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 by site interactions as instruments 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.

1. Introduction

Crime varies dramatically across countries, states, cities, and, most relevant for the present paper, neighborhoods, representing what Glaeser, Sacerdote, and

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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 nonlinear 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 an 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 socioeconomic status, as in role model stories (Wilson 1987) or the willingness of neighbors to become involved in the maintenance of local order, which Sampson, Raudenbush, and Earls (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—explain neighborhood variation in crime is important because only with contagion are policy interventions and other external shocks amplified through social multipliers (Glaeser, Sacerdote, and Scheinkman 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 use stronger research designs often provide stronger evidence that "like begets like" for other

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[1] Glaeser, Sacerdote, and Scheinkman (1996) document excess variation in crime across areas beyond what can be explained by standard sociodemographic determinants of crime. Their results suggest that 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, Morenoff, and Gannon-Rowley (2002).

outcomes, such as student test scores (Hoxby 2000), investment behavior (Hong, Kubik, and Stein 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 these models is responsible for any observed neighborhood effects on criminal behavior (Case and Katz 1991; Manski 1993; Moffitt 2001). Youth growing up in the same neighborhood are 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 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 on where the vouchers could be redeemed under the MTO program design. 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 postprogram neighborhoods.

Previous studies used MTO's experimental design to compare average arrest outcomes across the three randomly assigned mobility groups and found 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. The MTO intervention has few detectable effects on adult arrests (Kling, Ludwig, and Katz 2005; Ludwig and Kling 2005).

[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. The MTO vouchers required residence in these tracts for a minimum of 1 year for renewal of the subsidy. Families in the experimental group were provided with mobility assistance and in some cases other counseling services as well.

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, relative-deprivation models suggest that 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, because 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 in criminal behavior across neighborhoods 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 by 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 by group differences in MTO effects on individual arrest outcomes. For example, assignment to the experimental, rather than the 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 experimental-control difference in violent crime arrests of MTO participants to also be larger (more negative) in Chicago than at other MTO sites.

Although the experimental by 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 account for neighborhood crime plus some measure of neighborhood sociodemographic 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 has suggested. 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 subgroups defined by sex and age, and it also holds when we simultaneously control for neighborhood racial segregation or poverty rates.

Our results suggest that neighborhood racial segregation may play a more important role in understanding variation in violent crime across communities. In order to understand why racial segregation is related to violent criminal behavior among MTO participants, we examine the degree to which this relationship can be explained away by focusing on more detailed measures from the MTO surveys of neighborhood social processes that are predicted by leading theories to mediate neighborhood effects on crime. Our analysis suggests that neighborhood racial composition may affect violent behavior 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.

2. Data

Our analysis focuses on all adults who were part of MTO households at baseline and baseline youth who were 15–25 years old at the end of 2001 (the sample used in Kling, Ludwig, and Katz [2005]). We have baseline sociodemographic 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 include reports about neighborhood social processes. Details are in the 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 Aid to Families with Dependent Children (AFDC) at baseline. About three-quarters of households report getting away from gangs and drugs to be one of the top two reasons for joining MTO.

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

Table 1 shows that there are no statistically significant differences across MTO groups in the fraction of male or female adults or youth who have been arrested

[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) from when vouchers are issued to use them. In addition, families assigned to the experimental group are constrained by the requirement to move to a low-poverty tract.

Table 1

Baseline Descriptive Statistics for Moving to Opportunity (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
Los Angeles	.155	.149	.158	.304	.351	.345
New York City	.257	.257	.264	.297*	.259	.185
HH on AFDC at baseline	.739	.752	.756	.579	.586	.491
Moved because:						
Drugs and/or 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 before RA arrest	.258	.231	.260	.375	.423	.354
Missing 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
Los Angeles	.165	.185	.199	.195	.189	.196
New York City	.270	.271	.242	.269	.248	.270
HH on AFDC at baseline	.732	.744	.749	.743	.706	.727
Moved because:						
Drugs and/or 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 arrest data	.057	.048	.055	.059	.063	.061
N	966	651	716	988	691	739

Note. HH = head of household; RA = random assignment.
* $p < .05$ on experimental versus control or Section 8 versus control difference.

prior to random assignment or, for that matter, in other baseline characteristics. These results, together with those in Kling, Liebman, and Katz (2007), suggest that assignment was in fact random.[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

[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 or ethnic group in the propensity of women to be married or to 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 sex and age subgroups.

Table 2
Mobility Outcomes by Moving to Opportunity Treatment Group, Age Group, and Sex

	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
21%–40%	.266	.409*	.292	.261	.407	.320
Over 40%	.371*	.379*	.598	.406*	.359*	.559
% Tract black	.532*	.537*	.566	.389	.454	.402
% 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
21%–40%	.290	.399*	.290	.274	.403*	.282
60% and Over	.382*	.386*	.606	.396*	.390*	.620
% Tract black	.536	.527	.555	.524	.531	.542
% 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

Note. 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.
* $p < .05$ on experimental versus control or Section 8 versus control difference.

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, although MTO had more pronounced effects on economic than racial residential integration. In principle, neighborhood mobility under MTO could differ by sex 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 sex 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] The 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, whereas Boston has 11, Chicago has 279, Los Angeles has 18, and New York City has 76. We discuss the potential for bias from measurement error with our beat-level crime variables in detail below.

[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."

3. 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. Because 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 (2007), 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 socioeconomic 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).[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]

$$W = Z \times S\pi_1 + X\beta_1 + \varepsilon_1. \tag{1}$$

The second-stage estimates in equation (2) using Z by S interactions as excluded instruments show how the effects on neighborhood characteristics in the MTO sample are related to treatment effects on outcomes (Y).

$$Y = W\gamma_2 + X\beta_2 + \varepsilon_2. \tag{2}$$

[7] We control for a set of individual and household characteristics taken from the MTO baseline surveys 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, app. 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 instrumental variables (IV) models parameterize the site by group variation in outcomes to be linear in the endogenous neighborhood variable for which we instrument. Overidentification 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 (that is, large-sample) theory to justify statistical inference with our standard errors (see, for example, Donald and Lang 2007).

Our analysis differs from that of Kling, Liebman, and Katz (2007) 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). Second, Kling, Liebman, and Katz (2007) 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 crime rates by beat as well as class and race composition. That is, we use the 10 treatment by site interactions to instrument for multiple neighborhood measures simultaneously. The literature on neighborhood effects suggests that each of these measures may have a conceptually distinct effect on criminal behavior. Contagion models that predict neighborhood crime rates should be positively related to individual criminal behavior, even after controlling for tract poverty rates 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, the share of the tract that is minority (tract share minority), and the share of the tract that lives in poverty (tract share poverty) in turn, the corresponding F-statistics for the instruments excluded from the second-stage equation are 6.1, 10.2, and 28.9, respectively, with partial R^2-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 instrumental variables (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 postrandomization neighborhood characteristics. This 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

[9] They also examine the fractions of college graduates, households headed by females, and median income.

[10] Hahn and Hausman (2002) present two alternative tests, based on comparing standard IV estimates with "reverse" estimates that switch the dependent and endogenous right-hand-side variables, to determine whether weak instruments are a problem. Applying their tests to our MTO data provides some indication that limited information maximum likelihood (LIML) estimation 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 estimations is very similar, and so in our tables below we show 2SLS estimates for simplicity.

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 control for 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 socioeconomic characteristics and measures from the MTO surveys about neighborhood social processes that previous theories suggest are important.

4. Results

In what follows, we begin by demonstrating that the application of standard nonexperimental regression methods to our MTO data yields findings similar to those reported in previous studies, which suggests 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 data set.

We then show that when we use MTO site by 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 controlling for tract share minority or tract share poverty, which suggests that a contagion effect is not simply being offset by a third factor. We believe that 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.

4.1. Nonexperimental Estimates of Neighborhood Effects on Crime

In Table 3 we show that applying the standard nonexperimental estimation method to our MTO data yields evidence like that of previous studies that criminal behavior may be contagious. Note that we have some nonexperimental variation in our data that comes from the fact that, within MTO groups, variation in neighborhood attributes results from the mobility decisions made by individual families. This nonexperimental variation is the basis for Table 3.

The nonexperimental results in Table 3 include data only on adults and youth assigned to the MTO experimental group and use ordinary least squares estimation to regress our measure of arrests of individuals on our measures of post-random-assignment neighborhood characteristics and a set of baseline control

Table 3

Nonexperimental Estimates for Violent Crime Arrests since Random Assignment

	Full Sample	Youth		Adults	
		Female	Male	Female	Male
Beat violent crime rate:					
Violent crime only	.017	−.002	.075+	−.013	.084
	(.017)	(.024)	(.039)	(.016)	(.059)
Violent crime | tract share minority	.010	−.018	.074+	−.023	.098+
	(.018)	(.025)	(.041)	(.016)	(.058)
Violent crime | tract share poverty	.015	−.001	.08+	−.031+	.115+
	(.020)	(.026)	(.046)	(.018)	(.061)
Violent crime | tract share poverty and tract share minority	.013	−.005	.078+	−.034+	.115+
	(.020)	(.026)	(.046)	(.018)	(.060)
Tract percentage minority:					
Minority only	.016*	.027+	.017	.014+	−.031
	(.008)	(.015)	(.025)	(.008)	(.027)
Minority | tract share poverty	.016	.042*	.009	.011	−.013
	(.01)	(.019)	(.03)	(.009)	(.029)
Minority | beat violent crime rate	.012	.029+	−.002	.019*	−.042
	(.009)	(.016)	(.026)	(.009)	(.027)
Minority | beat violent crime rate and tract share poverty	.014	.040*	.002	.012	−.015
	(.010)	(.019)	(.031)	(.010)	(.030)
Tract share poverty:					
Poverty only	.007	−.006	.02	.012	−.047
	(.008)	(.015)	(.026)	(.008)	(.034)
Poverty | tract share minority	−.002	−.031	.016	.007	−.038
	(.010)	(.02)	(.032)	(.009)	(.039)
Poverty | beat violent crime rate	.002	−.005	−.010	.025*	−.072*
	(.010)	(.016)	(.032)	(.008)	(.034)
Poverty | beat violent crime rate and tract share minority	−.004	−.028	−.009	.020*	−.063
	(.011)	(.02)	(.037)	(.009)	(.039)

Note. Values presented are coefficients (standard errors) from a separate ordinary least squares estimation of equation (2) using data from the Moving to Opportunity experimental group, with rows describing the components of W in equation (2). For example, in the first row, W contains only neighborhood violent crime rate; in the second row, W contains neighborhood violent crime rate controlling for tract share minority, and the coefficient reported is for local violent crime rate. Endogenous variables are 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 share minority, 14% for tract share poverty, 185% for beat violent crime rate, and 525% for beat property crime rate.

+ Significant at the 10% level.

* Significant at the 5% level.

variables.[11] Identification of neighborhood effects with these and other nonexperimental estimates assumes that the process through which families select neighborhoods can be ignored 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 sex 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

[11] Note that, in principle, we could have instead followed convention and conducted our nonexperimental 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 for the control group, whereas the variance for tract share minority is about three-quarters larger for the experimental group. 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.

include a set of indicators for whether each MTO participant had one, two, or three or more arrests for violent crimes prior to random assignment, with similar indicators for prior arrests for property or other crimes.

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 result for male youth suggests that an increase of 1 standard deviation in the local area violent crime rate increases arrests for violent crimes of MTO male youth by .075 arrests per person ($p < .10$), equal to 16 percent 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, tract share poverty, or both 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.

4.2. *Experimental Estimates of Neighborhood Effects on Crime*

In contrast to the nonexperimental 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 in which the only baseline covariates are site indicators, two-stage least squares estimation of equation (2) with one endogenous neighborhood variable reduces the data to 15 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] Under the assumption that there are 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 sexes). The solid line 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 slope, which could arise if MTO participants are more likely to use violence when this will be a successful strategy and if, for a given person, winning a fight is more

[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.

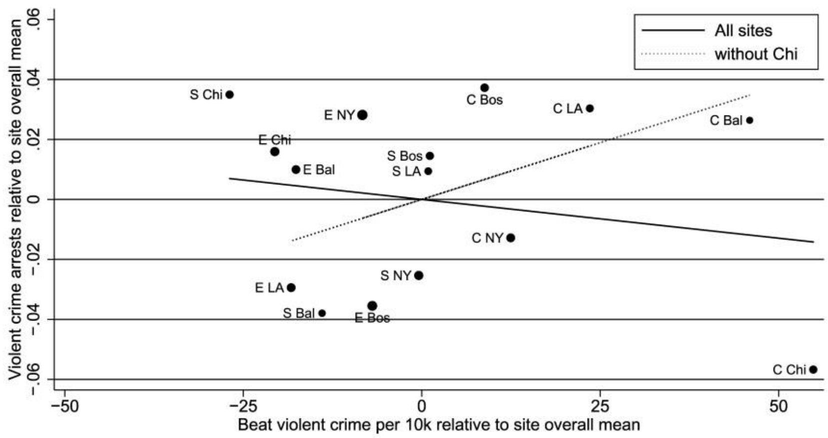

Figure 1. Own violent crime rate on local violent crime rate (Y = arrests, W = beat violent crime rate).

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

[13] Kling, Ludwig, and Katz (2005) hypothesize that the positive treatment by 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 data sets are for assault), then MTO participants may be less likely to have a comparative advantage in fighting in more violent neighborhoods; see, for example, 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).

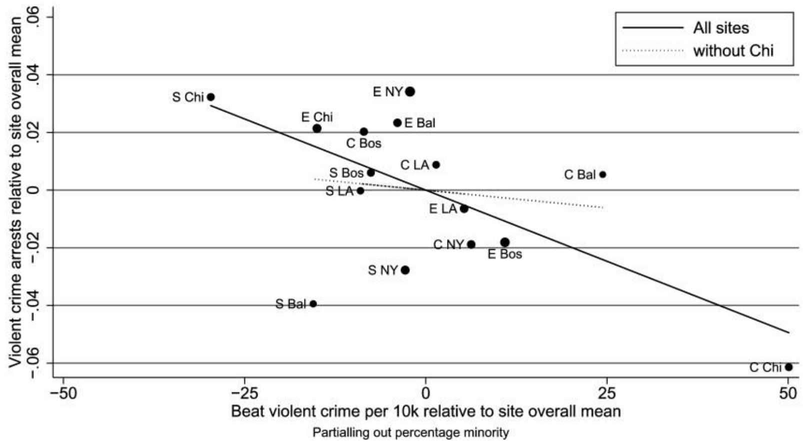

Figure 2. Own violent crime rate on beat violent crime rate, conditioning on tract share minority (Y = arrests, W = beat violent crime rate and tract share minority).

share minority. In this case, we now observe a negative relationship between neighborhood violent crime and arrests of MTO participants, with or without data from Chicago in the sample.

More generally, we find no statistically significant evidence that violent crime is contagious for the full sample or for any subgroup of MTO participants, even after conditioning on census tract poverty rates or racial composition, as summarized in 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 of MTO participants.

Table 4 also provides information about the degree of collinearity between our different neighborhood variables and, thus, our ability to use our 10 excluded instruments to estimate the effects of multiple neighborhood measures at once. 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 share poverty (by 77 percent) or both tract share 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 to avoid severe multicollinearity.

Our estimates enable us to rule out large contagion effects but not more modest effects. For example, in Table 4 the estimated effect of beat violent crime on arrests of male youth in MTO controlling for tract share minority and tract share poverty is −.109, with a standard error of .117. The upper bound of the

Table 4

Experimental Instrumental Variables Estimates for Violent Crime Arrests since Random Assignment

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

Note. Values presented are coefficients (standard errors) from a separate two-stage least squares estimation of equation (2), with rows describing the components of W in equation (2). For example, in the first row, W contains only neighborhood violent crime rate; in the second row, W contains neighborhood violent crime rate controlling for tract share minority, and the coefficient reported is for violent crime rate. Endogenous variables are 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 share minority, 14% for tract share poverty, 185% for beat violent crime rate, and 525% for beat property crime rate.

+ Significant at the 10% level.

* Significant at the 5% level.

95 percent confidence interval thus implies that an increase of 1 standard deviation in beat violent crime rates would increase arrests of 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 in the analytic sample. Table 4 shows that a decrease in tract percentage minority of 1 standard deviation, 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.[15]

[15] As described above, the estimates shown in Table 4 calculate standard errors that are clustered at the level of the MTO household. An alternative is to aggregate the data to the level of the site and treatment group and estimate the regression on these cell means; this approach yields a point estimate almost identical to what is shown in Table 4 but with a slightly larger standard error (.041 versus .033) and p-value (.14 versus <.05).

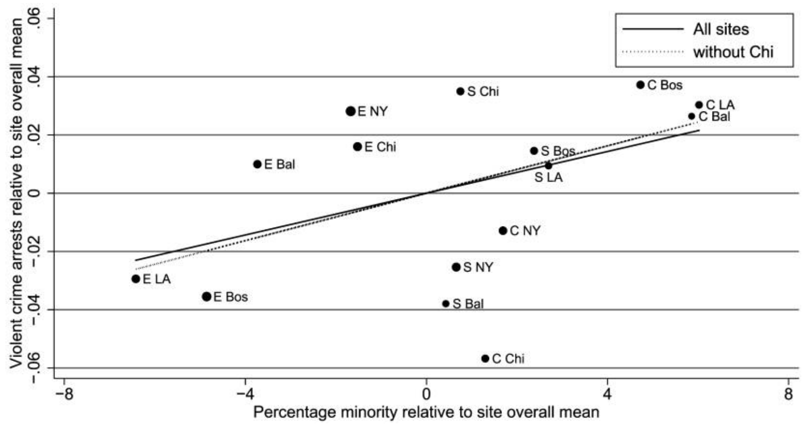

Figure 3. Own violent crime rate on tract share minority (Y = arrests, W = tract share minority).

Our finding for the influence of neighborhood racial composition on individual violent behavior holds even after we also control for beat violent crime rate or tract share minority in the instrumented set of neighborhood attributes, as seen in Table 4. Whereas 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. 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 included in the analysis.

4.3. Extensions and Sensitivity Checks

One particularly important question is whether any contagion or other peer effects vary nonlinearly 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. Reestimating 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 process of contagion that becomes less strong as neighborhood crime increases.[16] However, the quadratic term in beat violent crime rate is difficult to disentangle from

[16] For the full-sample results, the linear term for local area violent crime rate is .392 (standard error, .182), whereas the quadratic term is −.087 (.039). These coefficients are driven by the results for male youth, with coefficients of .702 (.313) and −.170 (.085), respectively.

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 nonlinear 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 nonexperimental analyses shown above—even despite the fact that our nonexperimental estimates draw on just the 40 percent of the MTO sample assigned to the experimental mobility group.[17]

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 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).[18] 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

[17] 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 Los Angeles MTO sample in 1999, the correlation between beat- and tract-level violent crime is +.25. A linear regression suggests that a 1-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 Los Angeles 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.

[18] The 2003 and 2004 American Housing Surveys (U.S. Bureau of the Census, Housing Vacancies and Homeownership: Second Quarter 2005, Table 3: Rental and Homeowner Vacancy Rates, by Selected Characteristics and Percent Distribution of All Units: Second Quarter 2004 and 2005 [http://www.census.gov/hhes/www/housing/hvs/qtr205/q205tab3.html]) show rental vacancy rates for one- and two-room units of 24.7 percent, compared with 10.9 percent for three-room units, 10.4 percent for four-room units, 9.0 percent for five-room units, and 7.5 percent for units with six rooms or more.

across cities.[19] 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, Devereux, and Salvanes 2005; Angrist, Lavy, and Schlosser 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.[20]

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 in which we instrument simultaneously for all three of our neighborhood measures (tract share poverty, tract 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 socioeconomic characteristics that might influence violent behavior, such as welfare receipt, female-headed households, unemployment, or the presence of affluent (college-educated) adults. Tract share poverty is also correlated with most of the social processes that previous theories predict should mediate neighborhood effects on crime.[21]

Why is violent behavior among MTO participants more strongly affected by tract share minority than other tract characteristics, such as poverty or beat-

[19] Data from the 1998 and 1999 American Housing Surveys (U.S. Census Bureau, Metropolitan Data [http://www.census.gov/hhes/www/housing/ahs/metropolitandata.html]) show a rental vacancy rate in Chicago of 12.4 percent for two-bedroom apartments, compared with 6.5 percent for those with three bedrooms and 8.9 percent for those with four or more bedrooms. The rental vacancy rates in Los Angeles and New York show a similar, although more attenuated, gradient, with lower overall vacancy rates for every rental size, whereas the Baltimore and Boston metropolitan areas show slightly higher vacancy rates for apartments with three or four or more bedrooms versus two-bedroom units.

[20] Using this expanded set of instruments typically increases the size of the first-stage partial R^2-values for our instruments by around 20–25 percent for local area violent crime and tract share minority and by around 5 percent for tract share poverty, whereas the first-stage F-statistics for the instruments decrease by around 30–40 percent.

[21] The correlations of tract share poverty with other neighborhood measures are as follows (correlations with tract share minority are in parentheses for comparison): female-headed households, $+.73$ ($+.47$); employment rate, $-.85$ ($-.55$); welfare receipt, $+.87$ ($+.55$); share of college-educated adults, $-.65$ ($-.62$); problem with police not coming when called, $+.25$ ($+.15$); fraction of neighborhood problems such as graffiti, trash, or youth hanging out, $+.22$ ($+.16$); discriminated against by police, $+.05$ ($+.03$); overall satisfaction with neighborhood, $-.27$ ($-.15$); and local drug market activity (from youth reports), $+.26$ ($+.15$).

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. Process measures that help explain away the direct relationship between neighborhood racial composition and violent crime arrests of MTO participants are interpreted to be candidate mediators for this relationship. We focus initially on results for the full MTO sample, given that our main IV specification (Table 4) yields evidence of relatively large effects of neighborhood minority composition on violent crime arrests for each of our subgroups except male youth. As noted above, using an expanded instrument set that further interacts family size with MTO site and treatment group indicators provides evidence for an effect for male youth as well. These results taken together suggest that local drug market activity may be an important mechanism through which racial segregation affects violent behavior among MTO participants.

Table 5 shows that the estimated effect of tract share minority on violent behavior by our full MTO sample is only modestly affected by also controlling for measures of neighborhood social process implicated by leading theories such as local policing quality,[22] 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, Raudenbush, and Earls (1997) term "collective efficacy." Nor are any of these measures of social process themselves statistically significant predictors of violent behavior by MTO participants, with many of the point estimates the opposite sign of what these leading theories would predict. However, the standard errors are sometimes large, particularly for male youth, which limits our ability to draw firm conclusions about the importance of these theories for 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 our full sample of MTO participants. Our drug measure has a positive and large association with violent criminal behavior in the full sample and for three of our subgroups, even when controlling for tract share minority, and is particularly large (and statistically significant) for

[22] 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, given that 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 "underpolicing," 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 victim reporting problems are presumably less severe.

Table 5

Experimental Instrumental Variables Effects of Neighborhood Social Processes on Violent Crime Arrests

	Full Sample	Youth		Adults	
		Female	Male	Female	Male
Tract share minority:					
Minority | problems with police	.151+	.066	−.072	.074	−.324
	(.087)	(.073)	(.117)	(.071)	(.287)
Minority | neighborhood problems	.085	.019	−.163	.027	−.213
	(.068)	(.063)	(.099)	(.059)	(.253)
Minority | collective efficacy	.112	.022	−.112	.011	−.222
	(.086)	(.076)	(.111)	(.074)	(.276)
Minority | drugs	.005	.005	−.181+	−.026	−.055
	(.067)	(.064)	(.1)	(.063)	(.136)
Problems with police not coming when called:					
Policing	−.048	−.042	−.093	−.187	−.038
	(.061)	(.094)	(.176)	(.206)	(.058)
Policing | minority	−.192	−.072	−.085	−.245	−.113
	(.105)	(.108)	(.182)	(.242)	(.101)
Neighborhood problems index:					
Problems	0	.063	.165	−.141	.034
	(.084)	(.107)	(.326)	(.147)	(.087)
Problems | minority	−.115	.047	.412	−.136	−.056
	(.13)	(.124)	(.345)	(.16)	(.13)
Collective efficacy:					
Collective efficacy	−.012	−.061	−.217	−.034	.031
	(.064)	(.085)	(.172)	(.068)	(.091)
Collective efficacy | minority	.134	−.036	−.223	−.029	.018
	(.123)	(.112)	(.180)	(.127)	(.089)
Drug use or selling in neighborhood:					
Drugs	.088	.129	.289	.059	.015
	(.09)	.127)	(.212)	(.169)	(.117)
Drugs | minority	.083	.13	.432*	.107	.006
	(.094)	(.139)	(.21)	(.192)	(.13)

Note. Values presented are coefficients (standard errors) from a separate two-stage least-squares estimation of equation (2), with rows describing the components of *W* in equation (2). For example, in the first row, *W* contains tract share minority and problems with the police, and the coefficient reported is for tract share minority. The sample is limited to households in which at least one youth aged 15–19 years at the end of 2001 was surveyed and provided a valid response to the question about drug use or selling in the neighborhood. Endogenous variables are expressed in standard deviations relative to the standard deviation in the control group for that variable. The variable for neighborhood problems is defined as the number of positive responses to questions about whether the 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. The policing quality measure is taken from the neighborhood problem item for police not coming when called. Collective efficacy is 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. The drug variable comes from survey reports from youth aged 15–19 years 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 are assigned to everyone in the family as a measure of local drug activity. The control group standard deviations are 17% for tract share 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 reports of drug selling.

+ Significant at the 10% level.

* Significant at the 5% level.

male youth (Table 5).[23] We also find that our drug measure, unlike the other neighborhood process variables, has a pronounced relationship with violent crime arrests of MTO participants when we control for neighborhood poverty or beat violent crime rates (results not shown).

Drug market activity may be important in explaining 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. 2007).[24] It is possible that drug market activity congregates in disproportionately minority neighborhoods simply because minorities are more likely to be involved with drug use, drug selling, or gangs or, alternatively, because something about the residential concentration of minority residents itself could increase the volume of drug market activity within a city. Unfortunately, with our data, we cannot explore why drug market activity is more common in heavily minority neighborhoods.

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 measure of behavioral problems 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 antisocial 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).

5. Conclusion

Previous studies have claimed to produce evidence that crime is contagious, which if true has important implications for government policy and law enforcement, given that external shocks to criminal behavior will be amplified in this case through social multipliers. Applying the same nonexperimental 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, Sacerdote, and Scheinkman (1996) note that variation

[23] We focus on survey reports of local drug activity by youth who were 15–19 years old 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 observation of drug activity, because we do not find a strong correlation between youth reports of local drug activity and the youths' own drug use.

[24] Without Chicago in the sample, the effects of local drug activity on individual arrest outcomes are smaller than those shown in Table 5 but are still larger than for most other neighborhood measures.

across neighborhoods in sociodemographic and other observable population characteristics accounts for no more than 30 percent of the variation in neighborhood crime rates. By comparison, in the MTO data we find that about 25 percent 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 "nonviolent" male youth is equal to about one-quarter of a standard deviation. As noted above, the 95 percent confidence interval for our estimated effect of neighborhood violent crime rates (controlling for tract poverty and racial composition) implies that an increase in neighborhood violent crime of 1 standard deviation would increase violent crime arrests of male youth by no more than .12 arrest 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 percent of the difference in arrests of these youth for violent crimes.[25]

Our estimates seem to rule out an important role for contagion models that operate on information or constraints rather than preferences, because 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).

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 con-

[25] 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 deviation (Aseltine 1995; Matsueda and Anderson 1998; Liu 2000) up to .6 standard deviation (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 deviation, so we can rule out estimates at the upper end of the previous range but not some of the smaller estimates.

tagion. 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, given that 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 nonexperimental estimates by Glaeser, Sacerdote, and Scheinkman (1996, 2003). However, administrative criminal justice data may confound variation in criminal behavior across areas 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 in violent crime across neighborhoods 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 United States during the 1990s was due to a decline in neighborhood racial segregation over this period.[26] To the extent to which other studies have claimed that contagion is in fact the main source of variation in violent crimes across neighborhoods, the results would seem to be due instead to some combination of endogenous sorting (self-selection) and unmeasured aspects of neighborhood racial segregation.

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

[26] From 1991 to 2001, the Federal Bureau of Investigation (FBI) violent crime index rate declined by 34 percent (Levitt 2004), whereas residential racial segregation (defined as the tract share black for the average black in metropolitan areas) declined by around 10–15 percent, or 5 percentage points (Glaeser and Vigdor 2001). Data from the FBI's Uniform Crime Reporting system suggest that around 40 percent of those arrested for violent index crimes are black (the data unfortunately do not distinguish Hispanics from non-Hispanic whites) (U.S. Department of Justice 1998, p. 342). Our estimates show that a decline in the average tract share minority of 1 standard deviation (equal to around 17 percentage points; note that 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 percent 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 rates of violent offenses among minorities by around 10 percent. If minorities make up 40 percent of the population arrested for violent crimes, this implies a 4 percent reduction in the overall violent crime index due to reductions in offending among minorities, equal to $(.04/.34) \approx 12$ percent of the overall decline in violent crimes during the 1990s.

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 aged 15–20 years 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 percent of completed surveys were with female adults. The overall effective response rate for the adult survey was 90 percent[27] and was 88 percent for the youth survey. For both adults and youth, the survey response rates are quite similar across MTO treatment groups. The youth surveys include questions about risky and delinquent behavior, and both surveys capture a variety of other nonmarket 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 percent of MTO participants. As seen in 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, and, in most cases, information on the disposition of each charge. Because these are lifetime arrest histories, we are able to construct measures of arrest experiences both before and after random assignment and to examine how neighborhood effects change with time since randomization.

Whereas administrative arrest data are not susceptible to self-reporting problems, the main limitation for our purposes is that they may confound variation in criminal behavior across neighborhoods 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 in the likelihood that victims report crimes to the police or that police arrest suspects for more serious offenses than for less serious offenses.

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

Information on post-random-assignment addresses for MTO families comes from a variety of active and passive tracking sources that were updated regularly throughout the post-random-assignment period. In calculating average postrandomization neighborhood environments for MTO families, we weight neighborhood characteristics for each address found for someone in the MTO by the amount of time spent at that address after 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 Federal Bureau of Investigation's Part I Index offenses (Federal Bureau of Investigation 2006), for which consistent data are available across areas.[28] 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.[29] 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.[30]

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[28] These crime figures come from the FBI's Uniform Crime Reporting system, which is subject to a number of well-known problems such as nonreporting 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 replicated the analysis using only crime data for jurisdictions that report complete data and obtained similar results.

[29] 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 (so we count arrests for all assaults as violent crime arrests) or between grand and petite larceny.

[30] 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 1994–96. We use a similar procedure to estimate 2002 beat-level 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 1994–96 because of missing crime data for two of Boston's police districts in those years. Fully 77 percent of addresses are matched to beat-level data and 10 percent to city-level data in the five MTO cities; 7 percent of addresses are matched to place-level data outside of these cities, and 2 percent are matched to county data outside MTO cities.

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