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IS CRIME CONTAGIOUS?

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ABSTRACT

Understanding whether criminal behavior is "contagious" is important for law enforcement and for policies that affect how people are sorted across social settings. We test the hypothesis that criminal behavior is contagious by using data from the Moving to Opportunity (MTO) randomized housing-mobility experiment to examine the extent to which lower local-area crime rates decrease arrest rates among individuals. Our analysis exploits the fact that the effect of treatment group assignment yields different types of neighborhood changes across the five MTO demonstration sites. We use treatment-site interactions to instrument for measures of neighborhood crime rates, poverty and racial segregation in our analysis of individual arrest outcomes. We are unable to detect evidence in support of the contagion hypothesis. Neighborhood racial segregation appears to be the most important explanation for across-neighborhood variation in arrests for violent crimes in our sample, perhaps because drug market activity is more common in high-minority neighborhoods.

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

JEL classifications: H43, I18, J23.

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I. INTRODUCTION

Crime varies dramatically across countries, states, cities and, most relevant for the present paper, neighborhoods, 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 how high-risk people are sorted across areas is relevant for government policies that affect how people are distributed across neighborhoods and schools. This question is also relevant for the optimal allocation of law enforcement resources. For example, the possibility that the prevalence of peer delinquency affects behavior in a non-linear fashion ("tipping points") has been the focus of much public discussion and, if true, could generate large differences across areas in the marginal productivity of police spending.

A large body of theoretical literature has developed to explain why social context may affect an individual's propensity to engage in crime. One possibility is that criminal behavior is "contagious." 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), perceptions about the net returns to the behavior (information), or the actual probability of arrest (constraints) (see Cook and Goss, 1996; Becker and Murphy, 2000; Manski, 1993, 2000). An alternative possibility is that criminal behavior is affected by "contextual effects" -- other attributes of neighborhood residents, including socio-economic status (SES) as in role model stories (Wilson, 1987) or the willingness of neighbors to become involved in local order maintenance, which Sampson et al. (1997) term "collective efficacy." A third possibility is "correlated effects" – policing, schools or other institutional characteristics of neighborhoods may matter for criminal behavior (Jencks and Mayer, 1990, Levitt, 1997, 2002, Sherman, 2002, Lochner and Moretti, 2004). Determining whether any of these models – or selection – explains neighborhood variation in crime is important because only with contagion are policy interventions and other external shocks amplified through "social multipliers" (Glaeser et al., 1996, 2003).

Despite the large body of theoretical literature on this question, the available empirical evidence is limited. Most previous studies of how neighborhoods influence criminal behavior are susceptible to bias from unmeasured individual attributes associated with neighborhood selection.[1] Studies that employ stronger research designs often provide stronger evidence that "like begets like" for other outcomes such as student test scores (Hoxby, 2000), investment behavior (Hong et al., 2004, 2005; Hong and Kacperczyk, 2005), and college drinking (Sacerdote, 2001; Duncan et al., 2005). Crime might be at least as "contagious" as these other outcomes if Becker and Murphy (2000, p. 4) are correct that behaviors "most subject to strong social pressures from peers and others are those that take place publicly." The "public" nature of at least some crime is suggested by high levels of group offending by youth (Zimring 1998), and certainly many assaults involving people of any age are public spectacles.

Even in the absence of the selection problem, research in this area will typically have difficulty determining which of the models described above are responsible for any observed neighborhood effects on criminal behavior (Case and Katz, 1991; Manski, 1993; Moffitt, 2001). Youth growing up in the same neighborhoods will be exposed to similar peer influences, but also to similar adult role models, schools, and policing services.

In this paper we try to empirically test whether crime is contagious by drawing on data from the Moving to Opportunity (MTO) randomized housing-mobility experiment. Sponsored by the U.S. Department of Housing and Urban Development (HUD), MTO has been in operation

[1] Glaeser et al. (1996) document excess variation in crime across areas beyond what can be explained by standard socio-demographic determinants of crime. Their results suggest social interactions are more important for less-serious than more-serious crimes. Perhaps the most famous study providing more direct evidence for social multipliers is Crane (1991). For a comprehensive review see Sampson et al. (2002).

since 1994 in five cities: Baltimore, Boston, Chicago, Los Angeles, and New York. Eligibility is restricted to low-income families with children living in public or Section 8 project-based housing in selected high-poverty census tracts.[2]

From 1994 to 1997, a total of 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 MTO services but did not lose access to social services to which they were otherwise entitled such as public housing. Because of random assignment, MTO yields three comparable groups of families living in different kinds of post-program neighborhoods.

Previous studies use MTO's experimental design to compare average arrest outcomes across the three randomly-assigned mobility groups and find mixed effects of assignment to the experimental or Section 8 groups on criminal behavior. The experimental treatment reduces arrests for violent and property crimes for female youth and reduces arrests for violent crime for male youth, at least in the short run, but increases male problem behaviors and property crime arrests. MTO has few detectable effects on adult arrests (Kling, Ludwig and Katz, 2005, Ludwig and Kling, 2005).

However, estimates for the overall effects of MTO mobility assignments are not directly informative about whether crime is contagious because MTO moves change multiple neighborhood characteristics simultaneously, which could have offsetting effects. For example,

[2] Section 8 project-based housing is essentially privately-operated public housing (Olsen 2003).
[3] Housing vouchers provide families with subsidies to live in private-market housing. MTO vouchers required residence in these tracts for a minimum of one year for renewal of the subsidy. Experimental group families were provided with mobility assistance and in some cases other counseling services as well.

“relative deprivation” models suggest people may have adverse psychological or behavioral responses to being surrounded by more affluent peers (Jencks and Mayer, 1990), a possibility with some empirical support from Luttmer (2005). Disentangling the effects of specific neighborhood attributes on behavior necessarily requires analysis that ventures beyond MTO’s basic experimental design, since comparing average arrests across MTO groups identifies the net effect of all of the neighborhood changes that are induced by treatment-group assignment.

In this paper 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 area, as suggested by 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 MTO sites. This enables us to use site-treatment interactions as instrumental variables for specific neighborhood attributes in our analysis to examine how differences by MTO site and group in treatment effects on specific neighborhood attributes relate to site-group differences in MTO effects on individual arrest outcomes. For example, assignment to the experimental rather than control group has an unusually large effect in reducing neighborhood violent crime rates for participants in the Chicago MTO site. If crime is “contagious,” we would expect the treatment-control difference in violent crime arrests to MTO participants to also be larger (more negative) in Chicago than other MTO sites.

While the experimental-control difference in neighborhood violent crime rates is largest in Chicago, experimental group assignment has the largest effect on racial segregation in the Boston site and on neighborhood poverty rates in the Los Angeles and New York sites. We can exploit the fact that differences across sites in the effect of MTO treatment assignment on different neighborhood characteristics are not perfectly correlated to simultaneously instrument for neighborhood crime plus some measure of neighborhood socio-demographic composition, such as poverty or racial integration.

Our results are not consistent with the idea that contagion explains as much of the across-neighborhood variation in violent crime rates as previous research suggests. We do not find any statistically significant evidence that MTO participants are arrested for violent crime more often in communities with higher violent crime rates. Our estimates enable us to rule out very large contagion effects, but not more modest associations. This general finding holds for our full sample of MTO youth and adults as well as for sub-groups defined by gender and age, and it also holds when we simultaneously instrument for neighborhood racial segregation or poverty rates. Our results suggest that neighborhood racial segregation may play a more important role in understanding variation across communities in violent crime, perhaps because drug market activity appears to be more common in neighborhoods that contain a large share of minority residents.

The remainder of the paper is organized as follows. The next section describes our data. Section 3 discusses our empirical approach. Section 4 presents our results. Section 5 discusses the limitations of our analysis as well as policy implications.

II. 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 (the sample used in Kling, Ludwig and Katz, 2005). We have baseline socio-demographic information for everyone in MTO plus household information such as total income and welfare receipt. Outcome measures come from two sources: follow-up surveys conducted in 2002 (about 4-7 years after random assignment), which are available primarily for a random sample of MTO youth and, by virtue of the sampling scheme, most MTO female adults; and administrative arrest records, which are available for almost everyone in MTO and capture all arrests through the end of 2001. The follow-up surveys also include reports about neighborhood social processes. Details are in the Data Appendix.

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 report that getting away from gangs and drugs was one of their top two reasons for joining MTO.

For adults assigned to the experimental group, the fraction that used the MTO voucher was equal to 48 percent for females and 40 percent for males. For adults assigned to the Section 8 group, MTO voucher use rates equaled 62 percent for females and 53 percent for males.[4] The take-up rates are similar for youth within MTO groups.

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, or for that matter in other baseline characteristics. These results, together with those presented elsewhere, suggest that random assignment was in fact random (Kling, Liebman, and Katz, 2005).[5]

Eligibility for MTO 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, in fact, in the country as a whole. As shown in Table 2, the average post-random-assignment Census tract had a poverty rate of over 40 percent for people in the control group. Assignment to an MTO treatment group produced significant changes in average Census tract characteristics,

[4] Leasing up through MTO is complicated because many apartments are not affordable under HUD's voucher payment standards and some landlords may not accept vouchers. Families also have a limited time (usually no more than half a year) to use their vouchers from when they are issued. And families assigned to the experimental group are constrained by the requirement to move to a low-poverty tract.

[5] Note that for a given MTO group, baseline characteristics for male adults 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. Our results are not sensitive to the uneven distribution of adult males across MTO sites, as shown below in part by our separate estimates for other gender-age subgroups.

although 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, tract characteristics within MTO groups do not vary much by gender or age.

Table 2 also shows the average number of crimes reported to police per 10,000 residents for the police beats in which MTO families have lived since random assignment.[6] MTO treatment-group assignment generally has more pronounced effects on violent than property crime rates within police beats. Note that the resolution provided by these beat data varies across cities: Baltimore has 9 police beats, while Boston has 11, Chicago has 279, Los Angeles 18, and New York City 76. We discuss the potential for bias from measurement error with our beat-level crime variables in detail below.

III. EMPIRICAL METHODS

A key issue in the study of neighborhood effects on individual behavior is the selection problem arising from the likely systematic sorting of people across areas on the basis of important (unobserved) determinants of behavioral outcomes. To identify the causal effect of residential location on an outcome, we must compare people living in different locations who would have experienced the same outcome, at least on average, if they 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 Kling, Liebman and Katz (2005),

[6] In some cities these administrative units are districts or areas instead of beats, although for convenience in what follows we refer to all of these areas as "beats."

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 eq. (1), where the main site effects are subsumed in a set of baseline characteristics (X).[7] All regressions use sample weights (see Orr et al., 2003). We present robust standard errors clustered at the family level to account for the fact that observations from people within the same family are not statistically independent.[8]

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

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).

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

Our analysis differs from Kling, Liebman, and Katz (2005) in two important respects. First, we focus on criminal behavior, which for a variety of theoretical reasons may be more "contagious" than behaviors such as employment or mental health (Cook and Goss, 1996).

[7] 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 to 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.

[8] In principle an alternative would be to cluster standard errors at the level of the MTO site-and-group – that is, essentially use a model with site-by-group random effects. Our IV models parameterize the site-by-group variation in outcomes to be linear in the endogenous neighborhood variable for which we instrument. Over-identification tests do not reject this hypothesis. In addition, clustering on site-and-group would leave us with just 15 clusters, which limits our ability to use standard asymptotic (i.e. large sample) theory to justify statistical inference with our standard errors (see for example Donald and Lang, 2004).

Second, Kling, Liebman, and Katz (2005) focus on estimating the effects of neighborhood poverty rates and testing for nonlinear effects.[9] We extend this approach to also disentangle the effects of beat crime rates as well as class and race composition. That is, we use the 10 treatment-site interactions to instrument for multiple neighborhood measures simultaneously. The literature on neighborhood effects suggests each of these measures may have conceptually distinct effects on criminal behavior. Contagion models predict neighborhood crime rates should be positively related to individual criminal behavior, even after controlling for tract poverty or race composition.

How much explanatory power do our instruments have in predicting variation across MTO participants in post-random assignment neighborhood characteristics? When we estimate the first-stage equation (1) using as our neighborhood measure the local-area violent crime rate, tract share minority and tract share poor in turn, the corresponding F-statistics for the instruments excluded from the second-stage equation equal 6.1, 10.2 and 28.9, respectively, with partial R-squared values of .028, .042 and .118. That our instruments – based on across-site variation in MTO treatment effects on mobility outcomes – have more explanatory power for neighborhood poverty than other attributes is consistent with the focus of MTO to move families to lower-poverty areas.[10]

The key identifying assumption behind our IV analysis is that the only source of variation across sites in MTO's treatment effects on criminal behavior is the variation across sites in how treatment assignment influences post-randomization neighborhood characteristics. This

[9] They also examine fraction college graduates, households headed by females, and median income.
[10] Hahn and Hausman (2002) present two alternative tests to determine whether weak instruments are a problem, based on comparing standard IV estimates with "reverse" estimates that switch the dependent and endogenous right-hand-side variables. Applying their tests to our MTO data provides some indication that limited information maximum likelihood (LIML) may be preferable to two-stage least squares (2SLS) in estimating our equations (1) and (2). However, in practice the pattern of results from LIML and 2SLS are very similar, and so in our tables below we show estimates from 2SLS for simplicity.

assumption strikes us as plausible. There is no obvious reason why, for example, low-income minority families in New York should respond differently than low-income minority families in Baltimore or Boston to the same type of MTO-induced change in neighborhood environment.

The main concern with our empirical approach is that our ability to distinguish between the effects of different neighborhood attributes is limited by the number of available instruments. Because MTO engenders change in many neighborhood characteristics simultaneously, these IV estimates cannot be interpreted literally as the effects of changing a given neighborhood characteristic on criminal 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. However, our ability to simultaneously condition on other neighborhood measures such as poverty or racial composition should help account for other criminogenic neighborhood attributes. Our ability to also control for tract poverty is particularly important because this variable is strongly correlated with other tract socio-economic characteristics and measures from the MTO surveys about neighborhood social processes that previous theories suggest are important.

IV. RESULTS

In what follows we begin by demonstrating that the application of standard non-experimental regression methods to our MTO data yields findings similar to those reported in previous studies, suggesting that criminal behavior is contagious. This helps establish that any difference in findings between our preferred IV analyses and previous studies results from our use of a different (we believe superior) research design, rather than from something peculiar or problematic about our own dataset.

We then show that when we use MTO site-group interactions to instrument for neighborhood measures in our preferred IV research design we do not find evidence for a large positive effect of beat crime rates on individual criminal behavior by MTO participants, contrary to the prediction of contagion models. Nor are the beat crime variables significant after conditioning on tract race or poverty, suggesting that a contagion effect is not simply being offset by a third factor. We believe the lack of a detectable association between neighborhood crime and individual arrests is quite informative. Although not conclusive, the pattern of results suggests to us that there are aspects of residential neighborhoods that affect crime, particularly racial segregation, but that the role of neighborhood crime is more limited.

A. Non-experimental estimates of neighborhood effects on crime

In Table 3 we show that applying the standard non-experimental estimation method to our MTO data yields evidence like that of previous studies that criminal behavior may be contagious. By virtue of random assignment, variation in neighborhood characteristics *across* MTO groups is driven by the experiment and so is exogenous (that is, unrelated to other unmeasured determinants of criminal behavior). However, variation in neighborhood attributes *within* MTO groups results from the mobility decisions made by individual families and is non-experimental, and this variation forms the basis for Table 3.

The non-experimental results in Table 3 include data just on adults and youth assigned to the MTO experimental group and use ordinary least squares to regress our measure of arrests to individuals against our measures of post-random-assignment neighborhood characteristics and a set of baseline control variables.[11] Identification of neighborhood effects with these and other

[11] Note that in principle we could have instead followed convention and conducted our non-experimental analyses using the sample assigned to the MTO control group. But there is more variation in most of our neighborhood measures within the experimental group and thus more power to detect relationships between neighborhood attributes and individual arrest outcomes. The variance in tract share poor is a third larger for the experimental than control group, while the variance for tract share minority is about three-quarters larger for the experimental group.

non-experimental estimates assumes that the process through which families select neighborhoods is “ignorable” conditional on observed individual and family characteristics. In our case the set of observables includes powerful demographic predictors of criminal involvement such as age, race and gender, and family background characteristics such as the household head’s baseline educational attainment and work status. Importantly, we also control for another strong predictor for future criminal involvement – past criminal involvement. Specifically, we include a set of indicators for whether each MTO participant had 1, 2, or 3 or more arrests for violent crimes prior to random assignment , with similar indicators for prior arrests for property or other crimes.

The first panel of Table 3 provides suggestive evidence that criminal behavior might be contagious, particularly among the group at highest risk for criminal offending more generally – males. The first row shows results of estimating regressions where the right-hand side neighborhood measure is each respondent’s local-area violent crime rate, and each column shows the estimates for this local crime rate variable from a separate estimation on the sample for that column. All endogenous neighborhood variables are scaled in standard deviation units to facilitate comparison across panels, with the standard deviation values themselves given in the notes to the table.

For example, the result in the first row for male youth suggests that a 1 standard deviation increase in the local-area violent crime rate increases arrests for violent crimes of MTO male youth by .075 arrests per person (p-value <.10), equal to 16% of the mean arrest rate for this group. The coefficient is of about the same magnitude for male adults, although it is not quite statistically significant. Controlling for tract share minority, share poor or both poor and

The distribution for beat violent crime rate has a slightly larger variance for the control group (about a fifth) but is also somewhat more skewed with extremely high values.

minority does not change the point estimate for male youth much but does serve to make the "contagion" effect for adult males statistically significant. These results provide one benchmark for comparison with our preferred estimates below.

B. Experimental estimates of neighborhood effects on crime

In contrast to the non-experimental estimates presented above, results that use the experimental design of the MTO data to try to parse out the separate effects of beat crime rates from other neighborhood characteristics yield no detectable "contagion" effects.

In a model where the only baseline covariates are site indicators, two-stage least squares estimation of equation (2) with one endogenous neighborhood 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 site-by-group means of the arrest outcome measure and the site-by-group means of the neighborhood variable.[12] Assuming no other confounders, this method estimates how the magnitude of the neighborhood treatment "dose" (such as the change in beat violent crime rates for a particular treatment group at a given site) is associated with the treatment response (the effect on the total number of violent crime arrests for MTO individuals in the experimental or Section 8 voucher groups at that site).

Figure 1 highlights the intuition behind our IV approach by plotting the 15 site-by-group values for beat violent crime rates and individual violent-crime arrest outcomes for our full MTO sample (adults and youth of both genders). The solid line in Figure 1 shows the linear regression relationship among these 15 site-by-group data points between neighborhood violent-crime rates and arrests for violent crime of individual MTO participants. This line has a modest *negative*

[12] 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.

slope, which could arise if MTO participants are more likely to employ violence when this will be a successful strategy, and that for a given person, winning a fight is more difficult in violent neighborhoods where residents are more adept at fighting.[13] In any case, a negative relationship between neighborhood violent crime and individual violent behavior is the opposite of what we would expect under a simple contagion story.

The estimated relationship between local-area violent crime and individual arrest outcomes is both more and less sensitive to outliers than the simple regression slope shown in Figure 1 would suggest. Figure 1 shows that MTO participants assigned to the control group in the Chicago demonstration site live in neighborhoods with unusually high violent crime rates relative to the overall Chicago mean,[14] but the average arrest rate for families in the MTO control group itself is below that site's mean. Yet the positive relationship between beat-level violent crime and individual arrest outcomes when we exclude data from the Chicago site as a whole (the dashed line in Figure 1) is itself an artifact of the correlation between neighborhood violent crime rates and minority composition. Figure 2 shows the results of using our indicators for site and treatment group interactions to simultaneously instrument for beat violent crime and tract minority composition. In this case we now observe a negative relationship between neighborhood violent crime and arrests to MTO participants, with or without data from Chicago in the sample.

[13] Kling, Ludwig and Katz (2005) hypothesize that the positive treatment-control difference they find for property crime arrests for male youth in MTO could be due to a comparative advantage in property-crime offending for experimental youth in their new lower-poverty neighborhoods. If there is "learning by doing" in fighting (most violent-crime arrests in our and other datasets are for assault), then MTO participants may be less likely to have a comparative advantage in fighting in more violent neighborhoods; see for example also the model for decisions about whether to use violence in Donohue and Levitt (1998).

[14] The very high beat violent crime rate for control group families in Chicago is not surprising given that most of these families were living in some of the nation's most notorious public housing projects on the city's South Side. For details on the geographic distributions of MTO families see Orr et al. (2003).

More generally we find no statistically significant evidence that violent crime is contagious for the full sample or any sub-group of MTO participants, even after conditioning on census tract poverty rates or racial composition, as summarized in panel A of Table 4. This table presents the results of using our experimental IV approach described by equations (1) and (2) to estimate the relationship between beat violent crime rates and individual arrest outcomes to MTO participants. As with the previous Table 3, each cell has results of a separate regression described by the row and column labels.

Table 4 also provides information about the degree of collinearity between our different neighborhood variables and thus our ability to use our ten excluded instruments to estimate the effects of multiple neighborhood measures at once. Moving from the first to the second row we see that conditioning on tract share minority increases the standard error for the estimated effect of beat violent crime rates on individual arrest outcomes by 36 percent. The standard error increases more markedly when we condition on tract poverty (by 77 percent) or both tract poverty and minority (79 percent). These results imply that neighborhood poverty and violent crime rates are strongly correlated in our data but that neighborhood minority composition is not as strongly correlated with these two other measures. We use no more than three endogenous variables in our IV estimations in order to avoid severe multicollinearity.

Our estimates enable us to rule out large contagion effects but not more modest effects. For example, in the last row of Table 4, Panel A, the estimated effect of beat violent crime on arrests per male youth in MTO is equal to -.109, with a standard error of .117. The upper bound of the 95% confidence interval thus implies that a 1 standard deviation increase in beat violent crime rates would increase arrests to male youth by +.125, a relative change of around one-quarter of a benchmark like the control mean.

Figure 3 suggests that what does seem to matter for individual arrest outcomes is neighborhood racial composition. The IV regression line between tract share minority and individual arrest outcomes for our full MTO sample is positive and not very sensitive to whether data from Chicago are included or not in the analytic sample. The coefficient (first row, first column of Table 4, Panel B) 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, around one-third of the control mean.

Our finding for the influence of neighborhood racial composition on individual violent behavior holds even after we also condition on beat violent-crime rate or tract share minority in the instrumented set of neighborhood attributes, as seen in Panel B of Table 4. While the IV estimates shown in Table 4 seem to suggest that the effects of tract share minority are weak among young males, as discussed below, estimates that double the number of instruments by also interacting MTO group and site indicators with indicators for family size yield larger positive point estimates even for male youth.

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 C of Table 4 shows large and significant effects only for female youth. Moreover, the coefficient for the tract poverty variable is sensitive to the choice of other neighborhood characteristics to include in the analysis.

C. Extensions and sensitivity checks

One particularly important question is whether any contagion or other peer effects vary non-linearly with neighborhood characteristics, in which case reallocating people or police resources across communities could change the overall level as well as distribution of violent crime in society. Re-estimating our basic IV model using a quadratic of the neighborhood violent crime rate for *W* in equation (2) yields a pattern that at first glance seems consistent with a contagious process that becomes less strong as neighborhood crime increases.[15] However the quadratic term in beat violent crime rate is difficult to disentangle from the effects of neighborhood racial segregation when all three measures are included simultaneously in the model. Moreover, the quadratic in beat violent crime rate – like the linear specification for beat violent crime shown in Figure 1, but unlike the effect for tract share minority – is highly sensitive to whether Chicago is excluded from the sample. We take this pattern of results as providing stronger support for an effect of racial segregation on violent criminal behavior than for a non-linear contagion effect.

Is our inability to detect a statistically significant "contagion" effect with our preferred IV research design simply an artifact of measurement error with our beat-level violent crime rates? Perhaps the strongest evidence against this interpretation of our results comes from the fact that any measurement error with our beat violent crime measure does not prevent us from identifying a statistically significant association with individual arrest outcomes in our *non-experimental* analyses shown above – even despite the fact that our non-experimental estimates draw on just the 40% of the MTO sample assigned to the experimental mobility group.[16]

The main concern with our analysis is that with only 10 instruments, our ability to control for every possible neighborhood attribute that might affect crime is limited. Partial consolation

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[15] For the full sample results the linear term for local-area violent crime rate is equal to .392 (se=.182), while the quadratic term is equal to -.087 (.039). These coefficients are driven by the results for male youth, with coefficients of .702 (.313) and -.170 (.085), respectively.

[16] In addition for Los Angeles, the site where our "beat" measures are largest (around 200,000 people per beat on average) we were also able to obtain crime data for part of our study period (through 1999) for census tracts (around 2,500 and 8,000 people per tract). For our LA MTO sample in 1999 the correlation between beat and tract-level violent crime is +0.25. A linear regression suggests a one-unit increase in the beat violent crime rate is associated with a 1.37 unit increase in the tract measure (standard error .113). When we replace beat with tract level violent crime for our LA sample through 1999 we get results similar to those in Table 4. Thanks to Jeffrey Grogger and George Tita for sharing these tract data with us.

comes from the fact that we have 10 more plausible instruments for specific neighborhood characteristics than previous studies in this literature.

A more constructive way to address this concern is to try to increase our power to disentangle the effects of different neighborhood attributes by using an expanded set of instruments that exploits differences by site *and* family size in how MTO treatment assignment affects neighborhood environments. Larger families have relatively greater difficulty moving when offered a MTO voucher and will face a more constrained neighborhood choice set because vacancy rates tend to be lower for larger rental units (Shroder, 2002).[17] The effects of MTO treatment assignment on mobility outcomes vary across demonstration sites because the gradient between rental unit size and vacancy rates seems to differ across cities.[18] At the same time, a growing body of research suggests that family size has little effect on children's outcomes conditional on birth order (Black et al., 2000, Angrist et al., 2005). In the absence of any main effect of family size on youth outcomes, there would seem to be little reason to believe that interactions of family size and MTO treatment assignment should affect youth outcomes other than through influencing mobility outcomes.[19]

When we replicate our estimates with this expanded set of instruments we generally obtain results qualitatively similar to those shown in Table 4. The one exception is with models where we instrument simultaneously for all three of our neighborhood measures (tract share

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[17] The 2003-4 American Housing Surveys show rental vacancy rates for 1 and 2 room units equal 24.7%, compared to 10.9% for 3 room units, 10.4% for 4 room units, 9.0% for 5 room units, and 7.5% for units with 6 rooms or more. http://www.census.gov/hhes/www/housing/hvs/qtr205/q205tab3.html, 10/17/05.

[18] Data from the 1998 / 1999 American Housing Surveys show a rental vacancy rate in Chicago of 12.4% for 2-bedroom apartments compared to 6.5% for those with 3 bedrooms and 8.9% for those with 4 or more bedrooms. The rental vacancy rates in the Los Angeles and New York show a similar although more attenuated gradient with lower overall vacancy rates for every rental size, while the Baltimore and Boston metropolitan areas show slightly higher vacancy rates for apartments with 3 or 4+ bedrooms vs 2 bedroom units. <http://www.census.gov/hhes/www/housing/ahs/metropolitandata.html> , accessed 10/17/05.

[19] Using this expanded set of instruments typically increases the size of the first-stage partial R-squareds for our instruments by around 20-25% for local-area violent crime and tract share minority and by around 5% for tract share poor, while the first-stage F-statistics for the instruments decrease by around 30-40%.

poverty, share minority, and local-area violent crime), which is where we might expect the greatest value-added from the expanded instruments. These results confirm that tract share minority is the most consistent predictor of individual arrest outcomes. This approach also yields estimates for the effects of tract share minority on arrests of MTO male youth that are larger than those presented above; we now cannot reject the hypothesis that neighborhood racial segregation has similar effects on individual arrest outcomes for male and female youth.

In terms of accounting for other neighborhood characteristics that might influence individual arrest outcomes, it is helpful for our purposes that neighborhood poverty is very highly correlated with most of the other neighborhood "structural" socio-economic characteristics that might influence violent behavior, such as welfare receipt, female-headed households, unemployment, or the presence of affluent (college-educated) adults. Tract poverty is also correlated with most of the social processes that previous theories predict should mediate neighborhood effects on crime.[20]

Why is violent behavior among MTO participants more strongly affected by tract share minority than other tract socio-economic characteristics, such as poverty or beat-level violent crime rates? Table 5 presents the results of estimating a series of "horse race" regressions that control for tract share minority plus some measure of neighborhood social process from our follow-up MTO surveys, which taken together suggest that local drug market activity may be an important mechanism through which racial segregation affects violent behavior among MTO participants.

[20] The correlations of tract poverty with other neighborhood measures are as follows [correlations with tract minority in brackets for comparison]: female-headed households, +.73 [+.47]; employment rate -0.85 [-0.55]; welfare receipt, +0.87 [+0.55]; share adults college educated, -0.65 [-0.62]; problem with police not coming when called, +0.25 [+0.15]; fraction of neighborhood problems such as graffiti, trash or youth hanging out, +0.22 [+0.16]; discriminated against by police, +0.05 [+0.03]; overall satisfaction with neighborhood, -0.27 [-0.15]; and local drug market activity, from youth reports, +0.26 [+0.15].

Table 5 shows that the estimated effect on individual violent behavior by tract share minority is only modestly affected by also controlling for measures of neighborhood social process implicated by leading theories such as local policing quality,[21] social disorder (emphasized by "broken windows" theories; see Wilson and Kelling, 1982; Harcourt, 2001; Harcourt and Ludwig, 2006), or the willingness of local residents to work together to maintain order and shared social norms, what Sampson et al. (1997) term "collective efficacy" (variable definitions in the notes to Table 5). Nor are any of these social process measures themselves statistically significant predictors of violent behavior by MTO participants, although the standard errors are sometimes large, particularly for male youth, which limits our ability to draw firm conclusions from these findings about the importance of these theories in explaining violent behavior.

In contrast we do find that controlling for our measure of local drug market activity seems to explain away the positive association between tract share minority and violent-crime arrests of individual MTO participants, and that our drug measure has a positive and statistically significant association with violent criminal behavior among male youth, even when controlling for tract share minority (Table 5).[22] Drug market activity may be important in explaining

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[21] This is a particularly important measure because criminologists have been concerned with the possibility that the probability (P) that a criminal event (C) results in arrest (A) varies across neighborhoods, which if true complicates our efforts to learn about neighborhood effects on actual criminal behavior since the three factors have a mechanical relationship $A=P\times C$. If the probability of arrest is higher in low-crime, low-poverty areas, our estimates would understate the effects of moving to a less distressed area on criminal behavior – that is, we might understate 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). In addition to possible "under-policing," a closely-related hypothesis is that victims are less likely to report crimes to police in high-minority areas. Yet we obtain qualitatively similar findings when we focus on just the most serious violent crimes for which presumably victim reporting problems are less severe.

[22] We focus on survey reports of local drug activity by youth who were ages 15-19 at the end of 2001 rather than on adult reports about drug activity because the youth reports seem to be more informative. The correlation between adult and youth reports is on the order of about .35, and the youth reports correlate more highly with other outcomes that we would expect to be related to local drug activity, such as whether the MTO youths report having ever sold drugs themselves. This last finding does not appear to be an artifact of increased drug involvement leading to more

individual arrest outcomes because violence, or at least the threat of violence, is common in many underground markets as a way of enforcing contracts (Blumstein, 1995, Miron and Zwiebel, 1995, Cook et al., 2005).[23]

Additional support for drug markets as the explanation for why racial segregation affects individual arrest outcomes comes from the fact that tract share minority does not have a statistically significant relationship with a behavior problems measure for MTO youth. Whatever is happening in predominantly minority neighborhoods appears to be specific to more serious criminal activity rather than general to all forms of anti-social behavior. We also find that tract share minority increases the likelihood that MTO youth report that they have sold drugs themselves (see Ludwig and Kling, 2005).

V. CONCLUSION

Previous studies claim to produce evidence that crime is "contagious," which if true has important implications for government policy and law enforcement since external shocks to criminal behavior will be amplified in this case through social multipliers. Applying the same non-experimental estimation techniques to data from MTO yields similar evidence for contagion, concentrated mostly among males. However, exploiting exogenous variation in neighborhood conditions generated by the experimental design of MTO yields no evidence that contagion is as important as much of the previous research would suggest in explaining across-neighborhood variation in crime rates.

For example, Glaeser et al. (1996) note that variation across neighborhoods in socio-demographic and other observable population characteristics accounts for no more than 30% of the variation in neighborhood crime rates. By comparison, in the MTO data we find that about

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observation of drug activity, since we do not find a strong correlation between youth reports of local drug activity and the youths' own drug use.

[23] Without Chicago in the sample, the effects of local drug activity on individual arrest outcomes are smaller than those shown in Table 8 but are still larger than for most other neighborhood measures.

25% of male youth experience at least one post-random-assignment arrest for violent crime, with a mean number of violent-crime arrests for this group of 1.84. The difference in neighborhood violent-crime rates between this "violent" quartile of male youth and the three quarters of "non-violent" male youth is equal to about one-quarter of a standard deviation. As noted above, the 95% confidence interval for our estimated effect of neighborhood violent crime rates (controlling for tract poverty and racial composition) implies that a one-standard-deviation increase in neighborhood violent crime would increase violent-crime arrests of male youth by no more than +0.12 arrests per person. Our estimates thus imply that differences in neighborhood violent crime rates between the violent quartile and other male youth in our MTO sample can explain no more than around 2% of the difference in arrests of these youth for violent crimes.[24]

Our estimates seem to rule out an important role for contagion models that operate on information or constraints rather than preferences, since we are measuring outcomes for MTO participants "only" 4-7 years after random assignment and only contagion models that emphasize peer effects on preferences would seem to plausibly depend on residential duration. One might wonder in this case how important contagion might be in general if peer influences require extended social exposure, given the high degree of residential mobility that has been documented for national samples of low-income minority families (South and Crowder, 1997; Briggs and Keys, 2005).

[24] An alternative way to think about magnitudes is in terms of effect sizes, although this is complicated by the fact that studies focus on slightly different outcome measures and draw on different samples. With this caveat in mind, previous estimates for the effects of neighborhood or peer violence or delinquency on individual involvement with the same behavior range from around .1 or .2 standard deviations (Aseltine, 1995; Matsueda and Anderson, 1998; Liu, 2000) up to .6 standard deviations (unpublished results from Stewart and Simons, 2006, which do not mediate the effects of neighborhood violence on individual violent behavior by also controlling for peer violence). Our estimates imply an effect size for neighborhood violent crime on violent-crime arrests of male youth of around .14 standard deviations, so we can rule out estimates at the upper end of the previous range but not some of the smaller estimates.

An alternative possibility is that race and violent behavior interact to affect preferences about violent behavior. If the predominantly minority population in MTO is most likely to socialize with others of the same race, it is possible that we cannot detect the effects of "contagion" because what matters is violent crime rates among the neighborhood's minority residents, not violent crime rates overall. However, there does not appear to be much room for divergence between violent crime rates for a neighborhood as a whole versus among a neighborhood's minority community given that most MTO families stay in census tracts that are predominantly minority.

A final concern has to do with the generalizability of our estimates for contagion. But there are 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, since the eligible public housing families who signed up for MTO would be those who expected to benefit the most from moving. And by far the most important reason families signed up for MTO was to escape from gangs and drugs.

In principle, less serious types of criminal activity might be more susceptible to endogenous peer effects, as suggested by previous non-experimental estimates by Glaeser et al. (1996, 2003). However, administrative criminal justice data may confound variation across areas in criminal behavior with variation in victim reporting of crimes to the police or the probability that police identify and arrest suspects, a problem that may be more pronounced for less-serious than more-serious offenses. For this reason our analysis is focused on arrests of MTO participants for violent crimes.

Our results taken together suggest that the role of neighborhood race segregation may play a more important role in understanding variation across neighborhoods in violent crime than is currently thought. One obvious question is – why? Our data provide suggestive support for one candidate explanation – drug market activity, which appears to be more common in racially segregated neighborhoods. If our MTO results generalized to the minority population as a whole, they would imply that around one-eighth of the decline in violent crimes in the U.S. during the 1990s was due to a decline in neighborhood racial segregation over this period.[25] 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.

[25] From 1991 to 2001 the FBI's violent crime index rate declined by 34% (Levitt, 2004) while residential racial segregation (defined as the tract share black for the average black in metropolitan areas) declined by around 10-15%, or 5 percentage points (Glaeser and Vigdor, 2001). Data from the FBI's Uniform Crime Reporting system suggest that around 40% of those arrested for violent index crimes are black (the data unfortunately do not distinguish Hispanics from non-Hispanic whites) (Sourcebook, 1998, p. 342). Our estimates show that a one-standard-deviation decline in the average tract share minority (equal to around 17 percentage points; note this figure does not distinguish between blacks and Hispanics) reduces individual arrests for violent crime among our MTO sample (which consists of both blacks and Hispanics) by around 33% of the control mean. If the tract share minority for the average minority also declined by around 5 percentage points during the 1990s, and if offending and arrest rates are proportional, then our estimates would suggest that declines in racial residential segregation reduced violent offending rates among minorities by around 10%. If minorities make up 40% of the population arrested for violent crimes, this implies a 4% reduction in the overall violent crime index due to reductions in offending among minorities, equal to $(.04 / .34) \approx 12\%$ of the overall decline in violent crimes during the 1990s.

Data Appendix

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.

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%[26] 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

[26] 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.

in the final row of Table 1, this "administrative data response rate" is quite similar across MTO groups. (We exclude the small share of observations for which we are missing arrest data).

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.

While administrative arrest data are not susceptible to self-reporting problems, the main limitation for our purposes is that they may confound variation across neighborhoods in criminal behavior with variation in the probability that a criminal event leads to arrest. In our empirical analysis we focus primarily on arrests of MTO participants for violent crimes (most of which are assaults, but the category also includes murder, rape, and robbery) because we expect there to be less variation across neighborhoods for more serious than less serious offenses in the likelihood that victims report crimes to the police or that police arrest suspects.

Information on post-random assignment addresses for MTO families comes from a variety of active and passive tracking sources that are updated regularly throughout the post-random assignment period (Goering et al., 1999). In calculating average post-randomization neighborhood environments for MTO families, we weight neighborhood characteristics for each address found for someone in MTO by the amount of time spent at that address following random assignment (that is, duration-weighted averages).

Our measures for local-area or neighborhood crime rates are average crime rates for the police beats in which MTO families have 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.[27] 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.[28] 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.[29]

[27] 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.

[28] The violent crime rate includes murder, rape, robbery, and aggravated assault. The property crime rate includes burglary, motor vehicle theft, and larceny. The only difference between the neighborhood crime 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

[29] 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; 7% of addresses are matched to place-level data outside of these cities, and 2% are matched to county data outside MTO cities.

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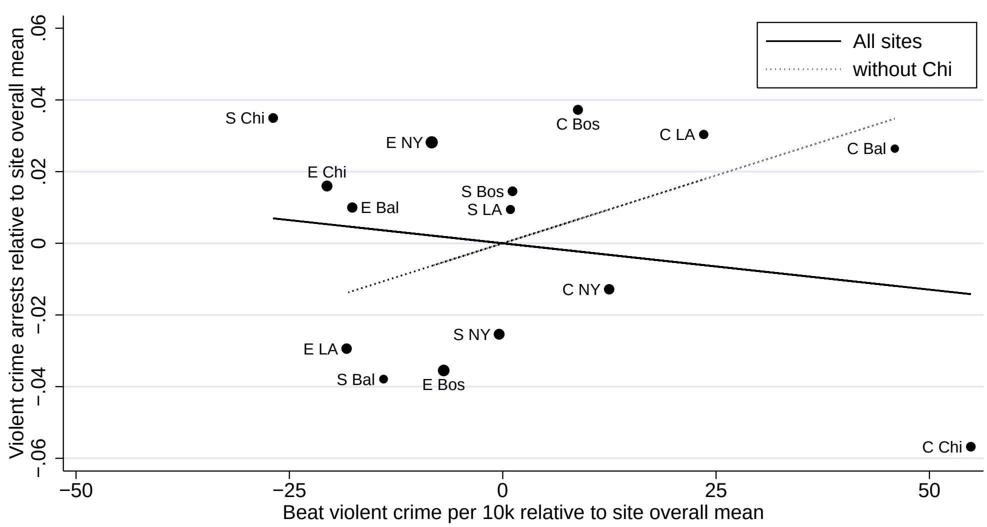
Figure 1

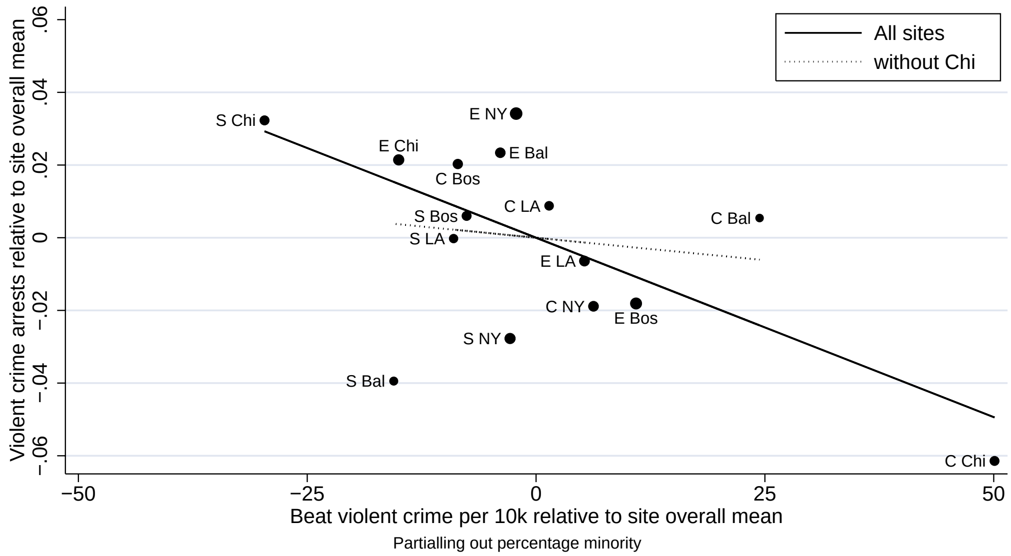
Own Violent Crime on Local Violent Crime



Y: Arrests *W*: Beat violent crime

Figure 2

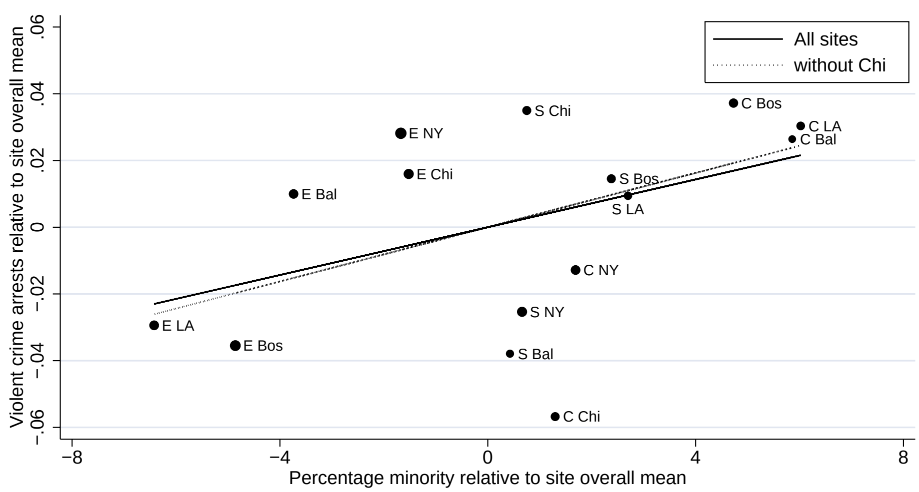
Own Violent Crime on Beat Violent Crime Rate, Conditioning on Tract Share Minority



Y: Arrests *W*: Beat Violent Crime Rate, Tract Share Minority

Figure 3

Own Violent Crime on Tract Share Minority



Y: Arrests *W*: Tract Share Minority

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
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

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
Non-Experimental Estimates for Violent-Crime Arrests Since Random Assignment

Explanatory variable:	Full sample	Female Youth	Male Youth	Female Adults	Male adults
A. Beat violent crime rate					
Violent crime only	.017	-.002	.075*	-.013	.084
	[.017]	[.024]	[.039]	[.016]	[.059]
Violent crime | Tract minority	.010	-.018	.074*	-.023	.098*
	[.018]	[.025]	[.041]	[.016]	[.058]
Violent crime | Tract poverty	.015	-.001	.08*	-.031*	.115*
	[.020]	[.026]	[.046]	[.018]	[.061]
Violent crime | Tract poverty & Tract minority	.013	-.005	.078*	-.034*	.115*
	[.020]	[.026]	[.046]	[.018]	[.060]
B. Tract percentage minority					
Minority only	.016**	.027*	.017	.014*	-.031
	[.008]	[.015]	[.025]	[.008]	[.027]
Minority | Tract poverty	.016	.042**	.009	.011	-.013
	[.01]	[.019]	[.03]	[.009]	[.029]
Minority | Beat violent crime	.012	.029*	-.002	.019**	-.042
	[.009]	[.016]	[.026]	[.009]	[.027]
Minority | Beat violent crime & Tract poverty	.014	.040**	.002	.012	-.015
	[.010]	[.019]	[.031]	[.010]	[.030]
C. Tract percentage in poverty					
Poverty only	.007	-.006	.02	.012	-.047
	[.008]	[.015]	[.026]	[.008]	[.034]
Poverty | Tract minority	-.002	-.031	.016	.007	-.038
	[.010]	[.02]	[.032]	[.009]	[.039]
Poverty | Beat violent crime	.002	-.005	-.010	.025**	-.072**
	[.010]	[.016]	[.032]	[.008]	[.034]
Poverty | Beat violent crime & Tract poverty	-.004	-.028	-.009	.020**	-.063
	[.011]	[.02]	[.037]	[.009]	[.039]

Notes: Each cell in the table contains the coefficient [with standard errors] from a separate OLS estimate of equation (2) using data just from the MTO experimental group, with each row label describing the one or two components of W in (2). E.g., in the first row W only contains neighborhood violent crime rate; in the second row, W contains neighborhood violent crime rate and tract percentage minority, and the coefficient reported is for local violent crime rate. 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. ** = significant at the 5% level; * = significant at the 10% level.

Table 4
Experimental IV Estimates for Violent-Crime Arrests Since Random Assignment

	Full sample	Female Youth	Male Youth	Female Adults	Male adults
A. Beat violent crime rate					
Violent crime only	-.016	-.031	.046	-.071	.016
	[.07]	[.077]	[.070]	[.072]	[.097]
Violent crime | Tract minority	-.137	-.173*	-.054	-.209**	-.103
	[.095]	[.100]	[.091]	[.098]	[.116]
Violent crime | Tract poverty	-.111	-.267**	-.078	-.243*	-.077
	[.124]	[.131]	[.114]	[.128]	[.146]
Violent crime | Tract poverty & Tract minority	-.118	-.285**	-.109	-.256*	-.112
	[.125]	[.136]	[.117]	[.131]	[.150]
B. Tract percentage minority					
Minority only	.067**	.114**	.006	.064**	.031
	[.033]	[.036]	[.057]	[.029]	[.061]
Minority | Tract poverty	.115**	.108**	.002	.152**	.091
	[.051]	[.053]	[.084]	[.043]	[.069]
Minority | Beat violent crime	.110**	.163**	.015	.131**	.057
	[.046]	[.045]	[.068]	[.040]	[.065]
Minority | Beat violent crime & Tract poverty	.115**	.070	.007	.137**	.099
	[.053]	[.058]	[.088]	[.045]	[.073]
C. Tract percentage in poverty					
Poverty only	.008	.082**	-.012	-.015	-.057
	[.02]	[.031]	[.050]	[.020]	[.059]
Poverty | Tract minority	-.041	.034	.034	-.106**	-.08
	[.030]	[.048]	[.073]	[.029]	[.060]
Poverty | Beat violent crime	.037	.174**	-.032	.071**	-.102
	[.034]	[.045]	[.062]	[.036]	[.071]
Poverty | Beat violent crime & Tract poverty	-.009	.156**	.014	-.009	-.117
	[.039]	[.068]	[.086]	[.040]	[.078]

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 neighborhood violent crime rate; in the second row, W contains neighborhood violent crime rate and tract percentage minority, and the coefficient reported is for violent crime rate. 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. ** = significant at the 5% level; * = significant at the 10% level.

Table 5
Experimental IV Effects of Neighborhood Social Processes on Violent Crime Arrests

	Full sample	Female Youth	Male Youth	Female Adults	Male adults
A. Tract share minority					
	0.151*	0.066	-0.072	0.074	-0.324
Minority | Problems with police	[0.087]	[0.073]	[0.117]	[0.071]	[0.287]
	0.085	0.019	-0.163	0.027	-0.213
Minority | Neighborhood problems	[0.068]	[0.063]	[0.099]	[0.059]	[0.253]
	0.112	0.022	-0.112	0.011	-0.222
Minority | Collective efficacy	[0.086]	[0.076]	[0.111]	[0.074]	[0.276]
	0.005	0.005	-0.181*	-0.026	-0.055
Minority | Drugs	[0.067]	[0.064]	[0.1]	[0.063]	[0.136]
B. Problems with police not coming when called					
	-0.048	-0.042	-0.093	-0.187	-0.038
Policing	[0.061]	[0.094]	[0.176]	[0.206]	[0.058]
	-0.192	-0.072	-0.085	-0.245	-0.113
Policing | Minority	[0.105]	[0.108]	[0.182]	[0.242]	[0.101]
C. Neighborhood problems index					
	0	0.063	0.165	-0.141	0.034
Problems	[0.084]	[0.107]	[0.326]	[0.147]	[0.087]
	-0.115	0.047	0.412	-0.136	-0.056
Problems | Minority	[0.13]	[0.124]	[0.345]	[0.16]	[0.13]
D. Collective efficacy					
	-0.012	-0.061	-0.217	-0.034	0.031
Collective efficacy	[0.064]	[0.085]	[0.172]	[0.068]	[0.091]
	0.134	-0.036	-0.223	-0.029	0.018
Collective efficacy | Minority	[0.123]	[0.112]	[0.180]	[0.127]	[0.089]
E. Drug use or selling in neighborhood (from youth surveys)					
	0.088	0.129	0.289	0.059	0.015
Drugs	[0.09]	[0.127]	[0.212]	[0.169]	[0.117]
	0.083	0.13	0.432**	0.107	0.006
Drugs | Minority	[0.094]	[0.139]	[0.21]	[0.192]	[0.13]

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 neighborhood violent crime rate; in the second row, W contains neighborhood violent crime rate and tract percentage minority, and the coefficient reported is for violent crime rate. Sample is limited to households in which at least one youth ages 15-19 at the end of 2001 was surveyed and provided a valid response to the question about drug use or selling in the neighborhood; specific analytic samples vary by column as indicated in the column headings. Endogenous variables are all expressed in standard deviation units relative to the standard deviation in the control group for that variable. Neighborhood problems variable is defined as number of positive responses to questions about whether respondent thinks the following are problems in their neighborhood: litter or trash on the streets or sidewalk, graffiti or writing on the walls, people drinking in public, abandoned buildings, groups of people just hanging out, and police not coming when called. Policing quality measure is taken from the neighborhood problem item for police not coming when called. Collective efficacy constructed from respondent reports about whether neighbors would do anything if a group of neighborhood children were skipping school and hanging out on a street corner, and if some children were spray-painting graffiti on a local building. Drug variable comes from survey reports from youth ages 15-19 at the end of 2001 to the question: During the past 30 days, have you seen people using or selling illegal drugs in your neighborhood? Youth responses assigned to everyone in the family as a measure of local drug activity. The control group standard deviations are 17% for tract minority, 1.1 for fraction of neighborhood problems, 48% for problem with police not coming when called, 43% for collective efficacy, and 50% for youth drug selling reports. ** = significant at the 5% level; * = significant at the 10% level.