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Abstract

This study reproduces and extends the analyses about the neighborhood-level effects of collective efficacy on criminal behavior originally reported by Sampson, Raudenbush, and Earls in a 1997 *Science* article entitled "Neighborhood and Violent Crime: A Multilevel Study of Collective Efficacy." Based on a 1995 citywide community survey of 8,782 residents in 343 neighborhood clusters conducted as part of the NIJ-sponsored Project on Human Development in Chicago Neighborhoods, they reported that collective efficacy directly affects perceived neighborhood violence, household victimization, and official homicide rates (Sampson, Raudenbush, and Earls 1997). They also reported that collective efficacy moderates the relationship of residential stability and disadvantage with each measure of violence. This study uses Earls, Brooks-Gunn, Raudenbush, and Sampson's (Earls et al. 1997) archived community survey database, archived U.S. Census summary data (United States Department of Commerce 1993) and Block and Block's (2005) archived *Homicides in Chicago, 1965-1995* study to assess the extent to which Sampson, et al.'s (1997) reported results can be reproduced by using

measures and statistical methods specified by Sampson, et al. (1997) and Morenoff, et al. (2001). We then extend the analyses conducted by Sampson, et al. (1997) by adding ten additional years of more detailed crime data in statistical models that address temporal and spatial correlation and multicollinearity. Our findings reproduce the direction and statistical significance of all the key theoretical results reported by Sampson, et al. (1997). In addition, our extension of their analyses finds a direct connection between collective efficacy and rates of homicide and rape from 1995 through 2004. However, we did not find that collective efficacy is negatively related to officially recorded measures of robbery and assaults in 1995, nor is collective efficacy related to most property crimes during any period covered by our study. These latter findings suggest some of the limits to the influence of collective efficacy on crime. Future research should seek to determine the extent to which these limits are valid or due to issues of measurement or to methodological considerations.

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Executive Summary

Neighborhood collective efficacy is an important theoretical and policy-relevant component of contemporary thinking about the causes of crime and the relative value of informal and formal mechanisms of social control. The recent preeminence of collective efficacy stems, in great part, from Sampson, et al.'s (Sampson, Raudenbush, and Earls 1997) *Science* article entitled "Neighborhood and Violent Crime: A Multilevel Study of Collective Efficacy." Their research built upon prior work on social disorganization theory (e.g., Bursik 1988; Sampson and Groves 1989; Shaw and McKay 1942; Toby 1957) to articulate and test several hypotheses about the direct and mediating effects of collective efficacy on official homicide rates and on the perceptions of crime and personal victimizations of Chicago residents. Their analytical tests provide consistent support for lower levels of violent crime in neighborhoods with higher levels of collective efficacy.

Since Sampson, et al.'s 1997 *Science* article, a robust body of empirical evidence generated by Sampson and his colleagues and by other scientists (e.g., Sampson, Morenoff, and Felton 1999; Morenoff, Sampson, and Raudenbush 2001; Browning 2002; Gibson et al. 2002;

Duncan et al. 2003; Cohen et al. 2005; Frankenberg 2004; Maimon and Browning 2010) attests to the contemporary salience of collective efficacy as a factor in explaining differences in levels of criminal behaviors between neighborhoods. Several cities across the United States (e.g., Foster-Fishman et al. 2007; Fox 2006; Gerding 2007) and beyond (e.g., Cerdá et al. 2011; Deuchar 2010) are implementing initiatives to improve the level of collective efficacy across their communities and neighborhoods.

Although research and policy has seen a widespread use and adoption of the underlying concept of collective efficacy, there does not appear to be a detailed critical review of the original research conducted in Chicago during the 1990s, any independent reproduction or verification of that research, or the extension of that research to other crime types or with additional years. The ability of independent scientists to reproduce (and the actual reproduction of) scientific research is an essential component for establishing the reliability of scientific work (Gezelter 2009). When original research can be reproduced, extensions of that research can produce rigorous tests of the temporal or substantive boundaries of the hypotheses and theories being tested. Our reproduction of the Sampson, et al.'s (1997) *Science* article became possible when the National Archive of Criminal Justice Data released the third version of this study for reuse in 2007 (Earls et al. 1997) and with access to independent sources of U.S. Census data not archived by Earls and his colleagues. Our ability to extend their analyses to additional crime types and to additional years stems from our access to additional crime data from the City of Chicago.

The Reproducibility of Sampson, et al. (1997)

There are two main components to conducting a reproduction. First, it is necessary to obtain well-documented data files that include all the cases and variables used in the original research. The second component involves the ability to use those data to generate the same substantive findings by reproducing the data analyses reported. We scrutinized Sampson, et al.'s (1997) article and Earls, et al.'s (1997) study and determined that three components used to conduct their analyses were not available in the archived databases. First, while the archived study included the three central factor scores — concentrated disadvantage, residential stability and immigrant concentration — it did not contain either the census tract-level data used to create these factor scores or summaries of these variables aggregated to the neighborhood cluster level used by Sampson, et al. (1997). Second, the archived study included no measure for their central concept -- collective efficacy. However, in the archived databases are variables for the ten respondent-level questions that Sampson, et al. (1997) used to compute their collective efficacy measure, as well as the summary measures for the two subcomponents of collective efficacy--social cohesion and informal social control. Thus, all the components of collective efficacy are in the archived study file but not the actual measure of collective efficacy.

The third omission from Earls, et al.'s (1997) study is the single measure of the respondent's occupational prestige that was collected via the community survey. Missing that variable, the archived study includes only two of the three measures used by Sampson, et al. (1997) to compute the factor score for the socio-economic status of the respondents. Without these measures, it is not possible to reproduce Sampson, et al. (1997); their work is not reproducible from Earls, et al.'s (1997) archived study.

We overcame these limitations in three ways. First, Earls, et al. (1997) included measures for the two component measures of collective efficacy — social cohesion and informal social control. With some difficulty, we were able to determine how these two measures were combined, and as a result, produced a measure of collective efficacy. Second, from Census tract data archived separately at the Inter-university Consortium for Political and Social Research (ICPSR), we were able to reconstruct summary measures at the neighborhood cluster level for all the demographic characteristics used by Sampson, et al. (1997) to create factor scores for concentrated disadvantage, residential stability and immigrant concentration. We addressed the SES issue by replacing the occupational status measure with a simpler measure of whether the respondent was employed or not. This variable, along with respondents' education and income, was used to create an alternative socioeconomic status (SES) factor score for use in the reproduction.

We used additional data to augment Earls, et al.'s (1997) study in order to produce two separate analyses, both of which successfully reproduced all of the substantive findings reported by Sampson, et al. (1997). Our first reproduction analysis is based on the measures computed at both the respondent and neighborhood cluster levels and provided in Earls, et al.'s (1997) study. The second reproduction analysis is based on augmenting the respondent level data in the PHDCN community survey (Earls, et al. 1997) with two additional data sources: (1) the U.S. Census tract level data (United States Department of Commerce 1993) and (2) an alternative source of homicide reports (Block and Block 2005). Both of these data collections are available from ICPSR. In the second reproduction, the two data sources are used to construct new factor scores and summary measures at the neighborhood cluster level.

The first reproduction more closely adheres to the study archived by Earls, et al. (1997) and reproduces the statistical analyses reported by Sampson, et al. (1997); the second reproduction tests the ability to reproduce the neighborhood-level summary measures from the original data source and uses the newly created summary measures to conduct the statistical analyses reported by Sampson, et al. (1997). The results of this second reproduction are that we were able to produce neighborhood cluster measures for social cohesion, informal social control, concentrated disadvantage, residential stability and immigrant concentration that were highly correlated with their counterparts in the Earls, et al.'s (1997) study. However, because these measures were not perfectly correlated, we produced and report the results following the statistical procedures set out in Sampson, et al. (1997) using both the Earls, et al.'s (1997) study and the augmented data files.

Reproducing the Statistical Analyses in Sampson, et al. (1997)

Using multivariate hierarchical models, Sampson, et al. (1997) reported that the relationships between collective efficacy and officially reported homicides, self reported personal victimizations and perceived neighborhood violence were negative and statistically significant. Moreover, they reported that the addition of collective efficacy to their models mediated the effect of concentrated disadvantage and residential stability on violence rates. For instance, in two models, the size of the coefficient for concentrated disadvantage was reduced; and, in one model, concentrated disadvantage was no longer a significant predictor of violent behavior. In both of our reproduction analyses, we reproduced their substantive results about the influence that neighborhood factors have on the likelihood of violence across the City of Chicago. In particular, we found a significant negative association between a neighborhood's level of collective efficacy and the quantity of violence reported by its residents, as well as the rate of homicides recorded by the police during 1995. We also reproduced their model that explained the variance in collective efficacy across Chicago's neighborhoods, as well as the mediating role that collective efficacy has on the relationship between structural disadvantage and violence rates. The only notable discrepancy between their reported findings and our results was our failure to reproduce the statistically significant, positive relationship they report between the rate of prior homicides (1989-91) and the current (1995) homicide rate. The discrepancy we found in this one relationship does not change the other key relationships in the model. In addition, in the two other regression models that included prior homicide rate as a control variable, we have reproduced the results reported by Sampson, et al. (1997). In both of these models, the 1989 to 1991 homicide measure was unrelated to the survey respondents' reported level of 1995 household victimization or perceived violence in the neighborhood.

Extending Sampson, et al.'s (1997) Analyses to other Crime Types and Years

Our ability to reproduce all of the substantive findings reported by Sampson, et al. (1997) is a major testament to the quality of their work and a major asset to our plan to extend Sampson, et al.'s (1997) analyses to additional crime types and to additional years. Using similar statistical procedures employed by Sampson, et al. (1997) and Morenoff, et al. (2001) and after adding additional data on homicides and nine other types of crimes reported to the Chicago Police Department for the years 1995 through 2004 to the analyses, we found that the effects of collective efficacy and other substantive relationships identified by the original analyses extend beyond the year 1995. However, we also found that the collective efficacy effect first reported by Sampson, et al, (1997) is not universal. While the majority of the collective efficacy - crime rate coefficients are in the negative direction (i.e., neighborhoods with more collective efficacy have less reported and recorded crime), in less than one-half of the tests we conducted did these coefficients reach the traditional level of statistical significance (i.e., p-value < 0.05). The remaining one-third of the coefficients either approached zero in size or are positive in their direction (i.e., neighborhoods with more collective efficacy have more police recorded crimes).

Across our results, there are several specific variations worth noting. First, we find that collective efficacy is consistently related to the rates of both homicide (as well as murder) and rape. Whether we used just the 1995 crime counts or the average of the 1995 to 1999 crime counts, neighborhoods with more collective efficacy in 1995 have fewer recorded homicides and rapes per resident than do neighborhoods with less collective efficacy. We also find that the neighborhood-level measure of collective efficacy is negatively related not only to the summary measures but also to each question that asked about household victimization or perceived violence.

These consistent negative relationships initially reported by Sampson, et al. (1997) and that we have reproduced, do not extend consistently to other forms of police recorded violent crimes (e.g., robbery and assaults), nor does it extend to all measures of property crimes across the City of Chicago. Moreover, we find that summary measures of all residential property crime and all violent crime occurring during the late 1990s were not significantly related to the level of collective efficacy. In addition, we found that more collective efficacy in a neighborhood in 1995 was not positively correlated with more rapid reductions in crimes over a ten-year period. In fact, among the handful of crimes that were related to collective efficacy in 1995, we found that homicide and rape did not go down as quickly in the neighborhoods with the greatest degree of collective efficacy.

Conclusions

We chose to reproduce and extend the work of Sampson, et al. (1997) because the theories tested in this work are central to criminology, because of the high quality and high visibility of this research, and the apparent utility of their findings. The tenets of science require that research findings be reproducible and that independent scientists can, in fact, use the original data to reproduce important published findings. The history of criminology is replete with examples of major findings that could not be reproduced (Maxwell and Garner 2009), or studies with serious methodological limitations that make them ineligible for reproduction.

This study overcame the limitations of the archived data and produced two separate reproduction analyses; one based on the PHDCN archived data and the other based in part on the archived data and on alternative sources of data from the U.S. Census Bureau and the City of Chicago. Both analyses reproduced all the substantive findings reported by Sampson, et al. (1997) on the relationships between collective efficacy, concentrated disadvantage, residential stability, immigrant concentration and three measures of crime in 1995. Such consistent support for the original analyses is typically not the result reported by published accounts of reproduction efforts (e.g., Blumstein, Cohen, and Gooding 1983; McCrary 2002; Garner and Maxwell 2008; Visher 1986). Our findings should be interpreted as confirming the high value and quality of the work of Sampson and his colleagues.

Sampson, et al. (1997) did not make any assertion about the extent of collective efficacy beyond the tests they reported for 1995. Our tests showed that collective efficacy does extend to other crime types and for additional years. This is an important extension of our understanding of the scope and depth of collective efficacy effects. Similarly, our findings also show that collective efficacy measured in 1995 does not extend to all crime types for 1995 or for future years. These findings suggest that there are limits to collective efficacy. Of course, these findings may also be due to issues of measurement or to other analytical shortcomings.

There are unknown amounts and sources for measurement error in official crime statistics in Chicago and elsewhere (MacDonald 2002; Skogan 1974, 1981; Zedlewski 1983). With these reproductions and extensions of Sampson, et al. (1997), our use of the collective efficacy concept will be enhanced if we can identify and confirm measurement limitations, if we can measure changes in collective efficacy in neighborhoods over time, and if we can de-construct the components of collective efficacy to separate out, if possible, the active from the inactive components. We have confirmed that the effects of collective efficacy are real. Future research needs to determine how valid, for which types of crime, and what are the costs and benefits of making changes in collective efficacy as a means to reduce crime.

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Collective Efficacy and Criminal Behavior in Chicago, 1995 – 2004

Neighborhood collective efficacy is an important component in contemporary theoretical thinking and policy considerations about the causes of crime, particularly the roles of social cohesion and of informal social control in crime causation (Sampson, Raudenbush, and Earls 1998). Collective efficacy emphasizes links between cohesion or mutual agreement, social trust, and shared expectations or values, and the willingness of neighborhood residents to act in support of these values to address a "specific task" such as neighborhood safety (Sampson 2004). Sampson (2004) argues that the "key casual mechanism in collective efficacy theory is social control enacted under conditions of social trust." This hypothesis – that social cohesion among neighbors combined with a willingness to intervene can influence crime rates – is grounded in Shaw and McKay's (1942) theories of community sources of juvenile delinquency (Triplett 2007). Their perspective connects residential stability, concentrated disadvantage, and ethnic diversity with neighborhood social organization and the capability to address criminal behavior. The concept of collective efficacy builds upon the more systematic contemporary hypothesis about how social disorganization and crime are casually linked (see Bursik and Grasmick 1993; Browning 2002; Elliott et al. 1996; Elliott et al. 2006).

Bandura's (2000) review of research in a variety of areas – sports teams, business organizations, educations systems, combat teams, as well as urban neighborhoods–concludes that "the higher the perceived collective efficacy within a group, the higher the groups' motivational investment in their undertakings, the stronger their staying power...., and the greater their performance accomplishments." Collective efficacy also has implications for crime control policy. For instance, both community policing and community crime prevention programs often operate with implicit underlying assumptions about how these formal government programs encourage and ultimately depend upon community mobilization and organization for the purposes of crime control (Cancino 2005; Kochel 2009; Sampson 2004; Serewicz 2009; Wells et al. 2006).[1]

Articulating and Testing the Effects of Collective Efficacy on Crime

Two articles have presented the central case for the role of collective efficacy in reducing criminal behavior, namely Sampson, et al.'s 1997 *Science* article, and Morenoff et al.'s 2001 *Criminology* article (Sampson, Raudenbush, and Earls 1997; Morenoff, Sampson, and Raudenbush 2001). These publications articulate a consistent position on the direct as well as moderating effects of collective efficacy on violent behavior and provide consistent results on tests of hypotheses about the correlates of collective efficacy and its effects on homicide.

Sampson, et al. (1997) asserts that a neighborhood's collective efficacy is connected directly and indirectly to the reduction in crime. They articulated and tested these and related hypotheses using data from a 1995 survey of 8,782 Chicago residents in 343 neighborhood clusters. These data were collected as part of the Program in Human Development in Chicago Neighborhoods (PHDCN), co-sponsored by the National Institute of Justice and by the John D. and Catherine T. McArthur Foundation.[2] These and other PHDCN data are distributed by the National Archive of Criminal Justice Data (NACJD) at The University of Michigan's Inter-

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[1] The role or prominence that collective efficacy plays in controlling crime has even invaded the *Facebook* milieu. There is now a *Facebook* "blog that discusses how collective efficacy helps to reduce crime levels in urban neighborhoods. This blog provides links to recent articles, journals, and opinions that describe both urban violence and collective efficacy." http://apps.facebook.com/blognetworks/blog/collective_efficacy_combats_urban_crime/opinions

[2] See http://www.icpsr.umich.edu/icpsrweb/PHDCN/about.jsp for further details about the Program in Human Development in Chicago Neighborhoods (PHDCN).

university Consortium for Political and Social Research (ICPSR).[3]

In their analyses of neighborhoods and violent crime, Sampson, et al. (1997: 920) used individual level responses to ten survey questions to construct two Likert scales called "informal social control" and "social cohesion," and then combined these "into a summary measure labeled collective efficacy." Using this summary measure, they then constructed and used a measure of collective efficacy for each neighborhood after adjusting for the composition of the informant sample with respect to eleven individual characteristics in a three level model (pp. 920-21).

Sampson, et al. (1997: 921) also created two measures of violence from the community survey. First, respondents were asked about their perceptions of violence in their neighborhood in the past six months, and second, if they or anyone in their family had ever been a victim of violence in the neighborhood. A third measure of neighborhood violence was derived from data on homicides for 1995 recorded by the Chicago Police Department. They also constructed three neighborhood factor measures – concentrated disadvantage (CD), immigrant concentration (IC), and residential stability (RS) – using the 1990 U.S. Census summary data for their 343 neighborhood clusters (Sampson, et al. 1997). These measures were computed using oblique rotated factor patterns from ten census characteristics.[4] Sampson, et al. (1997; table 3, p. 921) found that four individual level characteristics – being a home owner, mobility, age, and socio-economic status – and all three neighborhood-level measures were statistically significant predictors of their measure of collective efficacy.

The main tests for the effects of collective efficacy on neighborhood violence are

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[3] See http://www.icpsr.umich.edu/icpsrweb/NACJD/studies/2766/detail.

[4] The census characteristics are below poverty line, on public assistance, female-headed household, unemployment, aged under 18, Black, Latino, Foreign-born, same house since 1985 and owner-occupied house.

reported in their table 4 (p. 922). Three models were tested: two for each of the survey based measures of neighborhood violence (perceived violence and experienced violence); and one for the rate of recorded homicides in 1995. The model for perceived violence was a three-level HLM (with perceived violence scale in the first level, the eleven individual level characteristics in the second level, and neighborhood factors — CD, IC and RS in the third level). The model for experienced violence was a two-level HLM (with the eleven individual characteristics and the victimization question in the first level and neighborhood factors — CD, IC and RS in the second level). Homicide rate was a one-level regression model of the neighborhood factors. In Table 4, Sampson, Raudenbush and Earls (1997) reported effects of each of the three neighborhood factors across the three models of neighborhood violence. The results show that eight of nine tests were statistically significant and in the predicted direction. Only the quantity of immigrant concentration (IC) was not a statistically significant predictor of 1995 homicides. When Sampson, et al. (1997) added their measure of collective efficacy to these three models, they reported finding a direct negative effect for collective efficacy on all three measures of violence. In addition, based upon reductions in the size of the unstandardized coefficients for concentrated disadvantage (CD) and immigrant concentration (IC), they asserted that collective efficacy partially mediates their effects on violence. In three final models that control for prior rates of homicide, concentrated disadvantage and collective efficacy remained statistically significant predictors of all three measures of neighborhood violence. Sampson, et al. (1997) concluded that with adjustments for measurement error, neighborhood composition, social disorganization and prior violence, the consistent direct and indirect effects of collective efficacy remain a robust predictor for lower rates of violence.

Using much of the same data and analytical approaches, Morenoff, et al. (2001) provides additional tests of the robust nature of collective efficacy as a predictor of neighborhood violence and as a mediator for the effects of concentrated disadvantage in Chicago. Morenoff, et al. (2001) tested six models of the effects of collective efficacy using the mean of the 1996 - 1998 incident level homicide data from the Chicago Police Department. These same six models were also tested using just the 1996 vital statistics data on homicides from the Chicago coroner's office.

In these models, Morenoff, et al. (2001) replicated the measure of collective efficacy used by Sampson, et al. (1997) and retained their measures of concentrated disadvantage, concentrated immigration, and residential stability. Their analyses did not include individual level predictors, but they added two neighborhood-level control measures – adults per child and population density – as well as three measures of the community survey respondents' participation in voluntary associations, community organizations, and their kin and friendship ties in the neighborhood.[5] In addition, Morenoff, et al. (2001) inserted a measure of spatial proximity, which captured the spatial exposure to both measured and unmeasured characteristics of nearby neighborhoods. The final addition to the models in Morenoff, et al. (2001) is the introduction of an alternative measure of concentrated disadvantage. This measure is an index of the concentration in economic status (ICE) at the extremes of both affluence and poverty.

The results of Morenoff, et al.'s (2001) analyses are similar to those reported by Sampson, et al. (1997) in terms of the direct and indirect effects of collective efficacy. In eight tests, the relationship between collective efficacy and future homicide rates was negative and statistically significant. In addition, the coefficients for concentrated disadvantage were reduced

[5] Morenoff, et al. (2001: 527) describes the use of empirical Bayes residuals for all of the key survey based predictors as a method to correct for bias in regression coefficients with measurement error

but still statistically significant in models that included collective efficacy. The coefficients for concentrated immigration were not only reduced but were no longer statistically significant when measures of collective efficacy were introduced. Thus, in these analyses, collective efficacy continued to directly influence future homicides and also mediated the effects of concentrated disadvantage and concentrated immigration.

The analyses in Morenoff, et al. (2001) showed no effects on future homicides for the ratio of adults to children in a neighborhood but consistent negative and statistically significant effects for population density. All of the tests for the impact of spatial proximity show statistically significant effects in the predicted direction. None of the tests for the effects of participation in voluntary associations, number of organizations or the extent of kinship or friendship ties in the neighborhood showed any effect on future homicide rates.[6] Rate of prior homicides was statistically significant in all tests that used police data but in only one test that used vital statistics data.

Scientific Standing of Articles on Collective Efficacy

The findings reported in Sampson, et al. (1997) and Morenoff, et al. (2001) employed rigorous research designs, involved multivariate and multiple level tests of social disorganization theory, and have implications for community-based programs like community policing and community based crime prevention programs (Sampson and Morenoff 2004; Sampson 2004). Moreover, the findings of consistent spatial effects on neighborhood-level homicide rates adds to the growing evidence about the interdependence of contiguous neighborhoods and the significance of spatial dynamics in our understanding of the criminology of places.

[6] Additional analyses reported in Morenoff, et al. (2001) show that these three considerations do predict higher levels of collective efficacy

These two articles already have established standing in social research. As of March 2011, a total of 244 publications have cited Morenoff, et al.'s (2001) *Criminology* article. This is a substantial accomplishment given that a typical scientific article is never cited. The scientific impact of the Morenoff, et al.'s (2001) article, however, pales in comparison to the impact of Sampson, Raudenbush and Earls' (1997) *Science* article. This article had been cited in 1,853 peer reviewed papers, theses, books, abstracts, or other scholarly literature.[7] By comparison, the widely touted report of the NIJ-sponsored Minneapolis Domestic Violence Experiment by Sherman and Berk (Sherman and Berk 1984) has been referenced in 366 such scholarly works.[8]

The Importance of Reproduction for Science

The primary building block of this project are the published analyses reported in Sampson, et al. (1997) using data generated by the Project on Human Development in Chicago Neighborhoods (Earls and Buka 1997) and deposited for continued use at the National Archive of Criminal Justice at the University of Michigan (Earls et al. 1997). The reproduction of published findings by independent researchers is one of the expectations of the solicitation that funded this project (Justice 2007), the establishment of the NIJ Data Resources Program in 1976 (Garner 1981), and contemporary standards for research quality asserted by the National Academy of Sciences (Fienberg, Martin, and Straf 1985) and the American Association for the Advancement of Science.

Secondary data analysis (Hyman 1972; Bryant and Wortman 1978; Boruch, Sordray, and Wortom 1981; Cordray and Orwin 1983; Hedrick, Boruch, and Ross 1978) is a research method

[7] By March 2001, Google Scholar identified 3,427 articles that had cited Sampson, et al. (2007)

[8] The source of the citation counts is Thomson Reuter's *ISW Web of Knowledge*.

that uses some or all of the raw quantitative data from one or more prior studies to reproduce and perhaps build upon the originally reported analyses. Secondary data analysis is commonplace in the field of criminology. For instance, of the 20 articles published in the premier criminological journal *Criminology* between November 2006 and May 2007, 18 involved quantitative data analysis and only three of these articles involved new data collections. The other eleven quantitative articles were secondary analyses of previously collected and previously analyzed data. While none of these articles was a reproduction of prior analyses, secondary data analysis is a frequent method for advancing criminological thought.

In contemporary social research, the cost of data collection far exceeds the cost of analyzing data and disseminating research findings, and one of the goals of the National Institute of Justice's Data Resources Program is to increase the number and quality of analyses that can be produced with the limited financial resources available. The requirement that data from NIJ funded research be shared with the larger criminological community enhanced the rationale for NIJ's large long-term investment in the Project on Human Development in Chicago Neighborhoods. The allocation of a substantial proportion of the National Institute of Justice's research budget to one research team is more defensible when the benefits of that investment are shared beyond a single project team.

Reproduction is a form of secondary data analysis that is exclusively concerned with the exact production of previously generated empirical findings. Research that explicitly involves only the reproduction of prior analyses is not as widespread in scientific journals as other types of secondary data analyses but, in the field of criminology, there a several prominent examples of reproductions including, Blumstein, et al.'s (Blumstein, Cohen, and Gooding 1983) critique of Carlson, et al.'s (Carlson et al. 1980) assertions about the effect of prison capacity on prison population; or Visher's (Visher 1986) re-analysis of Chaiken and Chaiken's (Chaiken and Chaiken 1982) and Greenwood's (Greenwood 1982) inmate surveys which identified significant limitations that challenged the validity of the original estimates of offender crime commission rates and the incapacitation effects of imprisonment. More recently, errors identified by McCrary (McCrary 2002) demonstrated that Levitt's (Levitt 1997) assessment that increases in the number of police officers substantially reduced crime disappeared when those errors were corrected.

While the examples cited above show the power of reproduction to identify and correct errors in prior research, Vandaele's (Vandaele 1978) reproduction of Erhlich's (Ehrlich 1972) analysis of the deterrent effects of the criminal sanctions confirmed and thus enhanced the original author's calculations and conclusions. Sampson and Laub's (Sampson and Laub 1993) multivariate analysis of the data collected by the Gluecks (Glueck and Glueck 1950) upheld many of the substantive findings the Gluecks obtained through bivariate analyses.

There are two other benefits derived from conducting a reproduction. First, secondary analysis can itself include misunderstandings because the secondary analysis failed to fully understand the published article, the nature of existing data , or the complexities of the original analytical procedures. However, regardless of the source of these misunderstandings, the rigor imposed by the attempt to reproduce published findings invariably improves the secondary analyst's knowledge of the original publication.

Second, a successful reproduction provides a sound basis for any attempt to improve or extend the published analyses. Without a successful reproduction, secondary analysts cannot easily assert that the similarity or dissimilarity of their findings from the original publication stem from the newly proposed extensions. Thus, a successful reproduction can serve as a rigorous baseline against which the results of any extension analysis can be compared (e.g., (e.g., Rabe-Hemp and Schuck 2007).

Reproducing and Extending Sampson, et al. (1997)

Our assessment of the analyses published by Sampson, et al. (1997) and Morenoff, et al. (2001) is that they are rigorous tests of important criminological theories that bear directly on policy issues facing American criminal justice policymakers. The published analyses are sophisticated and complex. They involve technical adjustments for missing data and elaborate enhancements to traditional multivariate analyses to address the use of both individual level and neighborhood-level data. These analyses are products of a significant NIJ investment and have already had a widespread impact on the scientific community.

As acknowledged by the authors, their analyses have limitations. For instance, although the theory is dynamic–changes in collective efficacy lead to changes in crime–the data are cross-sectional, not longitudinal. In addition, these analyses are based on data from one city at one point in time and both of these considerations may limit the generalizability of the published findings. In this project, we cannot address either of these limitations but our detailed review of these two articles suggests that there may be other limitations to accepting these analyses as the definitive assessment of the role of collective efficacy in reducing violence.

We have identified two areas that we think are the most important potential limitations to these analyses – the measurement of violence and changes in violence over time. Therefore, an essential element of our design involves bringing new data and new analytical approaches to bear on these two issues. However, to accomplish this, our design calls first for the reproduction of the analyses about the role of collective efficacy in the published articles by Sampson, et al.

(1997) and Morenoff, et al. (2001). Thus, our design involves the combination of reproducing the original analyses from archived data and data documentation, and the extension of these analyses using additional measures of violence and other criminal behaviors that cover an extended period of time.

Measuring Violence

The measure of official violence used in these studies is restricted to a well measured but rare form of violence--homicide. All survey questions used to generate measures of collective efficacy, except for one, speak to the willingness by neighborhood residents to address *less severe* forms of criminal behavior, but none of the available analyses incorporates other measures of violence from official crime statistics besides homicide. Sampson, et al.'s (1997) use of only one official measure of violence and the limitation to just a one-year period may result in overestimating or underestimating the effect of collective efficacy on violence or as a mediator for concentrated disadvantage.

Our research team has access to crime data for the City of Chicago from 1990 through 2004 aggregated to the census tract level. We use these data to retest the role of collective efficacy: to influence several violent crime rates – homicide/murder, rape, robbery, and aggravated and simple assault; to influence measures of victimization or perceived violence held by the respondents that were not used in the original articles; and, to influence property crimes. These additional analyses will permit us to assess the extent to which the findings produced by Sampson, et al. (1997) and Morenoff, et al. (2001) are generalizable to a greater variety of officially recorded and respondent reported violence. We will first conduct these analyses using the methods and measures used in the original analyses. We will then expand the measure of violence by including incidents from a longer period and by using the capabilities of *HLM* to model the patterns of crime rates over an entire decade. The use of these alternative measures is expected to introduce more measurement error into the dependent variables but it will address the measurement problems created by the infrequency of homicide in most of the 343 neighborhood clusters in Chicago.

The most difficult part of this effort is in reproducing the six multivariate multi-level models reported in Sampson, et al. (1997). First, there are always difficulties inherent in reproducing someone else's published analyses. Second, the failure to reproduce is often difficult to diagnose. Failure to reproduce is co-produced by the lack of specificity in the original analyses, space limitations and typographical errors in publications, incomplete data documentation, and the capabilities of the secondary analyst. Despite the crisp and concrete language of both articles, neither of these articles includes the traditional descriptive statistics that can be useful in determining that we are, in fact, using the specific measures used in each of the multivariate models. In addition, while the archived data include the raw survey responses and the factor scores, they do not include the collective efficacy measure adjusted for measurement error by a three level HLM model.

Of particular concern was the construction of the adjusted measure of collective efficacy. Sampson, et al. (1997) describe in detail the role of missing data in computing this measure but do not report how many responses are missing. According to Earls, et al. (1997; 52-53), the individual items from which this measure was created are missing in as many as 18 percent of the community surveys. In addition, eight percent of the responses in the violence victimization questions are also missing responses (p. 74). These are not insurmountable problems but an indication that there are few benchmarks for reproducibility prior to the production of the multivariate, multi-level models. Nevertheless, several other related articles (i.e., Raudenbush and Sampson 1999; Raudenbush and Sampson 1999; Sampson and Raudenbush 1999) provided descriptions of the methods used to address missing data and to transform variables for use in multivariate models. This assisted us in the effort to reproduce substantive findings.

Methods

While examples of secondary analyses, reproductions and replications abound, we find little in the way of textbooks or descriptions of what such efforts should and would not entail. While secondary analyses involving investigations of entirely new hypotheses have their own internal logic, we determined that our design for reproducing and replicating the results produced by Sampson, et al. (Sampson, Raudenbush, and Earls 1997) required a detailed understanding of the published analyses, a comprehensive understanding of the archived data, a harmonizing of archived data with alternative data sources, and a matching of measures and regression methods that were used to generate the published findings. These are also the same steps we employed while completing another similarly designed National Institute of Justice-sponsored project entitled "*The Crime Control Effects of Prosecuting Intimate Partner Violence in Hamilton County, Ohio: Reproducing and Extending the Analyses of Wooldredge and Thistlethwaite*" (see Garner and Maxwell 2008). Below we describe in detail the steps we took to meet our own requirements, starting with an explanation of the various data collections we acquired for this project, followed by a detailed roadmap for how each measure (both dependent and independent) was identified and reproduced, and finish with descriptions of the applicable regression model. Also included in this section is a description of how we merge in additional data, and conduct further analytical steps to produce our "extension analysis."

Data Sources

The primary sources of data for this study are two databases produced by the Project on Human Development in Chicago Neighborhoods (PHDCN) study and archived by the National Archive of Criminal Justice Data (Earls et al. 1997). These two files are part of the Inter-university Consortium for Political and Social Research (ICPSR) study no. 2766 entitled *Project on Human Development in Chicago Neighborhoods: Community Survey, 1994-1995*. The Archive acquired the first version of these data in 1997, and has twice updated the database since first releasing them in 1999. The Archive released the third version of the study in 2007, which is the version utilized for this project.[9] Version three of this study was initiated when the Archive had acquired and processed the second of the two parts of this study (Earls et al. 1997).

Part (i.e., database) 1 of this ICPSR study contains respondent level information collected through face-to-face household interviews of residents over the age of seventeen. The interview team selected the applicable household using a probability-based sampling scheme that included the population of households that fell within 343 researcher-defined neighborhood clusters. Within each sampled household, the interview randomly selected a household respondent among the adult residents (Raudenbush and Sampson 1999). This part contains 8,782 respondent records and 383 variables. The Earls, et al. (Earls and Buka 1997) research team designed the community questionnaire/instrument to gather data from respondents about themselves and about their neighborhood, including the structure of their communities, organizational and political

[9] Besides using the PHDCN data available in ICPSR study no 2766, the former NACJD director and this project's principal investigator produced for this project a census tract to neighborhood cluster level crosswalk database to facilitate merging and aggregation of additional crime and census data.

association, cultural values, informal and formal social controls, and social cohesion. The instrument included measures of perceived crime and violence in the community, ratings of social order (gang activity, graffiti, and unruly teens), normative beliefs about violence, and crime-specific indicators of victimization, available resources, norms, and social organization. Other community variables measure the relationships among neighbors, including how many neighbors a respondent would recognize, how often neighbors socialized, and how often neighbors participated in other activities together. Variables that capture neighborhood social order include respondents' perceptions of neighborhood problems such as litter, graffiti, drinking, drugs, and excessive use of force by police. Respondents were also asked about their normative beliefs regarding violence, money, and various children's behaviors. Victimization variables cover how often the respondent was the victim of a fight with a weapon, a violent argument, a gang fight, sexual assault, robbery, theft, or vandalism. Other variables measure fear of crime and attitudes toward the police. Demographic variables include age, sex, education, residential situation, national origin, and employment status. The total number of respondents in the database is 8,782 (Earls et al. 1997). Raudenbush and Sampson (Raudenbush and Sampson 1999) reported a response rate of 75 percent. For their article entitled “Neighborhood and Violent Crime: A Multilevel Study of Collective Efficacy,” Sampson, et al. (Sampson, Raudenbush, and Earls 1997) reported using “the 7,729 cases that have sufficient date for all models estimated” (p. 924). While this database contains no variable that “flags” this subsample of cases, our approach is to identify the cases in this subsample by using one of the many researcher-constructed scale variables that have valid data for the same number of cases.

Part 2 of ICPSR study no. 2766 includes data that represent the aggregation of raw data from the community survey (i.e., Part 1), measured using the 1990 U.S. Census data and several homicide counts. More specifically, the community survey records were aggregated to the 343 Chicago neighborhood clusters (NC) created for the PHDCN. Most of the neighborhood-level variables were derived from a multi-step process used to aggregate the community survey respondent reports to the neighborhood cluster level. Raudenbush and Sampson (Raudenbush and Sampson 1999) describe thoroughly the process used to combine the variables and simultaneously aggregate the respondents' data to the NC level. To construct additional variables for these 343 records, Sampson, et al. (1997) aggregated the 1990 U.S. Census summary variables to the NC level and then combined them using the alpha-scoring factor analysis method. The Part 2 data file contained just the factors produced from the census variables; this file did not contain the original census tract level summary variables or the variables Sampson, et al. first produced by pooling select census summary fields before combining them into their factor analysis. This part also contains several measures of officially reported crime also aggregated to the neighborhood cluster level. One of crime variables included in the data files was the homicide measure that we believe Sampson, et.al used as one of their three key dependent measures. [10] While Sampson, et al. (1997) describe this variable as "1995 homicide counts" (p. 922), they also describe transforming it into "the homicide rate per 100,000 people in the neighborhood" (p. 922). The nature of this variable is further clarified in their *Science* article when they specify that their homicide measure only included incidents that occurred during the "months of the community survey" (p. 924). Sampson, et al. (1997) also describe using "the 3-year average homicide rate in 1988, 1989, and 1990" (p. 922) in their three regression models to address "possible confounding effects of prior crime" (p. 922). We did not

[10] They also included a logged transformed homicide rate for 1995 (LHOMR95), a logged transformed rate for the 1990 homicide rate (LHOMR90), and the number of murders in 1995 (MURDER95).

initially identify such a variable in this Part; however, we referenced an authoritative source on homicides in Chicago (see Block and Block 2005) to determine whether the variable named LHOMR90 is the three-year average homicide rate. After comparing the frequency distributions of LHOMR90 to one produced using a variable we constructed from this alternative source of data, we determined that LHOMR90 is likely the 3-year average homicide variable that Sampson, et al. used.[11] Thus, the most recently released data from the Program in Human Development in Chicago Neighborhoods provided most but not all the information we needed to reproduce independently the analyses reported in Sampson, et al. (1997). Fortunately, as described below, data needed to reproduce all of the analyses reported by Sampson, et al. (1997) are readily available in two other data collections archived by the National Archive of Criminal Justice Data.

The third source of data used in our study is ICPSR's study no. 6054, which provides the summary count data produced by the U.S. Department of Commerce's Bureau of the Census for the 1990 Census of Population and Housing. [12] This data file contains counts of attributes across the entire United States, sample data weighted to represent the total population, and 100-percent counts and unweighted sample counts for total persons and total housing units. Additional population and housing variables include age, ancestry, disability, citizenship, education, income, marital status, race, sex, travel time to work, rent, tenure, value of housing unit, number of vehicles, and monthly owner costs. While the U.S Census distributes these data at several

[11] The ICPSR study no. 6399 database had 19 more incidents during the 1988 to 1990 period, but that difference could have been caused by subsequent geo-coding of some incidents that had not spatial referenced when Sampson, et al. (1997) accessed these data.

[12] U.S. Dept. of Commerce, Bureau of the Census. Census of Population and Housing, 1990 [United State]: Summary Tape File 3C [Computer file]. Washington, DC: U.S. Dept. of Commerce, Bureau of the Census [producer], 1992. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 1993. doi:10.3886/ICPSR06054

levels of geographical aggregations, we selected the records that represented the census tracts that are within the borders of the City of Chicago. We then identified the census variables used by Sampson, et al. (1997) and used the neighborhood clusters identification numbers created by Earls, et al. (1997) to aggregate these variables to the NC level. We aggregated the census variables by summing their values. These data were then merged into neighborhood cluster level (i.e., Part 2) database archived by Earls, et al. (1997). We use these data to create the neighborhood social-structural factors used by Sampson, et al. (1997) and provided in Earls, et al. (1997).

The fourth source of data used in this project came from Carolyn Rebecca Block and Richard L. Block's *Homicides in Chicago, 1965-1995* study (ICPSR study no. 6399).[13] Their study contains information about every homicide that occurred between the years 1965-1995 and documented in the Chicago Police Department's murder analysis file. For our project, we used data stored at the victim level which contains one record for each homicide victim. Some of the incident attributes coded by Block and Block (2005) include the relationship of the victim to offender, time of occurrence and place of homicide, type of weapon used, cause and motivation for the incident, whether the incident involved drugs, alcohol, gangs, child abuse, or a domestic relationship, if or how the offender was identified, and information on the death of the offender(s). Demographic variables such as the age, sex, and race of each victim and offender are also included in each victim record. We merged into this database the PHDCN NC identification number by tract to link these homicide data to the other measures aggregated to the

[13] Block, Carolyn Rebecca, Richard L. Block, and Illinois Criminal Justice Information Authority. Homicides in Chicago, 1965-1995 [Computer file]. ICPSR06399-v5. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2005-07-06. doi:10.3886/ICPSR06399.

neighborhood cluster level.[14] We use these data to replicate the average homicide rate from 1988 to 1990 that Sampson, et al. (1997) reported using in their regression models.

With these four sources of data, we have all data needed to reproduce the analyses reported in Sampson, et al. (1997). However, our goal was to reproduce and to extend their analyses to additional crime types and to additional time periods. To accomplish this goal, we added additional crime count information from an independent source.

The fifth source of data used in this project are official recorded, incident level, geo-coded crimes reported in Chicago from 1990 through 2007. For the purposes of this project, we constructed a new database that contained a count of incidents within each census tract for ten select crimes for each of the 18 years. The incident types we included in this summary database are homicide, murder, sexual assault, robbery, aggravated assault, simple assault, burglary, theft, auto theft, and vandalism.

Measures

Selection and Production of the Dependent Measures

Sampson, et al. (1997) focused on modeling three dependent measures or violent crime "outcomes" (p. 922). They collected data for two of the three dependent measures using their community survey. These two measures are the respondent's "perceived neighborhood violence" and their reported prevalence of each household's "violent victimization." They are both found in Part 1 of ICPSR Study 2766 (Respondent Level). The former measure is named "*PVIOLNCE*," and the later measure is named "RVICT6MO." The third measure is the 1995 homicide counts (HOM). The City of Chicago Police Department (CPD) collected this third

[14] Sampson, et al. (1997) reported receiving their homicide data from the Richard Block as well.

outcome measure, and supplied it to Dr. Richard Block who in turn provided it to Earls, et al. (1997) for use in their PHDCN study.

The respondent's perceived violence measure was produced by combining the answers to five questions. Using a scale that ranged from "often" to "never", the PHDCN interviewers asked each respondent the following five questions: (*Q30A*) In the past six months how often was there was there a fight with a weapon; (*Q30B*) In the past six months how often was there a violent argument between neighbors; (*Q30C*) In the past six months how often was there a gang fight; (*Q30D*) In the past six months often how was there a sexual assault or rape; and (*Q30E*) In the past six months how often was there a robbery or mugging. The original response set for these questions was: (1) often, (2) sometimes, (3) rarely, and (4) never. Sampson, et al. (1997) reordered the response set of these variables such that high scores equaled the value "often" and low scores equaled the value "never." They then "pooled" these five answers into a single measure [*PVIOLNCE*] using a procedure where by each question became a record in another database that was linked to the record of each respondent's.. These respondent records were in turn "nested" within each neighborhood cluster (NC); therefore, each person had five records in this new database, and each NC had as many records as there were interviews within each NC.[15] At the person level (2nd level), the intercept represented the average response for each person across their five questions. At the third or the NC level, the intercept became each neighborhood's average of the averages of the five questions (see (Raudenbush and Sampson 1999) for more details about how and why these measures were produced). For the purposes of reproducing Sampson, et al.'s (1997) *Science* article, the key constructed variable is the neighborhood intercept, or in other words, each neighborhood's frequency of violence as viewed

[15] The number of interviews per neighborhood cluster ranged from eight to sixty-two. The mean was 26 interviews per neighborhood cluster.

from the collective perception of its residents.

The victimization measure used by Sampson, et al. (1997) was captured with just one question that asked (*Q31*) "Has anyone ever used violence against your or any household member." The Earl, et al.'s (1997) archived study had the answers to this question coded as 1 equal to "yes" and 2 equal to "no." We recoded this variable's values so that a "0" is equivalent to a "no" and a "1" is equivalent to a "yes." Because this measure contained just one item/question, Sampson, et al. (1997) only needed to specify a two level (vs. a three-level) HLM regression model. However, like the perceived violence measure, the key dependent variable was again the neighborhood intercept, which for this study is defined as the proportion of residents within each neighborhood that reported a violent victimization. Data for both the neighborhood-level perceived violence and proportioned victimized measures were located in Part 2 of ICPSR study no. 2766. We produced the perceived violence measures at levels two and three (neighborhood), and the victimization measure at the neighborhood-level (2nd level) by following the steps outlined in Sampson, et al. (1997).

The third dependent variable came from a quasi-independent source, the City of Chicago Police Department (CPD). According Sampson, et al. (1997), the homicide variable captured the count of homicides during 1995. Each incident was geo-coded and those occurring within a NC boundary were aggregated to produce a homicide count for each NC. Sampson, et al. (1997) then reported transforming each neighborhood count into a rate by dividing it by the neighborhood's populations [totalpop] (see p. 922). We located this transformed variable in Part 2 of ICPSR no. 2766, it is named *LHOMR95*, and it is labeled "*log of homicide rate 1995.*"[16]

[16] Sampson, et al. (1997) also noted that these "original data measured the address location of all homicides incident known to the Chicago police (regardless of arrest) during the months of the community survey" (note 23, p. 924).

Among the three dependent measures, we are able to provide one quasi-independent source for this data -- the 1995 homicide counts. The source is only quasi-independent because the CPD also collected these data, and they were geo-coded by Dr. Richard Block as well. We also note here that these homicide data may not perfectly reproduce the results reported by Sampson, et al. because geo-coding routines have improved since Sampson, et al. first obtained these data from Dr. Block fifteen years ago. In addition, Sampson, et al. did not include an explicit definition of the types of incidents that fell within their homicide definition. For the purposes of our study, we combined murders and justifiable homicide incidents to produce our 1995 homicide measure. Included in our murder count were murders, voluntary murders, and involuntary and reckless manslaughters.

Besides homicides, we also added to our database the counts for murder (separate from homicides), rape, robbery, aggravated assault, simple assault, burglary, theft, auto theft, and vandalism. Our rape variable includes aggravated sexual assault, attempted aggravated sexual assault, criminal sexual assault, and attempted criminal sexual assault). We then pooled five crime types (murder, rape, robbery, aggravated assault and simple assault) into a measure that captures the total count of all violent crimes. In addition, because residential crimes are-the locus of concern for collective efficacy, we produced several sub-counts for selected crimes and summary crime types that took place within a residential area (e.g., home, apartment etc.). These selected residential crime counts cover all personal, property crimes, as well as vandalism and burglaries separately. We also pooled the 1995 through 1999 counts into one variable to capture the annual average number of incidents across a five-year period. We then merged into this database the PHDCN NC number by tract number to facilitate further geographical aggregation of the crime counts to match the Sampson, et al. study. We then calculated crime rates (using the population variable named TOTPOP provided by Earls, et al. (1997)), and sequentially transformed each crime rate using the natural logarithmic function (after adding a constant value of 10.5 to prevent missing data). These constructed variables will facilitate our testing of whether the effect of collective efficacy extends to crimes other than homicide and burglary.

Selection and Production of Independent variables

The independent measures used by Sampson, et al. were constructed using variables extracted from three data sources: the PHDCN community survey, the Blocks' Chicago Homicide database, and the 1990 U.S Census. Below we describe in detail the steps we took to extract and produce each of the independent measures specified by Sampson, et al. (1997).

U.S. Census/Neighborhood Structural measures Using the 1990 decennial census of the U.S. population, Sampson, et al. (1997) reported constructing ten variables that "reflect neighborhood differences in poverty, race and ethnicity, immigration, the labor market, age composition, family structure, homeownership, and residential stability" (p. 920). They then conducted a series of factor analyses testing at least two approaches to combine the ten census variables into three constructs. One of the approaches they tested was an oblique rotation method (see results on Table 2, p. 920), but they also noted that they eventually used the alpha-scoring method because they were "analyzing the universe of NC in Chicago and are interested in maximizing the reliability of the measures (note no. 24, p. 924). They also noted that they used a "principal component analysis with varimax rotation" that produced "substantially identical results" (note no. 24, p. 924).

As a result of this factor analysis, they identified three factors which they labeled:

concentrated disadvantage, immigrant concentration, and residential stability. The concentrated disadvantage measure is a composite of the following six measures: percent below the poverty line; percent on public assistance; percent female-headed families; percent unemployed; percent less than age 18; and percent black. The immigrant concentration factor is a composite of the following two measures: the percent who are Latino and the percent that were foreign born. The residential stability factor is a composite score of the percentage living in the same house as in 1985, and the percentage of owner-occupied houses. We identified three variables in the Part 2 database that appear to represent the three constructs produced using the oblique factors scores; these three variables are named oblfac1, oblfac2, and oblfac3 in the Earls, et al.'s (1997) study. Their respective labels are "concentrated poverty," "immigrant concentration," and "residential stability." [17] As noted earlier, the second part of the PHDCN database contains neither the ten census measures used to produce the composite scores, nor the raw summary census count variables.

To maximize the extent to which we can independently reproduce the analyses reported in Sampson, et al. (1997), we chose not to rely on the factors scores found in Earls, et al. (1997) since we could reproduce them using the archived 1990 Census data. As mentioned above, we first identified the tract level summary census variables that Sampson, et al. (2001) reported using to produce the ten measures that are the subcomponents of their three factor/composite scores. We then aggregated the applicable tract level data to the neighborhood cluster level, and next used these new variables to compute the ten subcomponents. We subsequently applied the

[17] There are also a number of other closely labeled variables that may represent the Alpha factor regression methods for combining measures (e.g., *conpov90* labeled as "concentrated poverty 90," *himmig90* "labeled as "high immigration 90," and *condisad* labeled as "concentrated disadvantage." We use the three variables that have the prefix "obl" to stand for the three constructs because not only do the names appear to fit best but also because it seems that is what Sampson, et al. (1997) used to report the factor loadings in Table 2 (p. 920).

same factor modeling specification Sampson, et al. (1997) used to combine these ten subcomponents into the three composite/factor measures. In Table 1, we provide a comparison of characteristics of these three factor/composite measures as reported in the Earls, et al. (1997) data file and the characteristics we produced following the procedures set out in Sampson, et al. (1997).

Descriptions of Respondents and Neighborhood Clusters Sampson, et al. (1997) used the community survey as a source of data to capture descriptive information about both respondent and neighborhood-level characteristics. In terms of the respondent-level characteristics, Sampson, et al. used the following eleven measures: female, married, separated, single, homeowner, Latino, Black, mobility, age, years in neighborhood, and SES. We identified all eleven measures in Part 1 of ICPSR study no. 2766. However, one of the eleven variables (SES) is a composite of three questions. According to Sampson, et al. (1997), the respondent's SES measure was produced by a principal component factor analysis of three variables: "education, income, and occupational prestige" (p. 921). Unfortunately, we cannot reproduce this same SES score because in the archived data file the occupational prestige variable is blank, apparently to protect the respondent's identity and privacy. Nevertheless, we identified a way to address this omission by replacing the respondent's occupational status with their employment status. Like Sampson, et al. (1997), we combined the three variables using a principle component-factor model.

Missing Data in the Community Survey Like most social science surveys, the PHDCN community survey includes some missing data. Among the variables used to create both independent and dependent measures in Sampson, et al. (1997), the frequency of missing responses varied by the question asked and also by respondent. We found that all of these measures had some missing data and that the extent of missing data varied from seven percent for mobility questions to thirty-four percent for family income. Sampson, et al, (1997) do not address the existence, extent or implications of missing data in the community survey.

We address this missing information among the eleven key independent variables by producing another set of eleven variables with imputed values when valid data are missing. We accomplish this task by using IBM's *SPSS* 18 missing data routine. Our initial test of the pattern of missing data showed that responses were missing at random (MAR), and not missing completely at random (MCAR). Consequently, it is fair for us to use the *SPSS* EM method to estimate values for these missing data. We used seventeen demographic variables and the EM estimation routine in *SPSS* to estimate values for the key independent variables.[18] This process resulted in usable values for 8,780 cases. The remaining 12 cases still have missing data because they have missing information on all seventeen variables used in the imputation procedure. While locating the raw and transformed variables in the community survey Part 1 file, we also found five variables that contain what seem like imputed values for respondents with no valid responses (these five variables were grouped together in the data file and all began with the prefix imp). Unfortunately, the associated documentation is incomplete (just two of the five contained value labels) and does not describe how these variables were constructed or if they were used by Sampson, et al. (1997).

[18] MVA VARIABLES=english flang employ female married sepdiv single latino black homeown tmp5yrs hgrade rage yrsoneigh hgrade q71 employ SES /ID= rc_num /EM(TOLERANCE=0.001 CONVERGENCE=0.0001 ITERATIONS=25 OUTFILE = 'E:\... ….\CS_data_mva.sav').

Producing the Collective Efficacy Measure The core concept in Sampson, et al. (1997) is collective efficacy, which they define as "social cohesion among neighbors combined with their willingness to intervene on behalf of the common good" (p. 918). For Sampson, et al. (1997), collective efficacy is a combination of two other concepts, informal social control and social cohesion and trust. These two concepts are each measured by five questions on the PHDCN 1995 community survey. The informal social control measure is a composite score of the following questions: (Q12A) If a group of neighborhood children were skipping school and hanging out on a street corner, how likely is it that your neighbors would do something about it?; (Q12B) If some children were spray-painting graffiti on a local building, how likely is it that your neighbors would do something about it?; (Q12C) If a child was showing disrespect to an adult, how likely is it that people in your neighborhood would scold that child?; (Q12E) If there was a fight in front of your house and someone was being beaten or threatened, how likely is it that your neighbors would break it up?; and (Q12F) Suppose that because of budget cuts the fire station closest to your home was going to be closed down by the city; how likely is it that neighborhood residents would organize to try to do something to keep the fire station open? For each of these conditioned questions, there are five substantive responses: (1) very likely, (2) likely (3) neither likely nor unlikely (4) unlikely, and (5) very unlikely. The social cohesion measure is a composite score of the following five assertions: Q11B this is a close-knit neighborhood; Q11E people around here are willing to help their neighbors; Q11F people in this neighborhood generally don't get along with each other; Q11K people in this neighborhood do not share the same values; and Q11M people in this neighborhood can be trusted. Respondents were given the following possible responses: (1) strongly agree; (2) agree; (3) neither agree nor disagree; (4) disagree; and (5) strongly disagree. Sampson, et al. note (p. 924) that they recoded the "don't know" response to the middle category, and that they included in their analysis every respondent who answered at least one of the 10 questions.

Sampson, et al. (1997) stated that they "combined the two scales into a summary measure" which they call collective efficacy (p. 920), because the values of these two variables are significantly correlated when their values were aggregated to the neighborhood-level. As best we can determine, Sampson, et al. (1997) "combined" these ten questions into their collective efficacy measure using the same procedure they used to produce their perceived violence and household victimization measures described earlier in the methods section. More specifically, they transformed each question into a record in a third database that they then "link" to each respondent's record by the respondent's ID number (each record needs just two variables to work within HLM). One variable holds the respondent ID value and the other variable contains the answer to one of the ten questions). These respondent records were in turn "nested" within each neighborhood cluster (NC). Each person then had ten records in this new database, and each NC had as many records as there were interviews within each NC. At the person level (2^{nd} level), the intercept represented the average response for each person across their ten questions. At the third or the NC level, the intercept became each neighborhood's average of the averages of the ten measures. For the purposes of reproducing Sampson, et al.'s (1997) *Science* article, the key constructed measure is the neighborhood-level collective efficacy, a product of the collective perceptions of the residents living within each of the 343 neighborhood clusters. In Table 1, we provide descriptive statistics and various bivariate comparisons of different iterations of the Sampson, et al. (1997) collective efficacy measure and sub measures produced at the respondent and at the neighborhood cluster levels.

Neighborhood Context

Normally researchers use one of several factor-modeling techniques as their approach for combining many variables into a single construct. This is a straightforward approach for producing theoretically relevant constructs that are composites of many questions or case attributes. This technique was indeed the approach that Sampson, et al. (1997) used to calculate their three neighborhood context measures based upon the Census data, as well as the approach they used to produce each respondent's SES score (SES was a combination of three community survey questions). However, to produce both their collective efficacy and their perceived violence measures, Sampson, et al. (1997) employed a fairly novel routine for combining many variables into a single construct. More specifically, using the HLM framework, they nested or stacked like a set of cases the questions for the relevant questions within each respondent so that they could produce an average score for each group of five questions using a single regression model. Within this model specification, the average score for each respondent is the intercept at the second level of a two level HLM model.

To reproduce their three measures, we followed the same process laid out in Sampson, et al. (1997). We report in Table 1 a summary of the results of our efforts to reproduce their three factors. For all three measures, the distributions of our computed data were statistically identical to the corresponding variable values we found in their respondent level data file. The average scores of our respondent level data match all three of their mean scores at the first decimal point, and in two of three instances, they match at the second decimal point. Their corresponding bi-variate correlations are also all above 0.99. The only noticeable difference between their original data and our recomputed data is that our standard deviations are all smaller by about 33 percent. In addition, we produced valid data for two fewer cases than is found in the original database.

The Earls, et al. database have valid data for 7,729 cases while we have data for just 7,727 cases. Unfortunately, we did not find the same degree of continuity between their data and our data at the neighborhood-level. Again, table 2 reports these comparisons but under the section labeled "@ the Neighborhood Cluster Level." In all three instances, our matching paired-t tests show that the two data are not indistinguishable; although most data values are close to their counterparts, and their bi-variate correlations range from a low of 0.95 to a high of 0.97. At this point, we have not identified why two measures that match at the respondent level no longer match each other when aggregated to the neighborhood-level.[19]

Spatial dependence While Sampson, et al. did not speak to the problem of spatial correlations (as noted above this problem arose with the 2001 Morenoff, et al. *Criminology* paper), we took several steps during this project so that we could address this issue during our extension analysis. The first step was to determine the extent and type of spatial autocorrelation. To accomplish this task, we imported our data into *GeoDa*, computed four different weight matrixes, and tested their

[19] We located at both the respondent and neighborhood level the variables that represent both subcomponents of collective efficacy (as well as the ten specific questions that make up the two subcomponents). However, we could not find a variable at either level of aggregation that is labeled "collective efficacy." Therefore, we produce this measure to conduct our reproduction and extension analyses. Initially we produced our collective efficacy measure by adding together the values of its two subcomponent variables (e.g., collective efficacy = SUM(social control; social cohesion)). We took this approach instead of taking others such as by calculating the average of the two scores because Sampson, et al. (1997) reported that they "combined the two scales into a *summary* measure." We in turn operationalized their "summary" term as the *SPSS* "sum" command (which means to add together the values of two or more variables). However, our initial reproduction results were not closely matching those reported by Sampson, et al. (1997). After some further diagnostics and after producing an alternative collective efficacy measure within *HLM* that simultaneously combined the ten questions into one variable as Sampson, et al. (1997) did to produce their results reported in their table 3, we concluded that their term "summary" meant to take the average of the two variables. Therefore, all of the data analysis reported under the reproduction sections of the results section use a collective efficacy measure that is the average of the two subcomponents scores that are produced within HLM using a three-level model.

fits across different forms of an OLS and ML regression model (e.g., spatial error vs. spatial lag). Our results showed that a spatial lag is more appropriate than a spatial error term, and that the queen's matrix with one degree of continuity is more parsimonious than a Rook matrix. These finding are consistent with the approach taken by others (see Morenoff, Sampson, and Raudenbush 2001; Browning, Feinberg, and Dietz 2004) using these data but utilizing a different spatial analysis software application. The second step we took was to produce a series of variables that capture for each year (and for the combined 1995 through 1999 years) the total and average count by crime type across all the neighborhoods that touch each neighborhood. To calculate these neighboring indices entailed writing an *SPSS* syntax command file that would produce 343 *SPSS* system files. Each of these system files contains just 18 records (one for each year of crime data). To identify which neighborhoods touched each neighborhood, we used *GeoDa* to produce a queen's weight matrix (a weight matrix is a text file that contains a record for each neighborhood and lists out the adjoining neighborhood identification numbers).[20] We then modified each line in the weight matrix so that *SPSS* would recognize it as a "SELECT IF" command (e.g., select the records in the master data file that meet the following criteria: the record's neighborhood cluster value equaled the value of one of the adjoining neighborhood's cluster numbers). Between each SELECT IF command, we then inserted an *SPSS* AGGREGATION command. This AGGREGATION command produced two measures for each

[20] We reviewed the weight matrix produced automatically within *GeoDa* by manually identifying within *ArcMap* and documenting all adjacent neighborhoods. For no apparent reason, we found that the weight matrix produced by *GeoDa* was not entirely accurate. With the assistance of Ronald Wilson, our original project manager and the former director of the National Institute of Justices' Mapping and Public Safety Program, and the Data Resource Program, we tried to address this inconsistence using a number of adjustments to our *ArcMap* shape files. Unfortunately, no adjustment produced an accurate weight matrix in *GeoDa*. We therefore relied throughout this project on the weigh matrix we manually produced when we are addressing spatial lag issues.

crime type for each year. One of the two measures contained a summary count and the other contained the average count across all selected neighborhoods. This process of selecting and aggregating was repeated 343 times (once for each neighborhood). We then merged the 343 *SPSS* system files into the primary database by the neighborhood cluster number, transformed each of the count variables into a rate of crime per 100,000 population, and transformed these rate variables by the natural log (after adding a constant value of 10.5 to prevent missing data).

Multicollinearity We found no indication that Sampson, et al. (1997) or Morenoff, et al. (2001) had expressly considered the impact of multicollinearity among their independent variables on their results. Nevertheless, we choose to consider this as an additional issue to examine in our extension analysis. While extreme multicollinearity that is not perfect collinearity does not necessarily violate OLS regression assumptions, the presence of a great deal of multicollinearity can lead to large standard errors. Large standard errors in turn lead to wider confidence intervals and small t-statistics. Therefore, coefficients will need to become larger in order to be statistically significant. Unfortunately, multicollinearity is a matter of degree because there is no “irrefutable test that it is or is not a problem” (Williams 2011).

We first estimated the degree of multicollinearity by using *SPSS*’s Ordinal Least Squares (OLS) regression procedure with the “collinearity statistics” option checked. One of the statistics produced by this option is labeled the tolerance score. A small tolerance value indicates that the variable under consideration is almost a perfect linear combination of the independent variables already in the equation. Using the 1995 homicide rate as the dependent variable, we computed an OLS regression that included Sampson, et al.’s (1997) five key neighborhood-level independent variable measures (see their Table 5). The five tolerance values produced by this

OLS (from lowest to highest) are: 0.255 for concentrated disadvantage, 0.287 for 1988-90 homicide, 0.351 for collective efficacy, 0.749 for residential stability, and 0.819 for immigrant concentration. While we did not find any variable with a perfect collinearity score, several of them have quite low tolerance values. There are several directions we can take to deal with variables with low tolerance values (in our opinion some more preferable than others), but we chose to use just the approach of regressing one highly collinear variable against a number of the other key independent variables, and to save the residual values as a variable. This residual variable can then be included in subsequent substantive analysis as an alternative/replacement for the highly collinear independent variable we just modeled. Among the more highly collinear variables we identified above, we choose to focus on the 1988 to 1990 homicide rate because in many ways it was simply a “nuisance” variable in the Sampson, et al. (1997) model. We then took the remedial step described above to produce a residual variable for the 1988-1990 homicide counts. We then re-ran the OLS regression model specified above, but this time we used the residual of 1988-90 homicide rate variables in replace of the actual homicide rate. The regression results showed that our remedial step reduced some of the collinearity between the five independent variables. The tolerance score for the residual of homicide is 0.927, and the others variables in the model have likewise improved somewhat. The tolerance score for concentrated disadvantage is now 0.40, collective efficacy is now 0.34, residential stability is now 0.74, and immigrant concentration is now 0.85. This outcome seems like a reasonable improvement, and therefore we repeated the process of producing a residual variable for each of the crime types that would eventually become a dependent variable in our extension analysis. We also repeated this series of regression models for the neighboring crime rate variables as well to reduce the collinearity between this variable and the others that will eventually be added to the regression models. This regression was completed for each of the crime variables for every year we have crime data.

Regression Model Specifications

The Reproduction Analysis. For the reproduction analysis aspect of this project, we sought to duplicate the same analytical approaches outlined by Sampson, et al. (1997).[21] Beginning with their collective efficacy model, we reproduced and replicated their three-level HLM results reported in Table 3 and described in detail on pages 920 through 921. To do this, the ten collective efficacy questions are placed within level one (each of the ten measures represent a record in the database), the eleven respondent/person-level demographic variables are specified at level two, and three neighborhood cluster level measures are at level three. The eleven respondent characteristics included at level two in this and several subsequent outcome models are sex (0 = male; 1 = female), married (0 = no and 1 = yes), separated or divorced (0 = no and 1 = yes), single (0 = no and 1 = yes), homeownership (0 = no and 1 = yes), Hispanic (0 = no and 1 = yes), African-American (0 = no and 1 = yes); mobility (number of moves in the past five years), years in neighborhood, age and SES composite measure. The three neighborhood cluster measures incorporated at level three are concentrated disadvantage, immigrant concentration, and residential stability. We specified the dependent variables as normally distributed and only modeled the level two intercept by the level three neighborhood measures. We report the results

[21] We did not seek to reproduce Sampson, et al. (1997) results using the same version of the *HLM* software that they likely used. Based upon information from one reviewer, we suspect that they used version 2. We on the other hand used version 6.8. Because are results are not counter to their results, nor was this study about how statistical software version impact results, we choose not to invest time into finding and learning to use earlier version. In addition, we also assume that any changes made in to the *HLM* software that impacts the results would tend towards improving the results more so than harming them.

of this effort in Table 3. Our 1st reproduction uses all of Sampson, et al.'s (1997) measures as found in Earls, et al. (1997), while our 2nd reproduction analysis uses our recompilation of their eleven respondent characteristics (including our own attempt to address missing responses) and our recalculation of their three neighborhood measures using data directly from ICPSR's 1990 U.S. Census of Population study.[22]

Sampson, et al. (1997) produced their perceived neighborhood violence results reported in Tables 4 and 5 using a regression model specification that is like the one they used to model and produce their collective efficacy measure. As described earlier, Sampson, et al. (1997) placed the five perceived violence questions at level one (each represents as a case/record), the eleven respondent/person-level variables are at level two, and the three neighborhood cluster measures (e.g., concentrated disadvantage, immigrant concentration and residential stability) are at level three. We specified these five dependent variables as also having normal distributions, and we only specified that the level two intercept should be modeled by the level three neighborhood measures. One of two notable differences between the collective efficacy model described above and this model (as well as the later two regression models) is that Sampson, et al. (1997) introduced their collective efficacy measure as an independent variable at the neighborhood-level. They took this step after first running this regression model without collective efficacy so that they could assess after introducing collective efficacy whether it mediated (e.g., diminished the size of the coefficient) the association between the three

[22] The following lines of text are the collective efficacy HLM syntax using Sampson, et al. (1997) variables. Level-1 Model: Y = P0 + E; Level-2 Model: P0 = B00 + B01*(RFEMALE) + B02*(RMARRIED) + 03*(RSEPDIV) + B04*(RSINGLE);+ B05*(ROWNHH) + B06*(RNHBLACK) + B07*(RHISPAN) + B08*(MOBILITY) + B09*(RAGE) + B010*(IMPYRSNH) + B011*(IMPTDSEI) + R0; Level-3 Model: B00 = G000 + G001(OBLFAC1) + G002(OBLFAC2) + G003(OBLFAC3) + U00; B01 = G010; B02 = G020; B03 = G030; B04 = G040; B05 = G050; B06 = G060; B07 = G070; B08 = G080; B09 = G090; B010 = G0100; B011 = G0110.

neighborhood structural measures and the dependent variable. They reported the results from this two-step process in their Table 4 (p. 922). The second notable difference between the collective efficacy model and their latter three outcome models is that Sampson, et al. added a control for prior homicide (3-year average homicide rate in 1988, 1989, and 1990) to address "possible cofounding effect of prior crime." (p. 922). They reported the results of this third regression model specification in their Table 5 (p. 923).

For the violent victimization results reported in Tables 4 and 5, Sampson, et al. (1997) specified just a two level model because the dependent variable was measured with only one question (therefore there was no need to specify a measurement level model nested within the respondent level). Besides having just two levels, we specified the dependent variable as a Bernoulli or binary (0 = no victimization and 1= victimization) distribution, and we only specified that the level two intercept should be modeled by the level three neighborhood measures.[23] For this dependent variable/outcome, they otherwise followed the same sequence of regression model specifications as we outlined for the perceived neighborhood violence measure. Sampson, et al. (1997) first computed the regression model without their collective efficacy measure and the prior homicide rate; they then added their collective efficacy measure to the regression model and subsequently added the prior homicide measure.

For the 1995 homicide events results, Sampson, et al. (1997) specified a one level, neighborhood only model. Because the dependent measure was a count of events/homicides,

[23] The following is a summary of this model's HLM specification Level-1 Model: Prob(Y=1|B) = P; log[P/(1-P)] = B0 + B1*(RFEMALE) + B2*(RMARRIED) + B3*(RSEPDIV) + B4*(RSINGLE) + B5*(ROWNHH) + B6*(RNHBLACK) + B7*(RHISPAN) + B8*(MOBILITY) + B9*(RAGE) + B10*(IMPYRSNH) + B11*(IMPTDSEI); Level-2 Model: B0 = G00 + G01*(OBLFAC1) + G02*(OBLFAC2) + G03*(OBLFAC3) + U0; B1 = G10; B2 = G20; B3 = G30B4 = G40; B5 = G50; B6 = G60; B7 = G70; B8 = G80; B9 = G90; B10 = G100; B11 = G110

they specified this model as a Poisson regression with constant variance and over-dispersion.[24] Similar to the first two outcome regression models illustrated above, their first homicide regression model contained just the three neighborhood-level structural measures. They then added their collective efficacy measure the model (see Table 4), and next they added the prior homicide variable (see Table 5).

We ran all the regression models we just described two times within *HLM* v. 6.08. For the first time, we used data and measures produced and provided by Earls, et al. For the second instance, we used data we separately acquired and measures we produced using syntax that we created. The former models we refer to as the 1st reproduction models (i.e., their data and models), and the later as the 2nd reproduction models (i.e., alternative data source used when possible and Sampson, et al.'s models). The results from each of these two rounds of regressions are provided in Tables 1 through 5 in the results section. Each table displays the results as reported by Sampson, et al. (1997), and to the right of their results are the reproduced and replicated multivariate and multi-level analyses.

After running both series of regressions, we then sought to compare systematically Sampson, et al.'s reported results to our two sets of final multivariate results (our two reproduction results reported in Table 5). To make these comparisons systematically, we applied three criteria to determine whether our results matched Sampson, et al.'s results. The first criterion is a simple comparison of the regression coefficients and standard errors. The second criterion is a determination of whether the reproduced results conform to the direction and statistical significance levels of the original analyses. The third criterion is to apply a statistical

[24] The following is a summary of this model's HLM specification. Level-1 Model: E(Y|B) = L; V(Y|B) = L log[L] = B0 + B1*(MSUCE) + B2*(SRECD1AL) + B3*(SRECIMAL) + B4*(SRERSAL) + B5*(HAVE8890) Level-2 Model B0 = G00 B1 = G10 B2 = G20 B3 = G30 B4 = G40 B5 = G50

test to assess the significance of any differences in the sizes of original and reproduced coefficients (see Table 6 for results produced to assess the last criteria).

There are strengths and weaknesses to using each of these three criteria for reproducibility. The rationale for comparing of raw coefficients is based on the understanding that reproducibility is a mechanical process of applying exactly the same data using exactly the same statistical procedures, as if the original investigator had merely run the analyses twice. Sampson, et al. (1997) reported their findings to the third decimal point and this level of precision may be artificial if the standard for reproducibility is exactness to this degree. On the other hand, it is commonplace in social research to accept as consistent multivariate findings that are in the same direction and meet or exceed the traditional p-value < 0.05 level of statistical significance. Thus, this criterion seems appropriate in judging whether findings from a reproduction warrant changing our assessment about the direction and statistical significance of the original findings. Given the arbitrary nature of the 0.05 standard and the minor differences in coefficients or standard errors which could change the original findings, this criteria retains and perhaps amplifies the limitations of the arbitrary nature of $p < 0.05$ in frequentist (non-Bayesian) statistics.

The strength of our third criteria is that it uses statistical theory to bear on a judgment about whether the reported findings by the original investigators and the secondary analyst are different from zero. We adopted a test created by Clogg, et al. (Clogg, Petkova, and Haritou 1995) to determine if the introduction of a new statistical control affects the reported relationship between two variables in a multivariate analysis. Paternoster, et al. (Paternoster et al. 1998) adopted the same test to determine whether the relationship between two variables vary in separate analyses from two samples. This criterion for reproducibility assumes that the reproduction process is more stochastic than mechanical.

Extension across Crimes and Time

Our extension analysis mainly focuses on the implications that exist regarding the relationship (size and significance of the slope) between collective efficacy (CE) and crime when a data analyses address three substantive concerns. More specifically, the first question our extension analyses seeks to answer is what happens when one expands the number of crimes from homicide to eleven other types (both violent and property)? The second question our extension analyses examine is what happens to the CE effect when one expands the number of years covered by the crime data? The final question our extension analyses addresses is what happens to the CE effect when one captures in the regression models the influence of neighboring crime rates? In principal, to assess comparatively we wish to keep our analytical methods as similar as possible to those used by Sampson, et al. to facilitate answering the question about the dependence of the collective efficacy effect on competing hypotheses. In other words, we want to keep the gap between Sampson, et al. (1997) methods and our method as narrow as possible as we carry out the "extension" analysis. A close match between the two analyses should reduce (though not eliminate) the number of rationales as to why their results regarding the effect of collective efficacy diverged to those we produced; although we do wish to tackle the possible problem created by multicollinearity (see above) throughout the extension analysis. Below we provide details about the steps we took beyond those used by Sampson, et al. to complete the extension analysis.

Additional Crime Types. Our first set of extension analyses focus on the question of whether there is a connection between collective efficacy and crimes beyond homicide. In large part, besides changing the dependent variable from homicide to each of the eleven other crime types, we duplicate the analytical methods laid out by Sampson, et al. Although, we make a few adjustments to the regression models because of the nature of some alternative data and several analytical issues we raised above (e.g., multicollinearity and spatial effects). One adjustment included specifying only two-level rather than three-level regression models when utilizing the community survey database as alternative source of crime type data. This reduction of one level is due to our focus on the individual crime types rather than a collection of crime types (recall that their perceived violence measure was a collection of questions about what the respondent thought was happening in their neighborhoods). A second adjustment we made was using the residual of the 1990-1993 crime rate measure (see above for description) rather than the direct 1990-1993 crime rate. We specify the residual variables to help insure that this "nuisance" crime measure will adjust for a neighborhood's underlying crime propensity (beyond the propensity captured by the structural variables) without influencing the connection between collective efficacy and the crime variable of interest.

A third adjustment was to systematically account for the likelihood of a spatial effect throughout the modeling process (just not with the 1995 data, but with all of our extension analysis). We take two approaches to address this issue. First, within our HLM-structured models, we add to the list of independent variables in each regression the neighboring crime measure (more precisely the residual of the neighboring crime measure that we earlier described calculating). We intend for this measure to statistically control for the simultaneously crime processes (e.g., spatial lag that will not influence any measure within the model) that occurs just outside a neighborhood. This modeling approach simulates a simultaneous spatial autoregressive model.

The second approach taken to capture and control spatial effects involved repeating our entire analyses within *GeoDa. GeoDa* is a statistical application principally designed to conduct various formulations of exploratory spatial data analysis, spatial autocorrelation, and spatial modeling (as described earlier we used this application to determine the nature of the spatial autocorrelation and to produce the spatial weight matrix). However, this application has several shortcomings that required us to take additional steps within *HLM* before importing (via *ArcMap*) data into *GeoDa*. The two most relevant limitations that influence our analyses are that *GeoDa* cannot properly model "nested" data (e.g., respondents within neighborhoods) or dependent variables that are not normally distributed (e.g., Poisson and binomial distributions). Morenoff, et al. (2001) also addressed this shortcoming with the spatial application that they used to capture spatial effects (they used an application called *SpaceStat*) for homicide and burglary in Chicago.

Similar to Morenoff, et al.'s (2001) approach, we took steps to address these two shortcomings by first using *HLM* to produce proxy measures for the key dependent variables (we used *HLM* because it can simultaneously address these issues but it cannot yet also explicitly address the spatial effects). More specifically, we modeled as intercept only regressions (e.g., we included no independent measure at the second level) each relevant crime measure within *HLM*, and we then saved the appropriate predicted crime values within *GeoDa* as dependent variables for later reference. Within this process, the distribution of the dependent variable (normal, binomial or Poisson) determined the model's link function. The number of levels of data nesting was determined by whether we modeled the official-crime counts (one-level models), or we used the community respondent-survey data (two-level models). For example, for those instances where we were interested in a survey-based crime measure, we specified a two-level regression model to produce a proxy dependent variable to export to *GeoDa*. In this circumstance, the proxy measure was the level-two intercept (i.e., neighborhood intercept). If the dependent measure was one of the self-reported victimization questions that used a "no" or "yes" response-set, we specified a binominal distribution so as to produce an intercept at level two that represented the percentage of respondents within each neighborhood that answered "yes." If the response set was a Likert-scale (e.g., one of the perceived violence measures), we specified a normal link function to produce a second-level intercept that represented the average response across all subjects within each neighborhood. In all of the two-level models, we included the eleven respondent control variables noted above to address how neighborhood-level values might first vary as a function of the demographic backgrounds of each neighborhood respondent. We repeated this process for all the dependent variables modeled within *GeoDa*; thus we simultaneously addressed both the nesting and the distribution issues before using *GeoDa* to address the spatial effects. This entire process was repeated to compute proxy measures for the average 1995-1999 crime variables.

After we produced the proxy dependent variables (36 measures), we then merged them into an Environmental Systems Research Institute, Inc. (ESRI) shape file using *ArcMap*. ESRI's shape files are the standard database format for *GeoDa*. Once we imported these data into *GeoDa*, we specified our queen weight matrix,[25] and then we repeated the thirty-six regression models that we first produced in *HLM*. The *GeoDa* regression model specification included the six neighborhood measures: three census structural measures, collective efficacy, the residual of prior crime, and the spatial lag indicator.

[25] *Queens Weight matrix* defines spatial neighbors as those areas with shared borders and vertexes.

Additional Years Besides considering other crime types, we also considered whether collective efficacy was related to crime more broadly defined by the inclusion of more years of data. More specifically, we sought to test whether collective efficacy remains significantly associated with homicide and other crime measures when five years of data are pooled into a single dependent measure (rather than just one year). Besides pooling these crime data together, we followed the same procedures we outline above for modeling the 1995 crime data. The only significant change is that we did not include the survey data because data collection took place once during the five-year span. Like above, the dependent variable was modeled within *HLM* as a Poisson regression model with overdispersion, and we produced a proxy dependent measure for use within *GeoDa* to address the Poisson distribution of the variable (although descriptive analyses of these data showed that the distribution of all these data were far closer to normal when more years are combined and other more frequent crimes were considered).

Beyond considering the effect of collective efficacy on crime rates between neighborhoods within a fixed time, we also considered whether collective efficacy is related to changes in prospective crime rates over a ten-year period (1995-2004) within the 342 neighborhoods. This idea is analogous to considering how an individual's crime propensity (or lambda) changes overtime as a reflection of one or more factors measured at the time someone is first observed (in our case the first year was 1995). This extended model specification requires several adjustments to the *HLM* regression models that we have already described. The most notable of these differences are: (1) we used a two-level *Hierarchical Multivariate Linear Model* (*HMLM*) routine instead of the one-level HLM; (2) we produced an indicator variable (0=all other years and 1 = a specific year) for each of the ten years to include in the HMLM; and (3) we added a first-order autoregressive correction because we have ten repeated measurements of the same unit at fixed periods of time. In addition, we specified a two level model that "nested" the ten years of crime (both the neighborhood crime rates and the neighboring crime rates) data within each neighborhood (NC). We then specified a HMLM regression that was much like the level one model described above in terms of explained variations in the intercept (e.g., between neighborhoods), but we also added an additional parameter to the second level to test whether there was an association between crime rates overtime within neighborhoods and collective efficacy.[26] This model specification treats the level one intercept like it was the 1995 only model, and then we use the added year term at level one as the means for assessing whether crime rates beyond 1995 were influenced by collective efficacy measured in 1994-1995.[27]

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[26] An example of the HMLM model specified is Level-1 Model: Y = YEAR95*Y1* + YEAR96*Y2* + YEAR97*Y3* + YEAR98*Y4* + YEAR99*Y5* + YEAR00*Y6* + YEAR01*Y7* + YEAR02*Y8* + YEAR03*Y9* + YEAR04*Y10*; Y* = P0 + P1*(YEAR) + P2*(RHOMSLLN) + e; Level-2 Model: P0 = B00 + B01*(OBLFAC1) + B02*(OBLFAC2) + B03*(OBLFAC3) + B04*(EBCOLEF) + B05*(RHOM91LR) P1 = B10 + B11*(EBCOLEF) P2 = B20.

[27] In many regards the Sampson, et al. (1997) article and our initial analyses have indeed examined how crime rates change between the average crime rates for the early 1990s and 1995 to 1999 crime rates (e.g., a model of the difference between the early and later part of the decade). However, these initial analyses were not expressed within a crime change framework, and even if they were discussed in these terms, they only specified the difference between two periods that were five or more years apart. Our more explicit "growth-curve" model examine the average change over many one-year periods, and in doing permits us to illustrator how collective efficacy influence future crime rates given that not everyone neighborhoods starts at the same place (i.e., the time order is well specified).

Results

Table 3 provides three sets of results produced using a three level model that simultaneously tests for associations between individual and neighborhood social composition with the quantity of collective efficacy. This table reports the coefficients for levels two and three, and the percent of the total model fit that is explained by level three measures; it does not also include the coefficients produced by the level-one regression because Sampson, et al (Sampson, Raudenbush, and Earls 1997) did not provide this information. The first set of results (those shown in the columns to the far left side of the page and shaded gray) are those reported by Sampson, et al. (1997) in Table 3 on page 921. The second set of results (those in the middle three columns) under the column heading first reproduction are produced by us using measures Earls, et al. (Earls et al. 1997) provided and Sampson, et al. (1997) used. The third set of results (those showing in the columns to the right side of the page) we also produced, but we used measures we constructed from Earls, et al.'s (1997) unprocessed variables. Overall, we find remarkable consistencies across the three sets of results. Like Sampson, et al., (1997) we find using their measures that four of the eleven individual-level measures and all three neighborhood-structure measures are statistically significant (p-value < 0.05) factors in the model. The four respondent factors with some significant effect on collective efficacy are homeownership (b=0.139), residential mobility (b=-0.028), respondent age (b=0.001), and household Social economic status (i.e., SES) (b=0.001). In comparison to Sampson, et al.'s results, while all four of these significant coefficients also point in the same direction, none is equal to the size of their coefficients though three of the four matched until the second decimal point. Among the non-significant factors, six of the seven coefficients were in the same direction and six of seven coefficients were within 1/100 of a point of their respective coefficients (only one of our coefficients matches one of theirs perfectly).

We turn next to review the association between the three neighborhood structure factors and the neighborhood-level collective efficacy measure. Here we find that our results derived from their three neighborhood factors are even closer to Sampson, et al.'s original coefficients than those results we produced using the individual/respondent-level measures. All three of our neighborhood-level coefficients point in the same direction as those produced by Sampson, et al. (1997) (two are negative and one is positive), and all three coefficients are significantly associated with the quantity of collective efficacy across the neighborhoods. However, just one of our three coefficients matches theirs exactly in terms of size, although the other coefficients are just 1/100 of a point different.

Besides the direction and statistical significance of the regression coefficients, the relative percentage of variance explained between the respondent and neighborhood-level measures nearly match across the three models. More precisely, we find that there is just a one percentage point difference in the relative model fits, with our two regression models explaining just one percent more variance at the neighborhood-level than did their model.

The one notable difference between Sampson, et al.'s (1997) and our results is with the t-scores for the level one intercept (even though the coefficients nearly match each other across the three models); our respondent-level t-score for the intercept using their measure is 69.25 while their reported t-score for the same measure is 263.20. We find a similar pattern of results when replacing their independent measures with our computation of both the individual and neighborhood-level measures (see results under the columns labeled "Second Reproduction"). Besides these different t-scores, the one other difference across all three models is that we do not find using our measures that a household's SES significantly explains their quantity of collective efficacy. However, this difference between the two regression models is not surprising given that our SES measure is missing one (e.g., occupational prestige) of its three SES subcomponents.

Table 4(a) and 4(b) report more of their results and our reproductions in a similar format as in Table 3. The only difference is that we took what they reported in a single table (see their Table 4 on page 922) and divided it into two parts (Tables 4(a) and 4(b)). We also do not report the first reproduction analysis of their two homicide models because they did not specify any respondent-level coefficients nested within the neighborhoods (i.e., the homicide models are just a one level, neighborhood only regression). Part 4(a) shows their model one results (or what they label as the *social composition* model) and our two reproductions of their model one, and Table 4(b) shows their model 2 results and our two reproductions of their model 2. The difference between their models one and two is that they introduced their collective efficacy measure into the second regression model (they label this model as the *social composition and collective efficacy* model). For all applicable regressions, they (as well as us) report only the neighborhood-level coefficients and test statistics. They chose not to report the eleven respondent-level coefficients when they were used in the relevant regression models (e.g., the perceived violence and violent victimization models). Similar to the first set of results reported in Table 3, the results we produced for Table 4(a) are remarkably comparable to their reported results, although none match perfectly. Overall, all fifteen of the coefficients point in the same direction as Sampson, et al.'s (1997) corresponding coefficients, all the coefficients that are significant in the Sampson, et al. analysis are also significant in our analysis, and seven of the fifteen coefficients are equivalent until the hundredth decimal point.

The comparative results for the second part of Table 4 (see 4b) are again similar, although not quite as comparable as the comparative results produced for part 4(a). For some reason, the addition of the collective efficacy measure has negatively influenced the similarity of Sampson, et al. and our results. Overall, two of the twenty coefficients (10%) change direction; three of the coefficients are either now or are no longer significant; and in no instance do any of our twenty coefficients match across the models in terms of size. In regards to the collective efficacy measure, all five of our regression models produced significant coefficients similar to Sampson, et al.'s findings, and all of our collective efficacy coefficients point in the same direction as those produced by Sampson, et al. (1997). However, only one of our five collective efficacy coefficients matches its comparable coefficient at the tenth decimal point. We explored several possibilities that we thought might make our collective efficacy measures produce coefficients more similar to theirs, but none could produce results that are closer than those we have reported in Table 4(b). One explanation for these differences is that we produced the collective efficacy measure used in both reproduction regressions because we could not find a collective efficacy measure anywhere in their databases.

Table 5 reports our last series of reproduction analyses. The regressions reported in this table are different from the previous analyses because we have added the "average of the 1988 to 1990 crime rates" as a sixth control measure. Sampson, et al. (1997) included this measure to address the possible confounding effect of prior crime (p. 922) or what they later describe as "the possibility that the association between collective efficacy… … and homicide rates is really a reflection of the downward spiral of neighborhoods caused by prior violence" (Morenoff, Sampson, and Raudenbush 2001). They explicitly define this measure as the "3-year average homicide rate in 1988, 1989, and 1990. " Overall, for the third time, our regression analyses are generally equivalent to their reported analyses regardless of whether we use Earl, et al.'s independent measures or we use our produced measures. However, there a few notable differences between the two sets of results, particularly those reporting the association between prior homicide and later violence. Sampson, et al.'s (1997) analysis reports finding that this measure was positively associated with their 1995 homicide rate measure, but not associated with the neighborhood-level of perceived violence or with average rate of violent victimization (e.g., a neighborhood's prior homicide rate did not predict the proportion of respondents who would report recent victimization or their perception of violence in their neighborhood). We also found a similar pattern of results, except we did not find a significant association between past and future homicide rates as they did. Our analysis using their measure of homicide found nearly no association between the two measures of the same crime.

The lack of an association between past and future homicide rates did not have a substantive impact on the other five coefficients in either of the two 1995 homicide models. Under the 1st Reproduction model, while all of our five coefficients are somewhat larger than reported by Sampson, et al., none changed direction or became statistically significant (nor do they lose statistical significance). This lack of mediating effect for the inclusion of prior homicide was likewise consistent across the other five regression models. In other words, the addition of the homicide rate measured six years before any dependent measures did not change the coefficients within each model, and therefore the contemporaneous link between the level of collective efficacy and the quantity of violent crime remained intact regardless of the level of prior violence.

Our Table 6 reports two of the three results produced using the 1995 homicide count data. The first set is Sampson, et al.'s, three hierarchical/blocked models reported in their *Science* article, and the second set represents the same three regression models, but the coefficients were produced using measures archived in Earls, et al.'s (1997) study. Reported in this table are the results produced by the third set of three regressions (those to the far right side of the page). This third set of results (reported under the column entitled "*MGS-2nd Reproduction*") was produced using Sampson, et al.'s (1997) regression model specification and with measures and underlying data sources that are almost entirely independent of data utilized by Sampson, et al. The only data we use from Earl, et al. are the unprocessed variables derived from their community-based survey that are necessary to compute Sampson, et al.'s collective efficacy measure. All of the other variables are from the other data sources. These analyses are the most extensive reproduction of the Sampson, et al. analyses feasible using existing data within the City of Chicago. As we described above in the methods section, other data we used to produce these regression results were also acquired from the same organization that collected them (e.g., the U.S. Census Bureau and the City of Chicago Police Department), but what is different about them is that we acquired them independent of what Earls, et al. (1997) supplied to the National Archive of Criminal Justice Data (NACJD).

The first regression represents the initial outcome model specified by Sampson, et al. (1997). In comparison to their results, our two efforts (comparing from left to right) to duplicate their findings fall somewhat short of producing an exact match. While both of our regression models produced coefficients that point in the same direction as their coefficients and both of our models produce the same two statistically significant measures, none of our six coefficients matches any of their corresponding coefficients. Although, both regressions produced coefficients that are in the same relative order of size as their coefficients (starting with the concentrated disadvantage measurement having the largest coefficient followed by immigrant concentration and residential stability). Interestingly, while neither of our two sets match their coefficients, our two sets of coefficients are very close to each other (e.g., 1st reproduction vs. 2nd reproduction). The total size of the three coefficients produced by our two models is only 0.06 different from each other, while on average these two models are 0.29 smaller than Sampson, et al.'s regression results. This gap exists largely because of the difference between the slope they report for concentrated disadvantage and the two that we produced for this measure (their slope is nearly 50% larger than either of our two concentrated disadvantage slopes).

The second set of results shown in Table 6 contains the regression coefficients produced after Sampson, et al. (1997) added their collective efficacy (CE) measure to their homicide count model. Overall, in comparison to the first set of three regressions, the consistence across the three regression models is somewhat reduced by the addition of their CE measure. While both of our sets of four coefficients remain pointed in the same direction as those reported by Sampson, et al., our reproduction of their model finds that all four measures (versus just three found by Sampson, et al) are significantly associated with the rate of homicide in 1995. More explicitly, while Sampson, et al. did not find a significant association between the quantity of immigrant concentration and the rate of homicide, we find an association between these two measures using Earls, et al.'s (1997) study. However, at the same time we did not find that immigrant concentration was associated with homicide using the Inter-university Consortium for Political and Social Research (ICPSR)'s U.S. Census managed database and homicide data we acquired directly from the City of Chicago Police Department. In addition, while none of our coefficients match Sampson, et al.'s (1997) parallel coefficients, we did find that our 2nd reproduction of their regression model produced a total slope score that is just 0.02 different from their reported results (recall that without their CE measure in the model our 2nd reproduction results are closer to our reproduction results than they are to Sampson, et al.'s results). In terms of their CE measure, our reproduction analysis found a stronger correlation between CE and homicide than what Sampson, et al. report, but our 2nd reproduction analysis found a CE slope somewhat smaller than their slope (in other words, their CE slope fell about right in the middle between our two CE slopes).

Besides the additional benefit of adding collective efficacy to the overall model, Sampson, et al. (1997) remark that "when collective efficacy was controlled, the coefficient for concentrated disadvantage was substantially diminished, which indicated that collective efficacy can be viewed as partially mediating the association between concentrated disadvantage and homicide." (p. 922). More explicitly, their results showed that the slope for concentrated disadvantage went from 0.727 to 0.491 (a 33% smaller slope). At the same point, they also footnoted that residential stability was significant in this model even though its "zero-order correlations with homicide was insignificant" (footnote 33, p. 924). We too find similar changes in these two measures between models 1 and 2 for both our reproductions. Under the 1st reproduction model, the concentrated disadvantage slope was reduced in size by 50 percent, and under the 2nd reproduction model, this same slope was reduced by 32 percent.

The only evident difference between Sampson, et al.'s (1997) results and either of our two sets of results is the impact we found after adding collective efficacy on the immigrant concentration slope. In fact, under our 1st reproduction analysis, the impact of adding collective efficacy on immigrant concentration was larger (its slope increased by 60%) in comparison to how adding collective efficacy changed the size of the concentrated disadvantage slope, and the immigrant concentration slope became statistically significant (it is not statistically significant under the Sampson, et al. model). While collective efficacy reduced the consequence of concentrated disadvantage, it exposed the negative effect of more immigrant concentration on homicide if one accepts our reproduction analysis. We cannot argue for this conclusion because we did not reproduce the large change in the immigration concentration slope under our 2nd reproduction analysis.

The final set of regression results reported in Table 6 adds the prior homicide measure to the model. Above we have already discussed the differences between what Sampson, et al. reports with this measure in the model and our parallel reproduction analysis. However, by adding our 2nd reproduction analysis (as reported in Table 7) to this discussion, we provide some confirmation of our earlier reproduction analysis; which is to say that we again find only a weak to nearly non-existing relationship between past and future homicide rates when several other structural measures are in included in a model. More specifically, the prior homicide coefficient in the 2nd reproduction analysis is 0.005 while the corresponding coefficient produced under our 2nd reproduction analysis is once more nearly nonexistent with a value of 0.006. The one prominent difference between our two models that include the prior homicide measure is how adding this measure has influenced the size of their respective collective efficacy coefficients. In the 1st reproduction analysis, the collective efficacy slope went from a -1.641 value to a -1.202 value (a reduction of 0.439 or 25%) while under the 2nd reproduction analysis the collective efficacy slope went from a -1.269 to a -1.237 (a reduction of 0.032). Besides this difference, one other finding worth mentioning is the fact that under our 2nd reproduction analysis, our residential stability measure now produces a larger coefficient than our concentrated disadvantage measure (largely because concentrated disadvantage went from a value of 0.465 to 0.179, or a near three fold decrease in size). This reduction supports Sampson, et al. point that factors beyond concentrated poverty and other similar attributes can explain homicide rates.

Table 7 duplicates the last regression of the three sets reported in Table 6 as well as to two regression results reported in Table 5. All five regressions shown in Table 7 contain all six independent measures used by Sampson, et al. (1997). What is different about Table 7 is that to the right of the regression we report our systematic analysis of the extent to which we have reproduced Sampson, et al. final regression models across their three key dependent measures. In this section, we calculate the absolute difference in the sizes of the coefficients, report whether the coefficients are point in the same direction, and report their *Clogg z-value* (Clogg, Petkova, and Haritou 1995). Among these three indicators of reproducibility, our key assessment is the *Clogg Z test value*. At this moment, we utilize the *Clogg Z-value* to systematically test whether or not two coefficients (one reported by Sampson, et al. (1997) and the others we produced) are derived from the same distribution of possible coefficients describing the relationship between two equivalent measures. A z-value smaller than 1.95 supports the null hypotheses (e.g., the two coefficients appear to come from the same population) while a z-value larger than 1.94 support the alternative hypothesis.

Overall, our pattern of comparisons show reasonable congruency between Sampson, et al.'s (1997) reported results and our subsequent two reproductions of their models. Ten out of the 28 coefficients match each other at the 10^{th} decimal point (although just one matches at the 100^{th} decimal point), 25 out of the 28 coefficients point in the same direction and are still statistical significant, and 21 of the 28 model coefficients are likely from the same population of coefficients that produced Sampson, et al. results. But if one removes the model intercept from this summary exercise, then only one of the five substantive measures in three of the four models have coefficients that are statistical different from each other. Among these three models, the prior homicide measure represents two of these three significantly different coefficients while the one other different coefficient belongs to the collective efficacy measure (see their *perceived violence* model). Across the five models, the one that reproduces the closest to the original analysis is the self-reported violent victimization model. Four of the five substantive coefficients match at the 10th decimal point, four of five coefficients point in the same directions and match statistical significance, and all five coefficients are likely from the same population. In terms of substantive compatibility, the collective efficacy measure is always negatively associated with homicide rates and is at all times significant, and in three out of the four comparisons there is no difference between Sampson, et al.'s and our collective efficacy produced coefficients. In the one instance where there is a significant difference, our collective efficacy coefficient is significantly largely (-0.594 vs. 0.916) than their parallel coefficient (see their *Violent Victimization* model reported in Table 7).

Corresponding to the results provided in the prior two tables, the finding of note is the difference between their prior homicide coefficient and our prior homicide coefficients. Their reported prior homicide coefficient is 0.397 and is statistically significant, while our prior homicide coefficient is just 0.006 and is not statistically significant. What is particularly surprising about this incongruence is that it exists even though the bivariate correlating between their prior homicide rate and our prior homicide rate is r=0.99, and the two homicide measures have similar means value (x=29.2 vs. x=28.7), and their standard deviations scores are nearly identical (31.6 vs. 31.1). In addition, our results when we use their prior homicide data compared to when we use our prior homicide data are similar to each other (b=0.005 vs. b=0.006). Thus, while our prior homicide data match their comparable data, neither of the two regression coefficients that we produced match what they report. Similarly, the prior homicide coefficient produced in the perceived violence and the violent victimization models are statistically identical, and all four are statistically insignificant. In other words, knowing the level of homicide in one neighborhood around the year 1990 will not help you to know the extent of exposure to violence in 1995. Nevertheless, while there are differences between what they report and what we produced, we still reach the same conclusion about the benefits of adding collective efficacy to the models. We both find that collective efficacy is negatively related to violent crime rates regardless of whether crime is reported by residents or documented by the police, and we both find that the addition of collective efficacy to the models substantially reduces or mediates the size of the concentrated disadvantage measure.

Extending Collective Efficacy

Up to this point in this project, we have only sought to reproduce the results reported by Sampson, et al. (1997). After finding a great deal of continuity between what they report and we produced, we now turn to our final step in the project (see their results reported in Table 8). This step seeks to explore what happens to the value (i.e., the size, direction, and significance) of Sampson, et al.'s collective efficacy measure when we extend their violence model in several dimensions. In other words, in this section we investigate whether relationship between collective efficacy and crime remains when crime is measured more broadly than just homicide and self-reported violence, and the crime measured covers more than just one year. More specifically, the first extension analysis we undertake is to replace the 1995 homicide measures with alternative 1995 crimes measures across the neighborhoods. Like in Sampson, et al. (1997), we use both the community survey and the official police reports as sources for our dependent measures. Within the realm of official data, we test for a connection between collective efficacy and thirteen measures of crime and one composite crime measure. We provide in Table 8 under the column heading entitled *1995* the collective efficacy coefficients produced within *HLM* (Raudenbush et al. 2004) and in *GeoDa* (Anselin, Syabri, and Kho 2004) for each of the 13 crime measures. Under the sub-column heading entitled *official* are the coefficients that we produced using a multivariate regression model for each one of these measures based upon the official police data (we provide in the Appendix A the full complement of model results for the entire set of regressions). Besides using official data, we also model six community survey questions; the choice of which question we use was based upon whether the question approximately matched the official crime measure. For some crimes, the survey measure came from the questions asking about perceived violence, while for some other crimes the measure came from one of the self-reported victimization questions. Because we have just one period covered by the survey, we used each survey-based crime question's corresponding official crime measure to capture prior crime in the neighborhood. Sampson, et al. (1997) also used prior homicide in every model regardless of whether homicide was the dependent measure.

The regression model specification we employ to produce these are the same as Sampson, et al.'s, except for two changes. First, we replaced their 1988-1990 homicide rate measure with one that matched the dependent measure in the model, that captured crime from 1990 to 1992 (like Morenoff, et al. 2001), and represented the natural log of the rate per 100,000 residents. Second, we added a spatial lag measure for each crime to the model (again similar to the step taken by Morenoff, et al. (2001)). For the regression results we produced using *HLM*, we specified the1990-1992 crime's residual value that was produced by regressing the neighboring crime measure (either official or survey based) against the same five independent measures used by Sampson, et al. We took this step to reduce the impact that multicollinearity can have on other coefficients in a model (we speculate that crime in the neighboring area is also likely caused partially by each neighborhood's social structure and prior crime).

Turning now to the outcomes of our efforts (see Table 8), we find that the association between collective efficacy and the rate of homicide in 1995 is similar to the one reported by Sampson, et al., and to those we discussed earlier under the reproduction section. More specifically, regardless of whether we produced the results using *HLM* or *GeoDa*, the collectively efficacy measure remains negatively associated with the homicide rate in 1995. We similarly replicate in both software applications the negative relationship between collective efficacy and the respondents' perceived level of neighborhood violence (b=-0.41 and b=-0.33), as well as for every other respondent reported crime measures. Indeed, a neighborhood's quantity of collective efficacy is always negatively associated with its collective perception and experiences with crime (e.g., the more collective efficacy a neighborhood's respondents jointly believe exist the less they perceive that crime occurs in their neighborhood and the less likely they are to report their own victimization). The collective efficacy measure is also negatively associated with the rate of official murder (a significant subset of the homicide measure) and rape, as well as with the rate of residential burglary. However, collective efficacy is not significantly associated with the rate of robbery, aggravated assault, simple assault, or the overall level of overall violent crime reported to the police in 1995; nor is collective efficacy significantly related to the amount of all residual property crimes, auto thefts, thefts generally, or vandalisms (regardless of its location). Thus, Sampson, et al.'s collective efficacy measure is significantly related in a negative direction to just three (if you treat homicide and murder as one measure) of the eight 1995 crimes measures with just one exception. The one exception is that we found that collective efficacy is significantly related but in a positive direction (b=1.16) to a neighborhood's 1995 burglary rate within *GeoDa* while at the same time these two measures are not significant related when modeled within *HLM* (b=0.01). We did take several diagnostic steps but we still cannot account for why the same official crime measure is functioning differently in the two software packages.

Table 8 also provides the results from the same series of regression models we just described, but with dependent measures that contain five years of official data rather than just one year (in this section we did not model the survey-based dependent measures again because they were only collected during years 1994-1995). The addition of four more years of data provides us with a more precise time ordering than what was afford to us by just the 1995 data. With just the 1995 data, it is possible that all crimes occurred before the interviews took place, and without a doubt, all self-reported crime incidents happened before the respondents answered the questions about collective efficacy. The addition of these four years of data also should provide us with a more robust measure of the underlying criminality of neighborhood.

Once more, we find that collective efficacy is negatively related to the average five-year rate of homicide and rape to about the same degree as before and regardless which statistical application we used. In addition, within the *HLM* application, we find that collective efficacy is negatively associated with residential burglary, auto theft, and both types of vandalism (total and residential only). We similarly find that collective efficacy is related with these same four crime measures within *GeoDa*, but for one of these four crimes (Residential Burglary) the relationship is in the positive rather than negative direction. For the residential burglary measure, the negative relationship in 1995 changed to a positive relationship when we added the 1996 and 1999 incidents. This crime measure is the only one where the collective efficacy coefficient changed directions after more years of data were added (robbery also changed direction within *GeoDa* between 1995 and 1995-1999 models, but it was not significant in both models).

Our last set of regression models produces results that are the product of a somewhat different specification of the regression models and dependent measures than what we used in the first two sets. In the first two sets of regression, we did not consider time nor change as a factor to explain. However, for this last series of regressions, we not only consider whether the social context explains differences in crime rates between neighborhoods at one time point, but we also explore whether the social context at a point in time influences how crime changes into the future (e.g., in this particular analysis we look at the trajectory over ten years starting with the year 1995). More specifically, in addition to demonstrating how collective efficacy influences the rate of crime between neighborhoods net other social context factors, we also present results that demonstrate the extent to which collective efficacy is linearly related to the annual crime changes between the years 1995 and 2004.

We present the outcomes of this two-part effort in the last two columns on the right side of Table 8 (under the section labeled *1995 to 2004*). The first of the three columns of coefficient represent the relationships between collective efficacy and each crime at the models' intercept (for this particularly analysis the intercept represent the year 1995). The second column of coefficients represent how the average drop in crime over the two 1995-2004 period, and the third column of coefficients represent how collective efficacy measured at one point in time (1994-95) influences the pattern of crime change between 1995 and 2004. In other words, this last aspect of the model assesses whether collective efficacy measured in 1995 can influence the trajectory of crime over an entire decade. For this final set of regression models, we choose not to produce another fifteen regression models. Instead, we selected just the crimes which were influenced by collectively efficacy for the year 1995, or the average for the years 1995 through 1999 (because we did not see much value in assessing whether collective efficacy is related to how a particularly crime changed overtime if it was not initially related to a crime's base rate after controlling for its prior crime rate). In addition, we only present the coefficients produced by the trajectory subpart of the model that specified the collective efficacy measure as the sole independent variables (this coefficient tells us whether a neighborhoods crime trajectory over a decade is partially a function of the quantity of collective efficacy at the beginning of the decade). This collective efficacy coefficient is net the effect of each neighborhood's neighboring crime rate for each of the 10 years (e.g., we produce a measure for each year that captured the spatial lag affect and added that measure as level one variable). We had also examined how these trajectories behaved when we included the three other social context measures (all measured in 1990), but what we found is that none of these measures were related to the trajectory of any crime.

Overall, within this last series of seven regression models, we more often than not confirm our earlier analyses and we add some new results regarding how crime changed. First, like reported in other publications (i.e., Sampson, et al. 1993; Morenoff, et al. 2001) and shown in our previous regression analyses (see tables 3 through 8), collective efficacy remains negatively associated with rates of both homicide-murder and rape. The models shows that for the year 1995, the more collective efficacy that exist within a neighborhood net other social context factors the fewer homicides and rapes per resident are recorded by the police. These robust relationships do not exist for the other four crimes for the year 1995. This too is similar to what we found when we modeled just crime incidents reported in 1995. Besides finding that homicide and rape rates vary across neighborhood as function of the level of collective efficacy, we find that these crimes change on average within each neighborhood as a function of collective efficacy as well, but in a different direction. More specifically, we find that for both homicide-murder and rape rates, collective efficacy measured in 1995 is positively related to their pattern of change over a ten-year period. Thus, for those areas starting a relative low point in 1995 did not drop as quickly as those areas were crime was the highest. In other words, the downward trajectory of crime was flatter in areas with high collective efficacy.

Summary and Conclusions

For more than one hundred years, scientists have speculated about and tested for whether variations in neighborhood demographics and processes lead to different (often negative) outcomes for residents (Pratt and Cullen 2005; Sampson, Raudenbush, and Earls 1997). The most current thesis in this area of criminology is that collective efficacy is a key social process by which cohesion among residents coupled with their willingness to intervene will achieve many common goals (Sampson, Raudenbush, and Earls 1997). While the concept has been around for some time, it has experienced a surge of interest among scientists, and now practitioners, following the publication of Sampson, Raudenbush and Earls' 1997 *Science* article entitled "Neighborhood and Violent Crime: A Multilevel Study of Collective Efficacy." Their article reports that the impact of systemic neighborhood structural disadvantage is mediated (e.g., reduced in magnitude) by a process that they attribute to neighbors' willingness to intervene when their shared expectations are violated or at risk. Since the article's publication, a fairly robust body of empirical evidence, generated by Sampson, et al. and other scientists, attests to the relevance of collective efficacy as a factor in explaining different outcomes between communities within Chicago and beyond. Scientists have now linked collective efficacy to positive social outcomes like less interpersonal violence, better well-being, and superior educational outcomes (e.g., Browning 2002; Browning and Cagney 2002, 2003; Cohen et al. 2005; Kirk 2009; Sampson 2003; Simons et al. 2005; Vega et al. 2011). Community-based initiatives such as *Yes we can!* were also designed and implemented to improve the collective efficacy of the intervention communities (e.g., Foster-Fishman et al. 2007).

Despite all this attention, researchers and proponents of collective efficacy had not completed a thorough review, reproduction, and replication of Sampson, et al.'s (1997) original *Science* article, which was the primary purpose of our project. In addition, we sought to add to the growing body of evidence regarding the extent of the influence of collective efficacy on neighborhood outcomes. While other post-Sampson, et al. (1997) studies have tested collective efficacy in other communities and against dependent variables other than crime, we sought in this study to provide a more focused but rigorous assessment of the influence of their original collective efficacy model on many crime rates over time. More specifically, we assessed whether collective efficacy would affect homicide rates in Chicago in 1995 and throughout the subsequent decade and whether it would influence other crime types during those same years.

Two Reproductions

In two separate analyses, we reproduced Sampson, et al.'s (1997) substantive results about the positive influence that a neighborhood's collective efficacy has on influencing violence, particularly the significant negative association between a neighborhood's level of collective efficacy and the quantity of homicides recorded by the police in the City of Chicago during 1995. Using the best available data from the original Sampson, et al. (1997) analyses, and following the same analytical steps that they employed, we produced the same direction and met or exceeded the same level of statistical significance of all the substantive findings reported in 1997 by Sampson and his colleagues. In addition, using an alternative source for much of the same measures and, once more, the same analytical procedures, we have confirmed their substantive findings. This pattern of reproduction was not just for the central concept of collective efficacy and for one set of analysis, but for all substantive results that they reported in their 1997 *Science* article. This reproduction occurred at both the second level (between individuals nested within neighborhoods) and third levels (between neighborhoods) of a three level HLM regression model.[28]

Explaining Collective Efficacy

Overall, like Sampson, et al. (1997), we found that about 70 percent of collective efficacy's variance is explained by the neighborhood's quantity of concentrated disadvantage, immigrant concentration, and residential stability. The results show that all three measures play key independent roles in the formation of collective efficacy, although in differing degrees and directions. Among these three measures, the concentrated disadvantage factor likely provides the strongest foundation for communities to develop collective efficacy. In other words, the results demonstrate that the less concentrated disadvantage is imbedded in a neighborhood, the more likely it can build (a sense of) collective efficacy regardless of what else arises in the neighborhood or who is asked to report about collective efficacy. Besides the neighborhood factors, we also found a few respondent-level factors that helped to explain differences across the residents' perceptions of their neighborhood's level of collective efficacy. In particular, residents who owned their homes, moved less frequently, were older, and were wealthier reported higher levels of perceived neighborhood collective efficacy when compared to those in neighborhoods who did not report the same degree of these three attributes.

Direct Relationship between Collective Efficacy and Criminal Behavior

We reproduced the connection between Sampson, et al.'s (1997) collective efficacy construct and their three measures of violence across the neighborhoods. Our final series of analyses (see

[28] Sampson, et al. (1997) did not report the results from their level one HLM regression model.

tables 4(b) and 5) illustrate that after adjusting for several neighborhood structural factors (captured by three census-derived constructs), the more residents reported a feeling of collective efficacy in their neighborhoods (1) they reported fewer recent violent victimizations against themselves, (2) they perceived that there were fewer violent criminal incidents in their neighborhoods and (3) their neighborhoods had fewer homicides per resident recorded by the Chicago police.

<u>Collective Efficacy Mediates the Negative Effects of Concentrated Disadvantage</u>

Likewise, we reproduced Sampson, et al.'s (1997) key finding that collective efficacy mediates (reduces) the direct link between concentrated disadvantage and violence. Our reanalysis shows that when one compares the regression models without collective efficacy to those with collective efficacy (see tables 4(a) vs. 4(b)), the value (i.e., slope) of the concentrated disadvantage measure is reduced substantially in size across the board. For example, the size of the coefficient between concentrated disadvantage and neighborhood perceived violence decreased in size by sixty-two percent (0.279 vs. 0.107), and the coefficient between concentrated disadvantage and violent victimization went from a value of 0.25 to 0. This mediation effect is a key finding because it suggests that the consequence of systemic, structural factors, which have long been believed to have indelible influences on crime and delinquency across cities (Sampson and Groves 1989; Shaw and McKay 1942), may be attenuated by informal social processes within a neighborhood. Overall, while the numeric results are not identical to Sampson, et al.'s (1997), our pattern of outcomes parallel their results and conclusions regardless of the source of individual or aggregate level data. This level of reproduction attests to the overall quality of their work and exceeds the contemporary experiences of reproducing other criminological research (Maxwell and Garner 2009).

Prior Homicides as a Predictor of Future Homicides

While we did confirm Sampson, et al.'s findings about the correlation of collective efficacy and the interrelationship between collective efficacy, concentrated disadvantage and criminal behavior, we failed to reproduce one of the reported analyses in Sampson, et al. — the association between prior homicides and later violence. Sampson, et al. (1997) reported that this measure was positively associated with their 1995 homicide rate, but not associated with a neighborhood's level of perceived violence or with average rate of violent victimization. We did not find a significant association between past and future homicide rates. Our analyses using their measure of prior homicide found nearly no association between the two measures of the same crime. This lack of an association between past and future homicide rates did not substantively affect the other five coefficients in either of the two 1995 homicide models. For this reason, we conclude that we reproduced Sampson, et al.'s (1997) substantive findings.

Missing Data

Our reproduction of Sampson, et al. (1997) is imperfect for at least two reasons. First, it lacks information about how they addressed missing responses/data within their community survey. As noted in the methods section, there are missing responses spread across all the variables, and the frequency of missing responses varied by the question (the survey questions lacked responses for 7 to 34% of the cases). While Earls, et al. (Earls et al. 1997) provided both the raw and imputed variables, what Sampson, et al. (1997), did not provide was documentation that explained how they addressed the missing data beyond a brief discussion in another article about how their HLM framework adjusted for the missing data (see Raudenbush and Sampson 1999). Had our substantive findings not been so close to their published findings, this omission may have been a bigger problem.

The second reason our reproduction is imperfect is the failure of Earls, et al. (1997) to include the measure representing Sampson, et al.'s (1997) collective efficacy construct. Earls, et al. (1997) archived nearly every other key raw variable and every computed measure used by Sampson, et al. (1997) except for the central concept of their entire effort. This would be a fatal limitation had their study not included measures for the two concepts — social cohesion and informal social control — that Sampson, et al. (1997) assert are the basis for creating the collective efficacy measure. Moreover, Sampson, et al. (1997) were less clear about how these two measures were combined to create the measure of collective efficacy. It was only with some effort (and luck) that we were able to generate a measure that produced results so similar to those reported by Sampson, et al. (1997) that we are confident that it is their measure of collective efficacy.

Measurement Error

There are unknown amounts and many sources for measurement error in official crime statistics in Chicago and elsewhere (MacDonald 2002; Skogan 1974, 1981; Zedlewski 1983). MacDonald (2002) claims that the "disparity between crime rates suggested by victimization surveys and the rates suggested by Official Statistics … …is primarily a consequence of under-reporting by victims and under-recording by the police." Gibson and Kim (2006: 247) have also demonstrated that time-varying factors affect the propensity of victims to report crimes which can in turn "attenuate both cross-sectional and panel estimates "of the effect of ecological variables on crime. Therefore, it is possible that measurement error in the official data is not constant across Chicago's neighborhoods. For example, the reporting of a crime or the recording of the incident by the police may partially depend upon the ecological conditions of the neighborhoods.[29] It is also possible that due to changes in a neighborhood's ecological conditions, or because of administrative practices by the Chicago Police Department, that the measurement error in the official data was not stable over time.[30] In addition, since Sampson et al.'s collective efficacy measure is derived from their community survey research with known distributions of error, and dependent crime measures is produced by a process that is largely unknown and whose error distributions are largely unknown, the potential for a skew in the error terms is not zero.[31] Unfortunately, it is beyond the scope of this study to systematically assess how much measurement error exists or how it may have impacted our results. With these reproductions and extensions of Sampson, et al. (1997), our use of the collective efficacy concept will be enhanced if we can later identify and confirm measurement limitations, if we can measure changes in collective efficacy in neighborhoods over time, and if we can de-construct the components of collective efficacy to separate out, if possible, the active from the inactive components. We have confirmed that the effects of collective efficacy are real. Future research needs to determine how real, for which types of crime, and the costs and benefits of making

[29] An example of a cross-unit measurement problem was reported by Crockett, Randall, Shen, Russell, and Driscoll (2005) when they demonstrated the existence of measurement errors in depression scales across Latino and Anglo adolescents.

[30] One anonymous reviewer noted that the Chicago Police Department made changes around 2001 in the recording of assaults and batteries which he/she felt might change the trajectories for these crimes. However, it is not clear to us that a city-wide change would influence the comparisons across neighborhoods in the collective efficacy-crime relationship.

[31] How these two data sources were produced and the consequence of these two processes on the error term was initially raised by one of the anonymous reviewer. This reviewer was concerned that we were not sufficiently attuned to the measurement errors in the official data in our summary of our trajectory analysis.

changes in collective efficacy as a means to reduce crime.

Extending the test of Collective Efficacy.

Sampson, et al. (1997) initially focused on using collective efficacy and other measures to predict Chicago homicides in 1995. They and other scholars have subsequently expanded their investigation by changing some of their models and by adding several other crime measures and data collected from a few more years (e.g., Morenoff, Sampson, and Raudenbush 2001; Browning 2002). Like these researchers, we thought it was important to assess how much collective efficacy might influence criminal behavior beyond homicide in order to test whether collectively efficacy measured in 1995 was associated with many forms of violence and property crime types within and beyond 1995. Because we had successfully reproduced Sampson, et al.'s (1997) published findings, we were able to produce a rigorous test of the extent to which collective efficacy extends to other types of criminal behaviors and extends its influence beyond 1995. We tested a modified version[32] of their regression model against 19 different crime measures. Using this model, we found the same significant negative relationship between collective efficacy and the respondents' reporting of violence in their neighborhoods and household-victimization reported in Sampson, et al. (1997). This significant negative relationship exists regardless of the specific crime-interview question we modeled, or how we specified the regression models. However, in our adjusted model, the associations between collective efficacy and other officially recorded crimes vary in 1995 and for years following 1995. Several crime types, specifically the crimes of simple assault and theft, were influenced neither positively nor negatively by a neighborhood's level of collective efficacy.

[32] We added to the regression models a correction for spatial autocorrelation and reduced the quantity of multicollinearity.

Several patterns emerged by crime collection year(s) and crime type that are worth noting. For the year 1995, as noted above, we find that a neighborhood's quantity of collective efficacy is consistently associated with the residents' collective perception of and experiences with violent crime. In other words, the respondents' level of collective efficacy regularly influenced their reported rates of victimization and perceptions of violence within their neighborhoods. The more collective efficacy across a neighborhood the less a respondent reported violence and victimization. The collective efficacy measure is also negatively associated with the official homicide (and murder) and rape rates, as well as with the rate of residential burglary. However, collective efficacy is not significantly associated with the 1995 rate of robbery, aggravated assault, simple assault, or the overall level of violent crime reported to the police; nor did we find that collective efficacy is significantly related to the rate of all residual property crimes, thefts of any type, or vandalisms (regardless of its location). Thus, collective efficacy was related to less than half of the crime types in the City of Chicago for the year 1995. Using data from 1995 through 1999, we also find that collective efficacy is significantly related in a negative direction to the same three official crime measures as above -- official homicide (and murder), rape and residential burglary, and also with auto theft, residential vandalism, and total vandalism. These findings suggest that the influence of collective efficacy not only extends beyond 1995, but its influence on six of eight crime measures is as strong or stronger after 1995.

Finally, our last set of regression models considers not only the question of how collective efficacy influence crime rates between neighborhoods but also how it influences the rates of crime change within these neighborhoods over a decade long period. The findings from these analyses are largely consistent with many of our earlier findings. First, like patterns reported in prior publications (i.e., Sampson, et al. 1997; Morenoff, et al. 2001) and shown in our previous regression analyses (see tables 3 through 8), collective efficacy remains negatively associated with both homicide and rape rates during 1995, but it is not significantly related with other crime types. These analyses show that for the year 1995, the more collective efficacy that exists within a neighborhood, the fewer homicides and rapes are recorded by the police per resident in that neighborhood. Besides these robust findings, we also find that for homicides and rapes, the change in their annual frequency within neighborhoods is a function of collective efficacy. More specifically, we find that for both homicide and rape, collective efficacy (measured just in 1995) is positively related to their patterns of change over a ten-year period. In other words, while these two crimes started at lower rates in 1995 in neighborhoods with higher collective efficacy (as demonstrated by a negative slope for their respective intercept coefficient), they did not drop in frequency as quickly as in neighborhoods with less collective efficacy over the subsequent ten years. These crimes decreased at a significantly slower rate in high collective efficacy neighborhoods compared to neighborhoods with less collective efficacy. While these positive relationships initially seem counterintuitive, we realized this is because these crimes occurred relatively less frequent in 1995 as demonstrated by the *HLM*-intercept subpart of these models. Accordingly, we conclude that collective efficacy exerted its positive influence on these neighborhoods by initially suppressing the overall levels of these two crimes; but as time passed, a static measure of collective efficacy cannot explain additional crime decreases. Another way of looking at this it is to say that neighborhoods with the quickest reductions in homicides and rapes are those that started with the highest levels of these crimes and the lowest levels of collective efficacy. This leads us to ask the question, what then led to these significant reductions in homicides and rapes if this was not because of collective efficacy (in fact collective efficacy may impede further declines wherever crime is already low)? At this time, all that we can say is that crime rates drop most rapidly in neighborhoods with relatively low collective efficacy. The drop is unchanged or constant in neighborhoods with high or low concentrated disadvantage, residential stability, and immigrant concentration (as demonstrated by slopes approaching zero for all three measures for the six crime rates modeled overtime). Answering this question is beyond data available to us at this time, and it may well go unanswered for the City of Chicago because the time-dependent measures we need to answer this question may not exist at the levels of aggregation that match our data.

<u>Reproduction, Extension, and the Future of Collective Efficacy Theory</u>

The findings from our reproductions strongly support the reliability of the analyses reported by Sampson, et al., (1997). The findings from our extension of Sampson, et al.'s analyses identify some of the boundaries of collective efficacy theory. The statistically significant negative influence of collective efficacy varies by crime type and also remains when extending the number of years of crime data considered. Sampson, et al. (1997) do not address whether other crime types should be affected most by collective efficacy or for how many years it is reasonable to expect the effects of collective efficacy to persist. Using their data and their analytical approach, we have extended their analyses and identified what appears to be some of the boundaries of the effects of collective efficacy.

Sampson, et al. (1997: 923) made three important conclusions that have subsequently shaped more than a decade of criminological thinking. Their three conclusions were (1) "collective efficacy is an important construct that can be measured reliably at the neighborhood-level by means of survey research strategies," (2) collective efficacy… ... mediated a substantial portion of the association of residential stability and disadvantage with multiple measures of violence," and (3) "the combined measure of informal social control and cohesion and trust [is] a robust predictor of lower rates of violence." Based upon our comprehensive review, reproduction and extension of their research, there is nothing in our assessment that leads us to question their three specific conclusions even after we addressed several potential methodological issues such as multicollinearity, missing data, and spatial effects. Nonetheless, our project has identified some limitations in the scope or generalizability of their collective efficacy model. More specifically, while collective efficacy seems related to some forms of violence, both concurrently and for a fairly long period, it is not necessarily related to all forms of violence or to most forms of property offenses. We are somewhat perplexed by the failure of collective efficacy to affect property crimes given that the questions used to capture the collective efficacy construct are not particularly specific to violence per se, nor is the collective efficacy construct limited to affecting only the most infrequent and serious of crimes. In fact, it seems to us that collective efficacy should influence the crimes most likely to occur in places and in degrees that are amenable to informal social interference, intervention, and control (such as property crimes). Yet, just the opposite happened in the City of Chicago. For example, the crime of rape, whether it is committed by a stranger or by an acquaintance, does not come to our mind as one that is undertaken in a manner that others can frequently control through neighborhood social processes. In fact, a sizable body of research has shown that rape is not preventable by school-based educational programs and other informal social interventions that should fall under the collective efficacy rubric (Maxwell and Post 2002); nevertheless our research shows that this crime, like homicides, is influenced by the presence of neighborhood collective efficacy.

We and other students of the collective efficacy paradigm need to build upon the consistent findings and confront the paradoxes identified in this research. We need to address other methodological and substantive challenges in this area of research. For example, researchers using the PDHCN study should consider using alternative definitions of neighborhoods or nesting neighborhoods into the larger Chicago community network. Future data collections should consider measuring collective efficacy and structural changes over time in Chicago and elsewhere. As one of our anonymous reviewers pointed out, "it is hard to imagine that the neighborhood context in Chicago remained the same" during the ten years that we captured in this study. For example, this reviewer pointed out that during the same ten year period coved by our study the Chicago Housing Authority closed Cabrini Green and the Robert Taylor Homes and several neighborhoods were redeveloped. He/she also pointed to several changes in Chicago police practices and policies including their new Gang Ordinance, and their implementation of Project Safe Neighborhood and Operation Ceasefire, the latter of which likely positively impacted their targeted neighborhoods but not all neighborhoods (Skogan et al. 2009). Unfortunately, we do not believe we can test for some of these additional effects due to the mismatch between data capturing these "shocks" and PDHCN data. For instance, we had considered adding a time-dependent independent measure to capture community policing reforms across many Chicago precincts, but because precinct boundaries do not match the PHDCN neighborhood clusters, we do not believe we can pursue this question. Finally, if indeed collective efficacy is an important factor for controlling crime, work needs to commence in earnest to explore how communities without formal controls can produce or stimulate collective efficacy, and then assess whether this constructed process works like the organic collective efficacy measured by Sampson, et al. (1997) and others. If this latter set of questions goes unanswered, if communities cannot learn to capture the power of collective efficacy to improve their collective quality of life, then this body of research will not have served the purpose of advancing knowledge to transform communities.

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Table 1. Comparison between the Sampson et al 1997 Measures and the Replicated Measures

		N	Mean	Std. Dev.	Minimum	Maximum	Correlation	Paired-T	
@ the Respondent Level									
Collective Efficacy	EBRS*	7729	3.42	0.71	1.0	5.0	0.99	-0.39	
	MGS	7727	3.42	0.58	1.0	5.0			
Social Control	EBRS	7729	3.46	0.93	1.0	5.0	0.99	-0.41	
	MGS	7720	3.46	0.72	1.0	5.0			
Social Cohesion	EBRS	7729	3.37	0.68	1.0	5.0	0.99	-0.11	
	MGS	7626	3.36	0.49	1.0	5.0			
Violent Victimization	EBRS	7565	0.13	0.34	0.0	1.0	1.00	NA	
	MGS	7565	0.13	0.34	0.0	1.0			
Perceived Neighborhood Violence	EBRS	7729	1.81	0.79	1.0	4.0	0.99	2.46	***
	MGS	7290	1.80	0.83	1.0	4.0			
@ the Neighborhood Cluster Level									
Collective Efficacy	EBRS*	342	3.61	0.28	2.88	4.41	0.97	62.33	***
	MGS	342	3.43	0.28	2.70	4.29			
Social Control	EBRS	342	3.88	0.30	3.08	4.72	0.95	7.72	***
	MGS	342	3.49	0.31	2.62	4.38			
Social Cohesion	EBRS	342	3.35	0.26	2.65	4.11	0.97	-39.75	***
	MGS	342	3.38	0.23	2.78	4.08			
Concentrated Disadvantage	EBRS	342	0.00	0.99	-1.65	3.81	0.98	0.03	
	MGS	342	0.00	0.99	-1.16	4.33			
Concentrated Immigration	EBRS	342	0.00	0.97	-1.63	3.07	0.94	0.06	
	MGS	342	0.00	0.90	-0.89	2.70			
Residential Stability	EBRS	342	0.00	0.98	-2.18	2.33	0.94	0.86	
	MGS	342	0.00	0.87	-1.80	2.01			
Ave. Homicide Rate from 1988 to 1990	EBRS*	342	29.17	31.59	0.00	157.10	0.99	2.63	**
	MGS	342	28.72	31.14	0.00	157.10			
Homicide Rate for 1995	EBRS	342	30.64	35.68	0.00	238.00	0.93	-1.87	
	MGS	342	32.07	37.25	0.00	263.85			

EBRS= measure produced by Earls, Brooks-Cunn, Raudenbush, & Sampson

MGS=measure computed by Maxwell, Garner & Skogan

* = Maxwell, Garner, & Skogan computed measures because it is not available in the database acquired by the NACJD.

Table 2. Oblique Factor Roation Factor Patterns (n=343)

Variable	Factor Loadings SRE Reported	MGS Analysis
Concentreated disadvantage		
Percent below poverty line	0.93	0.94
Percent on public assistance	0.94	0.97
Percent female-headed families	0.93	0.95
Percent unemployed	0.86	0.95
Percent less than age 18	0.94	0.84
Percent black	0.60	0.59
Immigratn concentration		
Percent Latino	0.88	0.85
Percent foreign-born	0.70	0.85
Residential stability		
Percent same house as in 1985	0.77	0.78
Percent owner-occupied house	0.86	0.88

SRE=Reported by Sampson, Raudenbush, & Earls (1997) in Table 2.
MGS=Produced by Maxwell, Garner, & Skogan

Table 3. Correlates of Collective Efficacy

	SRE Reported			MGS-1st Reproduction			MGS-2nd Reproduction		
Variables	b	s.e.	t	b	s.e.	t	b	s.e.	t
Intercept	3.523	0.013	263.20	3.319	0.048	69.25 ***	3.303	0.045	72.89 ***
Person-level predictors									
Female	-0.012	0.015	-0.76	-0.014	0.015	-0.89	0.000	0.016	0.03
Married	-0.005	0.021	-0.25	0.001	0.022	0.04	-0.018	0.022	-0.82
Separated or divorced	-0.045	0.026	-1.72	-0.038	0.027	-1.39	-0.045	0.027	-1.67
Single	-0.026	0.024	-1.05	-0.024	0.025	-0.10	-0.020	0.025	-0.81
Homeowner	0.122	0.020	6.04	0.139	0.020	6.90 ***	0.109	0.021	5.17 ***
Latino	0.042	0.028	1.52	-0.038	0.031	-1.23	0.048	0.028	1.68
Black	-0.029	0.030	-0.98	0.022	0.028	0.78	-0.037	0.031	-1.19
Mobility	-0.025	0.007	-3.71	-0.028	0.007	-4.08 ***	-0.026	0.007	-3.82 ***
Age	0.000	0.001	3.47	0.001	0.001	2.34 ***	0.003	0.000	4.35 ***
Years in neighborhood	0.001	0.001	0.78	0.000	0.001	0.05	0.001	0.001	0.06
SES	0.004	0.008	4.64	0.001	0.000	2.27 ***	0.065	0.013	5.03
Neighborhood-level predictors									
Concentrated disadvantage	-0.172	0.016	-10.74	-0.186	0.016	-11.50 ***	-0.169	0.017	-10.09 ***
Immigrant concentration	-0.037	0.014	-2.66	-0.045	0.014	-3.12 **	-0.043	0.015	-2.96 **
Residential stability	0.074	0.130	5.61	0.074	0.014	5.42 ***	0.077	0.013	5.72 ***
Variance Component		var. comp.			var. comp.			var. comp.	
Intercept — Within neighborhoods		0.320			0.321			0.319	
Level-2 — Between Neighborhoods		0.026			0.027			0.027	
Percent of variance explained within neighborhoods		3%			3%			3%	
Percent of variance explained between neighborhoods		70%			71%			71%	

SRE=Reported by Sampson, Raudenbush, & Earls (1997) in Table 3.

F.Reproduction=Used of all variables measures provided by EBRS

Q.Reproduction=Used raw response variables provided by EBRS, but calculated new measures to address missing responses

Table 4(a). Neighborhood correlates of perceived neighborhood violence, violent victimization and 1995 homicide event

	Model 1. Social Composition								
	SRE Reported			MGS-1st Reproduction			MGS-2nd Reproduction		
Variables	b	s.e.	t	b	s.e.	t	b	s.e.	t
Perceived neighborhood violence									
Concentrated disadvantage	0.277	0.021	13.30	0.279	0.614	13.53 ***	0.275	0.021	13.32 ***
Immigrant concentration	0.041	0.017	2.44	0.045	0.021	2.63 **	0.049	0.018	2.72 **
Residential stability	-0.102	0.015	-6.95	-0.101	0.015	-6.85 ***	-0.103	0.015	-7.05 ***
Collective efficacy									
Violent Victimization									
Concentrated disadvantage	0.258	0.045	5.71	0.249	0.044	5.59 ***	0.228	0.047	4.89 ***
Immigrant concentration	0.141	0.046	3.06	0.132	0.045	2.95 **	0.146	0.047	3.01 **
Residential stability	-0.143	0.050	-2.84	-0.146	0.050	-2.94 **	-0.148	0.150	-2.99 **
Collective efficacy									
1995 homicide events									
Concentrated disadvantage	0.727	0.046	14.91	0.516	0.051	7.92 ***			
Immigrant concentration	-0.022	0.057	-0.04	-0.083	0.065	-1.30			
Residential stability	0.093	0.042	2.18	0.108	0.064	2.24 *			
Collective efficacy									

Table 4(b). Neighborhood correlates of perceived neighborhood violence, violent victimization and 1995 homicide event

	Model 2. Social composition & Collective efficacy											
	SRE Reported			MGS-1st Reproduction				MGS-2nd Reproduction				
Variables	b	s.e.	t	b	s.e.	t		b	s.e.	t	
Perceived neighborhood violence											
Concentrated disadvantage	0.171	0.024	7.24	0.107	0.022	4.77	***	0.167	0.022	7.50	***
Immigrant concentration	0.018	0.016	1.12	0.009	0.016	0.05		0.030	0.017	1.74	
Residential stability	-0.056	0.016	-3.49	-0.015	0.014	-1.06		-0.041	0.015	-2.69	**
Collective efficacy	-0.618	0.104	-5.95	-0.912	0.085	-10.70	***	-0.552	0.065	-8.39	***
Violent Victimization											
Concentrated disadvantage	0.085	0.054	1.58	-0.009	0.057	-0.17		0.059	0.055	1.08	
Immigrant concentration	0.098	0.044	2.20	0.060	0.044	1.38		0.100	0.046	2.18	*
Residential stability	-0.031	0.051	-0.60	0.000	0.051	0.01		-0.037	0.050	-0.74	
Collective efficacy	-1.190	0.240	-4.96	-1.533	0.246	-6.24	***	-1.166	0.236	-4.95	***
1995 homicide events											
Concentrated disadvantage	0.491	0.064	7.65	0.254	0.085	2.98	**				
Immigrant concentration	-0.078	0.050	-1.45	-0.135	0.060	-2.25	*				
Residential stability	0.208	0.046	4.52	0.217	0.054	4.04	***				
Collective efficacy	-1.471	0.261	-5.64	-1.641	0.310	-5.30	***				

Table 5. Predictors of neigborhood level violence, victimzation and homicide in 1995 with prior homicide controlled

	SRE Reported			MGS-1st Reproduction			MGS-2nd Reproduction		
	b	s.e.	t	b	s.e.	t	b	s.e.	t
Perceived Neighborhood Violence as outcome									
Intercept	3.772	0.379	9.95	5.517	0.328	16.81 ***	4.118	0.223	18.43 ***
Concentrated disadvantage	0.157	0.025	6.38	0.111	0.026	4.22 ***	0.191	0.030	6.30
Immigrant concentration	0.020	0.016	1.21	0.008	0.016	0.50	0.026	0.017	1.49 ***
Residential stability	-0.054	0.016	-3.39	-0.016	0.015	-1.07	-0.045	0.016	-2.80 *
Collective efficacy	-0.594	0.108	-5.53	-0.916	0.085	-10.78 ***	-0.552	0.001	-8.47 ***
Prior Homicide	0.018	0.014	1.27	0.009	0.028	-0.30	0.001	0.651	-1.27
Violent Victimization as outcome									
Intercept	-2.015	0.042	-49.24	3.855	1.083	3.56 **	2.084	0.848	-2.46
Concentrated disadvantage	0.073	0.060	1.22	0.008	0.065	0.13	0.064	0.075	0.85
Immigrant concentration	0.098	0.045	2.20	0.059	0.004	1.33	0.100	0.047	2.11 *
Residential stability	-0.029	0.052	-0.56	-0.001	0.051	-0.02	-0.037	0.051	-0.75
Collective efficacy	-1.176	0.251	-4.69	-1.549	0.253	-6.12 ***	1.164	0.235	-4.94 ***
Prior Homicide	0.017	0.049	0.34	0.037	0.095	-0.39	0.000	0.002	-0.08
Homicide in 1995 as outcome									
Intercept	3.071	0.050	62.01	7.199	0.941	7.65 ***			
Concentrated disadvantage	0.175	0.072	2.42	0.219	0.106	2.07 *			
Immigrant concentration	-0.034	0.044	-0.77	-0.085	0.616	-1.38			
Residential stability	0.229	0.043	5.38	0.211	0.054	3.89 ***			
Collective efficacy	-1.107	0.272	-4.07	-1.202	0.278	-4.33 ***			
Prior Homicide	0.397	0.070	5.64	0.005	0.003	1.67			

Table 6. Homicide Event Models reported in Sampson et al. (1997)'s *Neighborhoods and Violent Crime: A Multilevel Study of Collective Efficacy*

Table 4., Model 1

	SRE Reported			MGS-1st Reproduction			MGS-2nd Reproduction		
	b	s.e.	t	b	s.e.	t	b	s.e.	t
Intercept		NR			NR			NR	
Concentrated Disadvantage	0.727	0.046	14.91	0.516	0.051	7.92 ***	0.465	0.062	7.55 ***
Immigrant Concentration	-0.022	0.057	-0.04	-0.083	0.065	-1.30	-0.108	0.072	-1.49
Residential Stability	0.093	0.042	2.18	0.108	0.064	2.24 *	0.127	0.064	1.96 *
Collective Efficacy									
Ave. 1988-90 Homicide Rate									

Table 4., Model 2

	SRE Reported			MGS-1st Reproduction			MGS-2nd Reproduction		
	b	s.e.	t	b	s.e.	t	b	s.e.	t
Intercept		NR			NR			NR	
Concentrated Disadvantage	0.491	0.064	7.65	0.254	0.085	2.98 **	0.314	0.074	4.27 ***
Immigrant Concentration	-0.078	0.050	-1.45	-0.135	0.060	-2.25 *	-0.128	0.069	-1.85
Residential Stability	0.208	0.046	4.52	0.217	0.054	4.04 ***	0.252	0.068	3.71 ***
Collective Efficacy	**-1.471**	**0.261**	**-5.64**	**-1.641**	**0.310**	**-5.30 *** **	**-1.269**	**0.266**	**-4.77** ***
Ave. 1988-90 Homicide Rate									

Table 5., Model 3

	SRE Reported			MGS-1st Reproduction			MGS-2nd Reproduction		
	b	s.e.	t	b	s.e.	t	b	s.e.	t
Intercept	3.071	0.050	62.01	7.199	0.941	7.65 ***	7.354	0.897	8.20 ***
Concentrated Disadvantage	0.175	0.070	2.42	0.219	0.106	2.07	0.179	0.103	1.73
Immigrant Concentration	-0.034	0.044	-0.77	-0.085	0.616	-1.38	-0.096	0.070	-1.36
Residential Stability	0.229	0.043	5.38	0.211	0.054	3.89 ***	0.255	0.069	3.72 ***
Collective Efficacy	**-1.107**	**0.272**	**-4.07**	**-1.202**	**0.278**	**-4.33 *** **	**-1.237**	**0.265**	**-4.67** ***
Ave. 1988-90 Homicide Rate	0.397	0.070	5.64	0.005	0.003	1.67	0.006	0.003	1.93

Reproduction = SRE's data & variables, and their model specifications

Replication = MGS data (original census & updated crime data; and their communisty survey data but our calculation of their Collective Efficacy Measure), and their model specification

Table 7. Test of Reproducibility: 1995 Homicide SRE's Model

	MGS-1st Reproduction							Direction &	Clogg
	SRE			MGS				Statistical	Z-
Perceived Violence	b^1	s.e.	t	b^2	s.e.	t	$(b^1 - b^2)$	Significance	value
Intercept	3.772	0.379	9.950	5.517	0.328	16.808	-1.75	Same	-3.48 ***
Concentrated Disadvantage	0.157	0.025	6.380	0.111	0.026	4.217	0.05	Same	1.26
Immigrant Concentration	0.020	0.016	1.205	0.008	0.016	0.504	0.01	Same	0.52
Residential Stability	-0.054	0.016	-3.390	-0.016	0.015	-1.072	-0.04	Same	-1.76
Collective Efficacy	-0.594	0.108	-5.530	-0.916	0.085	-10.783	0.32	Same	2.35 ***
Ave. 1988-90 Homicide Rate	0.018	0.014	1.270	0.009	0.028	-0.300	0.01	Same	0.30
Violent Victimization									
Intercept	-2.015	0.042	-49.240	3.855	1.083	3.561	-5.87	Same	-5.42 ***
Concentrated Disadvantage	0.073	0.060	1.220	0.008	0.065	0.125	0.06	Same	0.73
Immigrant Concentration	0.098	0.045	2.200	0.059	0.004	1.330	0.04	Not Same	0.87
Residential Stability	-0.029	0.052	-0.560	-0.001	0.051	-0.020	-0.03	Same	-0.38
Collective Efficacy	-1.176	0.251	-4.690	-1.549	0.253	-6.123	0.37	Same	1.05
Ave. 1988-90 Homicide Rate	0.017	0.049	0.340	0.037	0.095	-0.394	-0.02	Same	-0.19
1995 Homicide Counts									
Intercept	3.071	0.050	62.010	7.199	0.941	7.654	-4.13	Same	-4.38 ***
Concentrated Disadvantage	0.175	0.070	2.420	0.219	0.106	2.074	-0.04	Same	-0.34
Immigrant Concentration	-0.034	0.044	-0.770	-0.085	0.616	-1.382	0.05	Same	0.08
Residential Stability	0.229	0.043	5.380	0.211	0.054	3.891	0.02	Same	0.26
Collective Efficacy	-1.107	0.272	-4.070	-1.202	0.278	-4.333	0.10	Same	0.24
Ave. 1988-90 Homicide Rate	0.397	0.070	5.640	0.005	0.003	1.669	0.39	Not Same	5.60 ***
1995 Homicide Counts	MGS-2nd Reproduction								
Intercept	3.071	0.050	62.010	7.354	0.897	8.198	-4.28	Same	-4.77 ***
Concentrated Disadvantage	0.175	0.070	2.420	0.179	0.103	1.729	0.00	Not Same	-0.03
Immigrant Concentration	-0.034	0.044	-0.770	-0.096	0.070	-1.362	0.06	Same	0.74
Residential Stability	0.229	0.043	5.380	0.255	0.069	3.715	-0.03	Same	-0.32
Collective Efficacy	-1.107	0.272	-4.070	-1.237	0.265	-4.673	0.13	Same	0.34
Ave. 1988-90 Homicide Rate	0.397	0.070	5.640	0.006	0.003	1.931	0.39	Not Same	5.58 ***

Table 8. Conditional Relationship between Collective Efficacy and Crime

	Year 1995				Average for years 1995 to 1999		Change from years 1995 to 2004		
	HLM [$P]		*GeoDa* [&]		*HLM* [#]	*GeoDa* [&]	*HLM*-Intercept	*HLM*-Time	
	Official	Survey	Official	Survey	Official	Official	Official	Intercept	Change
Homicide	-0.10 ***		-8.51 **		-0.09 ***	-6.00 ***	-68.3 ***	-0.27 ***	0.03 ***
Violence	0.00	-0.41 ***	-0.01	-0.33 ***	0.00	0.00			
Murder	-0.11 ***		-9.03 **		-0.09 ***	-0.99 ***	-68.3 ***	-0.27 ***	0.03 ***
Rape	-0.07 ***	-0.27 ***	-0.06 ***	-0.13 ***	-0.04 **	-0.37 **	-69.2 ***	-0.27 ***	0.03 ***
Robbery	-0.01	-0.46 ***	0.00	-0.26 ***	-0.01	-0.02			
Agg. Assault	-0.01		-0.01		-0.01	-0.90			
Simple Assault	0.00		0.00		0.00	0.00			
Property									
Residential Property	0.00		0.00		0.00	0.00			
Burglary	0.01		1.16 *		0.01	0.04			
Residential Burglary	-0.02 **	-0.34 **	0.01	-0.07 **	-0.03 ***	0.08 **	-12.8	-0.07 ***	0.01
Auto Theft	-0.01		0.00		-0.01 *	-0.02 *	0.8	-0.02	0.00
Theft	0.00	-0.24 *	0.00	-0.08 *	0.00	0.00			
Vandalism	0.00		-0.05		-0.01 *	-0.02 **	-8.6	-0.04	0.00
Residential Vandalism	-0.01	-0.25 *	0.00	-0.09 **	-0.02 **	-0.02 **	-11.4	-0.06 ***	0.01

P= Poisson Regression

&= OLS Regression

$= log[L] = B0 + B1*(CD) + B2*(IC) + B3*(RS) + B4*(Col. Eff.) + B5*(Ave. 1991-92 Crime Residual)

#= log[L] = (P0 = B00 + B01*(CD) + B02*(IC) + B03*(RS) + B04*(Coll. Eff.) + B05*(Ave. 1991-92 Crime Residual)) + P1*(Year) + P2*(Spatial Crime Lag)

**= p-value < 0.01; ***=p-value < 0.001

Appendix A.1. Homicide

HLM 6.8

Model 1 (y1995); d.f.=336 $		Coefficient	SE	t	p-value
Intercept	G00	1.95	0.217	0.900	0.000
Concentrated Disadvantage, 1990	G10	0.10	0.018	5.738	0.000
Immigrant Concentration, 1990	G20	-0.02	0.010	-2.291	0.023
Residential Stability, 1990	G30	0.02	0.011	1.717	0.086
Collective Efficacy, 1994-95	G40	-0.10	0.030	-3.404	0.001
Crime Rate, 1990*	G50	0.15	0.025	6.101	0.000
Model 2 (y1995-1999); d.f.=1,702 $					
Intercept					
Intercept	B00	1.81	0.128	14.134	0.000
Concentrated Disadvantage, 1990	B01	0.11	0.011	10.345	0.000
Immigrant Concentration, 1990	B02	-0.02	0.007	-2.579	0.010
Residential Stability, 1990	B03	0.02	0.007	3.355	0.001
Collective Efficacy, 1994-95	B04	-0.09	0.018	-4.818	0.000
Crime Rate, 1990*	B05	0.12	0.015	7.591	0.000
Year	B10	-0.01	0.003	-3.548	0.001
Spatial Lag of Crime Rate	B20	-0.04	0.074	-0.576	0.564

*=Residual of G10,G20,G30

$=HLM v6.08 Poisson restricted likelihood regression and population-average model with robust standard errors

GeoDA 0.9.5-i5

Model 3 (y1995); d.f.=335	Coefficient	SE	z	p-value
Spatial Lag of Crime Rate	0.13	0.070	1.854	0.063
Intercept	61.57	20.797	2.963	0.003
Concentrated Disadvantage, 1990	9.36	1.612	5.807	0.000
Immigrant Concentration, 1990	-2.29	1.045	-2.189	0.029
Residential Stability, 1990	1.59	1.102	1.443	0.149
Collective Efficacy, 1994-95	-8.51	2.870	-2.964	0.003
Crime Rate, 1990*	12.74	2.228	5.718	0.000
	Log Likelihood=	11460.420	R-Sqr.=	0.491

Model 4 (Aveage of y1995-y1999); d.f.=335	Coefficient	SE	z	p-value
Spatial Lag of Crime Rate	0.35	0.050	6.941	0.000
Intercept	43.36	11.226	3.862	0.000
Concentrated Disadvantage, 1990	8.23	0.928	8.871	0.000
Immigrant Concentration, 1990	-1.44	0.566	-2.537	0.011
Residential Stability, 1990	1.45	0.596	2.433	0.015
Collective Efficacy, 1994-95	-6.00	1.550	-3.872	0.000
Crime Rate, 1990*	6.92	1.229	5.631	0.000
	Log Likelihood=	-1256.070	R-Sqr.=	0.763

Appendix A.1. Homicide

Model 5 (y1995-2004); d.f=?		Coefficient	SE	t	p-value
Intercept	B00	535.13	97.646	5.480	0.000
Concentrated Disadvantage, 1990	B01	0.22	0.024	9.157	0.000
Immigrant Concentration, 1990	B02	-0.05	0.015	-3.020	0.003
Residential Stability, 1990	B03	0.04	0.016	2.657	0.008
Collective Efficacy, 1994-95	B04	-68.25	13.383	-5.100	0.000
Crime Rate, 1990*	B05	0.21	0.032	6.458	0.000
Year Intercept	B10	-0.27	0.049	-5.473	0.000
Collective Efficacy, 1994-95	B11	0.03	0.007	5.087	0.000
Spatial Lag of Crime Rates Intercept	B20	1.35	0.110	12.288	0.000

*=Residual of G10,G20,G30

$=HLM v6.08 Poisson restricted likelihood regression and population-average model with robust standard errors

Appendix A.2. Violence

HLM 6.8

Model 1 (y1995); d.f.=336 $		Coefficient	SE	t	p-value
Intercept	G00	1.155	0.042	27.584	0.000
Concentrated Disadvantage, 1990	G10	-0.004	0.002	-2.147	0.032
Immigrant Concentration, 1990	G20	0.005	0.001	4.008	0.000
Residential Stability, 1990	G30	0.003	0.001	2.948	0.004
Collective Efficacy, 1994-95	G40	-0.001	0.003	-0.381	0.703
Crime Rate, 1990*	G50	0.117	0.004	33.335	0.000

Model 2 (y1995-1999); d.f.=1,702 $					
Intercept					
Intercept	B00	2.133	0.022	94.890	0.000
Concentrated Disadvantage, 1990	B01	0.064	0.002	37.162	0.000
Immigrant Concentration, 1990	B02	-0.015	0.001	-14.292	0.000
Residential Stability, 1990	B03	-0.013	0.001	-11.026	0.000
Collective Efficacy, 1994-95	B04	0.000	0.003	0.141	0.888
Crime Rate, 1990*	B05	0.116	0.003	34.360	0.000
Year	B10	-0.003	0.000	-10.567	0.000
Spatial Lag of Crime Rate	B20	-0.056	0.010	-5.539	0.000

*=Residual of G10,G20,G30

$=HLM v6.08 Poisson restricted likelihood regression and population-average model with robust standard errors

GeoDA 0.9.5-i5

Model 3 (y1995); d.f.=335	Coefficient	SE	z	p-value
Spatial Lag of Crime Rate	0.046	0.026	1.771	0.077
Intercept	-0.005	0.020	-0.253	0.800
Concentrated Disadvantage, 1990	0.063	0.002	33.132	0.000
Immigrant Concentration, 1990	-0.015	0.001	-15.247	0.000
Residential Stability, 1990	-0.014	0.001	-13.617	0.000
Collective Efficacy, 1994-95	0.001	0.003	0.242	0.809
Crime Rate, 1990*	0.106	0.003	32.226	0.000
	Log Likelihood= 929.548		R-Sqr.= 0.963	

Model 4 (Aveage of y1995-y1999); d.f.=335	Coefficient	SE	z	p-value
Spatial Lag of Crime Rate	0.125	0.038	3.281	0.001
Intercept	-0.002	0.012	-0.187	0.852
Concentrated Disadvantage, 1990	0.022	0.001	19.210	0.000
Immigrant Concentration, 1990	-0.004	0.001	-6.673	0.000
Residential Stability, 1990	-0.004	0.001	-6.470	0.000
Collective Efficacy, 1994-95	0.000	0.002	-0.007	0.995
Crime Rate, 1990*	0.047	0.002	22.698	0.000
	Log Likelihood= 1092.270		R-Sqr.= 0.913	

Appendix A.3. Murder

HLM 6.8

Model 1 (y1995); d.f.=336 $		Coefficient	SE	t	p-value
Intercept	G00	1.983	0.216	9.174	0.000
Concentrated Disadvantage, 1990	G10	0.099	0.018	5.520	0.000
Immigrant Concentration, 1990	G20	-0.026	0.011	-2.396	0.017
Residential Stability, 1990	G30	0.021	0.011	1.875	0.061
Collective Efficacy, 1994-95	G40	-0.107	0.030	-3.582	0.001
Crime Rate, 1990*	G50	0.156	0.025	6.166	0.000

Model 2 (y1995-1999); d.f.=1,702 $					
Intercept					
Intercept	B00	1.832	0.128	14.350	0.000
Concentrated Disadvantage, 1990	B01	0.109	0.011	10.214	0.000
Immigrant Concentration, 1990	B02	-0.017	0.007	-2.532	0.012
Residential Stability, 1990	B03	0.025	0.007	3.489	0.001
Collective Efficacy, 1994-95	B04	-0.089	0.018	-5.025	0.000
Crime Rate, 1990*	B05	**0.116**	0.016	7.381	0.000
Year	B10	-0.011	0.003	-3.304	0.001
Spatial Lag of Crime Rate	B20	-0.013	0.074	-0.174	0.863

*=Residual of G10,G20,G30

$=HLM v6.08 Poisson restricted likelihood regression and population-average model with robust standard errors

GeoDA 0.9.5-i5

Model 3 (y1995); d.f.=335	Coefficient	SE	z	p-value
Spatial Lag of Crime Rate	0.120	0.070	1.697	0.897
Intercept	65.346	20.848	3.134	0.002
Concentrated Disadvantage, 1990	9.192	1.613	5.699	0.000
Immigrant Concentration, 1990	-2.504	1.052	-2.378	0.017
Residential Stability, 1990	1.767	1.107	1.596	0.111
Collective Efficacy, 1994-95	-9.029	2.877	-3.138	0.002
Crime Rate, 1990*	13.139	2.264	5.804	0.000
Log Likelihood= -1462.150		R-Sqr.= 0.487		

Model 4 (Aveage of y1995-y1999); d.f.=335	Coefficient	SE	z	p-value
Spatial Lag of Crime Rate	0.389	0.050	7.810	0.000
Intercept	6.912	1.636	4.226	0.000
Concentrated Disadvantage, 1990	1.063	0.134	7.930	0.000
Immigrant Concentration, 1990	-0.079	0.082	-0.967	0.334
Residential Stability, 1990	0.238	0.087	2.737	0.006
Collective Efficacy, 1994-95	-0.993	0.226	-4.391	0.000
Crime Rate, 1990*	0.923	0.181	5.101	0.000
Log Likelihood= -600.245		R-Sqr.= 0.759		

Appendix A.3. Murder (cont.)

HLM 6.8

Model 5		Coefficient	SE	t	p-value
Intercept	B00	535.129	97.000	5.480	0.000
Concentrated Disadvantage, 1990	B01	0.217	0.024	9.157	0.000
Immigrant Concentration, 1990	B02	-0.046	0.015	-3.020	0.003
Residential Stability, 1990	B03	0.042	0.016	2.657	0.008
Collective Efficacy, 1994-95	B04	-68.253	13.380	-5.100	0.000
Crime Rate, 1990*	B05	0.021	0.032	6.458	0.000
Year Intercept	B10	-0.267	0.049	-5.473	0.000
Collective Efficacy, 1994-95	B11	0.034	0.007	5.087	0.000
Spatial Lag of Crime Rates Intercept	B20	1.350	0.110	12.288	0.000

Appendix A.3. Sexual Assault

HLM 6.8

Model 1 (y1995); d.f.=336 $		Coefficient	SE	t	p-value
Intercept	G00	1.987	0.143	13.904	0.000
Concentrated Disadvantage, 1990	G10	0.101	0.010	10.443	0.000
Immigrant Concentration, 1990	G20	-0.065	0.006	-10.080	0.000
Residential Stability, 1990	G30	-0.030	0.008	-3.959	0.000
Collective Efficacy, 1994-95	G40	-0.074	0.020	-3.728	0.000
Crime Rate, 1990*	G50	0.147	0.018	8.017	0.000

Model 2 (y1995-1999); d.f.=1,702 $					
Intercept					
Intercept	B00	1.739	0.104	16.761	0.000
Concentrated Disadvantage, 1990	B01	0.111	0.007	15.563	0.000
Immigrant Concentration, 1990	B02	-0.052	0.004	-12.571	0.000
Residential Stability, 1990	B03	-0.021	0.005	-4.489	0.000
Collective Efficacy, 1994-95	B04	-0.041	0.014	-2.901	0.004
Crime Rate, 1990*	B05	0.148	0.010	14.340	0.000
Year	B10	-0.006	0.002	-2.391	0.017
Spatial Lag of Crime Rate	B20	0.144	0.034	4.242	0.000

*=Residual of G10,G20,G30

$=HLM v6.08 Poisson restricted likelihood regression and population-average model with robust

GeoDA 0.9.5-i5

Model 3 (y1995); d.f.=335	Coefficient	SE	z	p-value
Spatial Lag of Crime Rate	0.158	0.059	2.701	0.007
Intercept	0.452	0.135	3.353	0.001
Concentrated Disadvantage, 1990	0.091	0.011	8.365	0.000
Immigrant Concentration, 1990	-0.061	0.007	-8.287	0.000
Residential Stability, 1990	-0.025	0.007	-3.373	0.001
Collective Efficacy, 1994-95	-0.063	0.019	-3.365	0.001
Crime Rate, 1990*	0.011	0.014	7.678	0.000
	Log Likelihood=	261.777	R-Sqr.=	0.713

Model 4 (Aveage of y1995-y1999); d.f.=335	Coefficient	SE	z	p-value
Spatial Lag of Crime Rate	0.339	0.046	7.312	0.000
Intercept	2.488	0.886	2.808	0.005
Concentrated Disadvantage, 1990	0.701	0.074	9.534	0.000
Immigrant Concentration, 1990	-0.285	0.047	-6.122	0.000
Residential Stability, 1990	-0.165	0.048	-3.465	0.001
Collective Efficacy, 1994-95	-0.371	0.122	-3.028	0.002
Crime Rate, 1990*	1.144	0.098	11.710	0.000
	Log Likelihood=	-387.522	R-Sqr.=	0.818

Appendix A.3. Sexual Assault

HLM 6.8					
Model 5 (y1995-2004); d.f=?		Coefficient	SE	t	p-value
Intercept	B00	542.844	88.241	6.152	0.000
Concentrated Disadvantage, 1990	B01	0.283	0.025	11.336	0.000
Immigrant Concentration, 1990	B02	-0.163	0.016	-10.176	0.000
Residential Stability, 1990	B03	-0.037	0.016	-2.315	0.021
Collective Efficacy, 1994-95	B04	-69.178	12.077	-5.728	0.000
Crime Rate, 1990*	B05	0.406	0.031	13.080	0.000
Year Intercept	B10	-0.270	0.044	-6.120	0.000
Collective Efficacy, 1994-95	B11	0.035	0.006	5.713	0.000
Spatial Lag of Crime Rates Intercept	B20	0.830	0.071	11.647	0.000

*=Residual of G10,G20,G30

$=HLM v6.08 Poisson restricted likelihood regression and population-average model with robust standard errors

Appendix A.4. Robbery

HLM 6.8

Model 1 (y1995); d.f.=336 $		Coefficient	SE	t	p-value
Intercept	G00	1.979	0.052	37.960	0.000
Concentrated Disadvantage, 1990	G10	0.080	0.004	20.540	0.000
Immigrant Concentration, 1990	G20	-0.025	0.002	-11.732	0.000
Residential Stability, 1990	G30	-0.032	0.003	-9.945	0.000
Collective Efficacy, 1994-95	G40	-0.011	0.007	-1.575	0.116
Crime Rate, 1990*	G50	0.146	0.005	27.397	0.000

Model 2 (y1995-1999); d.f.=1,702 $					
Intercept					
Intercept	B00	1.938	0.048	40.357	0.000
Concentrated Disadvantage, 1990	B01	0.077	0.004	20.859	0.000
Immigrant Concentration, 1990	B02	-0.024	0.002	-12.254	0.000
Residential Stability, 1990	B03	-0.034	0.002	-13.791	0.000
Collective Efficacy, 1994-95	B04	-0.009	0.007	-1.377	0.169
Crime Rate, 1990*	B05	0.140	0.005	30.812	0.000
Year	B10	-0.006	0.001	-5.061	0.000
Spatial Lag of Crime Rate	B20	0.086	0.011	7.862	0.000

*=Residual of G10,G20,G30

$=HLM v6.08 Poisson restricted likelihood regression and population-average model with robust

GeoDA 0.9.5-i5

Model 3 (y1995); d.f.=335	Coefficient	SE	z		p-value
Spatial Lag of Crime Rate	0.186	0.041	4.508	#	0.000
Intercept	0.028	0.048	0.583	#	0.560
Concentrated Disadvantage, 1990	0.068	0.005	14.856	#	0.000
Immigrant Concentration, 1990	-0.023	0.003	-9.282	#	0.000
Residential Stability, 1990	-0.026	0.003	-9.607	#	0.000
Collective Efficacy, 1994-95	-0.004	0.007	-0.604	#	0.546
Crime Rate, 1990*	0.119	0.006	20.987	#	0.000
	Log Likelihood=	613.781	R-Sqr.=	0.913	
				#	

Model 4 (Aveage of y1995-y1999); d.f.=335	Coefficient	SE	z		p-value
Spatial Lag of Crime Rate	0.448	0.047	9.539	#	0.000
Intercept	0.117	0.103	1.134	#	0.257
Concentrated Disadvantage, 1990	0.059	0.009	6.699	#	0.000
Immigrant Concentration, 1990	-0.015	0.005	-2.851	#	0.004
Residential Stability, 1990	-0.035	0.006	-5.934	#	0.000
Collective Efficacy, 1994-95	-0.018	0.014	-1.299	#	0.194
Crime Rate, 1990*	0.141	0.011	12.755	#	0.000
	Log Likelihood=	345.228	R-Sqr.=	0.841	

Appendix A.5. Aggrevated Assault

HLM 6.8

Model 1 (y1995); d.f.=336 $		Coefficient	SE	t	p-value
Intercept	G00	1.970	0.038	51.175	0.000
Concentrated Disadvantage, 1990	G10	0.112	0.003	38.638	0.000
Immigrant Concentration, 1990	G20	-0.012	0.002	-6.411	0.000
Residential Stability, 1990	G30	-0.013	0.002	-5.747	0.000
Collective Efficacy, 1994-95	G40	-0.009	0.005	-1.689	0.092
Crime Rate, 1990*	G50	0.150	0.005	27.513	0.000

Model 2 (y1995-1999); d.f.=1,702 $					
Intercept					
Intercept	B00	1.961	0.038	51.413	0.000
Concentrated Disadvantage, 1990	B01	0.112	0.003	37.786	0.000
Immigrant Concentration, 1990	B02	-0.011	0.002	-5.960	0.000
Residential Stability, 1990	B03	-0.007	0.002	-3.170	0.002
Collective Efficacy, 1994-95	B04	-0.009	0.005	-1.760	0.078
Crime Rate, 1990*	B05	0.149	0.005	29.296	0.000
Year	B10	-0.002	0.001	-2.337	0.020
Spatial Lag of Crime Rate	B20	0.094	0.013	7.220	0.000

*=Residual of G10,G20,G30

$=HLM v6.08 Poisson restricted likelihood regression and population-average model with

GeoDA 0.9.5-i5

Model 3 (y1995); d.f.=335	Coefficient	SE	z	p-value
Spatial Lag of Crime Rate	0.163	0.033	5.011	0.000
Intercept	0.058	0.043	1.358	0.174
Concentrated Disadvantage, 1990	0.098	0.004	23.400	0.000
Immigrant Concentration, 1990	-0.012	0.002	-5.915	0.000
Residential Stability, 1990	-0.010	0.002	-4.545	0.000
Collective Efficacy, 1994-95	-0.008	0.006	-1.373	0.170
Crime Rate, 1990*	0.116	0.006	19.780	0.000
	Log Likelihood= 673.358		R-Sqr.= 0.941	

Model 4 (Aveage of y1995-y1999); d.f.=335	Coefficient	SE	z	p-value
Spatial Lag of Crime Rate	0.200	0.029	6.829	0.000
Intercept	6.459	3.821	1.690	0.091
Concentrated Disadvantage, 1990	9.409	0.375	25.067	0.000
Immigrant Concentration, 1990	-1.158	0.184	-6.286	0.000
Residential Stability, 1990	-0.537	0.195	-2.756	0.006
Collective Efficacy, 1994-95	-0.899	0.527	-1.705	0.088
Crime Rate, 1990*	11.111	0.527	21.080	0.000
	Log Likelihood= -863.957		R-Sqr.= 0.952	

Appendix A.6. Simple Assault

HLM 6.8

Model 1 (y1995); d.f.=336 $		Coefficient	SE	t	p-value
Intercept	G00	2.101	0.025	84.985	0.000
Concentrated Disadvantage, 1990	G10	0.060	0.002	34.923	0.000
Immigrant Concentration, 1990	G20	-0.016	0.001	-14.790	0.000
Residential Stability, 1990	G30	-0.014	0.001	-10.074	0.000
Collective Efficacy, 1994-95	G40	-0.002	0.003	-0.640	0.522
Crime Rate, 1990*	G50	0.114	0.005	24.076	0.000

Model 2 (y1995-1999); d.f.=1,702 $					
Intercept					
Intercept	B00	2.094	0.027	77.054	0.000
Concentrated Disadvantage, 1990	B01	0.059	0.002	30.210	0.000
Immigrant Concentration, 1990	B02	-0.016	0.001	-13.991	0.000
Residential Stability, 1990	B03	-0.012	0.001	-8.732	0.000
Collective Efficacy, 1994-95	B04	-0.001	0.004	-0.302	0.763
Crime Rate, 1990*	B05	0.117	0.004	27.424	0.000
Year	B10	0.000	0.000	-0.889	0.374
Spatial Lag of Crime Rate	B20	0.084	0.007	11.636	0.000

*=Residual of G10,G20,G30

$=HLM v6.08 Poisson restricted likelihood regression and population-average model with robust

GeoDA 0.9.5-i5

Model 3 (y1995); d.f.=335	Coefficient	SE	z-value	p-value
Spatial Lag of Crime Rate	0.104	0.030	3.466	0.001
Intercept	0.005	0.025	0.193	0.847
Concentrated Disadvantage, 1990	0.056	0.002	26.455	0.000
Immigrant Concentration, 1990	-0.015	0.001	-11.488	0.000
Residential Stability, 1990	-0.014	0.001	-10.310	0.000
Collective Efficacy, 1994-95	-0.001	0.003	-0.202	0.840
Crime Rate, 1990*	0.102	0.004	26.132	0.000
	Log Likelihood=	850.848	R-Sqr.=	0.936

Model 4 (Aveage of y1995-y1999); d.f.=335	Coefficient	SE	z	p-value
Spatial Lag of Crime Rate	0.177	0.034	5.182	0.000
Intercept	-0.010	0.018	-0.562	0.574
Concentrated Disadvantage, 1990	0.031	0.001	20.775	0.000
Immigrant Concentration, 1990	-0.007	0.001	-7.716	0.000
Residential Stability, 1990	-0.007	0.001	-7.253	0.000
Collective Efficacy, 1994-95	0.001	0.002	0.365	0.715
Crime Rate, 1990*	0.071	0.003	24.915	0.000
	Log Likelihood=	966.759	R-Sqr.=	0.914

Appendix A.7. Auto Theft

HLM 6.8

Model 1 (y1995); d.f.=336 $		Coefficient	SE	t	p-value
Intercept	G00	2.011	0.037	54.855	0.000
Concentrated Disadvantage, 1990	G10	0.019	0.003	7.726	0.000
Immigrant Concentration, 1990	G20	-0.013	0.002	-7.134	0.000
Residential Stability, 1990	G30	-0.004	0.002	-1.977	0.048
Collective Efficacy, 1994-95	G40	-0.007	0.005	-1.482	0.139
Crime Rate, 1990*	G50	0.129	0.004	32.582	0.000

Model 2 (y1995-1999); d.f.=1,702 $					
Intercept					
Intercept	B00	2.019	0.033	60.534	0.000
Concentrated Disadvantage, 1990	B01	0.017	0.002	7.263	0.000
Immigrant Concentration, 1990	B02	-0.010	0.002	-6.554	0.000
Residential Stability, 1990	B03	-0.003	0.002	-2.013	0.044
Collective Efficacy, 1994-95	B04	-0.010	0.005	-2.234	0.026
Crime Rate, 1990*	B05	0.127	0.004	33.723	0.000
Year	B10	-0.002	0.001	-3.795	0.000
Spatial Lag of Crime Rate	B20	0.105	0.008	13.389	0.000

*=Residual of G10,G20,G30

$=HLM v6.08 Poisson restricted likelihood regression and population-average model with robust standard errors

GeoDA 0.9.5-i5

Model 3 (y1995); d.f.=335	Coefficient	SE	z	p-value
Spatial Lag of Crime Rate	0.205	0.044	4.642	0.000
Intercept	0.016	0.034	0.465	0.642
Concentrated Disadvantage, 1990	0.015	0.003	5.559	0.000
Immigrant Concentration, 1990	-0.012	0.002	-6.833	0.000
Residential Stability, 1990	-0.004	0.002	-2.333	0.020
Collective Efficacy, 1994-95	-0.002	0.005	-0.503	0.615
Crime Rate, 1990*	0.112	0.005	23.072	0.000
	Log Likelihood= 715.834		R-Sqr.= 0.832	

Model 4 (Aveage of y1995-y1999); d.f.=335	Coefficient	SE	z	p-value
Spatial Lag of Crime Rate	0.282	0.041	6.941	0.000
Intercept	0.165	0.085	1.951	0.051
Concentrated Disadvantage, 1990	0.030	0.007	4.435	0.000
Immigrant Concentration, 1990	-0.021	0.004	-4.742	0.000
Residential Stability, 1990	-0.013	0.005	-2.935	0.003
Collective Efficacy, 1994-95	-0.025	0.012	-2.104	0.035
Crime Rate, 1990*	0.281	0.012	23.208	0.000
	Log Likelihood= 406.386		R-Sqr.= 0.854	

Appendix A.7. Auto Theft (cont.)

HLM 6.8					
Model 5		Coefficient	SE	t	p-value
Intercept	B00	46.551	39.072	1.191	0.234
Concentrated Disadvantage, 1990	B01	0.017	0.016	1.010	0.313
Immigrant Concentration, 1990	B02	-0.059	0.011	-5.199	0.000
Residential Stability, 1990	B03	-0.019	0.012	-1.570	0.116
Collective Efficacy, 1994-95	B04	0.770	5.365	0.143	0.886
Crime Rate, 1990*	B05	0.585	0.026	22.650	0.000
Year Intercept	B10	-0.021	0.020	-1.085	0.278
Collective Efficacy, 1994-95	B11	0.000	0.003	-0.152	0.879
Spatial Lag of Crime Rates Intercept	B20	0.650	0.026	24.722	0.000

Appendix A.8. Burglary

HLM 6.8

Model 1 (y1995); d.f.=336 $		Coefficient	SE	t	p-value
Intercept	G00	1.908	0.040	47.674	0.000
Concentrated Disadvantage, 1990	G10	0.019	0.003	6.357	0.000
Immigrant Concentration, 1990	G20	-0.010	0.002	-5.271	0.000
Residential Stability, 1990	G30	-0.013	0.002	-5.883	0.000
Collective Efficacy, 1994-95	G40	0.010	0.006	1.748	0.081
Crime Rate, 1990*	G50	0.107	0.006	19.049	0.000

Model 2 (y1995-1999); d.f.=1,702 $					
Intercept					
Intercept	B00	1.927	0.040	48.063	0.000
Concentrated Disadvantage, 1990	B01	0.018	0.003	6.168	0.000
Immigrant Concentration, 1990	B02	-0.013	0.002	-6.638	0.000
Residential Stability, 1990	B03	-0.007	0.002	-3.204	0.002
Collective Efficacy, 1994-95	B04	0.005	0.006	0.952	0.342
Crime Rate, 1990*	B05	0.105	0.006	18.318	0.000
Year	B10	-0.003	0.001	-18.240	0.000
Spatial Lag of Crime Rate	B20	0.116	0.007	17.500	0.000

*=Residual of G10,G20,G30

$=HLM v6.08 Poisson restricted likelihood regression and population-average model with robust

GeoDA 0.9.5-i5

Model 3 (y1995); d.f.=335	Coefficient	SE	z	p-value
Spatial Lag of Crime Rate	0.313	0.057	5.536	0.000
Intercept	-8.476	3.651	-2.320	0.020
Concentrated Disadvantage, 1990	1.821	0.279	5.487	0.000
Immigrant Concentration, 1990	-0.832	0.190	-4.386	0.000
Residential Stability, 1990	-1.004	0.202	-4.973	0.000
Collective Efficacy, 1994-95	1.159	0.504	2.300	0.021
Crime Rate, 1990*	8.965	0.551	16.260	0.000
	Log Likelihood=	-875.831	R-Sqr.=	0.698

Model 4 (Aveage of y1995-y1999); d.f.=335	Coefficient fici	SE	z	p-value
Spatial Lag of Crime Rate	0.410	0.051	7.917	0.000
Intercept	-0.337	0.203	-1.661	0.097
Concentrated Disadvantage, 1990	0.080	0.016	5.125	0.000
Immigrant Concentration, 1990	-0.048	0.011	-4.536	0.000
Residential Stability, 1990	-0.026	0.011	-2.467	0.014
Collective Efficacy, 1994-95	0.044	0.028	1.570	0.114
Crime Rate, 1990*	0.480	0.031	15.610	0.000
	Log Likelihood=	108.379	R-Sqr.=	0.666

Appendix A.8. Burglary (cont.)

HLM 6.8

Model 5		Coefficient	#	SE	t	p-value
Intercept	B00	117.194		43.677	2.686	0.008
Concentrated Disadvantage, 1990	B01	0.0406		0.016	2.589	0.01
Immigrant Concentration, 1990	B02	-0.044		0.011	-4.089	0.000
Residential Stability, 1990	B03	-0.021		0.011	-1.821	0.068
Collective Efficacy, 1994-95	B04	-10.650		5.980	-1.781	0.074
Crime Rate, 1990*	B05	0.470		0.028	16.707	0.000
Year Intercept	B10	-0.057		0.022	-2.622	0.009
Collective Efficacy, 1994-95	B11	0.005		0.003	1.789	0.073
Spatial Lag of Crime Rates Intercept	B20	0.819		0.025	32.365	0.000

Appendix A.9. Residential Burglary Rates

HLM 6.8

Model 1 (y1995); d.f.=336 $		Coefficient	SE	t	p-value
Intercept	G00	2.119	0.068	31.185	0.000
Concentrated Disadvantage, 1990	G10	0.002	0.005	0.379	0.705
Immigrant Concentration, 1990	G20	-0.009	0.004	-2.420	0.016
Residential Stability, 1990	G30	-0.009	0.004	-0.112	0.911
Collective Efficacy, 1994-95	G40	-0.025	0.009	-2.616	0.010
Crime Rate, 1990*	G50	0.004	0.008	-0.526	0.599

Model 2 (y1995-1999); d.f.=1,702 $					
Intercept					
Intercept	B00	2.136	0.072	29.706	0.000
Concentrated Disadvantage, 1990	B01	0.003	0.005	0.053	0.596
Immigrant Concentration, 1990	B02	-0.012	0.004	-2.999	0.003
Residential Stability, 1990	B03	0.006	0.010	1.500	0.134
Collective Efficacy, 1994-95	B04	-0.029	0.008	-2.871	0.005
Crime Rate, 1990*	B05	-0.003	0.008	-0.397	0.691
Year	B10	-0.002	0.001	-4.227	0.000
Spatial Lag of Crime Rate	B20	0.122	0.007	17.643	0.000

*=Residual of G10,G20,G30

$=HLM v6.08 Poisson restricted likelihood regression and population-average model with robust standard errors

GeoDA 0.9.5-i5

Model 3 (y1995); d.f.=335	Coefficient	SE	z	p-value
Spatial Lag of Crime Rate	0.342	0.059	5.827	0.000
Intercept	-0.081	0.042	-1.921	0.055
Concentrated Disadvantage, 1990	0.014	0.003	4.573	0.000
Immigrant Concentration, 1990	-0.004	0.002	-2.050	0.040
Residential Stability, 1990	-0.005	0.002	-2.409	0.016
Collective Efficacy, 1994-95	0.011	0.006	1.916	0.055
Crime Rate, 1990*	0.086	0.006	14.101	0.000
	Log Likelihood=	653.764	R-Sqr.=	0.643

Model 4 (Aveage of y1995-y1999); d.f.=335	Coefficient	SE	z	p-value
Spatial Lag of Crime Rate	0.391	0.054	7.253	0.000
Intercept	-0.592	0.278	-2.132	0.033
Concentrated Disadvantage, 1990	0.118	0.021	5.746	0.000
Immigrant Concentration, 1990	-0.025	0.014	-1.761	0.078
Residential Stability, 1990	-0.002	0.015	-0.129	0.898
Collective Efficacy, 1994-95	0.079	0.038	2.062	0.039
Crime Rate, 1990*	0.612	0.041	14.828	0.000
	Log Likelihood=	5.567	R-Sqr.=	0.702

Appendix A.9. Residential Burglary Rates (cont.)

HLM 6.8					
Model 5		Coefficient	SE	t	p-value
Intercept	B00	132.330	51.829	2.550	0.011
Concentrated Disadvantage, 1990	B01	0.052	0.017	3.087	0.002
Immigrant Concentration, 1990	B02	-0.024	0.012	-2.112	0.034
Residential Stability, 1990	B03	-0.009	0.012	0.073	0.466
Collective Efficacy, 1994-95	B04	-12.782	7.124	-1.794	0.075
Crime Rate, 1990*	B05	0.434	0.030	14.728	0.000
Year Intercept	B10	-0.065	0.026	-2.507	0.012
Collective Efficacy, 1994-95	B11	0.006	0.004	1.802	0.071
Spatial Lag of Crime Rates Intercept	B20	0.855	0.026	32.344	0.000

*=Residual of G10,G20,G30

Appendix A.10. Residential Property Crime

HLM 6.8

Model 1 (y1995); d.f.=336 $		Coefficient	SE	t	p-value
Intercept	G00	2.050	0.026	79.432	0.000
Concentrated Disadvantage, 1990	G10	0.033	0.002	17.599	0.000
Immigrant Concentration, 1990	G20	-0.018	0.001	-14.656	0.000
Residential Stability, 1990	G30	-0.007	0.001	-4.709	0.000
Collective Efficacy, 1994-95	G40	0.002	0.004	0.654	0.513
Crime Rate, 1990*	G50	0.104	0.005	21.820	0.000

Model 2 (y1995-1999); d.f.=1,702 $					
Intercept					
Intercept	B00	2.075	0.027	77.431	0.000
Concentrated Disadvantage, 1990	B01	0.031	0.002	15.676	0.000
Immigrant Concentration, 1990	B02	-0.020	0.001	-17.351	0.000
Residential Stability, 1990	B03	-0.004	0.001	-2.470	0.014
Collective Efficacy, 1994-95	B04	-0.002	0.004	-0.479	0.632
Crime Rate, 1990*	B05	0.109	0.005	23.155	0.000
Year	B10	-0.002	0.000	-4.025	0.000
Spatial Lag of Crime Rate	B20	0.092	0.006	14.677	0.000

*=Residual of G10,G20,G30

$=HLM v6.08 Poisson restricted likelihood regression and population-average model with robust

GeoDA 0.9.5-i5

Model 3 (y1995); d.f.=335	Coefficient	SE	z	p-value
Spatial Lag of Crime Rate	0.214	0.032	6.779	0.940
Intercept	0.002	0.022	0.076	0.000
Concentrated Disadvantage, 1990	0.039	0.002	21.547	0.000
Immigrant Concentration, 1990	-0.009	0.001	-8.615	0.000
Residential Stability, 1990	-0.002	0.001	-1.355	0.176
Collective Efficacy, 1994-95	-0.001	0.003	-0.251	0.802
Crime Rate, 1990*	0.089	0.004	22.790	0.000
	Log Likelihoo	901.901	R-Sqr.=	0.921

Model 4 (Aveage of y1995-y1999); d.f.=335	Coefficient	SE	z	p-value
Spatial Lag of Crime Rate	0.314	0.041	7.572	0.000
Intercept	0.000	0.054	-0.003	0.997
Concentrated Disadvantage, 1990	0.051	0.004	12.097	0.000
Immigrant Concentration, 1990	-0.031	0.003	-10.000	0.000
Residential Stability, 1990	-0.006	0.003	-2.128	0.033
Collective Efficacy, 1994-95	-0.001	0.007	-0.086	0.932
Crime Rate, 1990*	0.194	0.011	17.974	0.000
	Log Likelihoo	571.232	R-Sqr.=	0.854

Appendix A.11. Theft

HLM 6.8

Model 1 (y1995); d.f.=336 $		Coefficient	SE	t	p-value
Intercept	G00	2.096	0.026	81.073	0.000
Concentrated Disadvantage, 1990	G10	0.007	0.002	3.610	0.001
Immigrant Concentration, 1990	G20	-0.273	0.001	-19.223	0.000
Residential Stability, 1990	G30	-0.028	0.001	-22.161	0.000
Collective Efficacy, 1994-95	G40	-0.001	0.004	-0.177	0.860
Crime Rate, 1990*	G50	0.114	0.002	52.917	0.000

Model 2 (y1995-1999); d.f.=1,702 $					
Intercept					
Intercept	B00	2.086	0.024	88.276	0.000
Concentrated Disadvantage, 1990	B01	0.006	0.002	3.083	0.003
Immigrant Concentration, 1990	B02	-0.027	0.001	-20.948	0.000
Residential Stability, 1990	B03	-0.029	0.001	-23.920	0.000
Collective Efficacy, 1994-95	B04	0.000	0.003	0.082	0.935
Crime Rate, 1990*	B05	0.114	0.002	54.708	0.000
Year	B10	-0.001	0.000	-2.796	0.006
Spatial Lag of Crime Rate	B20	0.070	0.007	10.347	0.000

*=Residual of G10,G20,G30

$=HLM v6.08 Poisson restricted likelihood regression and population-average model with robust standard errors

GeoDA 0.9.5-i5

Model 3 (y1995); d.f.=335	Coefficient	SE	z	p-value
Spatial Lag of Crime Rate	0.096	0.033	2.900	0.004
Intercept	0.005	0.026	0.203	0.839
Concentrated Disadvantage, 1990	0.004	0.002	2.088	0.037
Immigrant Concentration, 1990	-0.027	0.001	-18.855	0.000
Residential Stability, 1990	-0.027	0.002	-16.126	0.000
Collective Efficacy, 1994-95	-0.001	0.004	-0.219	0.826
Crime Rate, 1990*	0.116	0.003	40.778	0.000
	Log Likelihood= 818.373		R-Sqr.= 0.903	

Model 4 (Aveage of y1995-y1999); d.f.=335	Coefficient	SE	z	p-value
Spatial Lag of Crime Rate	0.146	0.036	4.012	0.000
Intercept	0.021	0.027	0.777	0.437
Concentrated Disadvantage, 1990	0.004	0.002	1.875	0.061
Immigrant Concentration, 1990	-0.026	0.002	-16.902	0.000
Residential Stability, 1990	-0.025	0.002	-14.050	0.000
Collective Efficacy, 1994-95	-0.004	0.004	-0.937	0.349
Crime Rate, 1990*	0.105	0.003	34.509	0.000
	Log Likelihood= 793.275		R-Sqr.= 0.880	

Appendix A.12. Residential Vandalism

HLM 6.8

Model 1 (y1995); d.f.=336 $		Coefficient	SE	t	p-value
Intercept	G00	1.967	0.040	49.542	0.000
Concentrated Disadvantage, 1990	G10	0.058	0.003	19.844	0.000
Immigrant Concentration, 1990	G20	-0.016	0.002	-7.980	0.000
Residential Stability, 1990	G30	0.012	0.002	5.136	0.000
Collective Efficacy, 1994-95	G40	-0.007	0.005	-1.363	0.174
Crime Rate, 1990*	G50	0.127	0.006	22.021	0.000

Model 2 (y1995-1999); d.f.=1,702 $					
Intercept					
Intercept	B00	2.022	0.037	54.217	0.000
Concentrated Disadvantage, 1990	B01	0.053	0.003	18.213	0.000
Immigrant Concentration, 1990	B02	-0.017	0.002	-10.843	0.000
Residential Stability, 1990	B03	0.012	0.002	6.132	0.000
Collective Efficacy, 1994-95	B04	-0.016	0.005	-3.050	0.003
Crime Rate, 1990*	B05	0.129	0.005	25.151	0.000
Year	B10	-0.002	0.001	-4.297	0.000
Spatial Lag of Crime Rate	B20	0.059	0.010	6.122	0.000

*=Residual of G10,G20,G30

$=HLM v6.08 Poisson restricted likelihood regression and population-average model with robust standard errors

GeoDA 0.9.5-i5

Model 3 (y1995); d.f.=335	Coefficient	SE	z	p-value
Spatial Lag of Crime Rate	0.096	0.042	2.272	0.023
Intercept	0.028	0.039	0.720	0.471
Concentrated Disadvantage, 1990	0.057	0.003	18.071	0.000
Immigrant Concentration, 1990	-0.015	0.002	-7.012	0.000
Residential Stability, 1990	0.010	0.002	4.394	0.000
Collective Efficacy, 1994-95	-0.004	0.005	-0.721	0.471
Crime Rate, 1990*	0.117	0.006	18.789	0.000
	Log Likelihood=	675.674	R-Sqr.=	0.849

Model 4 (Aveage of y1995-y1999); d.f.=335	Coefficient #	SE	z	p-value
Spatial Lag of Crime Rate	0.339	0.036	9.341	0.000
Intercept	0.148	0.075	1.970	0.049
Concentrated Disadvantage, 1990	0.090	0.006	15.006	0.000
Immigrant Concentration, 1990	-0.021	0.004	-5.257	0.000
Residential Stability, 1990	0.018	0.004	4.242	0.000
Collective Efficacy, 1994-95	-0.022	0.010	-2.101	0.036
Crime Rate, 1990*	0.235	0.012	19.135	0.000
	Log Likelihood=	450.197	R-Sqr.=	0.878

Appendix A.12. Residential Vandalism

HLM 6.8

Model 5		Coefficient	0	SE	t	p-value
Intercept	B00	120.578		52.840	2.282	0.022
Concentrated Disadvantage, 1990	B01	0.319		0.016	20.183	0.000
Immigrant Concentration, 1990	B02	-0.097		0.011	-9.012	0.000
Residential Stability, 1990	B03	0.060		0.011	5.454	0.000
Collective Efficacy, 1994-95	B04	-11.375		7.273	-1.564	0.188
Crime Rate, 1990*	B05	0.731		0.031	23.473	0.000
Year Intercept	B10	-0.057		0.026	-2.169	0.030
Collective Efficacy, 1994-95	B11	0.006		0.004	1.554	0.120
Spatial Lag of Crime Rates Intercept	B20	0.279		0.027	10.160	0.000

Appendix A.13. Vandalism

HLM 6.8

Model 1 (y1995); d.f.=336 $		Coefficient	SE	t	p-value
Intercept	G00	2.064	0.027	77.58	0.000
Concentrated Disadvantage, 1990	G10	0.030	0.002	14.08	0.000
Immigrant Concentration, 1990	G20	-0.011	0.001	-8.71	0.000
Residential Stability, 1990	G30	-0.007	0.001	-5.62	0.000
Collective Efficacy, 1994-95	G40	-0.002	0.004	-0.49	0.624
Crime Rate, 1990*	G50	0.109	0.004	25.25	0.000

Model 2 (y1995-1999); d.f.=1,702 $					
Intercept					
Intercept	B00	2.101	0.024	88.01	0.000
Concentrated Disadvantage, 1990	B01	0.027	0.002	13.66	0.000
Immigrant Concentration, 1990	B02	-0.012	0.001	-10.80	0.000
Residential Stability, 1990	B03	-0.007	0.001	-5.73	0.000
Collective Efficacy, 1994-95	B04	-0.008	0.003	-2.29	0.022
Crime Rate, 1990*	B05	0.118	0.004	29.82	0.000
Year	B10	-0.002	0.000	-4.49	0.000
Spatial Lag of Crime Rate	B20	0.055	0.008	7.11	0.000

*=Residual of G10,G20,G30

$=HLM v6.08 Poisson restricted likelihood regression and population-average model with robust standard errors

GeoDA 0.9.5-i5

Model 3 (y1995); d.f.=335	Coefficient	SE	z	p-value
Spatial Lag of Crime Rate	0.130	0.043	3.00	0.003
Intercept	0.326	2.360	0.14	0.890
Concentrated Disadvantage, 1990	2.882	0.197	14.63	0.000
Immigrant Concentration, 1990	-1.021	0.128	-7.99	0.000
Residential Stability, 1990	-0.687	0.131	-5.23	0.000
Collective Efficacy, 1994-95	-0.049	0.326	-0.15	0.880
Crime Rate, 1990*	10.281	0.457	22.50	0.000
	Log Likelihood=	-730.971	R-Sqr.=	0.840

Model 4 (Aveage of y1995-y1999); d.f.=335	Coefficient	SE	z	p-value
Spatial Lag of Crime Rate	0.279	0.037	7.54	0.000
Intercept	0.110	0.0432	2.54	0.011
Concentrated Disadvantage, 1990	0.043	0.004	12.05	0.000
Immigrant Concentration, 1990	-0.019	0.002	-8.10	0.000
Residential Stability, 1990	-0.010	0.002	-4.17	0.000
Collective Efficacy, 1994-95	-0.016	0.006	-2.67	0.007
Crime Rate, 1990*	0.214	0.009	24.89	0.000
	Log Likelihood=	633.028	R-Sqr.=	0.873

Appendix A.13. Vandalism (cont.)

HLM 6.8					
Model 5		Coefficient	SE	t	p-value
Intercept	B00	78.286	39.111	2.00	0.045
Concentrated Disadvantage, 1990	B01	0.016	0.001	13.50	0.000
Immigrant Concentration, 1990	B02	-0.069	0.008	-8.73	0.000
Residential Stability, 1990	B03	0.036	0.008	-4.36	0.000
Collective Efficacy, 1994-95	B04	-8.583	5.385	-1.59	0.111
Crime Rate, 1990*	B05	0.750	0.027	26.79	0.000
Year Intercept	B10	-0.036	0.020	-1.85	0.064
Collective Efficacy, 1994-95	B11	0.004	0.003	1.59	0.112
Spatial Lag of Crime Rates Intercept	B20	0.390	0.025	15.35	0.000

Appendix A.15. Perceived Violence

HLM		b	s.e.	t	
Neighborhood-level predictors					
Intercept		5.184	0.310	16.73	***
Concentrated Disadvantage, 1990		0.121	0.023	5.36	***
Immigrant Concentration, 1990		0.010	0.016	0.63	
Residential Stability, 1990		-0.025	0.014	-1.86	
Collective Efficacy, 1994-95		-0.413	0.043	-9.58	***
Crime Rate, 1990*		0.087	0.034	2.60	*
Spatial Lag of Crime Rate		0.451	0.304	1.48	
Variance Component		std. dev.	var. comp.	Chi-sqr.	
Intercept	U0	0.1339	0.0179		
Level-1	R	0.6987	0.4881		
GeoDa					
Intercept		4.431	0.315	14.08	***
Concentrated Disadvantage, 1990		0.115	0.021	5.49	***
Immigrant Concentration, 1990		0.034	0.013	2.64	**
Residential Stability, 1990		0.000	0.014	-0.04	
Collective Efficacy, 1994-95		-0.333	0.037	-9.11	***
Crime Rate, 1990*		0.028	0.036	0.44	
Spatial Lag of Crime Rate		0.145	0.061	2.38	**
		R2= 69%	Log Likelihood=	45.25	

* = p-value < 0.05; ** = p-value < 0.01, *** = p-value < 0.001

Appendix A.16.
In past 6 mos how often a fight with a weapon

HLM		b	s.e.	t	
Neighborhood-level predictors					
Intercept		5.974	0.395	15.13	
Concentrated Disadvantage, 1990		0.248	0.029	8.56	***
Immigrant Concentration, 1990		0.011	0.022	0.51	
Residential Stability, 1990		0.004	0.018	0.22	
Collective Efficacy, 1994-95		-0.493	0.056	-8.84	***
Crime Rate, 1990*		0.107	0.050	2.15	
Spatial Lag of Crime Rate		0.245	0.067	3.66	**
Variance Component		std. dev.	var. comp.	Chi-sqr.	
Intercept	U0	0.178	0.032		
Level-1	R	0.994	0.883		
GeoDA					
Intercept		4.431	0.315	14.08	***
Concentrated Disadvantage, 1990		0.115	0.021	5.49	***
Immigrant Concentration, 1990		0.034	0.013	2.64	**
Residential Stability, 1990		0.000	0.014	-0.04	
Collective Efficacy, 1994-95		-0.333	0.037	-9.11	***
Crime Rate, 1990*		0.028	0.036	0.77	
Spatial Lag of Crime Rate		0.145	0.061	2.38	**
	R2= 0.69		Log Likelihood=	45.25	

* = p-value < 0.05; ** = p-value < 0.01, *** = p-value < 0.001

Appendix A.17
violent argument between neighbors

HLM		b	s.e.	t	
Neighborhood-level predictors					
Intercept		4.594	0.409	11.23	***
Concentrated Disadvantage, 1990		0.129	0.029	4.47	***
Immigrant Concentration, 1990		-0.003	0.021	-0.14	
Residential Stability, 1990		-0.040	0.018	-2.27	*
Collective Efficacy, 1994-95		-0.326	0.057	-5.74	***
Crime Rate, 1990*		0.059	0.042	1.39	
Spatial Lag of Crime Rate		0.021	0.076	0.27	
Variance Component		std. dev.	var. comp.	Chi-sqr.	
Intercept	U0	0.17	0.03		
Level-1	R	0.88	0.77		
GeoDa					
Intercept		3.784	0.394	9.61	***
Concentrated Disadvantage, 1990		0.044	0.019	2.39	**
Immigrant Concentration, 1990		0.019	0.012	1.56	
Residential Stability, 1990		-0.031	0.011	-2.69	**
Collective Efficacy, 1994-95		-0.198	0.030	-6.61	***
Crime Rate, 1990*		0.003	0.028	0.10	
Spatial Lag of Crime Rate		-0.087	0.079	-1.10	
R2=	43%		Log Likelihood=		116

* = p-value < 0.05; ** = p-value < 0.01, *** = p-value < 0.001

Appendix A.18.
gang fight

HLM		b	s.e.	t	
Neighborhood-level predictors					
Intercept		6.618	0.408	16.22	***
Concentrated Disadvantage, 1990		0.204	0.031	6.60	***
Immigrant Concentration, 1990		0.114	0.023	4.93	***
Residential Stability, 1990		0.057	0.019	3.05	**
Collective Efficacy, 1994-95		-0.580	0.056	-10.42	***
Crime Rate, 1990*		0.094	0.048	1.95	*
Spatial Lag of Crime Rate		0.214	0.061	3.50	**
Variance Component		std. dev.	var. comp.	Chi-sqr.	
Intercept	U0	0.185	0.0342		
Level-1	R	0.9548	0.9116		
GeoDa					
Intercept		4.981	0.336	14.84	***
Concentrated Disadvantage, 1990		0.090	0.022	4.13	***
Immigrant Concentration, 1990		0.094	0.014	6.62	**
Residential Stability, 1990		0.038	0.015	2.60	***
Collective Efficacy, 1994-95		-0.406	0.039	-10.38	***
Crime Rate, 1990*		0.034	0.038	0.88	
Spatial Lag of Crime Rate		0.144	0.061	2.36	*

R2= 69% Log Likelihood= 23.58

* = p-value < 0.05; ** = p-value < 0.01, *** = p-value < 0.001

Appendix A.19.
sexual assault or rape

HLM		b	s.e.	t	
Neighborhood-level predictors					
Intercept		3.573	0.325	11.01	***
Concentrated Disadvantage, 1990		0.001	0.025	0.02	
Immigrant Concentration, 1990		-0.074	0.018	-4.06	***
Residential Stability, 1990		-0.039	0.016	-2.47	**
Collective Efficacy, 1994-95		-0.270	0.044	-6.13	***
Crime Rate, 1990*		0.056	0.029	1.96	*
Spatial Lag of Crime Rate		0.584	0.086	6.78	***
Variance Component		std. dev.	var. comp.	Chi-sqr.	
Intercept	U0	0.1627	0.0265		
Level-1	R	0.6997	0.4896		
GeoDa					
Intercept		2.198	0.235	2.63	***
Concentrated Disadvantage, 1990		-0.016	0.013	9.35	
Immigrant Concentration, 1990		-0.021	0.009	-1.24	*
Residential Stability, 1990		-0.020	0.010	-2.29	*
Collective Efficacy, 1994-95		-0.129	0.025	-2.00	***
Crime Rate, 1990*		0.026	0.018	-5.11	
Spatial Lag of Crime Rate		0.203	0.077	1.44	**
	R2=	28%	Log Likelihood=	157.9	

* = p-value < 0.05; ** = p-value < 0.01, *** = p-value < 0.001

Appendix A.20.
robbery or mugging

HLM		b	s.e.	t	
Neighborhood-level predictors					
Intercept		5.613	0.433	12.98	***
Concentrated Disadvantage, 1990		-0.005	0.034	-0.14	
Immigrant Concentration, 1990		-0.043	0.022	-1.92	*
Residential Stability, 1990		-0.079	0.021	-3.76	***
Collective Efficacy, 1994-95		-0.456	0.059	-7.72	***
Crime Rate, 1990*		0.059	0.031	1.89	*
Spatial Lag of Crime Rate		0.227	0.094	2.41	*
Variance Component		std. dev.	var. comp.	Chi-sqr.	
Intercept	U0	0.2034	0.0414		
Level-1	R	0.9676	0.9362		
GeoDa					
Intercept		4.265	0.331	12.90	***
Concentrated Disadvantage, 1990		-0.028	0.018	-1.54	
Immigrant Concentration, 1990		-0.013	0.012	-1.09	
Residential Stability, 1990		-0.055	0.014	-3.93	***
Collective Efficacy, 1994-95		-0.264	0.034	-7.68	***
Crime Rate, 1990*		0.045	0.020	2.28	*
Spatial Lag of Crime Rate		-0.030	0.081	-0.37	
	R2=	39%	Log Likelihood=	60.95	

* = p-value < 0.05; ** = p-value < 0.01, *** = p-value < 0.001

Appendix A.21.
has anyone ever used violence against you or any household member

HLM	b	s.e.	t	
Neighborhood-level predictors				
Intercept	3.222	1.143	2.82	***
Concentrated Disadvantage, 1990	0.061	0.086	0.72	
Immigrant Concentration, 1990	0.059	0.047	1.27	
Residential Stability, 1990	-0.024	0.05	-0.481	
Collective Efficacy, 1994-95	-0.672	0.124	-5.42	***
Crime Rate, 1990*	0.301	0.136	2.21	*
Spatial Lag of Crime Rate	-0.038	0.129	-0.30	
GeoDa				
Intercept	-0.756	0.250	-3.03	**
Concentrated Disadvantage, 1990	-0.016	0.015	-1.08	
Immigrant Concentration, 1990	0.019	0.010	1.89	*
Residential Stability, 1990	-0.004	0.011	-0.35	
Collective Efficacy, 1994-95	-0.150	0.029	-5.14	***
Crime Rate, 1990*	0.022	0.027	0.84	
Spatial Lag of Crime Rate	-0.016	0.086	-0.18	
R2= 19%	Log Likelihood= 125.6			

* = p-value < 0.05; ** = p-value < 0.01, *** = p-value < 0.001

Appendix A.22.

has your home ever been broken into

HLM	b	s.e.	t	
Neighborhood-level predictors				
Intercept	0.150	0.973	0.15	
Concentrated Disadvantage, 1990	0.104	0.074	1.40	
Immigrant Concentration, 1990	0.059	1.159	1.16	
Residential Stability, 1990	-0.050	0.043	-1.17	
Collective Efficacy, 1994-95	-0.338	-2.550	-2.55	**
Crime Rate, 1990*	0.421	4.025	4.03	***
Spatial Lag of Crime Rate	0.360	1.808	1.81	
GeoDa				
Intercept	-2.053	0.280	-1.69	***
Concentrated Disadvantage, 1990	0.009	0.015	-7.32	
Immigrant Concentration, 1990	0.019	0.010	0.56	
Residential Stability, 1990	-0.020	0.011	1.78	
Collective Efficacy, 1994-95	-0.075	0.029	-1.79	**
Crime Rate, 1990*	0.110	0.025	-2.60	***
Spatial Lag of Crime Rate	-0.153	0.091	4.31	

R2= 14% Log Likelihood= 115.3

* = p-value < 0.05; ** = p-value < 0.01, *** = p-value < 0.001

Appendix A.23.
had anything stolen from your property

HLM	b	s.e.	t	
Neighborhood-level predictors				
Intercept	1.093	0.775	1.41	
Concentrated Disadvantage, 1990	0.091	0.061	1.50	
Immigrant Concentration, 1990	0.136	0.047	2.91	**
Residential Stability, 1990	0.088	0.042	2.10	*
Collective Efficacy, 1994-95	-0.244	0.105	-2.33	*
Crime Rate, 1990*	0.024	0.070	0.34	
Spatial Lag of Crime Rate	-0.179	0.094	-1.90	
GeoDa				
Intercept	-0.076	0.256	-0.30	
Concentrated Disadvantage, 1990	0.017	0.019	0.92	
Immigrant Concentration, 1990	0.044	0.013	3.30	***
Residential Stability, 1990	0.025	0.014	1.80	
Collective Efficacy, 1994-95	-0.078	0.035	-2.24	*
Crime Rate, 1990*	0.014	0.025	0.54	
Spatial Lag of Crime Rate	-0.014	0.089	-0.16	
R2= 8%	Log Likelihood= 34.71			

* = p-value < 0.05; ** = p-value < 0.01, *** = p-value < 0.001

Appendix A.24.
have you had any property damaged

HLM	b	s.e.	t	
Neighborhood-level predictors				
Intercept	1.199	0.754	1.59	
Concentrated Disadvantage, 1990	-0.017	0.0544	-0.31	
Immigrant Concentration, 1990	0.123	0.048	2.57	**
Residential Stability, 1990	-0.017	0.039	-0.43	
Collective Efficacy, 1994-95	-0.249	0.103	-2.41	*
Crime Rate, 1990*	0.273	0.097	2.82	***
Spatial Lag of Crime Rate	0.040	0.127	0.31	
GeoDa				
Intercept	0.027	0.239	0.11	
Concentrated Disadvantage, 1990	-0.009	0.017	-0.51	
Immigrant Concentration, 1990	0.034	0.012	2.73	**
Residential Stability, 1990	-0.006	0.013	-0.46	
Collective Efficacy, 1994-95	-0.086	0.033	-2.62	**
Crime Rate, 1990*	0.095	0.034	2.77	**
Spatial Lag of Crime Rate	-0.125	0.091	-1.37	

R2= 9% Log Likelihood= 62.57

* = p-value < 0.05; ** = p-value < 0.01, *** = p-value < 0.001