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Formal Controls, Neighborhood Disadvantage, and Violent Crime in U.S. Cities: Examining (Un)intended Consequences

Purpose: This study examines the intended and unintended effects of formal social controls on violent crime within and across U.S. cities.

Methods: Using data from the National Neighborhood Crime Study, we assess whether greater police arrest activity and jail incarceration risk are associated with lower violent crime rates across cities. We also investigate whether greater use of these formal social controls exacerbates the relationship between extreme neighborhood disadvantage and violent crime.

Results: Results from multilevel analyses show that some formal controls (jail incarceration risk) reduce violent crime across cities, but other formal controls (police arrest activity) amplify the relationship between extreme neighborhood disadvantage and violent crime within cities.

Conclusions: Two main conclusions can be drawn from our analyses. First, we found evidence that some formal controls do reduce violent crime, while others do not. Second, our results support scholars' arguments that formal controls have unintended consequences (e.g., Clear, 2007, 2008; Rose & Clear, 1998), specifically, by amplifying the effect of extreme neighborhood disadvantage on violent crime.

Keywords: formal social controls, neighborhood disadvantage, violent crime, community crime

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The regulation of crime in communities is a process that is owed to both informal and formal social controls (Bursik & Grasmick, 1993; Clear, 2007; Sampson, 1986). Researchers of community crime have focused primarily on the crime-reducing effects of informal social controls such as social ties, community organizations, collective efficacy, and so forth (e.g., Kubrin & Weitzer, 2003; Sampson, Raudenbush, & Earls, 1997), with much less attention to the effects of formal social controls (e.g., police, jail incarceration) (Clear et al., 2003; Kubrin & Weitzer, 2003; Sampson & Loeffler, 2010; Wildeman & Western, 2010). Additionally, much of the research on formal controls has focused primarily on the consequences of state imprisonment (e.g., Levitt, 1996; Kovandzic & Vieraitis, 2006; Western, 2006) as opposed to the effects of more localized formal controls such as police and jails.¹ Such formal controls may reduce city crime rates (Sampson, 1986); however, they may also impact the neighborhood processes (e.g., informal controls) that affect neighborhood crime rates within cities, thereby having unintended consequences, such as higher crime, in these areas (Clear, 2007; Rose & Clear, 1998). Further, because formal controls such as police and jail incarceration may be applied differently within and across cities (Klinger, 1997), their impacts on crime rates may be different for various types of communities, particularly disadvantaged ones (e.g., Clear, 2007; Rose & Clear, 1998). In this study, we assess the impact of formal controls (police arrest activity, jail incarceration risk) on rates of violent crime across U.S. cities, as well as whether these formal controls affect the relationship between extreme neighborhood disadvantaged and violent crime within cities.

Formal Controls in Communities

As sources of social control, both formal and informal controls ultimately function to regulate behavior; however, some scholars have suggested that formal controls do not necessarily reduce crime in all cases, nor in all types of communities. Clear and his colleagues

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(Clear, 2007; Rose & Clear, 1998) have offered the most precise theoretical expectations regarding the potential “unintended consequences” (e.g., higher crime, reduced capacity for informal social control) of formal controls for certain types of neighborhoods. The core argument put forth by Clear and colleagues has suggested that while formal controls are typically expected to reduce crime rates within communities (a direct effect), they may actually *increase* crime if they impede the capacity of communities to regulate crime themselves through the use of informal control (a moderating effect). Rose and Clear (1998) explained that coercive mobility, or the forced removal of neighborhood residents through the use of formal controls (such as arrest), may exacerbate the relationship between neighborhood disadvantage and crime because it disrupts the ability of the informal social controls (e.g., social ties between residents, family supervision) in these areas to work properly. In other words, relying too heavily on the police or incarceration to reduce crime may undermine the ability of informal controls, such as families, to control crime (Clear et al., 2003; Frost & Gross, 2012; Kubrin & Weitzer, 2003; Rose & Clear, 1998). Further, this effect is expected to be most pronounced in areas characterized by *extreme* levels of disadvantage, primarily because the informal controls that are available in such areas are already depleted and/or strained (Clear, 2008; Clear et al., 2003).

A few studies have examined Clear’s expectations empirically (Clear, Rose, & Ryder, 2001; Clear et al., 2003; Lynch & Sabol, 2004a, 2004b; Lynch et al., 2002; Sabol & Lynch, 2003), but these studies have: 1) primarily focused on imprisonment as a formal control--not police or jails; 2) used data from only a few cities; and, 3) not focused on the impact of formal controls within extremely disadvantaged neighborhoods in particular. In the current study, we attempt to fill these gaps by examining the direct effects of city formal controls, as measured by police arrest activity and jail incarceration, on violent crime rates across 90 cities in the U.S., as

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well as the moderating effects of formal controls on the relationship between extreme disadvantage and crime within cities. We assess whether formal controls have intended consequences for cities (e.g., lower crime rates), but unintended consequences for some neighborhoods (e.g., extremely disadvantaged neighborhoods) within those cities.

Intended and Unintended Consequences of Formal Controls

Direct Effects of Formal Controls on City Crime Rates

Formal controls are expected to directly reduce crime levels, primarily by deterring crime or incapacitating offenders from committing future crimes. Wilson and Kelling (1982), for instance, argued that increased police attention to “minor” offenses, such as public intoxication, littering, and so forth might be associated with lower violent crime rates because it sends a message to potential offenders that crime – even minor forms – will not be tolerated.² Stark (1987) observed that more rigorous law enforcement decreased crime and deviance, and suggested that lenient law enforcement practices may unintentionally attract criminals. Wilson and Kelling (1982) also posited that less vigorous policing increases disorder and sends the message that “no one cares” about the area. Increased police arrest activity, then, may impact crime rates because offenders perceive that there is a high likelihood they will be detected and punished for criminal behavior (Sampson, 1986; Wilson & Boland, 1978) or because law enforcement is viewed to be more “proactive” (Kubrin et al., 2010).

Evidence from studies conducted at both the city- and neighborhood-levels of analysis indicates that more stringent policing practices are, in fact, associated with lower rates of crime and violence (e.g., Kane, 2006; Kubrin et al., 2010; Sampson & Cohen, 1988; Wilson & Boland, 1978; Wilson & Kelling, 1982). However, Kubrin and colleagues (2010) noted that prior studies of proactive or aggressive policing may have suffered from underspecification in their empirical models, particularly by failing to acknowledging that police behavior is impacted by macro-level

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factors such as the social and economic health of a city. Klinger (1997) has theorized that police actions are influenced by environmental factors, and scholars have found that racial (e.g., Kane, 2003; Smith, 1986) and socioeconomic (e.g., Sun, Payne, & Wu, 2008; Smith, 1986; Terrill & Reisig, 2003) characteristics influence police actions, with police potentially being more active in areas with increased racial heterogeneity or lower socioeconomic status. Thus, certain areas of a city may experience more proactive police tactics than others, but the extent to which police behaviors impact various communities differently remains to be determined.

In addition to the police, incarceration may also reduce criminal activity by removing and incapacitating offenders from a city, at least temporarily suspending their criminal activity.³ Extant studies have examined the effects of incarceration at the state and national level, with the majority of research concluding that large increases in prison populations lead to only slight reductions in crime rates, or are not associated with crime levels at all (DeFina & Arvanites, 2002; Johnson & Raphael, 2012; Lynch & Sabol, 2004a; Marvell, 2010; Marvell & Moody, 1994; Spelman, 2000; Visher, 1987; Zimring & Hawkins, 1988). Perhaps this is because not all offenders have the same risk of being arrested and incarcerated (Visher, 1987), or, because incarceration at the city level is more relevant and meaningful (e.g., better signifies the likelihood of punishment) to potential offenders than incapacitation at the state or national level (Sampson, 1986). Indeed, contrary to evidence on state and national incarceration, Sampson (1986) found that a higher risk of jail incarceration reduced rates of robbery across cities. Very little research has examined the effects of jail incarceration on city crime rates; however, scholars have suggested that incarceration is differentially concentrated throughout areas within cities, with disadvantaged neighborhoods being particularly likely to experience higher rates of incarceration (Sampson & Loeffler, 2010; Western, 2007; Wildeman & Western, 2010). In this

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study, we examine the impact of police arrest activity and jail incarceration on city crime rates while controlling for important city and neighborhood covariates.

Moderating Effects of Formal Controls on the Relationship Between Extreme Disadvantage and Neighborhood Crime Rates

Clear and colleagues (Clear, 2007; Rose & Clear, 1998) have provided theoretical expectations regarding the potential moderating effects of formal controls on neighborhood-level processes, but the empirical validity of these expectations warrants continued investigation (for exceptions see Clear, Rose, & Ryder, 2001; Clear et al., 2003; Lynch & Sabol, 2004a, 2004b; Lynch et al., 2002; Sabol & Lynch, 2003). Clear has argued that over-reliance on, or over-use of, formal controls (e.g., policing, incarceration) can undermine the effectiveness of informal social control agents to regulate crime within disadvantaged neighborhoods, which in turn, may lead to increased levels of crime; further, these moderating effects may be most pronounced in areas that are characterized by *extreme* disadvantage because the sources of informal social control may already be limited, strained, or depleted in these areas (Clear et al., 2003; Rose & Clear, 1998; Shaw & McKay, 1942). In partial support of this idea, Clear et al. (2003) found that higher rates of admissions and releases from prisons within Tallahassee neighborhoods were associated with higher rates of neighborhood crime. Following Clear and colleagues (Clear, 2007, 2008; Rose & Clear, 1998), we expect that greater use of formal controls may unintentionally *exacerbate* the relationship between extreme neighborhood disadvantage and violent crime by reducing the number and/or effectiveness of informal social control agents in these areas. While we do not examine these intervening processes directly, we recognize the theoretical mechanisms which we believe to operate when formal controls are vigorously employed in extremely disadvantaged

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neighborhoods. That is, these linkages explain why the effects of neighborhood disadvantage on crime rates may be exacerbated in cities with greater use of formal controls.

Regarding the number of informal social control agents within disadvantaged neighborhoods, the social costs of arrests and incarceration can be detrimental to local human ecology systems, as incapacitation (both temporarily and long-term) removes individuals from neighborhoods which could otherwise benefit from their presence (Clear, 2007). For example, when a father is arrested and/or incarcerated, the household becomes headed by a single parent, a potential income-earner is removed, and the amount of supervision over children is reduced (Clear, 2008; Parker & Reckdenwald, 2008; Sabol & Lynch, 2003; Western, 2007; Western, Lopoo, & McLanahan, 2004). Removal of such an individual may have a broader impact within a neighborhood as well, since social ties between neighbors and the individual are disrupted and a potential income-generating member of the neighborhood is removed, creating increased strain on community resources (Clear, 2007; Fagan, 1997; Frost & Gross, 2012; Western, 2007).⁴

The overuse use of formal controls within disadvantaged neighborhoods may also reduce informal social control agents' effectiveness and capacity for crime control. For instance, when social ties within families and between neighborhood residents are disrupted (Western, Lopoo, & McLanahan, 2004), levels of cohesion, communication, and commitment—both to the area and among the residents within the area (Lynch & Sabol, 2004a)—may become strained and/or depleted (Clear, 2007). This can lead to a diminished sense of “community” in which neighborhood residents know each other, communicate, and work toward nurturing the health of the community (Kornhauser, 1978; Sampson et al., 1997; Ross, Mirowsky, & Pribesh, 2002; Warner, 2014).

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Residents of disadvantaged neighborhoods may also become cynical about the legitimacy of criminal justice agencies, such as the police or correctional institutions (Kirk & Matsuda, 2011; Kirk & Papachristos, 2011; Sampson & Bartusch, 1998). Residents may come to perceive that their community is being unfairly targeted for criminal justice sanctioning (e.g., Clear, 2007; Kane, 2005; Weitzer, 1999; Walker, 1992), or their trust in the criminal justice system could be eroded by personal or anecdotal experiences (Brunson, 2007; Carr, Napolitano, & Keating, 2007). Both scenarios could weaken mainstream belief systems regarding the importance of, and capacity for, criminal justice agencies to control crime within their neighborhood (e.g., Berg et al., 2012). If cynicism regarding the law and its agents becomes pervasive across neighborhood residents, then residents may adapt to this cultural circumstance by becoming more self-reliant, which could weaken residents' capability to come together to resolve problems and informally control crime (Sampson et al., 1997; Kornhauser, 1978), only furthering the need for additional criminal justice system involvement.

Finally, the burden that the social systems endure as a result of formal control actions may prove to be too much to handle, and this effect may be most detrimental in extremely disadvantaged neighborhoods. This is because the adverse effects of formal controls may be more devastating in disadvantaged areas compared to more affluent areas because residents in these areas have fewer resources to begin with (e.g., social and human capital, economic vitality, strong social ties; see Kornhauser, 1978; Rose & Clear, 1998; Wilson, 1987). Therefore, extremely disadvantaged neighborhoods may experience unintended consequences brought on by formal controls because the lack of resources in such communities would be further depleted, and in some cases, these resources would be completely exhausted (Clear et al., 2003). We are

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not aware of studies that have examined whether formal controls amplify the effect of neighborhood disadvantage on crime rates, as the theory described above would suggest.

To summarize, more research is needed regarding the intended and unintended effects of formal controls, such as police arrest activity and jail incarceration risk, on crime within communities. We know very little about the effects of formal controls (police and jail) on rates of crime across cities or neighborhood crime rates within cities. Further, much of what we know about the effects of formal controls on communities comes from data collected within select cities, despite calls for additional data collection efforts within other cities (e.g., Lynch & Sabol, 2004b). Finally, and perhaps most relevant to the current study, assessment of the impact of formal controls in *extremely disadvantaged* neighborhoods is still needed. Building from research on formal controls, coercive mobility, and the collateral consequences of incarceration (e.g., Clear, 2008; Hipp, Petersilia, & Turner, 2010; Petersilia, 2003; Sampson, 2011; Western, 2006; Wildeman & Western, 2010), we assess the direct and moderating impacts of formal controls on crime using a national sample of neighborhoods within large cities. We examine the main effects of formal controls (i.e., police arrest activity and jail incarceration risk) on city violent crime rates (e.g., robbery, aggravated assault, and homicide) and the moderating effects of these formal controls on the relationship between neighborhood disadvantage and crime.

Method

Sample and Data

The multi-level dataset compiled for this study consists of structural and demographic characteristics of neighborhoods (defined as census tracts) across U.S. cities as well as information regarding local arrest, jail, and crime data. The National Neighborhood Crime Study (NNCS) 2000 (Peterson & Krivo, 2010) provided the foundation for the data set. The NNCS

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involved a two stage sampling design with cities selected at stage one, followed by neighborhoods at stage two. The cities in the U.S. with a population greater than 100,000 (as of 1999) were stratified by census region (Northeast, Midwest, South, and West), and then cities were randomly selected from each strata, resulting in 91 cities. Next, all of the populated census tracts within those cities were selected; creating a final sample of 9,593 census tracts (hereafter referred to as neighborhoods) nested within 91 large U.S. cities.⁵

For the NNCS, sociodemographic information was collected for each neighborhood along with neighborhood-level crime data. For purposes of this study, we combined the NNCS data with information collected from several other data sources described below. The current analysis consists of 9,560 tracts nested within 90 cities. One city (Topeka, KS) was unable to provide arrest data due to a change in their computer operating systems and had to be dropped from the sample. For some of the analyses reported here, the sample was also restricted to a smaller number of cities, due to availability of neighborhood-level crime data in the NNCS.⁶ For the analysis of robbery, 9,560 census tracts located within 90 cities were examined, 8,580 census tracts within 78 cities were examined for the analysis of aggravated assault, and 8,979 census tracts within 88 cities were examined for the analysis of homicide. Despite the missing data for some neighborhoods and cities, the sample of NNCS cities generalizes to most large urban places regarding crime levels, racial/ethnic composition, segregation, and economic disadvantage (Krivo et al., 2009; Peterson & Krivo 2010).

Measures

All of the measures used in the analyses are described in table 1. The outcome measures included different types of violent crime (robbery, aggravated assault, and homicide).⁷ The neighborhood totals for these offenses were averaged over a three year period (1999-2001) in

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order to reduce the impact of annual fluctuations and provide a more reliable measures for small areas (see also Messner & Golden, 1992; Peterson, Krivo, & Harris, 2000; Sampson, 1987).

Robbery represents the natural log of the three-year average reported rate of robberies per 1,000 persons. *Aggravated assault* is the natural log of the three-year average reported rate of aggravated assaults per 1,000 persons. *Homicide* is the three-year sum of the number of murders and non-negligent manslaughters within each neighborhood. The natural log of the rates of robbery and aggravated assault were taken because the original distributions were skewed. Homicide was measured as a limited count rather than a rate (see also Browning, Feinberg, & Dietz, 2004; Browning et al., 2010).

-- Table 1 about here --

The predictor variables included both neighborhood- and city-level measures. For the neighborhood-level measures, we followed prior research (e.g., Browning, Leventhal, & Brooks-Gunn, 2005; Land et al., 1990) and conducted a principle components analysis of nine census indicators, which revealed a three component solution that explained 88 percent of the variation in these items (Kaiser-Meyer-Olkin Measure of Sampling Adequacy = .806). The three components reflect the structural characteristics of neighborhoods, concentrated disadvantage, concentrated immigration, and residential instability (e.g., Sampson et al., 1997). Based on the component loadings, *neighborhood disadvantage* was comprised of five items (Eigenvalue = 4.41): percent of civilian working age population unemployed; percent female-headed households; percent of population receiving public assistance; percent of population who did not graduate high school; percent of population living in poverty.⁸ *Extreme neighborhood disadvantage* is a dichotomous measure reflecting the top 10 percent of disadvantaged neighborhoods within each city based on the neighborhood disadvantage scale (Krivo &

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Peterson, 1996). Following Clear (2008), moderating effects of formal controls were only expected in neighborhoods where levels of informal social control are already depleted or strained, and so the measure of extreme neighborhood disadvantage was created for the analysis of moderating effects. *Concentrated immigration* (Eigenvalue = 2.49) consisted of three items: the percent of population Hispanic, percent of population foreign born, and percent of population linguistically isolated. *Residential instability* (Eigenvalue = 1.00) consisted of the percent of population age five and over who lived in a different residence in 1995.

Because formal controls may be exercised differentially across neighborhoods within cities, we controlled for other potential confounding effects of neighborhood composition (e.g., Kubrin et al., 2010; Stark, 1987). We included the percent of the population black (*percent black*) and the percent of the population Asian (*percent Asian*). Although the immigrant population in some cities is primarily Asian, preliminary analyses revealed that percent Asian did not correlate strongly with the items that comprise the concentrated immigration scale.

City-level covariates included three measures of formal controls. *Police arrest activity – minor offenses* was measured as the number of arrests for driving under the influence (DUI) and disorderly conduct per police officer (see Sampson, 1986; Wilson & Boland, 1978). *Police arrest activity – violent offenses* was included as a second measure of police arrest activity; this measure was operationalized as the number of arrests for violent offenses per police officer (see Kane, 2006).⁹ Local arrest data for these offenses were obtained from Uniform Crime Reporting Program Data [United States]: Arrests by Age, Sex, and Race, 2000.¹⁰ *Jail incarceration risk* represents the total county jail population per 100 county violent index offenses (see Sampson, 1986). The jail data were obtained through the Annual Survey of Jails: Jurisdiction-Level Data, 2000, which collects information on jails for the year preceding the survey data. Counties that

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were not listed in the survey ($n = 4$) were collected directly from the county jails.¹¹ Consistent with Sampson's (1986) analysis, a few cities were located within the same county and received the same jail incarceration risk measure.

Because city-level factors such as inequality and racial isolation may influence levels of formal controls and crime (Kubrin et al., 2010; Sampson & Wilson, 1995), we controlled for the *Gini index of inequality* for each city and a measure of *Black isolation*.¹² We also considered other measures of racial/ethnic isolation (e.g., Hispanic isolation), but preliminary analyses revealed that they were not related to any of the outcome measures once the other covariates were included in the models, and so they were excluded from our final analyses.

Analytic Strategy

Due to the hierarchical structure of the data (neighborhoods nested within cities), multilevel modeling techniques were used.¹³ Hierarchical linear regression was used for the analysis of robbery and aggravated assault, while homicide was examined with hierarchical Poisson regression. The distribution of homicide was skewed so the correction for overdispersion available in the HLM 7.0 software was used. The logged value of the population for each tract was also included as an independent variable in the analysis of homicide, and the parameter associated with the logged value of tract population was fixed to one (Osgood, 2000).

Unconditional models were estimated for each outcome. These models revealed significant variation in each crime outcome across the cities ($p \leq .05$). The intra-class correlation coefficients indicated that 36 percent of the variation in robbery rates, 31 percent of the variation in aggravated assault rates, and 16 percent of the variation in homicide rates existed across cities compared to 64 percent (robbery), 69 percent (aggravated assault), and 84 percent (homicide) within cities. Next, we estimated random coefficient models in order to assess the effects of the

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neighborhood-level variables on the outcomes, and determine whether the neighborhood effects varied across cities ($p \leq .05$), which would suggest these effects were stronger in some cities versus others. Neighborhood-level effects that did not vary across cities were treated as fixed or having a common slope across cities. All of the level-1 variables were grand-mean centered in order to control for compositional differences in neighborhoods across cities. For the third step of the analysis, the city-level measures were entered permitting an examination of the main effects of formal controls on the level-1 intercepts (neighborhood crime rates).¹⁴ Lastly, intercepts and slopes as outcomes models were estimated in order to examine the effects of formal controls on the relationship between extreme neighborhood disadvantage and violent crime rates.

Results

Before delving into the primary study findings, it is worth noting that the typical neighborhood included in this study was relatively safe in terms of violent crime. Over the three-year period (1999-2001), the median robbery rate was 2.86 per 1,000 persons and the median aggravated assault rate was 4.36 per 1,000 persons. Also, most neighborhoods (49%) did not experience a homicide within the three-year period; the median number of homicides across these neighborhoods was one homicide.

Table 2 contains the main effects of neighborhood characteristics and city-level formal controls on violent crime rates. The results in table 2 show that neighborhood disadvantage, residential instability, and percent black were associated with higher rates of violent crime within cities (robbery, aggravated assault, homicide). Concentrated immigration was associated with higher rates of robbery and homicide, but not aggravated assault. Neighborhoods with higher percentages of Asians had lower rates of all types of violent crime. The significant neighborhood

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characteristics accounted for 46, 67, and 39 percent of the within city variation in robbery, aggravated assault, homicide, respectively.

-- Table 2 about here --

Table 2 also shows that cities with a higher jail incarceration risk had lower rates of robbery and aggravated assault, but not homicide. More specifically, a one-unit increase in the risk of jail incarceration corresponds to a .3% decrease in the robbery rate, while a one unit increase in the risk of jail incarceration corresponds to a .5% decrease in the aggravated assault rate. Neither police arrest activity for minor offenses nor police arrest activity for violent offenses had an effect on violent crime. Cities with a greater degree of black isolation had higher rates of robbery and homicide; however, black isolation did not impact rates of robbery. Altogether, the significant predictors in the model accounted for 61 percent of the variation in robbery across cities, 5 percent of the variation in aggravated assault, and 17 percent of the variation in homicide.

Next, we turned to the examination of the moderating effects of formal controls on the relationship between extreme neighborhood disadvantage and violent crime. Recall from the framework above that formal controls are expected to weaken sources of informal control, but primarily in neighborhoods where these resources are already depleted (Clear, 2008; Clear et al., 2003). Accordingly, we examined the moderating effect of formal controls (police arrest activity and jail incarceration risk) on the relationship between *extreme* neighborhood disadvantage and violent crime, where extreme neighborhood disadvantage reflects the top 10 percent of the neighborhoods within each city based on the neighborhood disadvantage scale. For these analyses, we also control for the main effects of neighborhood and city characteristics; the

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neighborhood-level results are not shown because they were not substantively different from those reported in table 2.

-- Table 3 about here --

The relationship between extreme neighborhood disadvantage and each type of violent crime varied across cities, permitting the analysis of the moderating effects of formal controls (depicted in table 3). The main effects of the city-level characteristics, also presented in table 3, show that the city-level main effects were virtually identical to those contained in table 2. The one difference was the significant inverse effect of jail incarceration risk on homicide rates across cities; cities that used this formal control to a greater extent also had lower rates of homicide.

The analysis of the moderating effects (table 3) showed that the positive relationship between extreme neighborhood disadvantage and violent crime (robbery, aggravated assault, and homicide) was *amplified* by police arrest activity for minor offenses. Police arrest activity for violent offenses did not impact the extreme neighborhood disadvantage effect on robbery, aggravated assault, or homicide. Jail incarceration risk did not moderate the effect of extreme neighborhood disadvantage on violent crime.

Supplementary Analyses

The inclusion of jail incarceration risk in the models raises the possibility of simultaneity because the risk of jail incarceration may not only deter crime, but also might be simultaneously influenced by the crime rate. Such a scenario would contribute to correlated error terms. It is unlikely that simultaneity is a problem for our analyses because: a) we focused on disaggregated offending rates for specific crimes (e.g., it is unlikely that the homicide rate influences the risk of jail incarceration), b) the jail data were collected in advance of the crime data, and c) the jail

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incarceration risk measures are based on county-level data, whereas the relevant crime data are city based (Sampson, 1986). However, to rule out the possibility of simultaneity we adopted a similar approach as Sampson (1986) and re-estimated all of the models using jail capacity per violent index offenses as an instrument for jail incarceration risk. Jail capacity accounted for over 80 percent of the variation in jail incarceration risk. Our analyses revealed no substantive differences in the findings (coefficients, standard errors, and statistical significance were unchanged), suggesting that simultaneity was not a problem here.

Discussion and Conclusions

Informal and formal social controls regulate behavior and control crime within ecological areas. While researchers have explored the impacts of informal controls (Kubrin & Weitzer, 2003; Sampson et al., 1997), less research has examined the impact of formal controls (Kane, 2006; Lynch & Sabol, 2004a; Sampson, 1986; Western, 2006). Following scholars' contentions that there may be both intended and unintended consequences of formal controls (Clear, 2007; Rose & Clear, 1998; Sampson, 1986; Western, 2006), we examined the effects of formal controls (police arrest activity and risk of jail incarceration) on violent crime across cities and whether their effects were detrimental in neighborhoods characterized by extreme disadvantage. We focused on neighborhoods characterized by extreme disadvantage because scholars have argued that those communities are most likely to experience the unintended consequences of formal controls, such as increased crime levels (e.g., Clear, 2007). Our analyses revealed two main findings, which we expand upon below.

First, consistent with results from existing studies (Kane, 2006; Sampson, 1986; Sampson & Cohen, 1988; Wilson & Boland, 1978; Wilson & Kelling, 1982), we found evidence that some formal controls do reduce violent crime, while others do not. Cities with a higher risk of jail

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incarceration experienced lower rates of robbery, aggravated assault, and homicide, but police arrest activity was not associated with reduced city violent crime. Furthermore, our findings are consistent with others (e.g., Sampson, 1986) who have suggested that the risk of being incarcerated in a local jail serves a strong deterrent effect on potential offenders; in the current study, we found that the risk of local jail incarceration was indeed associated with lower rates of violent crimes. As Sampson (1986) explained, local incarceration is much more visible than other types of sanctions (e.g., state imprisonment) and may act as a deterrent due to its more immediate threat of punishment. More research is needed to examine the effects of city jail incarceration since our findings suggest it is an inhibitor of city crime.

Second, our results support scholars' arguments that formal controls have unintended consequences (e.g., Clear, 2007, 2008; Rose & Clear, 1998), specifically higher rates of crime, for extremely disadvantaged neighborhoods. We found that the relationship between extreme neighborhood disadvantage and all types of violent crime was exacerbated by police arrest activity of minor offenses. In order to reduce violent crime in extremely disadvantaged communities, police officers should practice extra caution utilizing their arrest powers, particularly for minor offenses. In these communities, where resources are very scarce and informal social controls are already depleted, arresting suspects may cause more harm to the community by worsening these problems. It is also possible that police arresting for *minor* offenses in these areas erodes residents' trust in and perceived legitimacy of the police, which in turn, can lead to higher crime and violence (e.g., Kirk & Matseuda, 2011). Our findings suggest that police should be aware of the potential for their actions, especially if perceived as unnecessary or trivial, to be counterproductive. Furthermore, police arrest practices should focus

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on the most dangerous suspects in order to reduce violent crime in extremely disadvantaged areas.

Although we were unable to test the impact of formal controls on informal controls within neighborhoods (see Lynch & Sabol, 2004a) due to data limitations, our findings suggest that police arrest activity is associated with higher violent crime within extremely disadvantaged neighborhoods; it could be argued, as Clear and colleagues have theorized, that these results indicate that formal controls disrupt the ability of informal controls within these areas to regulate criminal behavior. Nonetheless, examining the impact that formal controls may have on measures of *informal* controls would provide additional information regarding the much-needed link between formal controls and their effects on neighborhood crime (see Lynch & Sabol, 2004a). The precise mechanisms linking formal controls to community crime remain under-examined (Wildeman & Western, 2010), and future research should consider directly testing the impact of formal controls on neighborhood informal social control agents and capacities.

Returning to our earlier point, our results also confirm researchers' (e.g., Kubrin et al., 2010) claims that controlling for economic and/or social indicators of cities, as well as neighborhood compositional effects, is important when examining the effects of formal controls within and across cities. We found that neighborhood disadvantage, residential instability, and racial demographic characteristics were significantly associated with violent crime rates, and are thus important to include in statistical models when determining the impact of formal controls. Additionally, at the city-level, isolation of black residents was associated with higher robbery and homicide rates. In all, including such controls in our models rendered the effects of police behavior nonsignificant while the effects of jail incarceration remained. The findings also raise

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questions for future research regarding the interplay between cities' allocation of formal controls and racial segregation across cities.

The current study has provided an examination of the intended effects of formal controls across cities and the potential unanticipated consequences of formal controls for extremely disadvantaged neighborhoods. Nonetheless, further research on formal controls is still needed in order to build upon and address the potential limitations of this current study, particularly regarding reciprocal effects of formal controls and crime (e.g., Clear, 2007; Lynch & Sabol, 2004a) and potential curvilinear relationships between formal controls (police behavior and/or incarceration, e.g., Clear et al., 2003; Kane, 2006) and community crime levels. Similarly, it might be that the deployment of formal controls at the neighborhood level, as opposed to the city-level, impacts neighborhood processes differently. We examined formal controls at the city-level in the current study, but future examinations may want to consider this as a potential research avenue (see Kane, 2005). Finally, as noted, we were unable to examine the effects of formal controls on indicators of informal controls, and more research in this area is needed.

Despite these limitations, this study adds to our understanding of the impact of formal controls on violent crime by examining their effects across a large sample of U.S. cities and the neighborhoods within them. Consistent with the few studies that have examined the direct effects of formal controls, we found that forms of public control can be resources that cities can draw upon to regulate and control crime. However, we also uncovered that formal controls can have harmful consequences for some neighborhoods within cities. These findings underscore the importance of tailoring the application of formal controls to the communities they are serving. For most neighborhoods, it seems that incarcerating law violators may complement existing sources of informal control and work to reduce crime levels. In contrast, in extremely

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disadvantaged neighborhoods that are depleted of such resources, applying formal controls, particularly police actions, too vigorously may only further diminish the crime-reducing effects of informal controls. These unintended consequences of formal controls are important to consider, given that crime is disproportionately concentrated in these neighborhoods to begin with. Local stakeholders may want to consider alternative responses to crime in these neighborhoods, such as those that strengthen, rather than deplete sources of informal control.

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Table 1 Description of sample and measures

	Mean	SD	Minimum-Maximum
<u>Outcomes</u>			
Robbery	1.37	.82	0 - 4.84
Aggravated assault ¹	1.69	.85	0 - 4.99
Homicide ²	1.52	2.52	0 - 33
<u>Neighborhood-level variables</u>			
Neighborhood disadvantage	.00	1.00	-1.69 - 4.11
Extreme neighborhood disadvantage	.10	.29	0 - 1
Concentrated immigration	.00	1.00	-4.50 - 4.17
Residential instability	.00	1.00	-3.28 - 4.22
Percent black	25.84	32.81	0 - 100
Percent Asian	4.46	7.29	0 - 92.89
$N_1 =$	9,560		
<u>City-level variables</u>			
Police arrest activity			
Minor offenses	3.37	2.43	.08 - 15.21
Violent offenses	1.44	1.02	.10 - 6.74
Jail incarceration risk	42.74	23.08	10.08 - 146.35
Gini index of inequality	.45	.05	.36 - .58
Black isolation	34.85	26.83	.75 - 90.26
$N_2 =$	90		

Notes: ¹ Based on 8,580 neighborhoods; ² Based on 8,979 neighborhoods

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Table 2 Neighborhood and city-level effects on violent crime

	<u>Robbery</u>		<u>Aggravated Assault</u>		<u>Homicide</u>	
	Coef.	(SE)	Coef.	(SE)	Coef.	(SE)
Intercept	1.34	(.03)	1.66	(.05)	-.18	(.05)
<u>Neighborhood-level</u>						
Neighborhood disadvantage	.45**	(.03)	.58**	(.03)	.63**	(.03)
Concentrated immigration	.12**	(.04)	-.01	(.03)	.18**	(.03)
Residential instability	.12**	(.01)	.11**	(.01)	.05**	(.01)
Percent black	.01**	(.001)	.01**	(.001)	.02**	(.001)
Percent Asian	-.01**	(.002)	-.01**	(.003)	-.01**	(.003)
Proportion of variation within cities	.64		.69		.84	
Proportion of variation within cities explained	.46		.67		.39	
N_1	9,560		8,580		8,979	
<u>City-level</u>						
Police arrest activity						
Minor offenses	-.001	(.01)	-.01	(.01)	.02	(.02)
Violent offenses	.02	(.01)	-.002	(.03)	-.03	(.03)
Jail incarceration risk	-.003**	(.001)	-.005**	(.001)	-.003	(.002)
Gini index of inequality	.92	(.55)	.02	(.83)	1.42	(1.12)
Black isolation	.01**	(.001)	.002	(.001)	.01**	(.001)
Proportion of variation between cities	.36		.31		.16	
Proportion of variation between cities explained	.61		.05		.17	
N	90		78		88	

Notes: Hierarchical linear models predicting robbery and aggravated assault, hierarchical Poisson model predicting homicide.

** = $p \leq .01$; * = $p \leq .05$

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Table 3 Main and moderating effects of city-level formal controls on violent crime

	<u>Robbery</u>		<u>Aggravated Assault</u>		<u>Homicide</u>	
	β	(SE)	β	(SE)	β	(SE)
<u>Level-1 intercept as outcome</u>	1.15	(.02)	1.44	(.05)	-.20	(.06)
Police arrest activity						
Minor offenses	.004	(.01)	-.01	(.01)	.01	(.02)
Violent offenses	.04	(.03)	.01	(.06)	.07	(.07)
Jail incarceration risk	-.004**	(.001)	-.01*	(.002)	-.01**	(.002)
Gini index of inequality	1.22*	(.60)	-.04	(1.12)	1.51	(1.22)
Black isolation	.01**	(.001)	.001	(.002)	.01**	(.003)
Proportion of variation between cities	.36		.31		.16	
Proportion of variation between cities explained	.66		.13		.13	
<u>Level-1 extreme neighborhood disadvantage effect as outcome</u>	.27**	(.03)	.31*	(.03)	.62**	(.04)
x Police activity						
Minor offenses	.02*	(.01)	.03*	(.01)	.06**	(.02)
Violent offenses	-.01	(.03)	-.01	(.03)	.04	(.02)
x Jail incarceration risk	.001	(.001)	.002	(.001)	.001	(.002)
N_2	90		78		88	

Notes: Hierarchical linear models predicting robbery and aggravated assault, hierarchical Poisson model predicting homicide.

** = $p \leq .01$; * = $p \leq .05$

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¹ Some scholars have focused on the consequences of formal controls (primarily imprisonment) for particular social groups (e.g., Clear, 2007; Holzer, Raphael, & Stoll, 2004; Rose & Clear, 1998; Western, 2006; Wakefield & Wildeman, 2011) and have uncovered unintended consequences associated with the use of formal controls (e.g., furthered marginalization) for particular groups of people (e.g., lower class blacks). Our study, in contrast, focuses on community crime levels.

² Throughout this manuscript, we focus on community level crime rates and therefore do not review the extensive literature on police effects on crime at the micro-level (e.g., Eck & MacGuire, 2000).

³ Jails typically serve an entire county; however, in large metropolitan areas such as those examined here, the city and the county are, from a practical standpoint, one and the same (Sampson, 1986).

⁴ Although some scholars recognize potential harmful effects to community and familial systems when offenders are removed from their neighborhoods, others explain the benefits of offender removal. For example, Wakefield and Wildeman (2011) discuss the improvement in a child's wellbeing when the father is arrested and incarcerated for a sexual or violent offense.

⁵ Census tracts have been frequently used to approximate local "neighborhood" context (Kohfeld & Sprague, 1988; Krivo & Peterson, 1996; Krivo, Peterson, & Kuhl, 2009; McClain, 1989).

⁶ The NNCS reported 10.2% of the neighborhoods were missing aggravated assault data and 6.1% of the neighborhoods were missing homicide data.

⁷ As types of violent crime, we expect that formal controls will be inversely related to these outcomes, but because each may involve different means and motives, we also expect that the impact of formal controls might vary across the crime types (Sampson, 1986); thus, we modeled each as a separate outcome.

⁸ Percent of population receiving public assistance was not available in the NNCS and was retrieved from American Fact Finder.

⁹ Kane (2006) excluded rape offenses from his measure of enforcement vigor, while the current study uses all violent offenses as the numerator. We performed the analyses that are subsequently described with the variable measured both ways. No substantive differences in the results emerged.

¹⁰ Arrest data were missing for 15 of the 91 cities in the sample. In these cases, police departments were directly contacted, with a response rate of 93 percent. One city (Topeka, KS) was unable to provide arrest data due to a change in computer operating systems and was subsequently dropped from the sample. Washington, D.C. arrest data were retrieved from the Metropolitan Police Department 2000 Annual Report. Louisville, KY, Memphis, TN, and Seattle, WA arrest data are from year 2001. Florida cities did not report the number of arrests for disorderly conduct; these offenses were included in an "other" offenses category and therefore were estimated using a five-step process. First, we added the number of disorderly conduct arrests to the number of arrests for "other" offenses in all other cities in the sample. Next, we computed the percentage of arrests for "other" offenses that were for disorderly conduct in these cities. Third, we stratified all of the cities into four groups based on city population size. Fourth, we computed the average percentage of arrests for "other" offenses that were for disorderly conduct within each stratum. Finally, we used the within stratum average percentage of arrests for "other" offenses that were for disorderly conduct to derive estimates of each Florida city's number of disorderly conduct arrests from their respective number of arrests for "other" offenses.

¹¹ Larimer County, Colorado jail data were retrieved from the 1999 National Jail Census.

¹² The Gini index of household income inequality = $\sum_{i=1}^n (X_i * Y_{i+1}) - \sum_{i=1}^n (X_{i+1} * Y_i)$, where X_i is the cumulative proportion of households in income category i ; Y_i is the cumulative proportion of household income in income

category i ; and n is the number of income categories. The black isolation index = $\left[\sum_{j=1}^n \left(\frac{b_j}{B} \right) \left(\frac{b_j}{t_j} \right) \right] * 100$, where b_j is

the number of black residents in tract j ; t_j is the total population in tract j ; n is the number of tracts in the city; and B is the total number of black residents in the city.

¹³ In order to avoid a misspecification error, owing to differences in incarceration practices across states (see, e.g., Sampson, 1986), we initially estimated tri-level unconditional models (neighborhoods within cities within states). These models revealed nonsignificant variation across states, which supported the estimation of the bi-level models reported here.

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¹⁴ Our main findings are at the city level, which are not spatially contiguous. Therefore, analyses taking into account spatial autocorrelation were not necessary.