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Spatial network effects on neighborhood violence and overall crime: A computational statistics analysis of employment-based econetworks*

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KEYWORDS: econetworks; extra-local effects; disadvantage; commuting; public control; neighborhood crime

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BIOGRAPHICAL NOTES

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Abstract

Research on the neighborhood determinants of violence and overall crime rates has typically focused on internal or geographically proximate processes. However, a growing body of research shows that people often engage in interactions away from home areas, contributing to dynamic connections between places. A few important studies highlight the implications of these connections, focusing on rare interactions like co-offending or gang conflicts. The current study expands this idea by analyzing more common interactions based on population mobility patterns measured through commuting flows across Chicago communities. It integrates standard demographic and spatial methods with machine learning and computational statistics approaches to investigate the extent to which neighborhood violence and overall crime depends not just on internal or surrounding disadvantage but also on the disadvantage of areas connected to it through commuting. The findings contribute to ecological theories of crime, social isolation, and ecological networks by showing that communities can influence each other from a distance and suggesting that connectivity to less disadvantaged employment hubs may decrease local crime.

KEYWORDS: econetworks; extra-local effects; disadvantage; commuting; public control; neighborhood crime

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INTRODUCTION

A century old body of research on the role of socioeconomic disadvantage in increasing neighborhood crime has predominantly focused on internal or, more recently, geographically proximate neighborhood structures and processes (Peterson & Krivo, 2010; Sampson, 2012; Shaw & McKay, 1942). However, evidence is increasingly suggesting that disadvantage outside of home or nearby areas is also associated with local outcomes such as crime (Graif & Matthews, 2017; Mears & Bhati, 2006). This is not surprising, given that many people spend a great deal of their day in activities outside their home area (Krivo, Washington, Peterson et al., 2013) and this affects their outcomes and behaviors (Browning, Soller, & Jackson, 2015; Hoeben & Weefrman, 2016; Mahoney, Stattin, & Magnusson, 2001). The focus of research on activity spaces has mostly been on individuals, yet neighborhoods may also be affected by residents' activity locations (Browning, Calder, Boettner, et al., 2017; Browning, Calder, Soller, et al., 2017; Gould, 1991; Matthews & Yang, 2013; Papachristos, Hureau, and Braga, 2013; Radil, Flint, & Tita, 2010; Sampson, 2012; Schaefer, 2012; Wikström, Ceccato, Hardie, et al., 2010). Indeed, research on public social control has shown that access to external resources, such as inflows of mortgage loans, decreased neighborhood level crime (Vélez, Lyons, & Boursaw, 2012).

In this study, we integrate recent findings and thinking from the activity space research and the public control studies with current and classic thinking on neighborhood mobility and crime to address an important next question: Does population exposure to places of higher disadvantage during daily activities contribute to increasing local crime above and beyond the role of local disadvantage? This question has not yet been explored, to our knowledge. Although the gap is largely due to great challenges in collecting activity data, addressing this question is important. First, studies have found that neighborhoods are affected by connections to other areas based on: co-offending ties (Schaefer, 2012), gang conflicts (Papachristos et al., 2013), residents moving to new neighborhoods (Sampson & Sharkey, 2008), and commuting and transportation (Boivin & Felson, 2017; Boivin & D'Elia, 2017; Felson & Boivin, 2015; Graif, Lungeanu, & Yetter, 2017; Matthews et al., 2010; Wang et al., 2016). We thus have many indications that neighborhoods are not isolated islands or closed systems, as they have been predominantly assumed to be (Browning & Soller, 2014; Graif, Matthews, & Gladfelter, 2014). If we nonetheless treat the observed interconnectivity as non-existent or inconsequential and omit it from our models, estimates of neighborhood effects on crime may be biased. If ties to extra-local neighborhoods matter, focusing just on the internal (or nearby) structures that link disadvantage to crime will miss the full range of forces that affect local crime.

Advancements in the growing literature on spatial spillovers (Anselin et al., 2000), have shown that geographic proximity to neighborhoods of high disadvantage increases local residents' victimization experiences (Graif & Matthews, 2017), involvement in crime (Graif, 2015; Vogel & South, 2016) and increases the overall crime in a neighborhood (Peterson & Krivo, 2009, 2010). This phenomenon, also called *extra-local effects*, has been observed across a broad spectrum of outcomes (e.g., Crowder and South, 2011). Independent of outcome, the assumption underlying typical spatial analyses is that spillovers are due to interpersonal interactions across neighborhood borders (Anselin, 2002; Anselin et al., 2000). Yet, spatial interactions are rarely modeled explicitly. Doing so can greatly advance our understanding of population mobility and spatial interdependencies. The current study gets a step closer to measuring such interactions through commuting flows of residents from home to work.

Second, when focusing on disadvantage in a particular neighborhood, programs and policies may be less successful in decreasing crime if we do not pay attention to spillovers from connected areas. If neighborhood crime depends on disadvantage levels in areas connected through social ties, reducing disadvantage in one neighborhood may be insufficient in reducing crime without also reducing disadvantage in socially connected areas. However, if we do pay attention to which neighborhoods are more connected and have stronger ties to other disadvantaged areas, interventions may lead to controlling crime across a wider range of areas.

To understand the complex role of extra-local disadvantage in shaping crime, we focus on neighborhoods in Chicago, a large and diverse city. Despite the national trend of declining crime rates, some of Chicago's communities recently experienced some increases in crime. Importantly, much of the violence is concentrated in a few neighborhoods in the South and West side of Chicago, which have been historically impoverished and segregated (Gorner, 2016a; Sanburn & Johnson, 2017; Wills & Hernandez, 2017). For instance, neighborhoods in the Harrison District on the West side, such as West Garfield Park and North Lawndale, experienced an increase in homicides in 2016, with 92 homicides compared to 48 in 2015 (Gorner, 2016b).

In sum, the current study explores the connection between crime and commuting patterns among Chicago communities and seeks to contribute to the communities and crime literature by extending the existing research in several ways. First, it addresses an important substantive need in the social disorganization literature to go beyond the *internal* or *geographically* proximate forces to examine the significance for crime of the *ecological network* forces forged through daily mobility of residents between their homes and their jobs. Data accessibility limits our focus to employment-based activity rather than the full range of activities that people engage with. Nonetheless, this is an important first step in extending the prior focus on residential areas, as employment-based activity is a daily activity that occupies a great deal of people's time (Lindström, 2008). Furthermore, work places may overlap to some extent with the location of other activities like leisure or grocery shopping. Importantly, unlike standard studies using survey samples, our data has the advantage of covering the full census of workplaces and workers, over multiple years.

Second, on a methodological level, the current study contributes to the literature by importing an approach from computational statistics in order to account for spatial and network interdependencies. Moreover, it extends prior spatial models by going beyond implicit assumptions of spatial interactions, to explicitly model a specific type of spatial mechanism -- daily commuting flows between neighborhoods. Third, the study contributes conceptually to the public control literature by highlighting that routine connections to less disadvantaged work environments may be important pathways through which neighborhood residents may secure access to outside resources. Finally, and importantly, this study responds to the need in the activity space literature to extend the predominant focus from the individual to the neighborhood implications of residents' activities such as commuting.

LOCAL AND EXTRA-LOCAL DISADVANTAGE EFFECTS

Neighborhood disadvantage has been traditionally understood as a contributor to local crime as a result of processes linked to social disorganization, public social control, and routine activities, among others. Socioeconomic disadvantage factors, such as poverty and unemployment, are thought to increase crime as a result of increased economic and social distress of neighborhood residents and *social disorganization* processes, weakened formal and informal social interactions, and decreased collective efficacy -- "the willingness of local residents to intervene for the common good [as related to] conditions of mutual trust and solidarity among neighbors" (Sampson, Raudenbush, & Earls, 1997, p. 919). People in disadvantaged neighborhoods were shown to be less likely to trust others, positively interact with each other, come together to monitor children acting up on the street, or to solve collective problems like graffiti or people getting drunk on the street. It is not surprising that disadvantage and related ills increase violent crime over time (Sampson et al., 1997).

The logic of social disorganization may be extended to the work area as well. When neighborhoods of work are disadvantaged, employees who commute from other areas may witness or experience mistrust or mistreatment. More physical and social disorder in the work area, may lead to commuters' stress, fear, or arguments during worktime. As they travel back home, stressed commuters may then sometimes fight with their spouse or neighbors (Bolger et al., 1989) or be left with little energy to stay engaged in the home community (Cisneros, 1996). Alternatively, when work areas are less disadvantaged, commuters may witness the crime reducing effects of collective action such as fixing lights on street corners, cleaning up litter and vacant lots, installing alarms, or signs about a neighborhood crime watch (Bennett, Holloway, & Farrington, 2006; Crowe, 2000; Welsh & Farrington, 2004) and perhaps get new ideas of crime prevention strategies to try in their own homes and neighborhoods.

Wilson's classic insights on the "truly disadvantaged" highlighted that neighborhood poverty and unemployment increase residents' *social isolation* from mainstream institutions, which then leads to further disadvantage and crime. Indeed, institutions within residential neighborhoods and in surrounding neighborhoods have been related to reductions in violent crime (Peterson, Krivo, & Harris, 2000). If work communities with lower levels of disadvantage have a wider range of organizations and services, work related ties may be useful in increasing peoples' access to resources that otherwise would not be available. Indeed, a qualitative study of teenage boys in Boston found that [...parents] “find programs for their sons to fill a specific need or *remove the young man from the neighborhood* because of concerns about negative peer effects” (Harding, 2010, p. 115, emphasis added). A quarter of the boys in this study participated in programs outside their neighborhoods (e.g., an anger management therapy group or summer programs). Furthermore, research has shown that organizations often connect affiliates to outside institutions and organizations that provide a great range of information, services, and resources that otherwise might not be accessible (Small, 2006). Primary involvement in an organization thus often leads to secondary involvements and to forming new interpersonal ties (Tran et al., 2013). When residents have access to resources outside their community of residence, they may be *less strained* by disadvantage in their community (Agnew, 1999; Merton, 1938). Research has shown that when at-risk youth participate in recreation programs, crime rates decrease (Molnar et al. 2008; Witt & Crompton, 1996). Conversely, institutional isolation is associated with increased violence (Thomas & Shihadeh, 2013).

Insights on *public social control* (Hunter, 1985) have also highlighted how connections to outside institutions and actors like mortgage lenders, police departments, or politicians can significantly decrease local crime by increasing the resource flow toward disadvantaged communities. Vélez (2001) found that a neighborhood’s ties to public officials and the police increased public control and decreases victimization, especially in disadvantaged neighborhoods. Similarly, access to home mortgage lending reduced subsequent violent crime rates in Seattle (Vélez et al., 2012). Securing lending from external actors may be in part related to learning about them at work or visiting the banks nearby people's workplace.

In sum, different theoretical perspectives converge to an expectation that work area disadvantage may contribute to local crime. Fully testing the possible mechanisms underlying this expected association is beyond what our data allows. Nonetheless, as a critical first step, we explore the extent to which extra-local effects on local crime can be observed empirically in our data. We thus investigate the following:

Work network hypothesis 1: Crime in a local community will be associated with disadvantage in the network of work communities connected to it through commuting.

If this association is observed, it may nonetheless be explained by selective homophily. Neighborhoods that are internally disadvantaged may be more likely to connect with work neighborhoods that are also disadvantaged, because of stigma or self-reinforcing neighborhood social networks that circulate limited information about job and resource availabilities (Graif et al., 2017; Krivo et al., 2013; Schaefer, 2012). Audit studies support the idea that neighborhood disadvantage affects individuals' job search experiences, such as receiving a call back after job applications (Bertrand & Mullainathan, 2004; Besbris et al., 2015). Ong (1996) noted that poor residents are limited in finding and keeping a job and in where the jobs they find are located. These insights contribute to the following expectation:

Selective homophily hypothesis 2: If an association between work network disadvantage and local crime is observed, it may be fully explained by local disadvantage effects.

If residents of a focal community commute mostly to jobs in the geographic proximity of the focal community, the effect of network disadvantage on crime may be confounded with the spatial spillover effects. Many studies have highlighted the importance of spatial spillovers of disadvantage in increasing local crime (e.g., Peterson & Krivo, 2009, 2010). Graif and Matthews (2017) showed that children's prevalence of being victimized is increased by living in poor neighborhoods or in proximity to poor neighborhoods. This research leads to the following:

Geographic spillovers hypothesis 3: Any association between work network disadvantage and local crime may be fully explained by spatial proximity to disadvantage and crime.

Studies have also found that population travel patterns related to work, are correlated with violent and property crime (Felson and Boivin, 2015; Stults & Hasbrouck, 2015; Wang et al., 2016). Indeed, large proportions of reported criminal incidents (e.g., about 70 percent of homicides in Pittsburgh for instance) have been shown to happen *outside* of the neighborhood of residence of the involved victim or offender (Groff & McEwen, 2007; Tita & Griffiths, 2005). This suggests that, as people travel for work, especially to disadvantaged areas, some may be victimized or exposed to crime. This would be consistent with the *routine activities theory,* which proposes that crime is likely when a motivated offender comes into contact with a desirable target in a context of weak guardianship (Cohen & Felson, 1979). Motivated offenders in disadvantaged work areas may make use of the same public transportation routes as commuters, to find *desirable targets* for crime. Coworkers and visitors from outside neighborhoods may be victimized locally while dating local residents, visiting friends in the area, or simply traveling through (Harding, 2010).

Disadvantage at work may increase the risk of *exposures to crime at work*, which may increase people's fear of future violence, which decreases psychological well-being (Rogers & Kelloway, 1997). Fear of violence may lead residents to isolate themselves from others (LeBlanc & Kelloway, 2002) or anticipate conflicts or respond with violence to perceived threats. Some co-workers may be involved in both legal and illegal work (Fagan & Freeman, 1999). Ties formed in the work community may be more diverse than at home and not necessarily prosocial (Hipp & Boessen, 2015), especially in disadvantaged work neighborhoods with presumably more social disorder and risk. Such ties, however weak, may make commuters the target of crime at home or motivate them to commit crime at home. Local residents and gangs may be targeted by outside gangs in a struggle for status and turf (e.g., access to drug buyers) (Papachristos et al., 2013). Disadvantaged work areas may also have multiple high-risk places, like bars and subway stops, that have been found to increase violent crime, property crime, and disorder offenses locally as well as in their spatial proximity (Groff & Lockwood, 2014; Haberman & Ratcliffe, 2015; Peterson, Krivo, & Harris 2000). This literature thus suggests the following:

> *Network crime hypothesis 4: If an association between the work network disadvantage and local crime exists, it may be due to crime spillovers through the work network.*

DATA AND MEASURES

Data on crime incidents reported to the police, including violent crime and property crime, in Chicago over time was obtained from the Chicago Police Department's Citizen Law Enforcement Analysis and Reporting System under privacy protection through the City of Chicago's Data Portal. Crime data was only available for city neighborhoods. Measures of concentrated disadvantage and other compositional and socioeconomic characteristics were based on data from the Decennial Census. Data for 2000 was used to calculate these scores consistently prior to the years in which crime was assessed. The neighborhood boundary data were obtained from the Census TIGER/Line shapefiles and the City of Chicago's Data Portal. Data on commuting were from the Longitudinal Employer Household Dynamics (LEHD), a U.S. Census Bureau program that combines Unemployment Insurance reports with administrative, business, and demographic data on business establishments and employees, including the location of the establishments and employees' homes (Abowd et al., 2009).

We aggregated commuting data to the community area level on a yearly basis during the duration of the study, between 2001 and 2013. Community areas are geographic units with about just under forty thousand residents on average. Aggregations to other geographic scales would be valuable as well, either larger (e.g. county or city level) or smaller geographic scale (e.g. census tract or block), each coming with its own advantages and limitations. We chose this working definition for the current study for several reasons: a) a smaller geographic scale than the county helps us understand variation in neighborhood processes within the city; b) a larger scale than blocks or tracts helps us distinguish the role of commuting from that of geographic proximity. People who commute within their community area nearby their home tract or block are thus not counted as commuters. In addition to controlling for spatial spillovers at the community level, this helps us understand the network disadvantage effects more clearly from surrounding effects; c) the Census Bureau applies a confidentiality preserving procedure in the commuting data that can affect the interpretation of data for small area units.

Dependent Variables. The overall crime index includes all types of crime incidents, including violent and property crime as well as drug related crimes, and others. The violent crime measure is calculated as the sum of homicide, sexual assault, battery, robbery, domestic violence, and assault. Each incident was counted once, with the exception of murders which were counted once per each victim. Property crime represents burglary, theft, and motor vehicle theft. Each index is calculated as a function of the number of its specific types of crimes located in a community area each year during the duration of the study divided by the population of that community as measured by the census. In order to address possible concerns regarding the reporting of certain types of violent crime, such as sexual assault and simple assaults (Baumer, 2002; Baumer & Lauritsen, 2010), we also assess robbery and homicide separately from the violent crime index. While there may be some differences in the effects by type of crime, the core patterns may be broadly similar across all types. For example, successful strategies to prevent or deal with burglaries or street assaults in a non-disadvantaged area at work may be noted by commuters and subsequently imported to their home community. The same may be said about strategies to prevent or deal with robberies. Some of the strategies may even be overlapping across property and violent types of crime. We estimate crime levels by category and aggregated over three years in order to adjust for some of the random year-to-year fluctuations and better capture the broad temporal pattern. We also estimate crime levels for each individual year during the duration of the study.

Work network disadvantage. The core predictor in our models is the work network disadvantage index, which is calculated following a similar logic as the surrounding disadvantage index (more common in the communities and crime literature, as discussed below). The difference is that, rather than using spatial weight matrix based on the geographic distance between communities (Anselin, 2002; Anselin et al., 2000), we use a network weight matrix (based on actual people moving across space on a daily basis between any two communities). The network matrix of commuting flows between all communities in Chicago is measured separately for each year during the study period. The diagonal of the network matrix is set to null in order to exclude a community's own disadvantage from the calculation of the network disadvantage and enable us to examine the role of network disadvantage independent of that of internal disadvantage. The commuting flow between a focal home community and a work community is measured by first taking the sum of outgoing commuters from a focal area to a work area and then normalizing it by dividing it by the sum of all outgoing commuters with residence in the home community, regardless of work location, during that year. The work network disadvantage for a focal home area is then calculated based on the disadvantage level of each of the work communities connected to that focal area weighted by the normalized commuting flow between that work area and the focal area. The flow-weighted disadvantage levels of work communities are then summed across all the work areas connected to the focal home area. This procedure ensures that the disadvantage levels in work communities that have weaker ties to a home community are given less weight while those with stronger ties (more commuters) are given more weight.

Socio-demographic controls. To assess the role of work community disadvantage on local crime, independent of selective homophily, we measure the internal concentrated disadvantage. Concentrated disadvantage is a composite scale based on a principal component analysis of proportion of residents with income under the poverty level, percent with public assistance income, unemployment rate, and proportion of female headed households with kids. Each item's contribution to the scale is weighted according to its load on the principal component. Other sociodemographic factors that tend to be connected to local crime and may confound the association between network disadvantage and crime are residential stability, population density, and racial and ethnic composition. Residential stability is a scale based on percent household units occupied by owners and percent residents five years of age and older who had lived in the same house five years. The items are weighted by the factor loads of a principal component analysis. Population density is based on the number of residents per square feet area. Racial and ethnic diversity is calculated as a Herfindahl index, following prior work (Blau, 1977). Its scores are one minus the sum of squares of the proportions of the population in each of the six racial or ethnic groups: Hispanics, non-Hispanic whites, non-Hispanic Blacks, Asians, Native Americans, and Others. Higher values mean higher diversity.

Geographic spillovers. To assess the role of network disadvantage independently of spatial spillovers, the models control for the levels of *surrounding disadvantage* and *crime*. These are calculated as spatially weighted averages of the disadvantage (or crime, respectively) in the geographic areas surrounding a focal neighborhood. The spatial weight is constructed based on geographic contiguity using the queen criterion (where two areas are considered contiguous if they have any common point on their boundaries). The boundary data are processed in R to calculate the spatial weights. The cells of the spatial matrix are first assigned a value of one if two communities are contiguous and zero otherwise. Next, the values are standardized by row such that they add up to a value of 1. Each cell is next used as the weight of a neighbor's value (of crime or disadvantage) in calculating a spatially weighted average of crime (or disadvantage) in the surrounding areas of a focal neighborhood.

Network spillovers and temporal spillovers of crime. Measures of network spillovers of crime are calculated like the network disadvantage measure, but based on work-area crime rather than disadvantage. Including this measure helps us understand the role of network disadvantage independent of the possible role of network crime. In the final models, we also include temporal lag measures of crime aggregated over three years, prior to the dependent variable. This helps us estimate the effect of our core predictors on crime at a given time, independent of prior crime, helping us get closer to understanding differences in crime levels over time.

METHODS

Negative Binomial Regression. To deal with right-skewed count dependent variables and over-dispersion, we start by using a negative binomial regression approach (Osgood, 2000), which uses a gamma prior over the lambda parameter in a Poisson regression. The standard errors are adjusted to account for overdispersion (Boessen & Hipp, 2015; Mears & Bhati, 2006; Osgood, 2000; Wo, Hipp, & Boessen, 2016). The dependent variables are crime indices, overall and disaggregated by crime type. The model specification uses population within the unit as an offset (log transformed and the coefficient constrained to one), effectively estimating the outcome as a crime rate. The core predictor variable is the network level of concentrated disadvantage. The set of controls include internal demographic and socioeconomic measures such as residential stability, population density, racial and ethnic heterogeneity, and concentrated disadvantage. Additional important control variables and possible mediators are the surrounding level of disadvantage, surrounding level of crime, and network level of crime.

If the coefficient estimates of network disadvantage are significant, they will suggest that network disadvantage has an effect on predicting levels of crime in the home neighborhood, independent of concentrated disadvantage in the home neighborhood, other demographic factors, surrounding effects of crime or disadvantage, and network levels of crime. A positive coefficient would indicate that increased network disadvantage increases crime in the residential neighborhood; a negative coefficient would indicate that increased network disadvantage decreases crime in the residential neighborhood. The theoretical motivation and the use of a longitudinal design increase our confidence in the temporal ordering of the observed patterns but caution is still needed in interpreting coefficients as effects in a strict causal sense.

Leave-One-Out Cross-Validation and Permutation Tests. Because we consider multiple types of interdependencies between community areas in the form of spatial and network lags, the assumption required by regular regression approaches that observations are independent is not reasonable. While approaches exist for handling spatial interdependencies with negative binomial regression, combining spatial and network indices introduces new interdependencies. To deal with this challenge, we use a technique common in computational statistics and machine learning, though less so in criminology, to separate the training data (used to fit the model) from the test data (used to evaluate model accuracy) (Hastie, Tibshirani, & Friedman, 2009).

The quality of a model is often estimated using a standard in-sample estimator like the Akaike Information Criterion (AIC), which adjusts for model complexity to counter the effects of overfitting and get more accurate estimates of error. However, because AIC is derived from an asymptotic analysis based on independent records, it requires large sample sizes and may be affected by interdependencies between observations (DeLeeuw, 1992). Thus, in addition to comparing AIC scores across models, we also evaluate model accuracy by comparing estimates of absolute reduction in error and relative reduction in error using *leave-one-out cross-validation*, which allows us to measure error out-of-sample. Leave-one-out cross-validation (Hastie et al., 2009) yields more accurate error estimates compared to typical in-sample estimates because the model is not evaluated on the same data it was trained.

For N community areas, the model error is estimated using the following procedure. For each community area C_i, we fit the model on the other community areas (by temporarily removing C_i from the data) and then we evaluate the model's accuracy on C_i. Repeating this procedure for each of the N community areas yields N error estimates that are averaged. Specifically, the mean absolute error (MAE) is calculated by taking the sum of the absolute value of the difference between each observation and its predicted value and then dividing it by the sample size: MAE = $\sum_i^n |y_i - \hat{y_i}| \ / N$, y_i refers to the crime index score for each C_i. The mean relative error (MRE) is calculated by taking the sum of the relative errors and then dividing by the sample size. The relative error is calculated by taking the absolute value of the difference between each observation and its predicted value, and then dividing by the observed value: MRE = $\sum_i^n (|y_i - \hat{y_i}|/y_i) \ /N$. In other words, we measure how well a model based on the rest of the community areas predicts crime in a community area on which it was not trained. This approach is designed to detect overfitting and does not need adjustments for model complexity. Without using leave-one-out, the model would be tested on a point it was trained on, and would have the ability to memorize the testing points. Such memorization would produce unusually optimistic error estimates. This scenario is prevented by leave-one-out.

Leave-one-out cross-validation gives model level error estimates but it does not give significance estimates for the predictors of interest. For this reason, we next use permutation tests from computational statistics (Breiman, 2001). The goal of permutations is to take a predicting variable (feature) and make it independent from the dependent variable (target). This is done by permuting the feature. We conduct multiple permutations and measure error for each permutation, which creates a null distribution of what the errors would look like if that predictor variable were independent of the target variable. The null hypothesis is that the inclusion of a variable does not improve model accuracy. The test statistic for p-value computation is the MAE. Hence, we are testing the significance of a variable on modeling accuracy. To obtain samples from the null distribution, we permute the value of that variable randomly (using a uniform distribution over permutations) across community areas (thus breaking any predictive value that the variable could have). For every permutation, we fit the model and measure accuracy. Repeating this permutation technique M times gives us M empirical samples from the null distribution. We then compare the accuracy on the original data to these M samples under the null distribution. The p value is calculated as the fraction of values from the null distribution that are greater than or equal to the accuracy on the original data and measures whether the inclusion of a variable reduces error in a statistically significant way.

Analytical Strategy. We conduct analyses using the following sequence of steps that enable us to test the hypotheses presented above and conduct robustness tests. First we estimate the relationship between network disadvantage and crime through a series of negative binomial models which gradually include different control variables (Table 2). We also assess whether the main effects are consistent from one period of time to another during the duration of the study. In subsequent models, we add temporal lags to estimate differences in crime over time, focusing on the overall crime and on different crime types (Table 3). Next, we use computational tests to compare the accuracy of models of gradually increased complexity using MAE, MRE and AIC (Table 4). We start with models that only include demographics, move to models that further add surrounding disadvantage and crime; then we additionally include network crime; add temporal lag; and finally, we estimate error reduction based on models that add network disadvantage. Finally, we move from a focus on each model overall to a focus on variables and conduct a series of permutation tests to assess the extent to which the inclusion of a variable reduces error significantly (Table 5). In supplementary analyses, we predict crime: a) separately for every year during this study period and b) with additional controls (Appendix Tables 1 and 2).

RESULTS

Crime decreased during our study period across Chicago's communities (Table 1), which is consistent with the national downward trend of crime. The decline over time is observed for the internal crime levels of communities, their network crime levels, and the surrounding crime levels. The average network crime values tend to be higher than the average surrounding crime, suggesting that models that account for spatial spillover but not for network spillovers may be missing important extra-local crime exposures. On the other hand, the average network disadvantage (-.42 in 2004) is lower than the average surrounding disadvantage (-.01) and internal disadvantage exposures (-.04), suggesting a reason why some disadvantaged areas may be more protected from crime than others. Further examinations show that the neighborhoods with above-median levels of both internal and network disadvantage (38% of all communities in 2004) have significantly higher rates of crime than areas with above-median internal disadvantage but below-median network disadvantage (12%). This suggests that including network disadvantage in modeling crime may improve our understanding of crime.

<Table 1 about here>

Next, we estimate a series of negative binomial models in which we gradually add other neighborhood covariates to test our hypotheses. The first model of Table 2 shows that network disadvantage is positively and strongly associated with local area crime level during the first period in our study, 2004-06. For each one-standard deviation increase in network disadvantage, the expected increase in overall crime is .455 units. Exponentiating this coefficient gives a value of 1.576, which means that a standard deviation increase in network disadvantage is associated with a 58% increase in overall crime rate (Osgood, 2000). This is consistent with the first hypothesis, that work network disadvantage is positively associated with local crime.

Model 2 adds controls for internal residential demographics and internal disadvantage. The effect of network disadvantage decreases, as expected, but remains positive and significant. Specifically, a one-standard deviation increase in network disadvantage increases overall crime .248 units, corresponding to a 28% increase in the overall crime rate. The internal disadvantage also has a significant positive effect, with a one-standard deviation increase leading to a .145 unit increase in overall crime. Population density, residential stability, and ethnic diversity all have a negative coefficient, which means that one unit increase in each of these demographic variables predicts a significant decrease in the overall crime rate. In sum, these patterns indicate that local disadvantage explains some but not all of the association between network disadvantage and local crime, in partial support of the *selective homophily* hypothesis 2. By showing that network disadvantage increases crime in a home community independent of internal disadvantage and other demographics, the results add further support for the *work network* hypothesis 1.

Model 3 of Table 2 adds surrounding disadvantage to the variables included in Model 2. Results show the coefficient of surrounding disadvantage (.089) does not reach significance, but its inclusion does slightly decrease the magnitude and significance of the network disadvantage coefficient (.208). Thus, the results indicate that while a part of the role of network disadvantage in predicting higher levels of crime may be mediated by spatial spillovers of disadvantage, the network disadvantage effect remains strong and significant. Model 4 of Table 2 additionally controls for surrounding and network crime. The coefficient of surrounding crime (.134) is positive and significant, while the network crime coefficient (.103) is not. The magnitude of the coefficient of network disadvantage decreases somewhat (.161), but remains significant ($p<.05$). In sum, these results indicate that the role of network disadvantage in predicting higher levels of crime in the home community is robust to controlling for spatial and network spillovers of crime. These patterns thus offer only weak support for the *geographic and network crime spillover* hypotheses 3 and 4 while further supporting the *work network* hypothesis 1.

The role of network disadvantage is stably positive and significant over time in predicting crime in 3-year spells between 2004 to 2013. Model estimates presented in Table 2 and corresponding models estimating crime year-by-year (Appendix Table 1 model 1) show broadly the same patterns of results as before, suggesting stability in effects across most of the years during the study. Some minor exceptions emerge for two of the 11 years. In predicting 2005 violent crime the coefficient of network disadvantage is non-significant and for overall crime in 2005 and 2013 the coefficient is marginally significant.

<Tables 2 and 3 about here>

Next, Table 3 shows the results from estimating models of different types of crime in 2004-06 and 2011-13 while controlling for the same measures as in Model 4 of Table 2 as well as for the prior level of the corresponding type of crime. The coefficients for network disadvantage are positive and significant for models predicting overall crime. The coefficient for network disadvantage in predicting overall crime is .162 for 2004-06 and .233 for 2011-13.

Analyses for the intermediary time periods (available at request) yield the same pattern of results. Together, the results indicate that the role of network disadvantage in predicting higher levels of crime is robust to controlling for prior levels of crime and stable in its significance over time. This suggests that network disadvantage increases crime over time and its effects may be robust to controlling for unmeasured characteristics that contributed to prior levels of crime.

The next models of Table 3 show results from analyses of the role of network disadvantage in predicting not just overall crime incidents but also violent crime, robbery, homicide, and property crime separately across the years. The coefficient for network disadvantage is .217 for violent crime in 2004-06 and is .238 in 2011-13. The coefficients for network disadvantage in predicting robbery are: .291 in 2004-06 and .278 in 2011-13; in predicting homicide: .347 in 2004-06 and .481 in 2011-13; and for property crime: .237 in 2004-06 and .280 in 2011-13. These results thus indicate that the role of network disadvantage is positive and significant in predicting not just overall crime incidents but also violent crime, robbery and homicide separately. It also predicts more property crime, including a measure that includes arson (not shown). This suggests that the results are not sensitive to measurement error in reporting violence (Baumer 2002; Baumer & Lauritsen, 2010).

Table 4 compares the performance of models with gradually more predictors in estimating overall crime, violent crime, and property crime across different periods of time during our study. The sequence of steps starts with Model 1 that only includes internal disadvantage and other demographic characteristics; then, moves to Model 2 that adds surrounding disadvantage and crime to the set of variables in Model 1; Model 3 builds on Model 2 and adds network crime; Model 4 builds on Model 3 and adds temporal lag of crime; and finally, Model 5 builds on model 4 and adds network disadvantage. The results indicate that models that include network disadvantage tend to have a higher accuracy (lowest errors and lowest AIC scores) in predicting overall crime, violent crime, and property crime compared to the other models. This pattern is not perfectly consistent across all year-crime-type combinations. For instance, in predicting for overall crime in 2004-06, the Model 5's MAE is the third lowest. However, in Model 5 for 2004-06 violent crime, the value for the MAE is 451.368, the value for the MRE is .336, and the AIC is 1148.7. All of these values are lower than the values in any of the other four models. The same is the case for property crime. The MAE levels seem less stable over time, but not for violent crime and less so for property crime. For 2011-13 overall crime, Model 5's value for the MAE is 1192.723, the value for the MRE is .305, and the AIC is 1304.5, all of which are lower than in any other model. Still, no other model matches the accuracy of the network disadvantage Model 5 on all three indicators, across crime types and time.

<Tables 4 and 5 about here>

Table 5 shows results from estimations using the leave one out method with 1000 permutations. A low p value indicates that including the corresponding variable significantly improves model accuracy (reduces error). Similar patterns for network disadvantage emerge in these models as in all previous models. Network disadvantage generally has a significant and positive effect on predicting crime, across crime types and years, with the exception of overall crime in 2004-06. For example, the coefficient for network disadvantage for violent crime in 2004-06 is .217 and the significance value is .007. This value means that about .7% of the values of model accuracy from the null distribution generated from the permutation distribution are greater than or equal to the accuracy on the original data. Thus, including network disadvantage in the model leads to a significant reduction in model error. These additional tests add further confidence in the results above in support of the *work network* hypothesis 1.

Supplementary Analyses. To further explore the sensitivity of the observed network disadvantage effects, we conducted a series of supplementary analyses. First, to examine the robustness of the results to varying our measure of crime, we estimated a set of models with a focus on year-by-year crime levels rather than three year periods. The results are shown in Appendix Table 1, where Model 1 corresponds to Model 4 of Table 2, which controls for demographics and surrounding indices as well as network crime; and Model 2 corresponds to the models in Table 3, which to the full set of controls also adds a control for prior crime. The results show the network disadvantage coefficient in yearly models for various types of crime while including the full set of controls and temporal lag. The same pattern is present in the yearly crime models as in the models for three-year spells of crime, described above. Network disadvantage is significant (or marginally significant) and positive for overall crime across all years. For instance, the coefficient for network disadvantage for overall crime in Model 2 is .169 in 2004, .224 in 2007, .230 in 2010, and .111 (marginally significant) in 2013. The effects of network disadvantage are also significant (or marginally significant) and positive for violent crime and property crime across all years, with the exception of 2005 violent crime. For violent crime, the coefficient for network disadvantage is .195 in 2004, .254 in 2007, .220 in 2010, and .159 in 2013. For property crime, the coefficient for network disadvantage is .257 in 2004, .282 in 2007, .239 in 2010, and .141 in 2013. These results are consistent with those presented in the main tables for the 3-year spells of crime and support the *work network* hypothesis 1.

Second, to examine the sensitivity of the results to a larger and more conservative set of controls, we conducted supplementary analyses that included measures of racial and ethnic composition of the neighborhoods in addition to the typical social disorganization measures of concentrated disadvantage, residential stability, and racial/ethnic diversity. Appendix Table 2 shows results from models that add controls for percent black and percent Hispanic estimating various crime types using three-year aggregated dependent variables in 2004-06 and 2011-13. The same general pattern present in the yearly crime models is present in these three-year aggregated models: the coefficients for network disadvantage are significant and positive across crime types and in both year ranges. An exception is for 2004-06 homicide. In corresponding year-by-year models (not shown), the coefficients for network disadvantage also remain broadly significant and positive across types of crime (overall, violent, and property) and across most years during our study, with the exception of violent crime in 2005 and other types of crime in 2013 (though, for violent crime in 2013, the coefficient is marginally significant).

Additionally, we reiterated the core estimations presented above on network data that defined ties based on all commuters (not shown) rather than low income commuters. The negative binomial estimates yielded the same overall pattern in the results that suggested that network disadvantage increases local crime. However, the permutation tests lead to more uneven results, which suggested that low income ties constitute stronger pathways of influence in the spatial distribution of crime across the city.

DISCUSSION

This study showed evidence that the disadvantage levels in the extra-local network of communities where people work predicts higher levels of crime in people's home communities. The patterns are broadly consistent across different crime types and years and robust to controlling for internal levels of concentrated disadvantage and other structural measures of social disorganization such as residential stability and population heterogeneity. The effect of extra-local disadvantage on crime is consistent with expectations from prior work on activity spaces, public control, social disorganization, routine activities, and further extends this work to highlight for the first time the significance for crime of extra-local forces related to commuting.

The findings suggest that when residents of a focal neighborhood are exposed at work to low disadvantage and, presumably, also to social organization forces such as high social cohesion and trust (Sampson, Morenoff, & Gannon-Rowley, 2002; Sampson et al., 1997), they may be inspired to apply these ideas at home -- to try similar strategies to control crime. Such a pattern has been found in the health literature, in studies of workers found to apply to their own lives strategies learned at work, related to healthy eating (Buller et al., 2000) or quitting smoking (Kouvonen et al., 2008). Workers may also communicate such information to influence others (Christakis & Fowler, 2013). When enough residents have positive exposures, a momentum may be created for local change. When, instead, people experience disorder and mistrust at work, it may increase strain and anomie (Agnew, 1999; Merton, 1938) leading to disengagement, frustration, or fights at home (Bolger et al., 1989).

Residents' exposures to less disadvantaged outside work environments may also contribute to lower local crime if such exposures increase access to external resources, services, and organizations that may not otherwise be available. This idea is consistent with classic thinking on social isolation (Wilson, 1987) and with research showing that increasing residents' participation in organizations and services decreases aggression and reduces crime (Molnar et al., 2008; Witt & Crompton, 1996). When residents have access to resources and institutions at work that they do not have access at home, this may mitigate the deprivation and strain effects of institutional deficiencies at home. The findings also support existing insights on *public control* (Hunter, 1985) and are consistent with evidence that external resources like home mortgage lending or ties to public officials and the police can decrease crime in a focal neighborhood (Vélez, 2001; Vélez et al., 2012). Ties to influential outside actors can be consequential in securing resources to improve the neighborhood (e.g., clean up brownfields)[1] or prevent political decisions like building a highway through the neighborhood that would displace people and decrease remaining home values (Logan & Molotch, 1987).

The results suggest that the observed effect of network disadvantage may also be in part mediated by possible *spatial and network spillovers* of crime. As disadvantage in work communities increases crime there, motivated offenders (or possible victims) may travel through commuting channels in search for targets (or safer activity locations) of crime. This is consistent with insights on *routine activities* (Cohen & Felson, 1979), *crime pattern theory* (Beavon et al. 1994; Brantigham & Brantingham, 1993, 1995), and empirical research that suggests that crime often occurs outside the area of offenders' or victims' residence (Tita & Griffiths, 2005). The findings are also related to prior work on routine activities and transportation flows and crime, including Wang et al. (2016)'s finding that taxi trips help predict crime and Felson and Boivin (2015)'s finding that people's' travel patterns to work are correlated with violent and property crime. Still, the network disadvantage spillovers are not explained away by crime spillovers over the same network, suggesting that other mechanisms are likely important.

Beyond the independent effect of work network disadvantage on local crime, the findings also show that the initial observed effects of network disadvantage can be in part explained by its association with *internal disadvantage*. This is consistent with prior findings that suggest that communities tend to be connected to similarly disadvantaged others (Graif et al., 2017; Schaefer, 2012) and that residents of disadvantaged communities tend to conduct routine activities in similarly disadvantaged areas (Krivo et al., 2013). The findings also suggest that the effects of network spillovers of disadvantage may be (though only in part) related to the *geographic*

[1] Many brownfields (areas with hazardous substances, pollutants, or contaminants) are located in or near Chicago's disadvantaged communities (https://cfpub.epa.gov/bf_factsheets/gfs/index.cfm?xpg_id=1864).

proximity of some of the residents' work areas. This is consistent with an increasing body of evidence that disadvantage in geographically proximate areas is significantly related to victimization, delinquency, crime, and other health risks in a neighborhood (Crowder & South, 2011; Morenoff & Sampson 1997; Morenoff, Sampson, & Raudenbush, 2001; Peterson & Krivo, 2009, 2010; Vogel & South, 2016). The results are also consistent with evidence that crime is affected by non-nearby poverty, several miles away (Graif and Matthews 2017), and suggests that people's activities may explain some of the previously observed geographic spillovers.

Results also show that work network disadvantage predicts higher levels of crime while controlling for prior levels of local crime. This suggests that work network disadvantage contributes to increases in local crime over time and its effects may be robust to controlling for unmeasured characteristics that may have contributed to prior levels of crime.

Limitations. Future work will benefit from comparing patterns at different geographic scales, which will speak to classical debates of differences between face blocks, nominal communities, communities of limited liability, and communities of extended liability (Hunter & Suttles, 1972). Caution is needed in interpreting the results as causal. The longitudinal design of the study and the controls for prior crime levels help indicate the temporal order of the observed patterns but further research is needed on testing possible causal mechanisms. For example, signs signaling the presence of a neighborhood watch in a work community may influence the later formation of such a group in commuters' home community. Examining this and other possible causal mechanisms is beyond the scope of this study but would constitute a valuable direction for future research (Hedstrom & Ylikoski, 2010; Matsueda, 2017).

The crime data is limited to neighborhoods within the city, which is a common limitation in communities and crime studies. Overcoming this limitation would help illuminate differences between suburbs and inner-city neighborhood dynamics (Singer, 2014). While Chicago has the advantage of size and diversity, its segregated configuration of disadvantage makes the generalizability of the findings to other cities uncertain. Future analyses comparing effects of network disadvantage among different cities, large versus small, and urban versus suburban versus rural communities will be invaluable. An important direction for future research is also to further differentiate between different types of ties, including strong and weak. Commuting is uniquely valuable as a direct measure of how people cross neighborhood boundaries on a routine basis, but other types of activity-based connections relevant for the public control of crime are likely important as well. Future research that assesses different types of activity locations, such as travel related to recreation or other activities, and data sources (e.g., using Uber, Lyft, or other taxi trips; Twitter and Facebook check-in locations) will be valuable. Adding organic big data such as these will be invaluable in complement high-cost prospective activity space data.

CONTRIBUTIONS AND IMPLICATIONS

In the current study, we examined the extent to which the ecological network contexts of neighborhoods significantly contribute to local crime. Results from analyses of networks based on commuting population flows among Chicago neighborhoods over more than a decade showed that local crime is positively associated with concentrated disadvantage in communities where residents go to work, independent of internal disadvantage, spatial spillovers, and prior crime levels. These findings contribute to the literature in several key ways and have significant implications for future research and policy. First, the results suggest that extra-local exposures are relevant for crime above and beyond the effects of internal disadvantage and geographically proximate spillovers -- the two major explanatory forces in the neighborhood effects and communities and crime literature to date. This highlights the importance of revisiting the predominant assumption in the literature that neighborhoods can be treated as *closed systems* or as *mostly affected by nearby areas* (Browning, Calder, Boettner, et al., 2017; Graif et al., 2014).

Second, as importantly, the results connect with and advance the *routine activities, crime pattern,* and *activity space* bodies of work (Boivin & D'Elia, 2017; Browning, Calder, Soller, et al., 2017; Browning et al., 2015; Cohen & Felson, 1979; Felson & Boivin 2015; Krivo et al., 2013; Wikström et al., 2010) by showing for the first time the significant neighborhood level implications of residents' daily mobility across space through activities such as commuting for work. Considering the costs of collecting activity space data through surveys, our study shows that using administrative data on commuting is a valuable, scalable, and feasible approach. The focus on employment-based activity spaces, while limited in the breadth of activities and exposures it can capture, reflects a major activity in which people spend a lot of their waking hours (Lindström, 2008). Importantly, people also develop a wider range of weak ties at work, which may be important in information transmission and norms diffusion (Granovetter, 1973).

These findings also contribute to the current knowledge in criminology of communities and place and in the spatial and neighborhood effects literature more broadly by showing evidence suggesting that disadvantage risks may spillover through population mobility channels that are not accounted for in the traditional neighborhood effects approaches. The current study builds on recent advances in spatial modeling in criminology (Graif, 2015; Morenoff et al., 2001; Peterson & Krivo, 2009) and pushes it further by getting at the heart of standard assumptions of interactions across space to *explicitly* model interactions across space, via commuting.

Furthermore, the study connects to important recent work on transportation and crime (Boivin & Felson, 2017) and expands it by moving beyond a focus on crime traveling through transportation channels to highlight for the first time the effects on crime of the disadvantage exposures through such channels. By pushing beyond geographic proximity and showing evidence in support of broader avenues of extra-local influence, the current findings support and advance a classic perspective on public social control (Hunter, 1985) -- a fundamental but under-explored category of mechanisms that complement rather than exclude the more commonly examined private and parochial forms of control (e.g., Vélez, 2001).

These results are also consistent with an emerging body of work on the importance of networks across space based on co-offending or gang conflicts (Papachristos 2015; Papachristos et al., 2013; Schaefer, 2012) and further extend the literature by demonstrating the importance of commuting ties across neighborhoods. Commuting dynamics present promising new possibilities for crime control because, compared to co-offending or gang conflicts, which are rare and negative forms of interactions, commuting is a more *frequent and routine* (predictable) form of interaction, with many possible positive implications. The results support the idea that extra-local resources and information flow through commuting networks to shape crime (Vélez et al., 2012).

The current findings suggest that new avenues for decreasing neighborhood crime may be possible through an area's connections to other areas. A great deal of attention has been paid to the recent increasing levels of crime in some Chicago communities, despite the overall downward trend in crime across the country (e.g., Gorner, 2016a, b; Wills & Hernandez, 2017), suggesting that old approaches need further refining. When intervening to prevent or control crime, this study suggests that attention must be paid not only to a community's disadvantage level but to the disadvantage level in the communities to which residents are exposed at work. When residents of poor communities have jobs in other disadvantaged areas, the burden of crime in their communities may be harder to overcome than when jobs are in less disadvantaged areas. Programs that encourage connections between communities of different disadvantage levels may open new avenues for crime prevention and control than possible otherwise.

The findings suggest that where people go to work can affect safety in their own neighborhoods. By focusing on the interconnectedness of neighborhoods and the spillovers of disadvantage through such networks, programs may be able to identify and intervene in certain well-connected neighborhoods to lower the crime in many more neighborhoods, ultimately benefitting the city as a whole. Through work ties parents in disadvantaged neighborhoods may become aware of and begin to access extra-local resources, services, and information that can protect them and their children from being victimized or becoming involved in crime (Harding, 2010). While it may be challenging to convince employers to bring jobs to a particular disadvantaged neighborhood, it may be more feasible to improve the neighborhood's connections (e.g., public transportation) to less disadvantaged areas where jobs are located, and to incentivize employers to hire workers from disadvantaged neighborhoods.

Future research will also benefit from further extending the models of extra-local dynamics proposed here to investigate possible heterogeneity in effects across different cities and periods. Qualitative and quantitative work will be needed to further understand the mechanisms of social interactions that contribute to the observed effects of work network disadvantage.

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Table 1. Descriptive Statistics

Variable	Mean	SD	Min	Max	Variable	Mean	SD	Min	Max
Local Crime Variables					Network Crime Variables				
2004-2006 Overall Crime	5899.02	4851.89	431.33	28709.67	2004 Network Overall Crime	8995.06	881.80	7146.09	11876.69
2011-2013 Overall Crime	4285.63	3636.49	326.67	21349.67	2011 Network Overall Crime	6640.02	570.57	5305.91	8539.80
2004 Overall Crime	6066.90	5004.79	402.00	29333.00	2004 Network Violent Crime	2071.57	243.95	1545.67	2975.14
2007 Overall Crime	5658.74	4761.10	416.00	28417.00	2011 Network Violent Crime	1428.34	135.72	1080.15	2053.43
2011 Overall Crime	4556.61	3852.92	368.00	3852.92	2004 Network Property Crime	3465.05	368.01	2802.84	4555.83
2013 Overall Crime	3952.36	3385.69	290.00	20100.00	2011 Network Property Crime	2945.21	294.88	2399.99	3717.99
2004-2006 Violent Crime	1672.80	1542.29	95.67	8755.00	Surrounding Crime Variables				
2011-2013 Violent Crime	1195.79	1117.75	81.33	6357.67	2004 Surrounding Overall Crime	6349.54	3082.83	1238.00	16603.00
2004 Violent Crime	1735.47	1598.77	105.00	9087.00	2011 Surrounding Overall Crime	4767.91	2326.49	1049.75	12550.50
2007 Violent Crime	1600.40	1506.40	103.00	8354.00	2004 Surrounding Violent Crime	1826.06	944.21	237.75	5065.25
2011 Violent Crime	1255.84	1166.84	93.00	6516.00	2011 Surrounding Violent Crime	1323.47	695.98	173.75	3832.50
2013 Violent Crime	1106.18	1040.93	68.00	5971.00	2004 Surrounding Property Crime	1910.88	939.72	446.75	5097.00
					2011 Surrounding Property Crime	1630.54	798.34	368.75	4145.00
2004-2006 Property Crime	1754.51	1354.14	124.00	5831.33					
2004-2006 Property Crime	1460.22	1205.54	104.00	5562.33	Demographic Variables				
2004 Property Crime	1842.39	1454.33	109.00	6376.00	Population Density	5.39	2.81	.43	14.00
2007 Property Crime	1662.44	1326.63	106.00	5889.00	Residential Stability	-.01	.97	-2.11	1.73
2011 Property Crime	1572.64	1281.84	121.00	5510.00	Ethnic Diversity	.00	1.00	-1.30	2.12
2013 Property Crime	1318.48	1099.81	93.00	5260.00	Percent Black	41.20	41.09	.29	98.56
					Percent Hispanic	21.76	25.16	.00	88.91
Network Disadvantage Variables					Internal Disadvantage	-.04	.91	-1.24	2.38
2004 Network Disadvantage	-.42	.17	-.72	-.14					
2011 Network Disadvantage	-.46	.15	-.74	-.14	Surrounding Disadvantage	-.01	.66	-1.02	1.29

NOTE: N=77.

ABBREVIATION: SD = standard deviation.

Table 2. Negative Binomial Regression Predicting 3-Year Spells of Overall Crime

	2004-2006				2007-2009	2010-2012	2011-2013
	M1	**M2**	**M3**	**M4**	**M4**	**M4**	**M4**
Network disadvantage	.455 ***	.248 ***	.208 **	.161 *	.228 ***	.241 ***	.233 ***
	(.059)	(.064)	(.074)	(.068)	(.066)	(.062)	(.068)
Population density		-.311 ***	-.310 ***	-.372 ***	-.370 ***	-.359 ***	-.377 ***
		(.051)	(.050)	(.047)	(.045)	(.044)	(.048)
Residential stability		-.361 ***	-.348 ***	-.255 ***	-.281 ***	-.244 ***	-.270 ***
		(.054)	(.055)	(.060)	(.058)	(.056)	(.059)
Ethnic diversity		-.212 **	-.190 **	-.092	-.105 †	-.102 †	-.086
		(.066)	(.069)	(.066)	(.062)	(.058)	(.065)
Internal Disadvantage		.145 *	.115	.145 *	.125 †	.145 *	.123 †
		(.069)	(.074)	(.067)	(.065)	(.060)	(.067)
Surrounding disadvantage			.089	.036	-.028	-.039	.011
			(.086)	(.074)	(.074)	(.074)	(.079)
Surrounding crime				.134 *	.173 ***	.139 **	.148 **
				(.054)	(.046)	(.044)	(.052)
Network crime				.103	.072	.156 **	.133 *
				(.071)	(.057)	(.053)	(.064)
Intercept	8.481 ***	8.405 ***	8.404 ***	8.391 ***	8.294 ***	8.119 ***	8.066 ***
	(.059)	(.041)	(.040)	(.036)	(.035)	(.034)	(.037)
Dispersion Parameter	3.703	7.872	7.979	9.945	10.672	11.375	9.477
	(.573)	(1.247)	(1.264)	(1.584)	(1.702)	(1.818)	(1.512)
Log Likelihood	-705	-674	-673	-664	-654	-639	-641
AIC	1415	1362	1363	1349	1329	1298	1303

NOTES: N =77. Standard Errors are in parentheses.
†p<.10; *p < .05; **p < .01; ***p < .001 (two-tailed tests).

Table 3. Negative Binomial Regression Predicting 3-Year Spells of Crime by Type, 2004-2006 and 2011-2013

	Overall Crime		Violent Crime		Robbery		Homicide		Property Crime	
	2004-06	**2011-13**	**2004-06**	**2011-13**	**2004-06**	**2011-13**	**2004-06**	**2011-13**	**2004-06**	**2011-13**
Network disadvantage	.162 *	.233 ***	.217 **	.238 ***	.291 **	.278 *	.347 **	.481 ***	.237 ***	.280 ***
	(.068)	(.068)	(.080)	(.070)	(.106)	(.110)	(.124)	(.128)	(.070)	(.074)
Population density	-.365 ***	-.376 ***	-.226 ***	-.218 ***	-.133 †	-.172 †	-.072	-.059	-.436 ***	-.424 ***
	(.050)	(.051)	(.053)	(.054)	(.075)	(.089)	(.095)	(.106)	(.050)	(.049)
Residential stability	-.260 ***	-.272 ***	-.204 ***	-.205 ***	-.203 **	-.172 †	-.030	-.045	-.258 ***	-.230 ***
	(.061)	(.060)	(.057)	(.057)	(.077)	(.094)	(.088)	(.095)	(.065)	(.066)
Ethnic diversity	-.100	-.087	-.123 †	-.092	-.072	-.032	-.069	.025	.004	-.006
	(.069)	(.068)	(.066)	(.065)	(.089)	(.107)	(.101)	(.102)	(.076)	(.072)
Internal Disadvantage	.149 *	.124 †	.303 ***	.277 ***	.147	.156	.355 ***	.388 ***	-.050	-.005
	(.068)	(.067)	(.072)	(.069)	(.093)	(.108)	(.101)	(.100)	(.067)	(.069)
Surrounding disadvantage	.032	.010	.058	.065	.196 †	.184	.099	.070	.081	.037
	(.074)	(.079)	(.077)	(.078)	(.103)	(.129)	(.119)	(.118)	(.074)	(.079)
Surrounding crime	.143 *	.150 **	.152 *	.154 *	.240 **	.139	.114	.166 *	.194 ***	.187 ***
	(.059)	(.057)	(.064)	(.064)	(.089)	(.103)	(.082)	(.074)	(.057)	(.056)
Network crime	.098	.132 *	.016	.059	.028	.269 *	.024	.090	.170 *	.220 **
	(.073)	(.065)	(.078)	(.067)	(.100)	(.117)	(.082)	(.109)	(.074)	(.070)
Temporal lag	-.019	-.005	-.046	-.045	-.025	.005	.037	.017	-.036	-.004
	(.049)	(.050)	(.052)	(.053)	(.069)	(.084)	(.045)	(.048)	(.054)	(.056)
Intercept	8.391 ***	8.066 ***	7.056 ***	6.720 ***	4.925 ***	4.706 ***	1.209 ***	1.137 ***	7.202 ***	7.007 ***
	(.036)	(.037)	(.037)	(.037)	(.049)	(.059)	(.067)	(.070)	(.035)	(.037)
Dispersion Parameter	9.96	9.48	9.62	9.59	5.73	4.05	403.43	403.43	10.58	9.77
	(1.59)	(1.51)	(1.55)	(1.56)	(1.00)	(.70)	(.55)	(.65)	(1.71)	(1.58)
Log Likelihood	-664	-641	-563	-538	-420	-415	-142	-138	-571	-559
AIC	1351	1305	1149	1098	863	852	306	298	1165	1141

NOTES: N=77. Standard Errors in parentheses.
†p<.10; *p < .05; **p < .01; ***p < .001 (two-tailed tests).

Table 4. Leave-One-Out Cross-Validation: Comparing Model Performance in Predicting 3-Year Spells of Crime by Type

		2004-2006			2007-2009			2010-2012			2011-2013		
		Overall Crime	**Violent Crime**	**Property Crime**	**Overall Crime**	**Violent Crime**	**Property Crime**	**Overall Crime**	**Violent Crime**	**Property Crime**	**Overall Crime**	**Violent Crime**	**Property Crime**
M1	MAE	1744.960	457.203	588.396	1572.869	414.415	555.406	1338.676	350.827	513.403	1319.130	329.759	493.407
	MRE	.371	.394	.402	.386	.408	.414	.384	.407	.420	.399	.406	.425
	AIC	1373.400	1170.500	1201.400	1361.100	1159.100	1189.300	1339.100	1134.400	1185.500	1333.300	1125.300	1178.300
M2	MAE	1688.182	453.927	568.394	1470.978	404.259	507.919	1249.469	335.313	504.002	1211.069	315.943	488.528
	MRE	.322	.345	.319	.327	.346	.322	.321	.342	.329	.333	.344	.337
	AIC	1353.700	1154.000	1170.800	1340.500	1137.800	1157.400	1316.500	1113.900	1154.200	1311.300	1106.400	1149.900
M3	MAE	1796.719	499.896	595.206	1529.527	402.851	532.931	1314.210	348.769	527.758	1233.421	338.482	508.491
	MRE	.314	.340	.319	.315	.321	.322	.303	.310	.321	.328	.339	.340
	AIC	1352.400	1152.000	1171.500	1338.000	1128.200	1158.000	1309.500	1102.800	1149.700	1311.400	1104.900	1149.800
M4	MAE	1759.377	474.540	591.245	1537.967	402.368	535.394	1295.765	335.162	539.952	1243.160	313.497	518.020
	MRE	.318	.345	.322	.316	.322	.324	.300	.309	.321	.333	.340	.342
	AIC	1354.300	1153.700	1173.300	1339.800	1129.900	1160.000	1311.200	1104.800	1151.600	1313.400	1106.600	1151.800
M5	MAE	1753.112	**451.368**	**543.626**	1509.161	**390.973**	**474.996**	1271.008	**332.218**	504.750	**1192.723**	**298.101**	**458.074**
	MRE	.315	**.336**	**.302**	**.304**	**.315**	**.300**	**.287**	**.309**	**.311**	**.305**	**.315**	**.310**
	AIC	**1350.800**	**1148.700**	**1164.500**	**1330.600**	**1122.100**	**1145.600**	**1299.500**	**1097.500**	**1140.600**	**1304.500**	**1097.800**	**1140.600**

NOTES: *N* =77. Bolded values indicate the models in which including network disadvantage (M5) produces the lowest error.

M1 includes demographic predictors (population density, residential stability, ethnic diversity, and internal disadvantage). M2 includes all variables in M1, surrounding disadvantage, and surrounding crime. M3 includes all variables in M2 and network crime. M4 includes all variables in M3 and temporal lag. M5 includes all variables in M4 and network disadvantage.

Table 5. Permutation Tests on 3-Year Spells of Crime by Type for 2004-2006 and 2011-2013 (Using 1000 Permutations)

	2004-2006			2011-2013		
	Overall Crime	Violent Crime	Property Crime	Overall Crime	Violent Crime	Property Crime
Network disadvantage	.162	.217 **	.237 **	.232 *	.238 *	.279 **
	(.178)	(.007)	(.008)	(.035)	(.012)	(.006)
Population density	-.364 ***	-.226 **	-.435 ***	-.375 ***	-.217 **	-.423 ***
	(.000)	(.008)	(.000)	(.000)	(.002)	(.000)
Residential stability	-.260 ***	-.203 †	-.258 ***	-.272 **	-.203 *	-.231 ***
	(.000)	(.056)	(.000)	(.002)	(.025)	(.000)
Ethnic diversity	-.101 †	-.123 †	.003	-.088 †	-.092 *	-.007
	(.095)	(.095)	(.872)	(.091)	(.029)	(.814)
Internal Disadvantage	.151	.306 ***	-.049	.126	.280 **	-.004
	(.807)	(.000)	(.377)	(.416)	(.001)	(.564)
Surrounding disadvantage	.031	.056	.080	.010	.063	.038
	(.909)	(.893)	(.975)	(.939)	(.862)	(.980)
Surrounding crime	.144	.151	.193 *	.150	.153	.187 *
	(.849)	(.809)	(.010)	(.689)	(.497)	(.012)
Network crime	.096	.016	.168	.130	.059	.217
	(.998)	(.984)	(.995)	(.927)	(.962)	(.935)
Temporal Lag	-.019	-.045 †	-.035 †	-.004	-.045 *	-.003
	(.413)	(.066)	(.075)	(.558)	(.014)	(.253)
Intercept	8.391 ***	7.056 ***	7.202 ***	8.066 ***	6.719 ***	7.007 ***
	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)

NOTES: *N*=77. P values in parentheses.
†p<.10; *p < .05; **p < .01; ***p < .001 (two-tailed tests).