty, and the coefficient reported is for poverty. Sample is all adults surveyed, who were 98% female. Endogenous variables are all expressed in standard deviation units relative to the standard deviation in the control group. The control group standard deviations are: 14.4 for tract percentage in poverty, 17.3 for tract percentage minority, 183 for beat violent crime rate per 10,000 population, and 72 for beat property crime rate per 10,000 population. * = p-value < .05

Appendix Table A
IV Estimates for Other Crime Arrests Since Random Assignment

	Full sample	Female Youth	Male Youth	Female Adults	Male adults
Endogenous variables in s.d. units					
Minority only	.008 [.052]	.071 [.049]	-.045 [.094]	-.032 [.054]	.023 [.078]
Minority | Tract poverty	-.076 [.103]	.064 [.080]	-.308* [.146]	-.07 [.111]	.031 [.096]
Minority | Beat violent crime	-.071 [.100]	.008 [.080]	-.257* [.129]	-.07 [.099]	.008 [.103]
Minority | Beat property crime	-.02 [.055]	.005 [.058]	-.019 [.1]	-.093 [.057]	-.014 [.086]
B. Tract percentage in poverty					
Poverty only	.04 [.038]	.063 [.052]	.113 [.083]	-.014 [.037]	-.027 [.103]
Poverty | Tract minority	.073 [.069]	-.032 [.072]	.338* [.123]	.007 [.079]	-.081 [.111]
Poverty | Beat violent crime	-.01 [.055]	.079 [.065]	-.185 [.108]	.049 [.059]	.078 [.119]
Poverty | Beat property crime	.035 [.045]	.054 [.061]	.093 [.085]	-.011 [.046]	.003 [.109]
C. Beat violent crime rate					
Violent crime only	.158 [.157]	.117 [.192]	.508* [.178]	.033 [.145]	.011 [.201]
Violent crime | Tract minority	.237 [.239]	.184 [.270]	.643* [.253]	.117 [.225]	.08 [.28]
Violent crime | Tract poverty	.185 [.258]	.068 [.292]	.579* [.26]	-.001 [.237]	-.034 [.283]
D. Beat property crime rate					
Property crime only	.14 [.237]	.106 [.213]	.493* [.179]	.082 [.223]	.034 [.198]
Property crime | Tract minority	.172 [.259]	.188 [.230]	.561* [.199]	.143 [.243]	.105 [.218]
Property crime | Tract poverty	.062 [.271]	.061 [.253]	.453* [.209]	.044 [.269]	-.002 [.234]

Notes: Each cell in the table contains the coefficient [with standard errors] from a separate 2SLS estimate of equation (2), with each row label describing the one or two components of W in (2). E.g., in the first row *W* only contains traction fraction minority; in the second row, *W* contains tract percentage minority and tract percentage in poverty, and the coefficient reported is for minority. Samples vary by column. Endogenous variables are all expressed in standard deviation units relative to the standard deviation in the control group for that variable and sample. The control group standard deviations are: 17% for tract minority, 14% for tract poverty, 185 for beat violent crime, and 525 for beat property crime.