Title
Accounting for Feedback Effects in Neighbourhoods and Crime Research: How Much Does It Matter?

Journal
The British Journal of Criminology

ISSN
0007-0955

Author
Hipp, John R

Publication Date
2025

DOI
10.1093/bjc/azaf111

Accounting for Feedback Effects in Neighborhoods and Crime Research: How Much Does It Matter?

John R. Hipp*

November 11, 2025

Post-print. Published in British Journal of Criminology https://doi.org/10.1093/bjc/azaf111

Published: 08 December 2025

Word count: 7,912

Word count (including references): 9,441

Running Head: "Crime Feedback Effects"

* Department of Criminology, Law and Society and Department of Sociology, University of California, Irvine. Address correspondence to John R. Hipp, Department of Criminology, Law and Society, University of California, Irvine, 3311 Social Ecology II, Irvine, CA 92697; email: john.hipp@UCI.edu. This research is supported in part by NIJ grant 2012-R2-CX-0010

Accounting for Feedback Effects in Neighborhoods and Crime Research:

How Much Does It Matter?

Abstract

Abstract:

Whereas much ecology of crime research employs cross-sectional study designs, these studies assume that crime does not *cause* various neighborhood measures to change over time. However, longitudinal research evidence suggests this is not a plausible assumption. The present study explores this question using neighborhood data in U.S. cities in Southern California covering over 6.7 million residents. Using data from two time points—2000 and 2010—instrumental variable models account for this endogeneity. The results demonstrate sharp differences between models that do or do not account for endogeneity: several measures yield completely reversed results from positive to negative or vice versa, some measures exhibit much stronger results, whereas others have much weaker, or nonsignificant, results. The findings highlight the importance of accounting for endogeneity in the U.S. context, and that the differences are potentially not modest, but large enough that failing to account for endogeneity may limit our understanding of these social processes that criminologists have theorized. Whether such results generalize to non-U.S. contexts will need to be a focus of future research.

Keywords: egohoods, crime, endogeneity, feedback effects.

Crime Feedback Effects
Bio

John R. Hipp is a Professor in the departments of Criminology, Law and Society, and Sociology, at the University of California Irvine. His research interests focus on how neighborhoods change over time, how that change both affects and is affected by neighborhood crime, and the role networks and institutions play in that change. He approaches these questions using quantitative methods as well as social network analysis. He has published substantive work in such journals as *American Sociological Review, Criminology, Social Forces*, *Social Problems, Journal of Quantitative Criminology, Journal of Research in Crime & Delinquency, Urban Studies* and *Journal of Urban Affairs*. He has published methodological work in such journals as *Sociological Methodology*, *Psychological Methods*, and *Structural Equation Modeling*.

Accounting for Feedback Effects in Neighborhoods and Crime Research: How Much Does It Matter?

A voluminous body of research has focused on the general research question of why certain neighborhoods tend to have higher levels of crime compared to other neighborhoods. Whereas some of these studies have used longitudinal data, the vast majority have used cross-sectional data to explore these questions. A necessary assumption of this modeling strategy is that the various neighborhood characteristics measured *cause* higher or lower levels of crime, but that crime does not *cause* those various neighborhood measures to change over time. But is this a reasonable assumption?

Indeed, there is ample evidence from the more limited number of longitudinal studies that this assumption is not reasonable. Research using longitudinal data has shown that neighborhoods with higher levels of crime at one time point tend to have lower average income at a later time point (Hipp and Steenbeek 2016), lower home values (Tita, Petras, and Greenbaum 2006); higher vacancy rates (Morenoff and Sampson 1997), greater concentration of racial/ethnic minority residents (Xie and McDowall 2010), fewer retail businesses (Hipp 2010; Hipp et al. 2019), and lower collective efficacy (Hipp 2016; Hipp and Wickes 2018). This is just a sample of the results from this body of research. The inescapable conclusion is that the common strategy of estimating models using cross-sectional data and assuming that there is no feedback effect from crime onto the various measures included in the model is likely not reasonable. The implication is that endogeneity is being ignored in this modeling strategy.

The question then, is how big of an issue is this for our knowledge in the field? That is, if there are indeed feedback effects from crime onto various neighborhood measures, as there almost certainly are, how consequential are these for existing models? One possibility is that these feedback effects are relatively modest, and therefore the bias in existing studies is relatively minor and not of much concern. An alternative possibility is that these feedback effects, at least for some measures, are quite strong and therefore our knowledge base is incorrect given that we have presumed that some neighborhood measures cause crime, when in fact a more reasonable argument is that crime causes change in the neighborhood in these measures.

This is the focus of the present study. I use a dataset with information at two timepoints to first estimate cross-sectional models that mimic those in the literature, and then to estimate models in which I account for potential endogeneity using instrumental variables (IV's). The comparison of these two sets of analyses provides an approximation of the magnitude of the problem. Indeed, the evidence will show that, if the instrumental variables used here are appropriate, this endogeneity appears quite substantial for some of the measures, and ignoring it may lead to results in which the substantive conclusions appear completely reversed. In the subsequent literature review, I mostly focus on the evidence regarding the extent to which crime might impact these various neighborhood characteristics, given that the extensive literature regarding how these measures might impact crime is quite well known. I then describe the dataset, and the methodology. Following that I present the results, and then discuss the implications of the results and conclude.

Literature Review

The concern with possible feedback effects from an outcome measure in a regression framework—in this case, the neighborhood crime rate—falls under the broader statistical category of endogeneity.[1] Endogeneity also occurs when covariates are correlated with the error term for other reasons, including measurement error, but here I am specifically focused on how it can be fostered by the outcome measure also impacting one or more of the exogenous variables. In short, if there are such feedback effects, then in a longitudinal model we would expect the particular neighborhood structural measure—say concentrated disadvantage—to cause higher crime in the neighborhood at the next time point; but we would also expect that higher levels of crime at one time point to result in increased concentrated disadvantage at the next time point (assuming an appropriate time gap). If we lack longitudinal data, this implies these two equations:

(1) $$Y_1 = \alpha + \beta_1 X_1 + \varepsilon$$

(2) $$X_1 = \alpha + \beta_2 Y_1 + \varepsilon$$

where Y_1 is the outcome variable (crime) and X_1 is the independent variable of interest (say concentrated disadvantage). The question then is the relative size of the effects for β_1 and β_2. It they each have relatively similar-sized causal effects on the other (with the same sign), then a regression model with crime as the outcome variable and concentrated disadvantage as an exogenous variable (equation 1) will obtain a coefficient that is approximately a combination of the two effects. Thus, it will be biased upwards, and will be approximately twice as large as it should be, as it will contain both the effect of disadvantage on crime (β_1) and the effect of crime on disadvantage (β_2). If instead, the effect of crime on disadvantage is relatively much smaller than the effect of disadvantage on crime, then the coefficient in the regression model will only be

[1] Note that by the term "regression" I mean any model with a single outcome variable (y) and a set of independent variables (x). The actual estimation can vary depending on the form of the outcome variable: either a linear regression, a logistic regression for a 0/1 outcome, a Poisson regression for a count outcome, etc. Regardless, the general issue of endogeneity due to possible feedback effects from the outcome measure are the same for all of these models.

biased slightly upwards—this would be of only minor concern. At the other extreme, if the effect of crime on disadvantage is much larger than the reverse, then whereas the coefficient in a properly specified regression model should be closer to zero it will instead contain almost all bias. Thus, the relative sizes of these effects are important. Existing cross-sectional research has implicitly assumed that any causal effect of crime on various neighborhood measures is quite small, and therefore can be safely ignored. Is this reasonable? I next discuss this question in relation to several key measures posited in the literature.

Impact of crime on collective efficacy and social capital

A body of literature has used cross-sectional studies to explore the relationship between collective efficacy and crime (Sampson, Raudenbush, and Earls 1997). The presumption is that neighborhoods with more collective efficacy can better address problems in the neighborhood, and therefore have lower levels of crime. Indeed, a study of residents in London neighborhoods at one point in time found that collective efficacy was negatively associated with fear of violent crime (Brunton-Smith, Jackson, and Sutherland 2014). Nonetheless, even an early contribution by Sampson considered the possibility that crime may indeed impact the level of collective efficacy (Sampson 2006), and a study of 100 neighborhoods in Seattle used instrumental variable models to find that violent crime impacted collective efficacy (Bellair 2000).

Collective efficacy is a perceptual construct (Sampson 2006), and therefore a common strategy is to survey a large number of people in each neighborhood in a city asking them their perceptions regarding how their neighbors will respond to problems. Given the difficulty of conducting such large-scale surveys, Weisburd and colleagues argued that voting behavior is a reasonable proxy for collective efficacy (Weisburd, Groff, and Yang 2012). Whereas voting might appear to be an individual action, a countervailing argument from political scientists is that it is not really rational to vote given that one's singular vote cannot possible impact nearly any election. Instead, the motivation for voting typically is based on altruism (Jankowski 2007) or some sense of the collectivity (Bali, Robison, and Winder 2020). There is evidence that those perceiving more neighborhood cohesion are more likely to vote (Gearhart 2020). Nonetheless, given that voting combines collective efficacy (a perception) with actual activity (the voting behavior), I argue that it is actually an imperfect measure of *neighborhood engagement*. This is an important distinction when considering the endogeneity induced by feedback effects from crime, as they would be impacting both a combination of perceptions and activity, as I describe next.

The feedback effects of crime onto collective efficacy versus neighborhood engagement differ. Longitudinal studies find that levels of crime in a neighborhood can reduce the level of collective efficacy at a subsequent time point. A study of block groups in three counties in North Carolina found that neighborhoods with higher perceived crime at one time point had lower collective efficacy at the next time point (Hipp 2016). A study of individuals nested in Brisbane neighborhoods found that the presence of more problems in the neighborhood reduced a sense of collective efficacy at the next timepoint (Hipp and Wickes 2018), although two studies of Brisbane neighborhoods found no relationship between various types of crime and collective efficacy at the next time point (Hipp and Wickes 2017; Wickes and Hipp 2018). Although a study in Utrecht, Netherlands found that neighborhoods with higher perceived violent crime at one time point had lower collective efficacy at the next time point, this same study found that in neighborhoods with higher burglary rates residents were *more* likely to express collective efficacy at the next time point (Hipp and Steenbeek 2016). Research in 151 neighborhoods in the U.K. found that higher levels of disorder and crime resulted in reduced cohesion four years later (Markowitz et al. 2001). In contrast, crime and neighborhood problems appear to impact actual behavior differently. For example, a study of Brisbane neighborhoods found that residents who perceived disorder were *more* likely to engage in various types of informal social control action (Hipp and Wickes 2018). Similarly, studies in Utrecht, Netherlands found that in neighborhoods with higher burglary rates residents were *more* likely to engage in activity to improve the neighborhood (Hipp and Steenbeek 2016), and that higher levels of neighborhood disorder resulted in increased activity to improve the neighborhood (Steenbeek and Hipp 2011). Thus, accounting for feedback effects from crime will be important for assessing how neighborhood engagement impacts crime.

The same possible feedback effect also exists for the relationship between voluntary organizations and crime. There is a large body of cross-sectional literature exploring the relationship between voluntary organizations and crime, and this literature has yielded very mixed results, as noted by Wo and colleagues (Wo, Hipp, and Boessen 2016). This same study noted that the geographic placement of voluntary organizations is almost certainly not random, but instead often is in response to need. As a consequence, we would expect that the needs of high crime neighborhoods would result in more organizations placed there. The implication is that studies viewing the relationship between voluntary organizations and crime could result in a null, or even a positive, relationship as detected in a study in the Hague (Bruinsma et al. 2013). Nonetheless, despite this plausibility, few studies have accounted for a possible feedback from crime to voluntary organizations.

Impact of crime on businesses

Various theories such as crime pattern theory (Brantingham and Brantingham 2008) and routine activity theory (Felson and Boba 2010) posit that the presence of businesses provide opportunities for crime, and therefore their presence will result in more crime (Bernasco and Block 2011; Hipp and Hodgen 2024; Yu and Maxfield 2014). Most existing research of this question is cross-sectional, and has assessed the extent to which the presence of various types of businesses are related to where crime occurs. Nonetheless, there is reason to expect that crime can discourage the locations of businesses. Indeed, a study of neighborhoods in New York City found evidence that higher levels of violent crime reduce commercial property values, and these effects were particularly strong in lower income neighborhoods dominated by racial minorities (Lens and Meltzer 2016). Research in neighborhoods across five cities found evidence that surges in increased violence resulted in fewer service-related establishments in low-crime neighborhoods (Greenbaum and Tita 2004), and another study over 10 years in 13 cities found that higher levels of violent crime resulted in fewer retail businesses 10 years later (Hipp 2010). Furthermore, a study using data on businesses in Southern California over a 15 year period found that retail businesses were more likely to leave locations with more property crime, whereas white collar businesses were more likely to leave locations with more violent crime (Hipp et al. 2019). Similarly, these same categories of businesses avoided opening in locations with higher levels of these same types of crime.

Impact of crime on socio-demographic characteristics

Theory and evidence suggest that crime can impact residential mobility. Given residents' preferences to avoid crime and disorder, Skogan argued that disorder and crime can lead to a sense of decline in a neighborhood and the out-mobility of residents (Skogan 1990). Building on the insights of Lyons and Lowrey (1989), the EVLN (exit, voice, loyalty, neglect) model posits that residents will respond to neighborhood problems through one of these four actions; crime is a particularly salient neighborhood problem that can drive these decisions (Hipp 2023). This desire to leave a neighborhood with more crime ("exit") has numerous consequences. One obvious one would be an increase in residential instability due to this mobility. Another would be that the household may not be able to leave, and that could impact their perceptions of the neighborhood. Yet another would be that although the exiting household finds a new place to live, there may not be another household willing to replace them in their unit: in this case, higher crime would result in reduced population and increased vacancy rates. Given the presumed importance of these measures for impacting crime, these are potentially important processes. This temporal aspect to residential instability was accounted for by a study of neighborhoods across the entire U.K. by lagging residential housing turnover one year to assess how it was associated with increased property crime in the following year (Braakmann 2023).

There is evidence that crime does indeed impact general mobility by households. One study using the National Crime Victimization Survey (NCVS) found that experiencing a crime incident increased the likelihood of exiting a neighborhood (Dugan 1999), whereas other research with the NCVS found that the awareness of victimization experienced by one's nearest four neighbors also increased residential mobility (Xie and McDowall 2008). A study using the American Housing Survey (AHS) found that households were more likely to leave the unit if the neighborhood had more violent crime (Hipp 2011). Higher homicide rates led to population loss ten years later in Chicago neighborhoods (Morenoff and Sampson 1997), and higher levels of crime led to more residential instability in high crime Los Angeles neighborhoods in the 1970s (Schuerman and Kobrin 1986). Research across 13 U.S. cities found that neighborhoods with higher levels of crime had higher levels of residential instability and vacancies ten years later (Hipp 2010). In neighborhoods in Utrecht, Netherlands, higher levels of burglary resulted in greater residential instability two years later (Hipp and Steenbeek 2016), and crime led to greater home sales (as a measure of residential instability) the following year in Los Angeles in the 1990s (Hipp, Tita, and Greenbaum 2009).

Neighborhood crime also has the ability to transform the composition of neighborhoods to the extent that there is disproportionate ability to exit high crime neighborhoods. The earliest social disorganization writings described residents moving to low crime neighborhoods based on their level of income, implying that crime drives change in neighborhood economic resources (Shaw and McKay 1942), and this idea was extended in the work of Skogan (1990) regarding disorder and decline. Indeed, a study of Dutch neighborhoods found that most neighborhood change that occurs is through selective mobility into higher crime neighborhoods (Van Wilsem, Wittebrood, and De Graaf 2006). A study of neighborhoods across 12 U.S. cities found that higher levels of both violent and property crime resulted in falling home values as well as fewer home purchases (Luo 2025), and research in Baltimore neighborhoods in the 1970s found that increasing violent crime resulted in falling home values (Taylor 1995). Research in Los Angeles neighborhoods in the 1970s found that high crime neighborhoods tended to experience increasing economic disadvantage (Schuerman and Kobrin 1986), and more recent work found that higher levels of crime resulted in lower home values the following year (Hipp, Tita, and Greenbaum 2009). Higher levels of violent crime resulted in subsequent higher concentrated disadvantage in 13 U.S. cities (Hipp 2010), as well as in Utrecht neighborhoods (Hipp and Steenbeek 2016) and in Brisbane neighborhoods (Hipp and Wickes 2017). Likewise, higher levels of property and drug crime resulted in subsequently more disadvantage (Wickes and Hipp 2018).

Crime levels can also change the racial composition over time. One U.S. study found that victimization among the nearest four households increased in-mobility by black households

(Xie and McDowall 2010), and greater perceptions of crime in a micro-neighborhood were associated with more white households leaving and more Latinos and African Americans entering the micro-neighborhood (Hipp 2010). Other U.S. research found that high violent crime neighborhoods experienced greater out-mobility and less in-mobility by white residents, but greater in-mobility by racial minorities (Hipp 2011). A study of neighborhoods in 13 U.S. cities found that higher violent crime led to a higher concentration of Black residents ten years later (Hipp 2010), whereas a study in Utrecht found that higher violent crime resulted in more ethnic minorities in the neighborhood at the next time point (Hipp and Steenbeek 2016).

Higher levels of crime might drive neighborhood transition from owners to renters based on the EVLN model (Hipp 2023). Whereas owners and renters likely both desire leaving high crime neighborhoods—to the extent that they have the economic means—they may differentially be replaced. In the case of renters, if the household leaves and is not replaced it will result in an increase in vacancies. Thus, crime could increase the vacancy rate, in addition to theories positing that the vacancy rate can increase crime. Likewise, this implies that crime would reduce the population. In the case of owners, they may not be able to leave if they cannot find a household to purchase the unit. They may instead choose to leave and rent out the existing one, which implies that higher crime levels can reduce the ownership rate in a neighborhood.

Summary

This review makes clear that there are many reasons to expect that crime can drive the socio-demographic change in neighborhoods. The implication is that these measures will be endogenous to crime in standard cross-sectional models. It is likely that measures such as voluntary organizations, collective efficacy, or neighborhood engagement are particularly vulnerable to such feedback effects from crime. Other measures, such as businesses or the racial composition are also likely impacted by crime, and thus there may be a need to account for this endogeneity. I explore the extent of this problem with instrumental variable models, as I detail in the next section.

Data and methods

Data

The study site for the analyses is the Southern California region. Various datasets were collected and combined, including a subset of the Southern California Crime Study (SCCS) (Kubrin and Hipp 2016), the U.S. Census and other data sources. Data were aggregated to ¼ mile egohoods, given prior evidence that these are a neighborhood unit that generally does an even better job of capturing the relationship between the environment and crime (Hipp and Boessen 2013).[2] An egohood is defined as a block at the center, and all blocks within a particular radius (1/4 mile in this case). This results in overlapping units that arguably better capture the activity patterns of residents. Furthermore, they are inherently spatial, and therefore obviate the need to account for spatial effects, given that they are designed to capture the context of interest.[3] Given the need for crime data from around 2000 and around 2010, I was limited to cities for which such crime data were available from the SCCS. Therefore, the sample cities include the following: one city of 3.8 million residents (Los Angeles); 2 cities with over 300,000 population (Santa Ana, Riverside); 5 cities with between 100,000 and 200,000 population (Rancho Cucamonga, Corona, Victorville, Downey, Burbank); 13 cities with between 50-

[2] I also tested models using ½ mile egohoods, and the results were similar. In general, ¼ mile egohoods are approximately the size of block groups, whereas ½ mile egohoods are approximately the size of census tracts. Given that the results were modestly stronger using the ¼ mile egohoods, these are the ones presented here.
[3] The overlapping nature of egohoods indicates that a spatial error model might be needed for the standard errors. However, it is indeterminant whether the standard errors would be larger or smaller in such a model, and Hipp and Boessen (2013) found that the standard errors were extremely similar when estimating the egohoods as a spatial error model.

100,000 population; 10 cities with between 25-50,000 population; and 14 cities with between 10-25,000 population. There are smaller cities, and portions of additional even smaller cities given that the data include three County Sheriffs (Orange; Riverside; San Bernardino), who often cover smaller jurisdictions; nonetheless, all are part of the Southern California region, and thus none are rural environments. This is a quite large sample, with a total of over 6.7 million residents living in the study area. All variables are constructed at two time points: 2010 for the models, and 2000 for the instrumental variables.

Dependent variables

The crime data for these cities were collected from each police agency as address-level incident crime data. The data come from crime reports officially coded and reported by the police departments.[4] The crime events are classified into six Uniform Crime Report (UCR) categories: homicide, aggravated assault, robbery, burglary, motor vehicle theft, and larceny. Crime events were geocoded for each city separately to latitude–longitude point locations using ArcGIS 10.2, and subsequently aggregated to egohoods, with an average geocoding match rate of about 95% overall. For the first time point, a three-year average was computed from 2000-2002, given that is the first year of most of the crime data. At the second time point, a three-year average was computed from 2009-2011.

Independent variables

Whereas Weisburd and colleagues (Weisburd, Groff, and Yang 2012) proposed using a measure of general political involvement as a proxy for collective efficacy, I instead argue that it should be conceptualized as an imperfect measure of neighborhood engagement and therefore

[4] As part of the data cleaning, it was also checked and confirmed that the crime cleaning resulted in crime totals for the cities that were generally within 5% of the reported totals by the city to the FBI for the UCR data.

construct a measure of the *percent who voted*. This variable was constructed based on the 2000 presidential election for time 1, and the 2008 election for time 2.[5] A measure of the number of *social service voluntary organizations* was constructed based on data from the National Center for Charitable Statistics (NCCS). I followed Wo and colleagues (Wo, Hipp, and Boessen 2016) and combined social service organizations based on the following dimensions into a single count variable: vocational; youth development; recreational; mental health; crime prevention; community association; civil advocacy.

Also constructed was a set of "opportunity" variables. To capture the presence of employees and patrons of businesses the Reference USA Historical Business data were used (Infogroup 2015), which provides point-level information. After geocoding these businesses, they were placed into the appropriate egohoods. Several business measures were constructed. To capture locations with workers who attract the public as customers, a measure of *consumer-facing employees (log transformed)* was constructed (these represent not only the workers, but proxy for the presence of customers as well). These are businesses that primarily serve the public as customers, and contains 31 classes of consumer-facing businesses (for a complete description of the classification codes, see Kane, Hipp, and Kim 2017).[6] The remaining non-consumer employees were split into *white collar employees* (e.g., office-based) and *blue collar employees* (e.g., factories and light industry) based on the 2-digit NAICS code (both log

[5] These data come from the Statewide Database at the University of California, Berkeley (https://statewidedatabase.org/election.html). One view is that non-presidential elections better capture neighborhood cohesion; however, tests showed that the 2008 and 2010 measures were correlated .95, so they appear quite similar.

[6] These classes are the following: Apparel Retailing; Auto Services; Beer, Wine, and Liquor Stores; Child Care Services; Convenience Stores; Deposit-taking Institutions; Drinking Places (Alcoholic Beverages); Drug Stores; Elementary and Secondary Schools; Full-Service Restaurants; Gas Stations; General Merchandise Retailing; Groceries; Hair Care Services; Healthcare Provider Offices; Home Products Retailing; Hospitals; Laundry; Limited-Service Food and Beverage; Medical Laboratories; Movie Theaters; Other Learning; Other Personal Services; Personal Financial; Personal Products Retailing; Recreational Facilities and Instruction; Religious Organizations; Repair Services; Social Service Organizations; Specialty Food; Specialty Retailing

transformed).[7] Following the work of Hipp and Hodgen (2024) who measured business heterogeneity in micro units, a measure of *business heterogeneity* was constructed as a Herfindahl Index of the employees of these 31 business types; this is computed as 1 minus a sum of squares of the proportions of employees in each of these categories.

Some Census variables are not aggregated to units smaller than block groups or tracts, and therefore the synthetic estimation of ecological inference strategy was used to impute values to blocks, before aggregating to egohoods.[8] To capture the economic resources of the neighborhood, a measure of *average household income* was computed, and *income inequality* was measured based on the standard deviation of logged household income.[9] The racial/ethnic composition is captured with measures of *percent Black* and *percent Latino*. *Racial/ethnic heterogeneity* is measured with a Herfindahl index of five groups (percent white, black, Asian, Latino, and other race). Given that neighborhoods with more immigrants tend to have less crime, a *percent immigrants* variable was created.

A measure of the *percent homeowners* was computed, given that homeownership is associated with greater collective efficacy (Sampson, Raudenbush, and Earls 1997; Wickes et al.

[7] I classified as white collar firms those with the following 2-digit NAICS codes (unless they were already were classified as consumer-facing): 51, 52, 53, 54, 55, 56, 92, 99, and the following as blue-collar firms: 11, 21, 22, 23, 31, 32, 33, 42, 48, 49.

[8] The synthetic estimation for ecological inference is described by Boessen and Hipp (2015), and imputes values to blocks based on the relationships between variables at the higher geographic unit of analysis (Cohen and Zhang 1988; Steinberg 1979). Variables used in the imputation model were percent owners, racial composition, percent divorced households, percent households with children, percent vacant units, population density, and age structure (percent aged: 0-4, 5-14, 15-19, 20-24, 25-29, 30-44, 45-64, 65 and up, with age 15-19 as the reference category).

[9] For both the average income and income inequality measures, the income distribution by race data in the block group were used, and then these income distributions were imputed to the blocks in the block group based on the racial composition of the block. Thus, the assumption is that the income distribution by racial group is the same in the block as in the block group. This gives imputed income distribution data for the egohood when the blocks are aggregated to an egohood. Then, given that the Census only reports household incomes in particular ranges, household income is assigned to the midpoint of each reported range. These midpoint values are logged, multiplied by the number of observations in each bin to get the incomes of these households, and then the mean logged income is computed (this is average logged income). Then the standard deviation of the incomes is computed based on these values for the inequality measure.

2013). The crime generating possibilities of vacant housing were captured with the *percent vacant units*. To capture the crime-prone population (Sampson and Laub 1993) I followed prior scholars and measured the *percent aged 16 to 29* (Kubrin and Hipp 2016). To capture the general presence of persons in the environment, a measure of the *population (logged)* was created. A measure of population within 5 miles of the center of the egohood was created given the evidence that the population over a broader area can impact crime in neighborhoods (Hipp and Kubrin 2017; Hipp, Kim, and Wo 2021; Hipp, Wo, and Kim 2017). This also measures the population density of the area around an egohood. The summary statistics for the variables used in the analyses are presented in Table 1.

<<<Table 1 about here>>>

Methods

To account for the potential endogeneity of several of the measures, instrumental variable (IV) models were estimated. This requires identifying instrumental variables. For each of the measures a variable is needed that would predict the variable in question, but not crime. Arguably, a suitable instrument is sometimes that same variable itself earlier in time. Therefore, for example, average household income in 2000 is used to predict average household income in 2010, under the assumption that there would indeed be a strong causal relationship (there is), but average household income in 2000 would have no effect on crime in 2010 after accounting for average household income in 2010 (which would impact crime in 2010). Thus, the argument is that each of the measures at the earlier timepoint serves as a suitable instrument. The logic for a 10-year lag comes from a study of U.S. cities over a 40 year period used an instrumental variable strategy and not only found that higher levels of violent crime—particularly robbery—resulted in stronger increases in racial minorities over time, but that a lag of about 10-15 years appeared optimal for capturing this effect (Liska and Bellair 1995).

Given that the model is exactly identified, I cannot test for the suitability of the exclusion assumptions of these instruments. Therefore, the obtained results need to be treated with caution. Nonetheless, the argument is that the instruments are theoretically plausible, and this strategy follows that of prior research using a temporally lagged measure as an instrument (MacDonald, Hipp, and Gill 2013). In that study, the logic was that the consequences of chain immigration meant that neighborhoods with more immigrants at one time point would very likely cause the level of immigrants at a later time point. In the current study, one reason for the theoretical justification of some of the measures is the relative stasis of the built environment. For example, businesses tend to be located in similar locations over time simply because of the built environment, and therefore we would expect that businesses at a prior time point would cause the level of similar businesses at the current time point. A similar logic would generally hold for service organizations, which are housed in buildings, as well as the presence of homeowners, neighborhood population, and the average income level of neighborhoods given the long-term nature of the housing stock. We would expect the earlier values of these measures to be causally related to their values at the current time point. Likewise, the voluminous literature on racial segregation in the U.S. implies that racial patterns are quite stable over time, and therefore these measures serve as suitable instruments.

There is also other research using lagged instrumental variables. A study using the British Crime Survey tested for feedback effects by using lagged variables as instruments (Markowitz et al. 2001). Likewise, a study of Chicago neighborhoods used lagged crime levels as instrumental variables in estimating the impact of crime on collective efficacy (Sampson and

Raudenbush 1999). A study of Seattle neighborhoods used change in a measure at a previous time point as an instrumental variable for the current timepoint (Velez, Lyons, and Boursaw 2017). An econometric study of Dutch manufacturing used a lagged version of the outcome variable to predict a change variable (van Ours and Stoeldraijer 2011), a study of the impact of export-oriented entrepreneurship on regional economic growth used a lagged change variable as an instrumental variable (González-Pernía and Peña-Legazkue 2015), and a study of the impact of digital infrastructure on the productivity of Tibet's cultural industry used lagged versions of a variable as instruments (Li et al. 2025).

Note that there is debate about the appropriateness of lagged variables as instruments (Greene 2000). The exclusion requirement asserts that whereas an instrumental variable causes the current value of itself, it does not impact the level of crime currently (other than through the current value of the variable). I have argued here that this is a plausible assumption for these instrumental variables. Whereas some might be concerned that a variable such as the business composition at an earlier time point would impact the average income of a neighborhood currently, this is not a problem as long as there is a measure of current average income in the model. However, what is a problem is if the instrumental variable causally affects an omitted variable from the model, which would then impact crime. This is a difficult assumption to meet. Of course, the issue of omitted variables is a general concern as well for the existing body of cross-sectional studies. Furthermore, the obtained instrumental variable (IV) results here comport with theoretical expectations, indicating plausibility.

Given that the outcome variables are crime counts with overdispersion, Poisson or negative binomial regression models are estimated for the second stage equation. Therefore, in the first stage equation each of the independent variables is regressed on all of the instrumental variables—that is, all of the variables in 2000. The equation for this linear regression is:

(3) $$Z_{i\,2010} = \alpha + \Phi_1 Z_{2000} + \varepsilon$$

where Z_i is the particular variable in 2010 (e.g., average household income), Z is a vector of all the instrumental variables in 2000, Φ is the set of coefficients, and ε is a normally distributed error term. From this equation the residual value is obtained. This same equation is estimated for each of the independent variables in the model. The results from the first stage equations show that these are indeed strong instruments: that is, they each independently strongly predict the outcome itself ten years later (beyond the effect of the other instruments, and therefore exhibit a strong incremental R-square).[10]

In the second stage count model the residual estimated from the first stage equation for each variable is also included, which accounts for the endogeneity (Terza, Basu, and Rathouz 2008). There was no evidence of overdispersion in the homicide models, so these are estimated as Poisson models; the remainder were estimated as negative binomial regression models as follows:

(4) $$E(Y_{2010}) = \exp(\alpha + Z_{2010}B_1 + Y_{2000}B_2 + \widehat{\varepsilon Z}_{2010}B_3 + \mu)$$

where y_{2010} is the count of crime events in the egohood in 2010, $\mathbf{Z}$ is the vector of independent variables in 2010 with $\mathbf{B_1}$ as a vector of parameters capturing the relationship between these measures and the crime count, Y_{2000} is the count of crime incidents in the egohood in 2000, $\widehat{\boldsymbol{\varepsilon Z}}_{\mathbf{2010}}$ are the residuals from the first stage equations with $\mathbf{B_3}$ as a vector of parameters, and μ is a parameter capturing the overdispersion based on a gamma distribution.

[10] The average first stage r-square was .74, and the average incremental r-square was .23, suggesting considerable power for the instrumental variable. (The incremental r-square is the difference between the r-square of the first stage equation either with, or without, the unique instrumental variable).

Results

In Table 2 the models are presented for the three violent crimes of aggravated assault, robbery and homicide. The first three models in Table 2 present the cross-sectional count model results assuming no endogeneity—these mimic what is typically done in the literature. The last three models in Table 2 present the preferred results for the IV models that attempt to account for endogeneity. The pattern of models is the same in Table 3 for the property crimes. Beginning with the results for service voluntary organizations, we observe a sharp difference in the pattern between the two sets of models. In the non-IV models, it appears that egohoods with more service organizations have higher levels of aggravated assault and robbery. However, in the IV models the interpretation is entirely reversed: now we see evidence that egohoods with more service organizations have *lower* levels of aggravated assault. The story is similar for property crime in Table 3. In the non-IV models there are modest negative relationships between service voluntary organizations and burglary and motor vehicle theft, but not larceny. However, in the IV models the negative relationship with burglary is nearly 5 times as large, the relationship with motor vehicle theft is 33% larger, and there is now a quite strong negative relationship with larceny. Thus, it appears that the feedback effect of crime onto service organizations may be biasing the estimate in the model that does not address this endogeneity, leading to a faulty inference.

<<<Table 2 about here>>>

<<<Table 3 about here>>>

We also see evidence that the negative relationship between the percent who voted and crime is much stronger in the IV models. In the initial models, there appears to be a positive relationship between voting and robbery, a nonsignificant one with homicide, and only a small negative one with aggravated assault. However, in the IV models there is a much stronger negative relationship with all three violent crimes: about 5 and 3 times stronger for aggravated assault and homicide, respectively, and now a negative relationship with robbery. A one standard deviation increase in percent voting is associated with 32% fewer aggravated assaults, 23% fewer robberies and 15% fewer homicides. The pattern is even stronger for property crime (Table 3): whereas egohoods with a higher percentage of voters appear to have higher property crime in the non-IV models, these all flip to strong negative relationships in the IV models. Again, a one standard deviation increase in percent voting is associated with 17-23% fewer property crimes.

There are also strong differences for the two socioeconomic status measures. The generally negative relationship between average income and violent crime becomes much weaker in the IV models, as the size of the coefficients for violent crime decrease 10% for homicide, 60% for aggravated assault, and become nonsignificant for robbery. For property crime it is an even more pronounced change, as the generally weak or negative relationships flip to strong positive relationships in the IV models. These results are consistent with the possibility that higher income households provide more attractive targets for property crime. The positive relationship with larceny is over 8 times larger in the IV model.

Although we might not have expected crime to decrease income inequality that much, the results of these models suggest that this may be the case. There are very large differences between the initial models and the IV models: the positive relationship between inequality and aggravated assault is 10 times larger in the IV model, it is almost 25 times larger in the robbery IV model, and the negative relationship between inequality and homicide in the initial model flips to a strong positive relationship in the IV model. The pattern is the same for property crime, with positive coefficients in the IV models 5 times larger for larceny, 7 times larger for burglary, and 17 times larger for motor vehicle theft. These are extremely large differences, suggesting that it may be important to account for endogeneity when measuring the impact of income inequality on crime.

There is also evidence that the negative relationships for owners and the positive relationships for vacant units with crime weaken in the IV models. Again, this is consistent with the theoretical expectation that higher levels of crime will reduce the number of owners and increase the number of vacant units. The negative relationships between homeowners and violent crime disappear in the IV violent crime models, and actually become positive for aggravated assault. The negative relationship is reduced 70 percent with larceny and 80 percent with motor vehicle theft in the IV models, whereas the negative relationship with burglary flips to a positive one. Likewise, the positive relationship between vacancies and homicides is reduced 15 percent in the IV models, whereas the relationships with aggravated assaults and robberies are reduced 50-60 percent. For the property crimes, the positive relationship between vacancies and motor vehicle thefts is reduced 20 percent and with burglaries 50 percent in the IV models.

Among the business measures, the strongest changes occur for the white-collar employees variable. Although there are few significant results for white-collar employees in the non-IV violent crime models, there is a relatively strong negative relationship between white-collar employees and violent crime in the IV models. The results for white-collar employees and property crime are even more pronounced, as the positive relationships in the initial models flip to strong negative relationships in the IV models. The consumer-facing employees measure has weakened relationships for property crime in the IV models, with coefficients only 35-70 percent as large. The other business measure exhibiting notable changes in the IV models is the consumer business heterogeneity measure. The positive relationship for consumer business heterogeneity is even stronger for the property crimes and robbery in the IV models, as it is over 3 times larger for burglary, over 6 times larger for motor vehicle theft, twice as large for robbery, and whereas the relationship with larceny appears negative in the initial model it flips to a strong positive relationship in the IV model.

The other set of measures that exhibit notable differences in the IV models are the race/ethnicity variables for the property crime models. The sizes of the positive coefficients for percent Black are reduced 20 percent for homicide and 35 to 50 percent for the property crimes in the IV models. In contrast, the positive coefficients for percent Latino are actually increased in the IV property crime models. The story is the same for racial/ethnic heterogeneity, which has positive relationships with property crime that are 65-120 percent larger in the IV models, and 85 to 200 percent larger for robbery and homicide. For percent immigrants, an apparent positive relationship with motor vehicle theft in the initial model flips to a strong negative relationship in the IV model, and the negative relationships for the other two property crimes are much stronger in the IV models. This is consistent with possible endogeneity for immigrants, at least for property crime (MacDonald, Hipp, and Gill 2013).

The patterns for percent aged 16 to 29 are opposite across the violent and the property crimes. There are stronger positive relationships for aggravated assault and robbery in the IV models. In contrast, the apparent positive relationships for the property crimes in the initial models flip and become negative relationships in the IV models. The two population measures do not change much in the IV models.

Discussion

This study has demonstrated the importance of considering the possible endogeneity of neighborhood measures that scholars typically assume impact crime levels in neighborhoods. For several of the measures, the consequences do not appear trivial, but instead the IV models yielded very different results. In several instances, the conclusions for variables were completely reversed in the IV models. The implication is that we may need to be cautious when interpreting the existing body of research. Given that there is strong theoretical justification for such feedback effects and empirical evidence from a growing body of literature using longitudinal data, the sometime dramatic differences noted in this study using an instrumental variable approach indicate that considerable caution in interpreting existing literature seems warranted. The fact that the results in the IV models were often more closely aligned with theoretical expectations was particularly encouraging. I next describe several particularly notable findings.

One key finding was the pattern for service organizations. Despite the theoretical expectation that service organizations should be beneficial to neighborhoods, and therefore result in lower crime rates, the existing evidence is quite mixed (Wo, Hipp, and Boessen 2016). Arguably, this is because of endogeneity in this measure that is typically unaccounted for in most existing cross-sectional research. Indeed, Wo and colleagues (Wo, Hipp, and Boessen 2016) explicitly focused on the timing of the placement of voluntary organizations and found that this yielded more robust evidence of their negative relationship with crime. Likewise, the present study showed that a typical cross-sectional strategy yielded a positive relationship between these service organizations and crime in egohoods. However, the IV models for these organizations completely flipped this result and yielded a strong negative relationship, consistent with theoretical expectations. Results such as these highlight that existing research failing to account for such feedback effects from crime can lead to misleading conclusions, and may explain the existing mixed findings. For example, a cross-sectional study in the Hague failed to find such a negative relationship (Bruinsma et al. 2013). In this case, one might incorrectly conclude that service organizations are not effective in reducing crime in neighborhoods, but the results in the present study highlight that this may simply occur because researchers need to account for this feedback effect.

A second key finding was the robust negative relationship between the measure of neighborhood engagement used here and all types of crime, at least in the IV models. It was notable that the percent voting, as a measure of neighborhood engagement, appears quite strongly impacted by crime. This is not necessarily surprising, as we might expect that whereas crime reduces collective efficacy, as some other studies have noted (Hipp and Steenbeek 2016; Hipp 2016; Hipp and Wickes 2018), it might positively impact the *behavior* of some individuals, as prior research has noted (Hipp and Wickes 2018; Hipp and Steenbeek 2016). Thus, when attempting to account for this potential endogeneity, the present study found a strong and robust negative relationship between percent voting and all types of crime in this large study of several cities. Given that collective efficacy is likely reduced by higher crime, we would expect that IV models would yield smaller negative coefficients for collective efficacy, rather than larger ones, compared to existing studies, though future research will need to account for this endogeneity. Nonetheless, neighborhood engagement, by accounting for actual behavior, might be an even stronger measure to explore more in future research.

A third notable result were the particularly strong positive relationships between income inequality and all types of crime in the IV models. Hipp and Boessen (2013) in their initial paper introducing the concept of egohoods found that income inequality had a particularly strong positive relationship with crime in egohoods, but not in typical census units, and argued that this was an advantage of egohoods as they are not constructed in a manner that attempts to minimize heterogeneity within units (as is the more common strategy). A similar result is found here, except that it was most pronounced in the IV models . Thus, the results here were consistent with a model in which inequality causes more crime, but crime reduces levels of inequality. Notably, the results were also consistent with lower income residents moving into higher crime neighborhoods given the large differences in the results for average income in the IV models. This inflow of lower income residents could result in reduced inequality if they are tipping the neighborhood towards a low income, low inequality, neighborhood (as opposed to a higher income neighborhood experiencing an inflow of lower income residents, which would increase inequality). It is likely *not* safe, however, to presume that such feedback effects from crime to inequality will always be in the same direction in all study settings; nonetheless, it is likely important to account for them in the particular study site. Similarly, the positive relationship between racial/ethnic heterogeneity and property crime was much stronger in the IV models, again highlighting the importance of considering this feedback process from crime onto neighborhood change.

It was also the case that several other measures were impacted by such feedback effects from crime, and thus yielded somewhat different results in the IV models. These effects were generally as expected: given that crime is expected to reduce the presence of owners over time, and increase the number of vacancies, both of these measures showed effects pushed towards zero in the models attempting to account for this endogeneity (Hipp 2010). And given that some minorities and immigrants may have reduced choices for neighborhoods to move into (Xie and McDowall 2010), the relationships between African Americans or immigrants with property crime were reduced in the IV models. Although the changes in the size of these effects were not as dramatic as for some of the other measures, they nonetheless highlight that getting proper estimates of the size of effects likely requires considering endogeneity.

Some limitations of this study should be acknowledged. The study was limited to a subset of cities, so caution should be exercised in how much the results would generalize to other cities. A second limitation is that it was not possible to statistically test the exogeneity assumptions of the instrumental variables given that the models were exactly identified. Therefore, although the instruments were plausible theoretically, we must rely on this plausibility when assessing the results. Third, a question is whether the feedback effects occur more slowly than the effects of these measures on crime. A possible slower effect would imply smaller coefficients for these feedback effects in the short-term. However, 1) the relatively strong feedback effects in longitudinal studies make this unlikely, and 2) even if there were *no* feedback effects, the instrumental variable model results would be asymptotically the same as the cross-sectional model results, which clearly was not the case. Another limitation is the reliance on crime data as reported by the police. There is always a concern that not all crime is reported; nonetheless, evidence that such reporting for the more serious crimes studied here is not systematically related to the socio-demographic features of neighborhoods is reassuring (Baumer 2002).

Additionally, whereas I have drawn a distinction between the standard cross-sectional models and the IV models, and highlighted the differences, caution is needed as there are other possible reasons for differences. For example, omitted variables can cause differences across the two estimation strategies. I have attempted to include the variables typically included in such cross-sectional studies of the ecology of crime, but this is a general issue that all such studies should consider. Likewise, measurement error can cause differences in the results across estimation strategies. Unfortunately, measurement issues are not given as much consideration in the general ecology of crime literature as one might hope, so this is a general issue for consideration across many studies. Nonetheless, it should be kept in mind.

Despite these limitations, an important takeaway point from this study is the need to account for the feedback effects from crime onto neighborhood features. Although these feedback effects are well theorized, and there is a growing body of evidence of these effects from longitudinal studies, the present study highlighted that there may be dramatic consequences from ignoring these feedback effects. Some of the conclusions were completely reversed when attempting to account for this endogeneity in instrumental variable cross-sectional models. This is notable, as there are a large number of studies that estimate cross-sectional models, but then note as a possible limitation at the end of the paper that there may be endogeneity that is being ignored. The implicit assumption of these studies is that this possible endogeneity is not very substantial, and therefore the results would not be greatly impacted. However, as highlighted in the present study, this is not necessarily the case. The results were completely reversed for several of the measures in the IV models. Whether this will be the case for other study sites is not certain, but nonetheless this should to be a source of considerable concern. The implication is that some of the existing studies that provide the empirical evidence that criminologists rely on may have results that will not hold up once accounting for the almost certain endogeneity that exists.

References
Bali, Valentina A., Lindon J. Robison, and Richard Winder. 2020. What Motivates People to Vote? The Role of Selfishness, Duty, and Social Motives When Voting. *Sage Open* 10 (4).
Baumer, Eric P. 2002. Neighborhood Disadvantage and Police Notification by Victims of Violence. *Criminology* 40 (3):579-616.
Bellair, Paul E. 2000. Informal Surveillance and Street Crime: A Complex Relationship. *Criminology* 38 (1):137-167.
Bernasco, Wim, and Richard L. Block. 2011. Robberies in Chicago: A Block-Level Analysis of the Influence of Crime Generators, Crime Attractors, and Offender Anchor Points. *Journal of Research in Crime and Delinquency* 48 (1):33-57.
Braakmann, Nils. 2023. Residential turnover and crime—Evidence from administrative data for England and Wales. *The British Journal of Criminology* 63 (6):1460-1481.
Brantingham, Patricia L., and Paul J. Brantingham. 2008. Crime Pattern Theory. In *Environmental Criminology and Crime Analysis*, edited by R. Wortley and L. Mazerolle. Portland, OR: Willan Publishing.
Bruinsma, G. J. N., L. J. R. Pauwels, F. M. Weerman, and W. Bernasco. 2013. Social Disorganization, Social Capital, Collective Efficacy and the Spatial Distribution of Crime and Offenders: An Empirical Test of Six Neighbourhood Models for a Dutch City. *British Journal of Criminology*.
Brunton-Smith, Ian, Jonathan Jackson, and Alex Sutherland. 2014. Bridging Structure and Perception. *British Journal of Criminology* 54 (4):503-526.
Cohen, Michael Lee, and Xiao Di Zhang. 1988. The Difficulty of Improving Statistical Synthetic Estimation. In *Statistical Research Division Report Series*. Washington, D.C.: Bureau of the Census.
Dugan, Laura. 1999. The Effect of Criminal Victimization on a Household's Moving Decision. *Criminology* 37 (4):903-930.
Felson, Marcus, and Rachel Boba. 2010. *Crime and Everyday Life*. Fourth Edition ed. Thousand Oaks, CA: Sage.
Gearhart, Michael C. 2020. Social cohesion, internal efficacy, and external efficacy: Studying voting behavior using collective efficacy theory. *Community Development* 51 (5):593-608.
González-Pernía, José L., and Iñaki Peña-Legazkue. 2015. Export-oriented entrepreneurship and regional economic growth. *Small Business Economics* 45 (3):505-522.
Greenbaum, Robert T., and George E. Tita. 2004. The Impact of Violence Surges on Neighbourhood Business Activity. *Urban Studies* 41 (13):2495-2514.
Greene, William H. 2000. *Econometric Analysis*. 4 ed. Upper Saddle River, NJ: Prentice Hall.
Hipp, John R. 2010. A dynamic view of neighborhoods: The reciprocal relationship between crime and neighborhood structural characteristics. *Social Problems* 57 (2):205-230.
———. 2010. The Role of Crime in Housing Unit Racial/ethnic Transition. *Criminology* 48 (3):683-723.
———. 2011. Violent crime, mobility decisions, and neighborhood racial/ethnic transition. *Social Problems* 58 (3):410-432.
———. 2016. Collective Efficacy: How is it Conceptualized, How is it Measured, and Does it Really Matter for Understanding Perceived Neighborhood Crime and Disorder? *Journal of Criminal Justice* 46 (1):32-44.

———. 2023. *The Spatial Scale of Crime: How Physical and Social Distance Drive the Spatial Location of Crime*. New York: Routledge.
Hipp, John R., and Adam Boessen. 2013. Egohoods as waves washing across the city: A new measure of "neighborhoods". *Criminology* 51 (2):287-327.
Hipp, John R., and Cheyenne Hodgen. 2024. The ecology of business environments and consequences for crime. *Criminology* 62 (4):859-891.
Hipp, John R., Young-an Kim, and James Wo. 2021. Micro-scale, meso-scale, macro-scale, and temporal scale: Comparing the relative importance for robbery risk in New York City. *Justice Quarterly* 38 (5):767-791.
Hipp, John R., and Charis E. Kubrin. 2017. From Bad to Worse: How Changing Inequality in Nearby Areas Impacts Local Crime. *Russell Sage Foundation Journal* 3 (2):129-151.
Hipp, John R., and Wouter Steenbeek. 2016. Types of crime and types of mechanisms: What are the consequences for neighborhoods over time? *Crime & Delinquency* 62 (9):1203-1234.
Hipp, John R., George E. Tita, and Robert T. Greenbaum. 2009. Drive-bys and Trade-ups: The Impact of Crime on Residential Mobility Patterns in Los Angeles. *Social Forces* 87 (4):1777-1812.
Hipp, John R., and Rebecca Wickes. 2017. Violence in Urban Neighborhoods: A Longitudinal Study of Collective Efficacy and Violent Crime. *Journal of Quantitative Criminology* 33 (4):783-808.
———. 2018. Problems, Perceptions and Actions: An Interdependent Process for Generating Informal Social Control *Social Science Research* 73 (1):107-125.
Hipp, John R., Seth A. Williams, Young-an Kim, and Jae Hong Kim. 2019. Fight or Flight? Crime as a Driving Force in Business Failure and Business Mobility. *Social Science Research* 82:164-180.
Hipp, John R., James Wo, and Young-an Kim. 2017. Studying Neighborhood Crime Across Different Macro Spatial Scales: The Case of Robbery in Four Cities. *Social Science Research* 68 (1):15-29.
Infogroup. 2015. Reference USA Historical Business Data, edited by Infogroup. Papillion, NE: Reference USA.
Jankowski, Richard. 2007. Altruism and the Decision to Vote. *Rationality and Society* 19 (1):5-34.
Kane, Kevin, John R. Hipp, and Jae Hong Kim. 2017. Analyzing Accessibility using Parcel Data: Is there Still an Access-Space Tradeoff in Long Beach, California? *The Professional Geographer* 69 (3):486-503.
Kubrin, Charis E., and John R. Hipp. 2016. Do Fringe Banks Create Fringe Neighborhoods? Examining the Spatial Relationship between Fringe Banking and Neighborhood Crime Rates. *Justice Quarterly* 33 (5):755-784.
Lens, Michael C., and Rachel Meltzer. 2016. Is Crime Bad for Business? Crime and Commercial Property Values in New York City. *Journal of Regional Science* 56 (3):442-470.
Li, Yuanyuan, Qianqian Du, Guihua Ma, and Caidan Gazang. 2025. Can strengthening digital infrastructure enhance productivity in the cultural industry? evidence from Tibet. *PLoS One* 20 (1):e0317366.
Liska, Allen E., and Paul E. Bellair. 1995. Violent-Crime Rates and Racial Composition: Convergence Over Time. *American Journal of Sociology* 101 (3):578-610.
Luo, Xiaoshuang Iris. 2025. Neighbourhood Housing Dynamics and Crime Interactions: A Multicity Analysis in the United States. *The British Journal of Criminology* Online.

Lyons, William E., and David Lowery. 1989. Citizen Responses to Dissatisfaction in Urban Communities: A Partial Test of a General Model. *The Journal of Politics* 51 (4):841-868.
MacDonald, John M., John R. Hipp, and Charlotte Gill. 2013. The Effects of Immigrant Concentration on Changes in Neighborhood Crime Rates. *Journal of Quantitative Criminology* 29 (2):191-215.
Markowitz, Fred E., Paul E. Bellair, Allen E. Liska, and Jianhong Liu. 2001. Extending Social Disorganization Theory: Modeling the Relationships Between Cohesion, Disorder, and Fear. *Criminology* 39 (2):293-319.
Morenoff, Jeffrey D., and Robert J. Sampson. 1997. Violent Crime and The Spatial Dynamics of Neighborhood Transition: Chicago, 1970-1990. *Social Forces* 76 (1):31-64.
Sampson, Robert J. 2006. Collective Efficacy Theory: Lessons Learned and Directions for Future Inquiry. In *Taking Stock: The Status of Criminological Theory*, edited by F. T. Cullen, J. P. Wright and K. R. Blevins. New Brunswick: Transaction.
Sampson, Robert J., and John H. Laub. 1993. *Crime in the Making: Pathways and Turning Points Through Life*. Cambridge: Harvard University Press.
Sampson, Robert J., and Stephen W. Raudenbush. 1999. Systematic Social Observation of Public Spaces: A New Look at Disorder in Urban Neighborhoods. *American Journal of Sociology* 105 (3):603-651.
Sampson, Robert J., Stephen W. Raudenbush, and Felton Earls. 1997. Neighborhoods and Violent Crime: A Multilevel Study of Collective Efficacy. *Science* 277 (August 15):918-924.
Schuerman, Leo, and Solomon Kobrin. 1986. Community Careers in Crime. *Crime and Justice* 8:67-100.
Shaw, Clifford, and Henry D. McKay. 1942. *Juvenile Delinquency and Urban Areas*. Chicago: University of Chicago Press.
Skogan, Wesley G. 1990. *Disorder and Decline: Crime and the Spiral of Decay in American Neighborhoods*. New York: Free Press.
Steenbeek, Wouter, and John R. Hipp. 2011. A Longitudinal Test of Social Disorganization Theory: Feedback Effects among Cohesion, Social Control and Disorder. *Criminology* 49 (3):833-871.
Steinberg, Joseph, ed. 1979. *Synthetic Estimates for Small Areas: Statistical Workshop Papers and Discussion*. Vol. 24, *National Institute on Drug Abuse Research Monograph Series*. Washington, D.C.: National Institute on Drug Abuse.
Taylor, Ralph B. 1995. The Impact of Crime on Communities. *The Annals of the American Academy of Political and Social Science* 539:28-45.
Terza, Joseph V., Anirban Basu, and Paul J. Rathouz. 2008. Two-stage residual inclusion estimation: addressing endogeneity in health econometric modeling. *Journal of Health Economics* 27 (3):531-43.
Tita, George E., Tricia L. Petras, and Robert T. Greenbaum. 2006. Crime and Residential Choice: A Neighborhood Level Analysis of the Impact of Crime on Housing Prices. *Journal of Quantitative Criminology* 22 (4):299-317.
van Ours, Jan C., and Lenny Stoeldraijer. 2011. Age, Wage and Productivity in Dutch Manufacturing. *De Economist* 159 (2):113-137.
Van Wilsem, Johan, Karin Wittebrood, and Nan Dirk De Graaf. 2006. Socioeconomic Dynamics of Neighborhoods and the Risk of Crime Victimization: A Multilevel Study of

Improving, Declining, and Stable Areas in the Netherlands. *Social Problems* 53 (2):226-247.

Velez, Maria B., Christopher J. Lyons, and Blake Boursaw. 2017. Neighborhood Housing Investments and Violent Crime in Seattle, 1981-2007. *Criminology* 50 (4):1025-1056.

Weisburd, David, Elizabeth Groff, and Sue-Ming Yang. 2012. *The Criminology of Place*. New York: Oxford.

Wickes, Rebecca, and John R. Hipp. 2018. The spatial and temporal dynamics of neighborhood informal social control and crime. *Social Forces* 97 (1):277-308.

Wickes, Rebecca, John R. Hipp, Elise Sargeant, and Ross Homel. 2013. Collective efficacy as a task specific process: Examining the relationship between social ties, neighborhood cohesion and the capacity to respond to violence, delinquency and civic problems. *American Journal of Community Psychology* 52 (1-2):115-127.

Wo, James C., John R. Hipp, and Adam Boessen. 2016. Voluntary Organizations and Neighborhood Crime: A Dynamic Perspective. *Criminology* 54 (2):212-241.

Xie, Min, and David McDowall. 2008. Escaping Crime: The Effects of Direct and Indirect Victimization on Moving. *Criminology* 46 (4):809-840.

———. 2010. The Reproduction of Racial Inequality: How Crime Affects Housing Turnover. *Criminology* 48 (3):865-896.

Yu, Sung-suk Violet, and Michael G. Maxfield. 2014. Ordinary Business: Impacts on Commercial and Residential Burglary. *British Journal of Criminology* 54:298-320.

Tables and Figures

Table 1. Summary statistics of variables used in analyses

	2010			2000	
1/4 mile egohoods	Mean	S.D.		Mean	S.D.
Dependent variables					
Aggravated assault	3.52	6.27		9.59	17.93
Robbery	3.61	7.38		4.99	10.46
Homicide	0.11	0.30		0.20	0.48
Burglary	7.19	7.47		8.72	10.04
Motor vehicle theft	6.09	7.66		9.75	12.64
Larceny	20.03	28.22		24.87	37.17
Independent variables					
Service voluntary organizations (logged)	0.24	0.41		0.19	0.36
Percent who voted	37.8	19.1		39.0	18.8
Consumer employees (logged)	3.28	2.17		3.05	2.20
Blue collar employees (logged)	2.91	1.79		2.81	1.95
White collar employees (logged)	3.39	1.92		3.04	2.03
Consumer business heterogeneity	0.57	0.31		0.52	0.33
Average household income	3.96	0.42		3.71	0.48
Income inequality	0.88	0.11		0.86	0.12
Percent Black	6.2	11.1		6.8	13.0
Percent Latino	39.1	27.5		33.5	27.1
Racial/ethnic heterogeneity	0.47	0.16		0.44	0.17
Percent immigrants	28.6	14.7		26.5	17.0
Percent owners	56.5	27.2		58.5	28.5
Percent vacant units	8.5	10.4		7.0	10.7
Percent aged 16 to 29	21.6	7.6		20.5	7.9
Population (logged)	6.94	1.19		6.88	1.23
Population within 5 miles (logged)	12.54	1.29		12.26	1.45
Note: N = 66,226 egohoods					

Table 2. Violent crime models. Non-instrumental variable and Instrumental variable count models

	Non-instrumental variable count models						Instrumental variable count models					
	Aggravated assault		Robbery		Homicide		Aggravated assault		Robbery		Homicide	
Service voluntary organizations (logged)	0.0543	**	0.0776	**	0.0115		-0.0668	**	-0.0181	†	0.0301	
	(6.91)		(9.74)		(0.42)		-(6.02)		-(1.75)		(0.73)	
Percent who voted	-0.0033	**	0.0013	**	-0.0024	†	-0.0175	**	-0.0117	**	-0.0080	†
	-(10.79)		(3.97)		-(1.78)		-(20.50)		-(12.75)		-(1.95)	
Consumer employees (logged)	0.1625	**	0.3095	**	0.0847	**	0.1646	**	0.2792	**	0.0621	**
	(49.95)		(86.52)		(6.45)		(31.94)		(52.21)		(2.97)	
Blue collar employees (logged)	0.0029		-0.0594	**	0.0388	**	-0.0019		-0.0999	**	0.0669	**
	(0.99)		-(19.18)		(3.67)		-(0.41)		-(21.34)		(3.64)	
White collar employees (logged)	-0.0018		0.0006		-0.0486	**	-0.0200	**	-0.0550	**	-0.0712	**
	-(0.53)		(0.17)		-(3.78)		-(3.54)		-(9.90)		-(3.21)	
Consumer business heterogeneity	-0.0287		0.2539	**	0.1556	†	-0.0089		0.5728	**	0.2829	†
	-(1.46)		(11.05)		(1.91)		-(0.24)		(14.34)		(1.81)	
Average household income	-0.4702	**	-0.5119	**	-0.7752	**	-0.1905	**	-0.0230		-0.7071	**
	-(31.52)		-(31.39)		-(13.36)		-(7.24)		-(0.86)		-(6.34)	
Income inequality	0.2431	**	0.1632	**	-0.2082		2.4527	**	4.0244	**	2.4784	**
	(6.31)		(3.96)		-(1.34)		(16.23)		(25.89)		(3.80)	
Percent Black	0.0270	**	0.0228	**	0.0216	**	0.0256	**	0.0202	**	0.0169	**
	(84.00)		(66.46)		(18.20)		(56.89)		(44.63)		(8.82)	
Percent Latino	0.0130	**	0.0129	**	0.0230	**	0.0122	**	0.0161	**	0.0261	**
	(54.88)		(51.38)		(21.73)		(29.99)		(38.91)		(14.15)	
Racial/ethnic heterogeneity	-0.3652	**	0.1181	**	0.2322	*	-0.2177	**	0.3496	**	0.4325	**
	-(15.49)		(4.66)		(2.37)		-(7.30)		(11.49)		(3.26)	
Percent immigrants	-0.0020	**	0.0037	**	-0.0178	**	-0.0027	**	0.0028	**	-0.0212	**
	-(4.85)		(8.41)		-(10.26)		-(3.49)		(3.42)		-(5.74)	
Percent owners	-0.0048	**	-0.0055	**	-0.0022	*	0.0015	**	0.0006		0.0013	
	-(22.24)		-(23.78)		-(2.39)		(4.07)		(1.46)		(0.75)	
Percent vacant units	0.0266	**	0.0222	**	0.0118	**	0.0228	**	0.0104	**	0.0049	
	(49.78)		(30.37)		(3.37)		(26.00)		(8.70)		(0.92)	

Crime Feedback Effects

Percent aged 16 to 29	0.0034	**	-0.0001		-0.0115	**	0.0071	**	0.0109	**	-0.0059	
	(5.91)		-(0.13)		-(4.46)		(6.79)		(10.74)		-(1.20)	
Population (logged)	0.5843	**	0.4273	**	0.6887	**	0.5999	**	0.3444	**	0.6555	**
	(101.10)		(71.78)		(26.60)		(57.03)		(32.29)		(13.16)	
Population within 5 miles (logged)	0.0959	**	0.5019	**	0.3897	**	0.0312	**	0.3988	**	0.2995	**
	(15.82)		(64.48)		(12.59)		(3.15)		(36.28)		(6.37)	
Logged crime 10 years ago							0.0085	**	0.0230	**	0.1824	**
							(38.77)		(69.27)		(10.92)	
Intercept	-4.1195	**	-9.2007	**	-10.3739	**	-6.3221	**	-12.6477	**	-11.7971	**
	-(41.31)		-(73.65)		-(21.44)		-(31.39)		-(58.56)		-(12.67)	
Pseudo R-square	0.279		0.326		0.293		0.291		0.356		0.300	

*** p < .01(two-tail test), * p < .05 (two-tail test), † p < .10 (two-tail test). T-values in parentheses. N=66,226 egohoods.*

Table 3. Property crime models. Non-instrumental variable and Instrumental variable count models

	Non-instrumental variable count models						Instrumental variable count models					
	Burglary		Motor vehicle theft		Larceny		Burglary		Motor vehicle theft		Larceny	
Service voluntary organizations (logged)	-0.0154	*	-0.0164	**	0.0035		-0.0736	**	-0.0218	**	-0.0878	**
	-(2.07)		-(2.62)		(0.46)		-(8.11)		-(2.76)		-(9.02)	
Percent who voted	0.0072	**	0.0030	**	0.0019	**	-0.0125	**	-0.0129	**	-0.0103	**
	(30.67)		(12.97)		(8.54)		-(21.71)		-(20.92)		-(18.27)	
Consumer employees (logged)	0.1121	**	0.1249	**	0.2198	**	0.0326	**	0.0785	**	0.1446	**
	(43.88)		(51.88)		(89.38)		(8.59)		(21.68)		(37.42)	
Blue collar employees (logged)	-0.0006		0.0043	†	-0.0165	**	-0.0140	**	0.0223	**	-0.0302	**
	-(0.24)		(1.89)		-(6.33)		-(3.74)		(6.58)		-(7.78)	
White collar employees (logged)	0.0228	**	-0.0023		0.0826	**	-0.0132	**	-0.0638	**	0.0218	**
	(7.80)		-(0.87)		(27.47)		-(2.92)		-(15.51)		(4.59)	
Consumer business heterogeneity	0.1234	**	0.0283	†	-0.0334	*	0.4559	**	0.1820	**	0.3550	**
	(8.22)		(1.90)		-(2.21)		(17.19)		(6.86)		(13.01)	
Average household income	-0.0034		-0.0672	**	0.0514	**	0.5583	**	0.4649	**	0.4336	**
	-(0.27)		-(5.62)		(4.05)		(29.30)		(25.20)		(22.52)	
Income inequality	0.5638	**	0.2569	**	0.3133	**	4.0033	**	4.3642	**	1.7210	**
	(17.78)		(8.66)		(9.88)		(37.24)		(40.77)		(16.24)	
Percent Black	0.0158	**	0.0117	**	0.0057	**	0.0095	**	0.0075	**	0.0030	**
	(50.89)		(44.68)		(17.53)		(27.58)		(22.81)		(8.13)	
Percent Latino	0.0066	**	0.0144	**	0.0006	**	0.0135	**	0.0237	**	0.0058	**
	(33.50)		(80.41)		(3.22)		(44.52)		(81.00)		(18.94)	
Racial/ethnic heterogeneity	0.4428	**	0.6261	**	0.3784	**	0.9657	**	1.3324	**	0.6244	**
	(22.62)		(34.55)		(19.37)		(42.80)		(61.12)		(26.80)	
Percent immigrants	-0.0039	**	0.0034	**	-0.0007	*	-0.0146	**	-0.0116	**	-0.0131	**
	-(11.59)		(10.46)		-(2.18)		-(25.97)		-(20.10)		-(23.35)	
Percent owners	-0.0021	**	-0.0074	**	-0.0050	**	0.0048	**	-0.0014	**	-0.0014	**
	-(11.91)		-(45.33)		-(28.40)		(18.36)		-(5.36)		-(5.40)	
Percent vacant units	0.0150	**	0.0140	**	0.0103	**	0.0078	**	0.0113	**	0.0108	**
	(35.85)		(28.14)		(25.41)		(12.73)		(15.06)		(17.65)	

Crime Feedback Effects

Percent aged 16 to 29	0.0093	**	0.0003		0.0093	**	-0.0008		-0.0116	**	-0.0026	**
	(19.27)		(0.64)		(19.56)		-(1.05)		-(15.05)		-(3.34)	
Population (logged)	0.4374	**	0.4090	**	0.3417	**	0.4163	**	0.3988	**	0.3292	**
	(99.15)		(97.56)		(89.88)		(57.89)		(53.97)		(49.14)	
Population within 5 miles (logged)	0.0901	**	0.3527	**	0.1910	**	0.0416	**	0.3230	**	0.2238	**
	(19.51)		(69.86)		(42.13)		(5.84)		(43.84)		(31.26)	
Logged crime 10 years ago							0.0403	**	0.0228	**	0.0111	**
							(113.49)		(90.86)		(96.50)	
Intercept	-4.4508	**	-7.4033	**	-3.9982	**	-8.6249	**	-12.3884	**	-6.5188	**
	-(57.09)		-(88.96)		-(52.35)		-(59.53)		-(83.74)		-(45.58)	
Pseudo R-square	0.136		0.255		0.138		0.176		0.286		0.162	

*** p < .01(two-tail test), * p < .05 (two-tail test), † p < .10 (two-tail test). T-values in parentheses. N=66,226 egohoods.*