



# HHS Public Access

Author manuscript

*Rev Econ Stat.* Author manuscript; available in PMC 2021 September 23.

Published in final edited form as:

*Rev Econ Stat.* 2021 March 01; 103(1): 18–33. doi:10.1162/rest\_a\_00860.

## The Effect of Social Connectedness on Crime: Evidence from the Great Migration

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### Abstract

This paper estimates the effect of social connectedness on crime across U.S. cities from 1970 to 2009. Migration networks among African Americans from the South generated variation across destinations in the concentration of migrants from the same birth town. Using this novel source of variation, we find that social connectedness considerably reduces murders, rapes, robberies, assaults, burglaries, and motor vehicle thefts, with a one standard deviation increase in social connectedness reducing murders by 21 percent and motor vehicle thefts by 20 percent. Social connectedness especially reduces murders of adolescents and young adults committed during gang and drug activity.

### Keywords

crime; social connectedness; Great Migration

## 1 Introduction

For almost 200 years, the enormous variance of crime rates across space has intrigued social scientists and policy makers (Guerry, 1833; Quetelet, 1835; Weisburd, Bruinsma and Bernasco, 2009). Prior work finds that standard covariates explain less than one-third of the cross-city variation in crime rates, which suggests a potential role for social influences (Glaeser, Sacerdote and Scheinkman, 1996). One possible explanation is peer effects, whereby an individual is more likely to commit crime if his peers commit crime (e.g., Case and Katz, 1991; Damm and Dustmann, 2014). Another explanation is that cities differ in the degree of social connectedness, or the strength of relationships between individuals, including those unlikely to commit crime.

This paper uses a new source of variation in social connectedness to estimate its effect on crime. Migration networks among millions of African Americans who moved out of the U.S. South from 1915 to 1970 generated variation across destinations in the concentration of migrants from the same birth town. For example, consider Beloit, Wisconsin and Middletown, Ohio, two cities similar along many dimensions, including the total number

of Southern black migrants that moved there. Around 18 percent of Beloit's black migrants came from Pontotoc, Mississippi, while less than five percent of Middletown's migrants came from any single town. Historical accounts trace the sizable migration from Pontotoc to Beloit to a single influential migrant, John McCord, getting a job in 1914 at a manufacturer in search of workers (Bell, 1933). Furthermore, ethnographic and newspaper accounts suggest that Southern birth town networks translated into strong community ties in the North (Stack, 1974; Associated Press, 1983; Laury, 1986; Crowder and Spencer, 2002; Smith, 2006). Guided by a simple economic model, we proxy for social connectedness using a Herfindahl-Hirschman Index of birth town to destination city population flows for African Americans born in the South from 1916 to 1936, who we observe in the Duke SSA/Medicare dataset. We focus on social connectedness among black migrants because birth town migration networks are especially strong among this group (Stuart and Taylor, 2018) and qualitative and quantitative evidence supports our empirical strategy.

We estimate regressions that relate cross-city differences in crime from 1970 to 2009 to cross-city differences in social connectedness. The historical literature suggests that, conditional on economic and social opportunities, variation in social connectedness stems from idiosyncratic factors, like the right migrant being in the right place at the right time. To exploit this variation, we control for population, manufacturing employment (the sector employing the largest number of African American migrants), and the black population share from 1920 to 1960. Our regressions also include the number of Southern black migrants that live in each city, to adjust for differences in the overall attractiveness of cities to black migrants, and contemporaneous population, land area, and state-by-year fixed effects. City-level crime counts come from FBI Uniform Crime Reports.

We find that social connectedness leads to sizable reductions in crime rates. The elasticity of the crime rate with respect to social connectedness ranges from  $-0.07$  to  $-0.25$  across the seven index crimes of murder, rape, robbery, assault, burglary, larceny, and motor vehicle theft, and is statistically distinguishable from zero for every crime besides larceny. At the mean, a one standard deviation increase in social connectedness leads to a precisely estimated 21 percent decrease in murder, the best measured crime in FBI data. Our estimates imply that replacing Middletown's social connectedness with that of Beloit would decrease murders, robberies, and motor vehicle thefts by 28 to 30 percent. By comparison, the estimates in Chalfin and McCrary (2018) imply that a similar decrease in murders would require a 44 percent increase in the number of police officers.

Because social connectedness arises from individuals' location decisions, a natural concern is whether our estimates reflect causal effects. The validity of our empirical strategy hinges upon whether social connectedness is correlated with unobserved determinants of crime from 1970 to 2009, conditional on the covariates described above. Historical accounts emphasize the importance of migrants who were well-connected in their birth town and who worked for an employer in search of labor in establishing concentrated migration flows from Southern birth towns to Northern cities (Scott, 1920; Bell, 1933; Gottlieb, 1987; Grossman, 1989). It is unlikely that these idiosyncratic factors are correlated with unobserved determinants of crime some 50 years later. These considerations provide qualitative support for our empirical strategy.

We marshal a wide range of quantitative support for our empirical strategy. First, 78 percent of the variation in social connectedness stems from a single birth town to destination city migration flow. This accords with historical accounts emphasizing the importance of idiosyncratic factors. Second, social connectedness is not correlated with murder rates from 1911 to 1916 or 1936 to 1939. This implies that connected groups of migrants did not simply move to low crime cities.

To provide even stronger support, we show that our results are robust to selection on both observed and unobserved variables. Our results are similar when including a battery of additional controls: contemporaneous economic and demographic factors, the number and concentration of white migrants and immigrants, and characteristics of counties from which migrants came. Our results also are robust to controlling for the share of migrants drawn to each destination by a birth town migration network. This variable, which we estimate using a structural model of location decisions, controls for a range of unobserved migrant characteristics. Finally, we develop a more general test of selection on unobserved variables. The main threat to identification is that connected groups of migrants moved to cities with low crime rates, and unobserved determinants of crime persisted over time. In the presence of this unobserved selection, controlling for the 1960–1969 crime rate would eliminate the relationship between crime and social connectedness from 1970 to 2009. In contrast, if our empirical strategy is valid, then controlling for the 1960–1969 crime rate would partly attenuate the estimated effect of social connectedness and this attenuation would diminish over time; this is exactly what we find, which rules out the main threat to identification. All of this evidence supports our empirical strategy.

A number of additional results clarify the mechanisms through which social connectedness reduces crime. Social connectedness reduces crimes that are more and less likely to have witnesses, which suggests that an increased probability of detection is not the only operative mechanism. The effect of social connectedness on crime is not driven by effects on employment, education, homeownership, the prevalence of single parents, or crack cocaine use. Other mechanisms, such as effects on non-cognitive skills, personality traits, and norms, likely matter. We see the largest reductions in murders of adolescents and young adults, committed by acquaintances or strangers, in the course of gang, drug, and other felonious activity. Furthermore, the effect of social connectedness on crime is persistent: even in the 2000s, when many of the original Southern migrants were no longer alive, crime rates were lower in cities with higher social connectedness. Natural explanations for this persistence include changing norms or skills, which are passed down across generations, and path dependence in crime (Nagin and Paternoster, 1991).

There is widespread interest in the effects of social connectedness and the related concept of social capital.<sup>1</sup> This interest partly stems from the possibility that relationships between individuals can address market failures and generate desirable outcomes that are difficult to accomplish with government policies. However, estimating the effects of social

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<sup>1</sup>Although definitions of social capital vary, Portes (1998) argues that a consensus definition is “the ability of actors to secure benefits by virtue of membership in social networks or other social structures” (p. 6). In discussing social capital, authors typically emphasize the role of trust and reciprocity (Fukuyama, 1995; Putnam, 2000; Bowles and Gintis, 2002). Social connectedness is a broader concept than social capital, as Karlan (2007) also notes.

connectedness and social capital has proven challenging. Some of the most influential evidence comes from correlations between outcomes, such as income and crime, and proxies for social capital, like individuals' participation in community organizations, their stated willingness to intervene in the community, and their stated willingness to trust others (Sampson, Raudenbush and Earls, 1997; Putnam, 2000). These proxies for social capital reflect individuals' contemporaneous decision to invest in their community, which raises the concern that these correlations reflect reverse causality or omitted variables bias. As a result, the empirical importance of social capital continues to be debated (Durlauf, 2002). This stands in contrast to several papers that credibly identify peer effects in crime.<sup>2</sup>

We use variation in social connectedness that has the unusual and attractive property of being established decades before we measure outcomes as the result of a known process — birth town migration networks. This facilitates our primary contribution, which is providing new, more credible evidence on the effect of social connectedness on crime. We also contribute to the literature in economics studying how social capital and trust relate to various outcomes, including growth and development, government efficiency and public good provision, financial development, microfinance, and intergenerational mobility.

More broadly, there is enormous interest in the causes and consequences of criminal activity and incarceration in U.S. cities, especially for African Americans (Freeman, 1999; Neal and Rick, 2014; Evans, Garthwaite and Moore, 2016), and this paper demonstrates the importance of social connectedness in reducing crime. Our results imply that policies which lower social connectedness, including mass incarceration, could have more negative consequences than commonly understood. We also add to the literature on the consequences of the Great Migration for migrants and cities, which has not considered the effects of social connectedness before (e.g., Scroggs, 1917; Smith and Welch, 1989; Margo, 1990; Carrington, Detragiache and Vishwanath, 1996; Collins, 1997; Boustan, 2009, 2010; Hornbeck and Naidu, 2014; Black et al., 2015). Our work complements research on the effects of immigration on crime (Bell and Machin, 2013). Much of this literature focuses on how crime depends on the number of immigrants and the types of countries from which immigrants originate. By comparison, this paper focuses on the role of social connectedness among a more homogenous group of domestic migrants. This paper draws on Stuart and Taylor (2018), which examines the role of birth town migration networks in more detail.

## 2 Historical Background on the Great Migration

The Great Migration saw nearly six million African Americans leave the South from 1910 to 1970 (United States Bureau of the Census, 1979).<sup>3</sup> Although migration was concentrated in certain destinations, like Chicago, Detroit, and New York, other cities also experienced dramatic changes. For example, Chicago's black population share increased from 2 to 32 percent from 1910 to 1970, while Racine, Wisconsin experienced an increase from 0.3 to

<sup>2</sup>Both peer effects and social connectedness reflect social influences, but are different concepts. Peer effects arise through interactions between individuals making decisions about whether to commit crime, while social connectedness in our setting is analogous to social cohesion among older individuals that arises because they share the same birth town. Recent research on peer effects in crime includes Ludwig, Duncan and Hirschfeld (2001); Kling, Ludwig and Katz (2005); Ludwig and Kling (2007); Bayer, Hjalmarsson and Pozen (2009); Drago and Galbiati (2012); Damm and Dustmann (2014); Billings, Deming and Ross (2016); Corno (2017); Stevenson (2017)

<sup>3</sup>Parts of this section come from Stuart and Taylor (2018).

10.5 percent (Gibson and Jung, 2005). Migration out of the South increased from 1910 to 1930, slowed during the Great Depression, and then resumed forcefully from 1940 to 1970.

Several factors contributed to the exodus of African Americans from the South. World War I, which simultaneously increased labor demand among Northern manufacturers and decreased labor supply from European immigrants, helped spark the Great Migration (Scroggs, 1917; Scott, 1920; Gottlieb, 1987; Marks, 1989; Margo, 1990; Jackson, 1991; Collins, 1997; Gregory, 2005). However, many underlying causes existed long before the war, including a less developed Southern economy, the decline in agricultural labor demand due to the boll weevil's destruction of crops (Scott, 1920; Marks, 1989, 1991; Lange, Olmstead and Rhode, 2009), widespread labor market discrimination (Marks, 1991), and racial violence and unequal treatment (Tolnay and Beck, 1991).

Migrants tended to follow paths established by railroad lines: Mississippi-born migrants generally moved to Illinois and other Midwestern states, and South Carolina-born migrants generally moved to New York and Pennsylvania (Carrington, Detragiache and Vishwanath, 1996; Collins, 1997; Boustan, 2010; Black et al., 2015). Labor agents, offering paid transportation, employment, and housing, directed some of the earliest migrants, but their role diminished after the 1920s, and most individuals paid for the relatively expensive train fares themselves (Gottlieb, 1987; Grossman, 1989).<sup>4</sup> African-American newspapers from the largest destinations circulated throughout the South, providing information on life in the North (Gottlieb, 1987; Grossman, 1989).<sup>5</sup>

Historical accounts and recent quantitative work indicate that birth town migration networks strongly affected location decisions during the Great Migration. Initial migrants, most of whom moved in the 1910s, chose their destination primarily in response to economic opportunity. Migrants who worked for an employer in search of labor and were well-connected in their birth town linked family, friends, and acquaintances to jobs and shelter in the North, sometimes leading to persistent birth town to destination city migration flows (Rubin, 1960; Gottlieb, 1987). Describing this behavior shortly after the start of the Great Migration, Scott (1920) wrote,

“The tendency was to continue along the first definite path. Each member of the vanguard controlled a small group of friends at home, if only the members of his immediate family. Letters sent back, representing that section of the North and giving directions concerning the route best known, easily influenced the next groups to join their friends rather than explore new fields. In fact, it is evident throughout the movement that the most congested points in the North when the migration reached its height, were those favorite cities to which the first group had gone”

(p. 69).

<sup>4</sup>In 1918, train fare from New Orleans to Chicago cost \$22 per person, when Southern farmers' daily wages typically were less than \$1 and wages at Southern factories were less than \$2.50 (Henri, 1975).

<sup>5</sup>The *Chicago Defender*, perhaps the most prominent African-American newspaper of the time, was read in 1,542 Southern towns and cities in 1919 (Grossman, 1989).

Consistent with these accounts, Stuart and Taylor (2018) provide quantitative evidence that birth town migration networks strongly influenced the location decisions of Southern black migrants.

The experience of John McCord captures many important features of early black migrants' location decisions.<sup>6</sup> Born in Pontotoc, Mississippi, nineteen-year-old McCord traveled in search of higher wages in 1912 to Savannah, Illinois, where a fellow Pontotoc-native connected him with a job. McCord moved to Beloit, Wisconsin in 1914 after hearing of employment opportunities and quickly began work as a janitor at the manufacturer Fairbanks Morse and Company. After two years in Beloit, McCord spoke to his manager about returning home for a vacation. The manager asked McCord to recruit workers during the trip, and McCord returned with 18 unmarried men, all of whom were soon hired. Thus began a persistent flow of African Americans from Pontotoc to Beloit: among individuals born from 1916 to 1936, 14 percent of migrants from Pontotoc lived in Beloit's county in old age (Stuart and Taylor, 2018).<sup>7</sup>

Qualitative evidence documents the impact of social ties among African Americans from the same birth town on life in the North. For example, roughly 1,000 of Erie, Pennsylvania's 11,600 African American residents once lived in Laurel, Alabama, and almost half had family connections there, leading an Erie resident to say, "I'm surrounded by so many Laurelites here, it's like a second home" (Associated Press, 1983). Nearly forty percent of the migrants in Decatur, Illinois came from Brownsville, Tennessee, and Brownsville high school reunions took place in Decatur from the 1980s to 2000s (Laury, 1986; Smith, 2006).<sup>8</sup> As described by a Brownsville native, "Decatur's a little Brownsville, really" (Laury, 1986).

Stack (1974) offers deeper insights into birth town and family social ties among African American migrants. This ethnography is set in an unidentified Midwestern city that lies on a major railroad connecting the South to Chicago. Stack documents "extensive networks of kin and friends" that originated during the Great Migration and continued to grow in the North (p. 28). These networks served many purposes, one of which was childrearing. Households typically contained three generations of kin (not all of whom were first or second degree relatives): "males and females beyond childbearing age, a middle generation of mothers raising their own children or children of close kin, and the children" (p. 123). Beyond the middle and older generations of adults in their household, children were raised by "discipliners ... allowed to participate in the control of children," "trainers [who] not only discipline but teach moral values and respect for adults," and older children who learned these behaviors from adults (p. 84). This environment clearly could have reduced crime. Motivated by these accounts, we now turn to a systematic analysis of the effect of social connectedness on crime.

<sup>6</sup>The following paragraph draws on Bell (1933). See also Knowles (2010).

<sup>7</sup>This is 68 times larger than the total share of Mississippi migrants that lived there in old age.

<sup>8</sup>The 40 percent figure comes from the Duke SSA/Medicare dataset, described below.



### 3 Conceptual Framework: Crime and Social Connectedness

To inform our empirical work, we next describe the ways in which social connectedness might affect crime. As a starting point, consider younger and older individuals, with the key distinction being that younger individuals might commit crime, while older individuals do not. In deciding whether to commit crime, younger individuals are influenced by non-social factors, peer effects, and social connectedness. The non-social determinants of crime include employment opportunities and the degree of policing, among other factors. Peer effects matter because youth are influenced by the crime decisions of other youth. In our setting, social connectedness is analogous to social cohesion among older individuals that stems from a shared birth town.

Social connectedness could directly affect crime in several ways. For example, it might reduce crime by increasing the probability a criminal is identified and punished, helping individuals find jobs, increasing the stock of cognitive and non-cognitive skills that boost earnings in the non-crime labor market, or promoting anti-crime norms. Alternatively, social connectedness could increase crime by reinforcing unproductive norms or providing trust that facilitates criminal activity, as with the Ku Klux Klan, Mafia, or gangs (Fukuyama, 2000; Putnam, 2000). The work of Stack (1974) suggests that social connectedness might decrease crime in our setting, but this is ultimately an empirical question.

The total effect of social connectedness depends on these direct effects plus peer effects and spillovers. Suppose that social connectedness among Southern black migrants directly affects only African American youth (e.g., because of segregation). Social connectedness could indirectly affect non-black youth through peer effects that link the crime decisions of black and non-black youth (e.g., because of gang activity). These peer effects could amplify the direct effect of social connectedness, providing one reason why social connectedness among Southern black migrants could affect city-level crime rates. Another reason is that, although African Americans account for a minority of the population, they account for a majority of the crimes reported to police in the cities we study.

Appendix A contains a simple model that formalizes these forces. We show that if social connectedness reduces the crime rate of African Americans with ties to the South, then social connectedness weakly reduces the crime rate of all groups, as long as the equilibrium is stable and peer effects are non-negative. In this situation, the crime-reducing effect of social connectedness among Southern African Americans is not counteracted by higher crime rates among other groups. A symmetric result holds if social connectedness instead increases the crime rate of African Americans with ties to the South. An additional takeaway from the model is that the Herfindahl-Hirschman Index emerges as a natural way to measure the degree of social connectedness in each destination city. Guided by this theoretical analysis, we next describe our empirical strategy for estimating the effect of social connectedness on crime. We return to mechanisms below.

## 4 Data and Empirical Strategy

### 4.1 Data on Crime, Social Connectedness, and Control Variables

We estimate the effect of social connectedness on crime from 1970 to 2009, since the Great Migration ended around 1970. We measure annual city-level crime counts using FBI Uniform Crime Reports (UCR) data, available from the Inter-university Consortium for Political and Social Research (ICPSR). UCR data contain monthly counts of the number of offenses reported to police, which we aggregate to the city-year level. We focus on the seven commonly studied index crimes: murder and non-negligent manslaughter (“murder”), forcible rape (“rape”), robbery, assault, burglary, larceny, and motor vehicle theft. Murder is the best measured crime, and robbery and motor vehicle theft are also relatively well-measured (Blumstein, 2000; Tibbetts, 2012). Missing crimes are indistinguishable from true zeros in the UCR. Because cities in our sample almost certainly experience property crime each year, in our main analysis we drop all city-years in which any of the three property crimes (burglary, larceny, and motor vehicle theft) equal zero.<sup>9</sup> We also use annual population estimates from the Census Bureau in the UCR data.

The Duke SSA/Medicare dataset provides the birth town to destination city population flows that underlie our measure of social connectedness. The data contain sex, race, date of birth, date of death (if deceased), and the ZIP code of residence at old age (death or 2001, whichever is earlier) for over 70 million individuals who received Medicare Part B from 1976 to 2001. In addition, the data include a 12-character string with self-reported birth town information from the Social Security Administration NUMIDENT file, which is matched to places (see Black et al., 2015). These data capture long-run location decisions, as we only observe individuals’ location at birth and old age.<sup>10</sup> Hence, our measure of social connectedness for each city does not vary over time. We focus on individuals born from 1916 to 1936 in the former Confederate states, which we refer to as the South. Out-migration rates for the 1916–1936 cohorts are among the highest of all cohorts in the Great Migration (Appendix Figure A.1), and coverage rates decline considerably for earlier and later cohorts (Black et al., 2015). We restrict our main analysis sample to cities with at least 25 Southern-born black migrants in the Duke dataset to improve the reliability of our estimates.

Census county and city data books provide covariates each decade from 1920 to 2000. In 1920 and 1930, we have county-level covariates. Starting in 1940, we have city-level covariates for cities with at least 25,000 residents. Consequently, our main sample contains cities with at least 25,000 residents from 1940–forward. We limit our sample to cities in the Northeast, Midwest, and West Census regions to focus on the cross-region moves that characterize the Great Migration. Our main analysis sample excludes cities with especially severe measurement errors in the crime data, as described in Appendix B. Appendix Tables A.1 and A.2 provide summary statistics, and Appendix Figure A.2 shows the geographic distribution of our sample. All 224 cities in our sample are in a county with a railroad.<sup>11</sup>

<sup>9</sup>At least one property crime equals zero in four percent of city-year observations.

<sup>10</sup>As described in detail below, there is relatively little migration for our sample after leaving the South, so our ability to observe individuals’ location only in old age is not particularly important.



## 4.2 Estimating the Effect of Social Connectedness on Crime

Our main estimating equation is

$$Y_{k,t} = \exp[\ln(\mathbf{HHI}_k)\delta + \ln(N_k)\theta + X'_{k,t}\beta] + \epsilon_{k,t}, \tag{1}$$

where  $Y_{k,t}$  is the number of crimes in city  $k$  in year  $t$ . The key variable of interest is our proxy for social connectedness among African Americans with ties to the South,  $\mathbf{HHI}_k = \sum_j (N_{j,k}/N_k)^2$ , where  $N_{j,k}$  is the number of migrants from birth town  $j$  that live in destination city  $k$ , and  $N_k \equiv \sum_j N_{j,k}$  is the total number of migrants. A Herfindahl-Hirschman Index is a natural way to measure social connectedness, as discussed in Section 3.  $X_{k,t}$  is a vector of covariates, including log population and other variables described below, and  $\epsilon_{k,t}$  captures unobserved determinants of crime.<sup>12</sup> We use an exponential function in equation (1) because there are no murders for many city-year observations (Appendix Table A.1).<sup>13</sup>

Our proxy for social connectedness varies only across cities, but the number of crimes varies across both cities and years. Instead of collapsing the data into city-level observations, we use equation (1) because our panel of cities is not balanced.<sup>14</sup> We cluster standard errors by city to allow for arbitrary autocorrelation in unobserved determinants of crime.

The key parameter of interest is  $\delta$ , which we interpret as the elasticity of the crime *rate* with respect to  $\mathbf{HHI}_k$ , because we control for log population and specify the conditional mean as an exponential function. If social connectedness reduces the city-level crime rate, then  $\delta < 0$ . We estimate  $\delta$  using cross-city variation in social connectedness, conditional on the total number of migrants and  $X_{k,t}$ . The key identifying assumption is

$$\epsilon_{k,t} \perp\!\!\!\perp \mathbf{HHI}_k \mid (N_k, X_{k,t}), \tag{2}$$

which states that, conditional on the number of migrants living in city  $k$  and the vector of control variables, social connectedness is independent of unobserved determinants of crime from 1970 to 2009. Condition (2) allows the total number of migrants,  $N_k$ , to depend arbitrarily on observed and unobserved determinants of crime.<sup>15</sup>

As discussed in Section 2, historical accounts suggest that variation in social connectedness, conditional on economic and social opportunities, arose largely from idiosyncratic factors like the right migrant being in the right place at the right time. For example, social connectedness in Beloit, Wisconsin stemmed from John McCord’s ability to convince 18 individuals from his birth town to come to Beloit in 1916. If John McCord had worked in

<sup>11</sup>Of these, 112 cities can be reached from the South via one railroad line, 111 cities via two lines (i.e., one connection is required), and one city (Lynn, Massachusetts) is linked via three lines.

<sup>12</sup>Because equation (1) includes  $\ln(\mathbf{HHI}_k)$ ,  $\ln(N_k)$ , and log population, our estimate of  $\delta$  would be identical if we instead used city population as the denominator of  $\mathbf{HHI}_k$ .

<sup>13</sup>We estimate the parameters in equation (1) using a Poisson quasi-maximum likelihood estimator. Consistent estimation of  $(\delta, \theta, \beta)$  requires the assumption that  $E[Y_{k,t} \mid \cdot] = \exp[\ln(\mathbf{HHI}_k)\delta + \ln(N_k)\theta + X'_{k,t}\beta]$ , but does not require any restriction on the conditional variance of the error term (Wooldridge, 2002). Given this, we use the representation in equation (1) to facilitate discussion of our assumptions about unobserved determinants of crime.

<sup>14</sup>Estimating regressions on data collapsed to the city-level yields nearly identical results.

<sup>15</sup>Condition (2) does not guarantee identification of the other parameters in equation (1) besides  $\delta$ . For example, identification of  $\theta$  requires exogenous variation in the total number of migrants. Boustan (2010) provides one possible strategy for identifying  $\theta$ , but we do not pursue that here. See also Derenoncourt (2019).

a different city that offered similar opportunities, these migrants likely would have followed McCord there. If a less influential migrant had worked in Beloit, these migrants likely would not have followed.

We construct  $\text{HHI}_k$  and  $N_k$  for migrants born from 1916 to 1936. The vast majority of these individuals moved out of the South between 1940 and 1960 (Stuart and Taylor, 2018). For this generation, the historical literature highlights the role of previous migrants' location decisions, contemporaneous economic conditions, and moving costs as the main factors determining where individuals moved (Gottlieb, 1987; Grossman, 1989). Moving costs mattered in a specific way: migrants moved along vertical routes established by railroad lines, but along a railroad line, there was little variation in the cost of moving to different destinations.

Our main specification includes several variables that bolster the credibility of condition (2). We control for the log number of Southern black migrants to account for a broad set of factors that, via revealed preference, influenced the attractiveness of destinations to black migrants. We also control for log population, the African American population share, and log manufacturing employment from 1920 to 1960, as these variables could affect the strength of social connectedness and be correlated with later determinants of crime. We control for log population in year  $t$  and log land area, so that we also control for log population density. State-by-year fixed effects flexibly account for determinants of crime that vary over time at the state-level, due to changes in economic conditions, government spending, and other factors. Below, we examine the robustness of our results to a battery of additional covariates. We also examine selection on unobserved variables in two distinct ways. The results support the validity of condition (2).

We construct  $\text{HHI}_k$  and  $N_k$  using migrants' location in old age, measured from 1976 to 2001. In principle, migration after 1970, when we first measure crime, could influence  $\text{HHI}_k$ . If migrants with a higher concentration of friends and family nearby were less likely to out-migrate in response to higher crime shocks, then  $\text{HHI}_k$  would be larger in cities with greater unobserved determinants of crime,  $\epsilon_{k,t}$ . This would bias our estimate of  $\delta$  upwards, making it more difficult to conclude that social connectedness reduces crime. Reassuringly, Appendix Table A.3 reveals very low migration rates among African Americans who were born in the South from 1916 to 1936 and living in the North, Midwest, and West. Around 90 percent of individuals stayed in the same county for the five-year periods 1955–1960, 1965–1970, 1975–1980, 1985–1990, and 1995–2000. This suggests that our inability to construct  $\text{HHI}_k$  using migrants' location before 1970 is relatively unimportant.

### 4.3 Initial Evidence on the Validity of the Empirical Strategy

Before discussing our results, we present initial evidence that supports the validity of our empirical strategy. We first examine whether social connectedness stems from a large concentration of migrants from a single birth town. If idiosyncratic factors drive social connectedness, then a single sending town should account for most of the variation. Consistent with this, Figure 1 shows that 78 percent of the variation in log HHI is explained by the leading term of log HHI, which equals the log squared share of migrants from the top sending town.<sup>16</sup>

Second, we examine whether crime rates in the early twentieth century are correlated with social connectedness. If connected groups of migrants moved to cities with low crime rates, and these low crime rates persisted into the 1970s and beyond, then this would threaten our empirical strategy. Table 1 reports regressions of  $\ln(\text{HHI}_k)$  on  $\ln(N_k)$  and several covariates. Column 1 shows a negative relationship between log social connectedness and the log number of migrants. This relationship is mechanical: because birth towns are smaller than destination cities, a city must attract migrants from many birth towns to attract a large number of migrants. Column 2 shows that social connectedness is stronger in cities with more manufacturing employment in 1940.<sup>17</sup> The relationship between social connectedness and the African American population share is positive, but not statistically significant. Column 3, which includes the log mean murder rate from 1936 to 1939, is the most important.<sup>18</sup> The point estimate is small and indistinguishable from zero. As a result, we find no evidence that cities with lower crime rates from 1936 to 1939 attracted more connected groups of migrants.<sup>19</sup>

## 5 The Effect of Social Connectedness on Crime

### 5.1 Main Results

Table 2 shows that social connectedness leads to sizable and statistically significant reductions in murder, rape, robbery, assault, burglary, and motor vehicle theft. The table reports estimates of equation (1) for an unbalanced panel of 224 cities.<sup>20</sup> As seen in column 1, the estimated elasticity of the murder rate with respect to HHI is  $-0.245$  (0.064). The estimates for robbery and motor vehicle theft, two other well-measured crimes in the FBI data, are  $-0.234$  (0.045) and  $-0.227$  (0.083). At the mean, these estimates imply that a one standard deviation increase in social connectedness leads to a 21 percent decrease in murders and a 20 percent decrease in robberies and motor vehicle thefts. Summed over the 40 years from 1970 to 2009, a one standard deviation increase in social connectedness leads to 80 fewer murders, 2,529 fewer robberies, and 5,566 fewer motor vehicle thefts per 100,000 residents.

Simple examples help further illustrate the effects of social connectedness on crime. First, consider Middletown, Ohio and Beloit, Wisconsin. These cities are similar in their total number of Southern black migrants, 1980 population, and 1980 black population share, but Beloit's HHI is over four times as large as Middletown's (0.057 versus 0.014).<sup>21</sup> The

<sup>16</sup>Appendix Table A.4 lists the HHI and top sending town migrant share for each city.

<sup>17</sup>This is consistent with Stuart and Taylor (2018), who find that birth town migration networks brought African Americans to cities with more manufacturing employment.

<sup>18</sup>We digitized FBI UCR data to construct this variable. UCR data are available for 81 cities from 1930 to 1936 (see Fishback, Johnson and Kantor (2010)) and not available before 1930. To examine crime rates before the Great Migration began, we construct log murder rates from 1911 to 1916 using historical mortality statistics for cities with at least 100,000 residents in 1920 (United States Bureau of the Census, 1922). As seen in Appendix Table A.5, we find no statistically or substantively significant relationship between social connectedness and early century murder rates, although power is limited by the smaller sample size. This conclusion holds when we use inverse probability weights to make this sample of cities, which has higher population, comparable to our main analysis sample on observed covariates.

<sup>19</sup>Results in Table 1 are extremely similar if we replace the 1940 covariates with 1950 or 1960 covariates. We use a single year of covariates to transparently describe the cross-sectional patterns that underlie our identification strategy. Because we include covariates from 1920 to 1960 in equation (1), our estimates of  $\delta$  also control for changes in covariates across decades.

<sup>20</sup>Appendix Table A.6 displays results for all variables.

<sup>21</sup>For Middletown and Beloit, the number of Southern black migrants is 376 and 407; the 1980 population is 35,207 and 43,719; and the 1980 percent black is 11.3 and 12.0.

estimates in Table 2 imply that replacing Middletown's HHI with that of Beloit would decrease murders, robberies, and motor vehicle thefts by 28–30 percent. By comparison, the estimates in Chalfin and McCrary (2018) imply that a similar decrease in murders would require a 44 percent increase in the number of police officers.<sup>22</sup> The effect of social connectedness is even larger in other examples. HHI in Decatur, Illinois is almost twenty times larger than that of Albany, NY (0.118 versus 0.006).<sup>23</sup> Replacing Albany's HHI with that of Decatur would decrease murders by 63 percent, robberies by 60 percent, and motor vehicle thefts by 58 percent. While these effects are sizable, they are reasonable in light of the tremendous variation in crime rates across cities (Appendix Table A.2).

## 5.2 Addressing Threats to Empirical Strategy

**Selection on Observed Variables**—We first examine the robustness of our results to a battery of additional covariates. We focus on the effect of social connectedness on murder, given its importance for welfare and higher measurement quality. Column 1 of Table 3 repeats our baseline specification to facilitate comparisons. In column 2, we control for the contemporaneous share of the population that is African American and female.<sup>24</sup> In column 3, we control for the share of the population age 5–17, 18–64, and 65+, and the share with a high school and college degree. In column 4, we control for log median family income, the unemployment rate, the labor force participation rate, and log manufacturing employment. We add these variables because they could be correlated with social connectedness and unobserved determinants of crime, biasing our estimate of  $\delta$ . However, social connectedness might affect some of these variables, in which case controlling for them would eliminate the original omitted variables bias while introducing another source of bias.<sup>25</sup> In practice, the coefficient on log HHI changes very little when including these variables.

Next, we control for log HHI and the log number of Southern white migrants and foreign immigrants, using country of origin for the latter group. In principle, social connectedness among these groups could affect crime. We focus on the results for Southern black migrants because previous work documents the importance of birth town migration networks (Stuart and Taylor, 2018) and we are most confident in the validity of condition (2) and the interpretation of HHI as reflecting social connectedness for this group. While we do not assign a causal interpretation to the additional variables, they could be correlated with omitted determinants of crime. As seen in column 5, our results are very similar when including these variables. Column 6 shows that our results also are similar when controlling for racial fragmentation (Alesina and Ferrara, 2000) plus the share of the population that is Hispanic, foreign born, speaks only English at home, and speaks English well or very well.<sup>26</sup>

<sup>22</sup>Chalfin and McCrary (2018) estimate an elasticity of murder with respect to police of  $-0.67$ , almost three times the size of our estimated elasticity for murder.

<sup>23</sup>For Decatur and Albany, the number of Southern black migrants is 760 and 874; the 1980 population is 94,081 and 101,727; and the 1980 percent black is 14.6 and 15.9.

<sup>24</sup>When explaining crime in year  $t$ , we use the variable from the decade in which  $t$  lies. Demographic and economic variables are available every ten years from 1970 to 2000, except for percent with a high school degree and college degree (not observed in 2000), log median family income (not observed in 2000), and manufacturing share (not observed in 2000). We use the 1990 values of these unavailable variables. Appendix B has additional details on the sample and data.

<sup>25</sup>In the language of Angrist and Pischke (2009), this is a “proxy control” situation (p. 66).

<sup>26</sup>Following Alesina and Ferrara (2000), we define racial fragmentation as one minus a HHI of the share of population that is white, black, American Indian, and any other race.

Another possible concern is that our results reflect the characteristics of migrants' birth place, as opposed to social connectedness. To examine this, we construct migrant-weighted averages of Southern birth county characteristics. We use the 1920 Census to measure the black farm ownership rate, black literacy rate, black population density, percent black, and percent rural. We also measure exposure to Rosenwald schools, which increased educational attainment among African Americans in the South (Aaronson and Mazumder, 2011). Our results are very similar when adding these variables, as seen in column 7.<sup>27</sup>

Columns 2–7 of Table 3 demonstrate that our results are robust to controlling for many additional variables. Next, we provide additional support for our empirical strategy by examining selection on unobserved variables in two distinct ways.

**Selection on Unobserved Variables: Using a Structural Migration Model**—One concern is that our estimate of  $\delta$  is biased by unobserved characteristics of certain migrants — those who chose the same destination as other migrants from their birth town. Census data reveal that Southern black migrants living in a state or metropolitan area with a higher share of migrants from their birth state have less education and income (Appendix Table A.7).<sup>28</sup> As a result, migrants who followed their birth town network likely had less education and earnings capacity than other migrants. This negative selection on education and earnings could generate a positive correlation between  $\text{HHI}_k$  and  $\epsilon_{k,b}$  biasing our estimate of  $\delta$  upwards, and making it harder to conclude that social connectedness reduces crime (e.g., Lochner and Moretti, 2004).

At the same time, migrants who followed their birth town network might display greater cooperation or other pro-social behaviors. To address this possibility, we estimate a structural model of location decisions, originally developed in Stuart and Taylor (2018), which generates the share of migrants that moved to each destination because of their birth town migration network. When used as a covariate in equation (1), this variable proxies for unobserved characteristics of migrants that chose to follow other migrants from their birth town. We sketch this model in the text, leaving some details to Appendix C.

In the model, migrants from birth town  $j$  are indexed on a circle by  $i \in \{1, \dots, N_j\}$ , where  $N_j$  is the total number of migrants. For migrant  $i$ , destination  $k$  belongs to one of three preference groups: high ( $H_j$ ), medium ( $M_j$ ), or low ( $L_j$ ). The high preference group contains a single destination. In the absence of social interactions between migrants, the destination in  $H_j$  is most preferred, and destinations in  $M_j$  are preferred over those in  $L_j$ .<sup>29</sup> A migrant never moves to a destination in  $L_j$ . A migrant chooses a destination in  $M_j$  if and only if his neighbor,  $i - 1$ , chooses the same destination. A migrant chooses a destination in  $H_j$  if his neighbor chooses the same destination or his neighbor selects a destination in  $L_j$ .

<sup>27</sup>County-level homicide data are available from historical vital statistics reports starting in the late 1940s. Results are extremely similar if we also control for the log mean homicide rate in 1950 in migrants' county of birth.

<sup>28</sup>Research on immigrants in the U.S. finds similar patterns of selection (Bartel, 1989; Bauer, Epstein and Gang, 2005; McKenzie and Rapoport, 2010).

<sup>29</sup>The assumption that  $H_j$  is a non-empty singleton ensures that migrant  $i$  has a well-defined location decision in the absence of social interactions. We could allow  $H_j$  to contain many destinations and specify a decision rule among the elements of  $H_j$ . This extension would complicate the model without adding new insights.

Migrants from the same birth town can differ in their preferences over destinations. The probability that destination  $k$  is in the high preference group for a migrant from town  $j$  is  $h_{j,k} \equiv \mathbb{P}[k \in H_i \mid i \in j]$ , and the probability that destination  $k$  is in the medium preference group is  $m_{j,k} \equiv \mathbb{P}[k \in M_i \mid i \in j]$ . These probabilities arise from expected utility maximization problems solved by migrants. We do not need to specify migrants' utility functions, but expected wages and transportation costs are among the relevant factors. We also do not need to specify why some migrants choose the same destination as their neighbor. For example, neighbors might provide information about employment opportunities, or migrants might value living near friends and family.

The share of migrants from birth town  $j$  that chose destination  $k$  because of social interactions equals  $m_{j,k}$ . As a result, the share of migrants that chose this destination because of social interactions is  $m_k = \sum_j N_{j,k} m_{j,k} / N_k$ . By including  $m_k$  in equation (1), we can assess whether our results stem from social connectedness or unobserved characteristics of migrants that chose the same destination as other migrants from their birth town.

Conditional on the number of migrants in a destination ( $N_k$ ) and the share of migrants that chose their destination because of social interactions ( $m_k$ ), variation in social connectedness ( $\text{HHI}_k$ ) continues to arise from concentrated birth town to destination city population flows. To see this, consider two hypothetical cities that each have 20 migrants, one-fourth of whom chose their destination because of social interactions ( $m_k = 0.25$ ). In the low HHI city, the 20 migrants come from five birth towns. Each town sends four migrants, one of whom moves there because of social interactions. As a result,  $\text{HHI}_{\text{Low}} = 0.2$ . In the high HHI city, the 20 migrants also come from five birth towns. One town sends 12 migrants, three of whom move there because of social interactions. Two towns each send two migrants, one of whom moves there because of social interactions, and two towns each send two migrants, neither of whom is influenced by social interactions. As a result,  $\text{HHI}_{\text{High}} = 0.4$ . This example is consistent with Figure 1 in that variation in social connectedness arises from the top sending town.

Column 8 of Table 3 shows that the estimated effect of social connectedness changes little when we control for the share of migrants that chose their destination because of their birth town migration network.<sup>30</sup> We find little evidence that our results are driven by unobserved characteristics of certain migrants. For completeness, column 9 includes all of the additional covariates previously described. The effect of social connectedness is similar in magnitude and statistically significant. As noted above, column 9 is not our preferred specification, because some of the covariates could be affected by social connectedness.

**Selection on Unobserved Variables: Using Lagged Crime Rates**—Although we have addressed many potential concerns, it is possible that cities with higher social connectedness have lower unobserved determinants of crime,  $\epsilon_{k,t}$ , for some other reason. For example, if connected groups of migrants moved to cities with low crime rates, and unobserved determinants of crime persisted over time, then our estimate of  $\delta$  could be biased downwards. We have already presented evidence against this threat by showing that log HHI

<sup>30</sup>Results are very similar when we use quadratic, cubic, or quartic functions of this variable.



is not correlated with murder rates from 1936 to 1939 (Table 1) or 1911 to 1916 (Appendix Table A.5).

To provide more direct evidence against this threat, we estimate the effect of social connectedness on crime for each five-year interval from 1970 to 2009 while controlling for the 1960–1969 log mean crime rate. If our results were driven by connected groups of migrants initially sorting into cities with low crime rates and unobserved determinants of crime persisting over time, then controlling for the 1960–1969 crime rate would eliminate the correlation between social connectedness and crime rates in later years. On the other hand, if condition (2) is valid and there is a true effect of social connectedness, then controlling for the 1960–1969 crime rate will not completely attenuate the estimate of  $\delta$ ; adding this control could attenuate estimates because unobserved determinants of crime are serially correlated, but the attenuation would diminish with time.

To see this more formally, consider a simple linear model,

$$Y_{k,t} = \ln(\mathbf{HHI}_k)\delta_t + \epsilon_{k,t} \tag{3}$$

$$\epsilon_{k,t} = \epsilon_{k,t-1}\rho + u_{k,t}, \tag{4}$$

where  $\delta_t$  is the effect of social connectedness on crime in year  $t$ ,  $\rho \in (-1, 1)$  captures serial correlation in unobserved determinants of crime,  $\mathbb{E}[u_{k,t} | \epsilon_{k,t-1}] = 0$ , and we ignore other covariates. We use a linear model to simplify the analysis, but we have used Monte Carlo simulations to verify that the main conclusion holds with an exponential conditional mean function in equation (3). Because there is little migration after 1960 (Appendix Table A.3), the main concern is that  $\text{cov}[\ln(\mathbf{HHI}_k), \epsilon_{k,1960}] < 0$  and  $\rho \in (0, 1)$ . We could have  $\text{cov}[\ln(\mathbf{HHI}_k), \epsilon_{k,1960}] < 0$  if connected groups of migrants moved to cities with low unobserved determinants of crime in 1960. If unobserved determinants of crime are positively correlated, then our estimate of  $\delta$  in 1970 could be biased by this selection.

Consider estimating a regression on 1970–2009 data that controls for the 1960 crime rate,

$$Y_{k,t} = \ln(\mathbf{HHI}_k)d_t + Y_{k,1960}b_t + e_{k,t}. \tag{5}$$

It is straightforward to show that

$$\text{plim} \hat{d}_t = \delta_t - \delta_{1960}\rho^t. \tag{6}$$

Equation (6) shows that controlling for the 1960 crime rate eliminates the selection bias that arises if  $\text{cov}[\ln(\mathbf{HHI}_k), \epsilon_{k,1960}] \neq 0$ . However, if there is an effect of social connectedness on crime in 1960 and unobserved determinants of crime are serially correlated, then  $\hat{d}_t$  is a biased estimator of  $\delta_t$ . As  $t$  increases, the bias declines as the correlation of  $\epsilon_{k,t}$  with  $\epsilon_{k,1960}$  declines. If  $\hat{d}_t$  approaches the coefficient on  $\ln(\mathbf{HHI}_k)$  from the regression that does not control for  $Y_{k,1960}$ , then our results are not driven by selection of connected groups of

migrants into cities with low  $\epsilon_{k,1960}$ . In contrast, if our results are driven by selection, so that  $\delta_t = \delta_{1960} = 0$ , then  $\text{plim } \hat{d}_t = 0$ .

Figure 2 plots coefficient estimates from our baseline specification and from a specification that includes the 1960–1969 log mean murder rate. The results are consistent with the prediction in equation (6) if our empirical strategy were valid: there is some attenuation, but this declines over time, and the two sets of point estimates converge. We conclude that our results are not driven by the sorting of connected groups of migrants into low crime cities, but instead reflect the effect of social connectedness on crime. This rules out a large set of threats to our empirical strategy.

Figure 2 also shows that the effects of social connectedness on crime are persistent. Even in the 2000s, when many of the individuals born from 1916 to 1936 were no longer alive, cities with higher social connectedness have lower murder rates. Natural explanations for this persistence include changing norms and non-cognitive skills, which are passed down across generations, and path dependence in criminal and gang activity (Nagin and Paternoster, 1991).

Appendix D describes several additional robustness tests, all of which support our findings.

### 5.3 Mechanisms

The previous results show that social connectedness reduces city-level crime rates, demonstrate the robustness of this finding, and support the validity of our empirical strategy. So far, we have estimated the overall effect of social connectedness on crime rates. We next present results that clarify our main finding and the underlying mechanisms.

Several potential mechanisms stem from previous theoretical and empirical work. For example, social connectedness could increase the probability that criminals are identified and punished, increase formal labor market opportunities through job referrals, increase homeownership through shared information or resources, increase the number of single-parent households (by providing additional resources for childrearing) or decrease the number of single-parent households (by increasing the costs faced by less-active parents), increase cognitive skills, increase non-cognitive skills such as self-control and forward-looking behavior, affect personality traits such as aggression, or increase pro-social norms. We consider these in turn.

If social connectedness reduces crime by increasing the probability that criminals are identified and punished, we should primarily see reductions in crimes that tend to be witnessed. However, Table 2 shows that social connectedness reduces crimes that are more and less likely to have witnesses: burglary and motor vehicle theft are less likely to have witnesses than robbery or assault, yet the estimates are similar in magnitude for all of these crimes.<sup>31</sup>

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<sup>31</sup>Unlike larceny or motor vehicle theft, a robbery features the use of force or threat of force. Consequently, robberies are witnessed by at least one individual (the victim).

We partly examine other mechanisms by adding observed proxies to equation (1). For example, consider the black unemployment rate. If social connectedness increases the probability of employment for young adults and this decreases crime, then including the black unemployment rate in equation (1) would attenuate the coefficient on HHI. However, an attenuated coefficient would not necessarily imply that employment is a mechanism, as the reduction in crime could cause higher employment, or social connectedness could independently cause lower crime and higher employment. An attenuated coefficient only suggests the variable in question as a potential mechanism. On the other hand, if the estimated effect of HHI on crime does not change when adding a variable, this implies it is not a quantitatively important mechanism.

Table 4 explores several possible mechanisms. We focus on years 1980–1989 because African American-specific covariates from the Census are not available for 1960 or 1990, and the crack index from Fryer et al. (2013) is only available from 1980–forward. The table presents results for the 222 cities with non-missing African-American specific covariates. Appendix Table A.13 contains results for the 66 cities for which the Fryer et al. (2013) crack index is also available.

Column 1 contains the estimate of  $\delta$  from our baseline specification. In column 2, we add black demographic and economic covariates, including the share of African Americans with a high school and college degree, and the black unemployment rate. Column 3 adds the black homeownership rate, column 4 adds the share of black households headed by a single female, and column 5 adds both of these variables. In column 6 of Appendix Table A.13, we add the crack index from Fryer et al. (2013), and column 7 adds all variables. Estimates of  $\delta$  are very similar across the specifications in Table 4. There is more variation in Table A.13, which is not surprising given the smaller sample size; the most important takeaway is the lack of evidence that crack cocaine use is an important mechanism. In sum, Tables 4 and A.13 indicate that the effect of social connectedness on crime is not mediated by short-run effects on employment, education, homeownership, the prevalence of single parents, or crack cocaine use.<sup>32</sup>

To better understand the underlying mechanisms, we use FBI Supplemental Homicide Reports (SHR), which provide additional details on offenders and victims for murders starting in 1976. Table 5 reports the estimated effect of social connectedness on murders, by victim characteristic and circumstance. The overall effect, in row 1, is very similar to the Table 2 estimate. Social connectedness leads to the largest reduction in murders committed during gang and drug activity. Social connectedness also leads to a sizable reduction in murders committed alongside other felonies. The effects are smaller, but still sizable and precisely estimated, for murders that occur during arguments. For 28 percent of murders, the circumstance is unknown, mainly because the murder is not cleared by arrest. The largest effects are on murders committed with guns and for victims age 10–25.<sup>33</sup> Finally, the effects

<sup>32</sup>Social connectedness also could affect the community's relationship with police. For example, individuals in more connected destinations might be more or less likely to report crimes to police or cooperate with investigations. Data limitations prevent a full examination of these issues, but the scope for under- or over-reporting of crimes is negligible for murder and relatively small for robbery and motor vehicle theft (Blumstein, 2000; Tibbetts, 2012). Consistent with this, we estimate similar effects on homicides measured in vital statistics data, which do not require a police report (Appendix D).

are larger in magnitude for murders committed by acquaintances and strangers than those committed by romantic partners or family members.<sup>34</sup>

To further examine mechanisms, Table 6 reports the effect of social connectedness by offender race, victim race, and circumstance. Column 2 reports the effect of social connectedness on murders committed by African Americans. While African Americans make up 16 percent of the population in our sample, they account for 56 percent of the murders for which offender race is available. African Americans constitute 82 percent of the victims of black murderers. Among this group, social connectedness especially reduces murders that coincide with gang and drug activity and other felonies. Social connectedness leads to a smaller, but still sizable, reduction in murders that occur alongside arguments. Social connectedness also reduces murders of non-black victims by black offenders.<sup>35</sup> We again see particular reductions in murders that coincide with gang and drug activity, other felonies, and arguments. Furthermore, murders of non-black victims are more likely to occur in these circumstances, partly explaining the row 8 coefficient being larger than row 2. Because African Americans are the vast majority of victims, the effect of social connectedness on murders of black victims by black offenders is quantitatively the most important.

Column 4 reports the effect of social connectedness on murders committed by non-black offenders. This reduction is driven primarily by fewer murders of black victims by non-black offenders, especially those associated with gang and drug activity, felonies, and arguments. Social connectedness also reduces murders of non-black victims by non-black offenders; these reductions are concentrated in gang and drug activity and felonies.

Overall, the results in Table 6 are consistent with social connectedness among African Americans from the South having a direct effect on black offenders and an indirect effect on non-black offenders through peer effects and spillovers. The simple framework in Section 3 describes this equilibrium. While most murders are intraracial, the presence of interracial spillovers, as seen in the SHR data and qualitative accounts of drug and turf wars (Block and Block, 1993; Quadracci, 2007; Audi, 2011), means that these spillovers are relevant. As crime can lead to more crime (e.g., through retaliatory murders), social connectedness can even reduce murders of non-black victims by non-black offenders. Further support for this interpretation comes from the fact that the interracial effects of social connectedness are on murders committed alongside gang, drug, and other felonious activity.

Based on all of these results and prior research (e.g., Stack, 1974; Nagin and Pogarsky, 2004; Heckman, Stixrud and Urzua, 2006; Heckman, Pinto and Savelyev, 2013; Heller

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<sup>33</sup>We also estimate significant reductions in murders of individuals age 36 and older. Most of these victims are killed by younger offenders. Furthermore, social connectedness likely has persistent effects on individuals, by changing norms and skills or by reducing the tendency of crime to beget more crime (Nagin and Paternoster, 1991); these persistent effects would reduce the probability of committing crime in adulthood, thus reducing the chances of being murdered.

<sup>34</sup>Appendix Table A.14 distinguishes between black and non-black victims. Results are similar for both groups, with the most notable difference being the relationship between victim and offender. For non-black victims, social connectedness has the largest effect on murders committed by strangers. For black victims, social connectedness has the largest effects on murders committed by family, acquaintances, and strangers, with a somewhat smaller effect on murders committed by romantic partners.

<sup>35</sup>While it would be interesting to distinguish non-black victims and offenders by Hispanic origin, SHR data do not identify individuals by both race and Hispanic origin before 2000.

et al., 2017; Stevenson, 2017), the most likely mechanisms appear to be non-cognitive skills, personality traits, and anti-crime norms. Prior research suggests that these factors play a large role in interactions that adolescents and young adults have with strangers and acquaintances. For example, non-cognitive skills such as self-control and forward-looking behavior could prevent the escalation of conflicts into violence.

## 6 Conclusion

This paper estimates the effect of social connectedness on crime across U.S. cities from 1970 to 2009. We use a new source of variation in social connectedness stemming from birth town migration networks among millions of African Americans from the South. A one standard deviation increase in social connectedness leads to a precisely estimated 21 percent decrease in murder and a 20 percent decrease in motor vehicle thefts. We find that social connectedness also leads to sizable and statistically significant reductions in rapes, robberies, assaults, and burglaries. Social connectedness reduces crimes that are more and less likely to have witnesses, which suggests that an increased detection probability is not the only mechanism. The effect of social connectedness on crime does not appear to be mediated by short-run effects on employment, education, homeownership, the prevalence of single parents, or crack cocaine use. Instead, effects on non-cognitive skills, personality traits, and norms are most likely. Social connectedness especially reduces murders of adolescents and young adults committed in the course of gang and drug activity.

Our results suggest that social connectedness, and the related concept of social capital, could help address market failures and generate desirable outcomes that are difficult to accomplish with government policies. The results also suggest that policies which disrupt social networks and communities, such as mass incarceration or the construction of interstate highways in the United States, could have negative consequences that are more severe than previously thought.

## Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

## Acknowledgments

Thanks to Martha Bailey, Dan Black, John Bound, Charlie Brown, Eric Chyn, John DiNardo, Steven Durlauf, Alan Griffith, James Heckman, Mike Mueller-Smith, Daniel Nagin, Seth Sanders, Jeff Smith, Lowell Taylor, Anthony Yezer, anonymous referees, and numerous seminar and conference participants for helpful comments. Thanks to Seth Sanders and Jim Vaupel for facilitating access to the Duke SSA/Medicare data. During work on this project, Stuart was supported in part by an NICHD training grant (T32 HD007339) and an NICHD center grant (R24 HD041028) to the Population Studies Center at the University of Michigan. Taylor thanks the Center for the Economics of Human Development at the University of Chicago.

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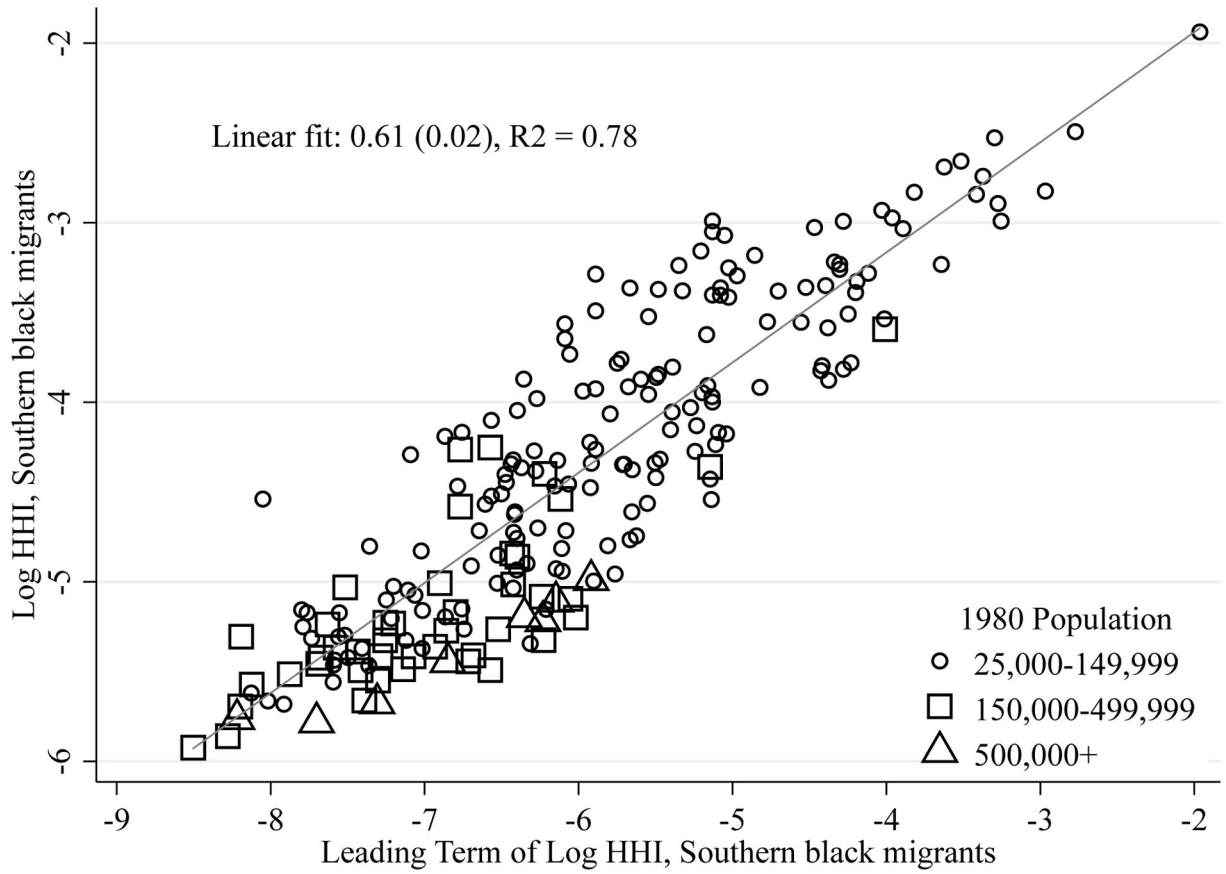
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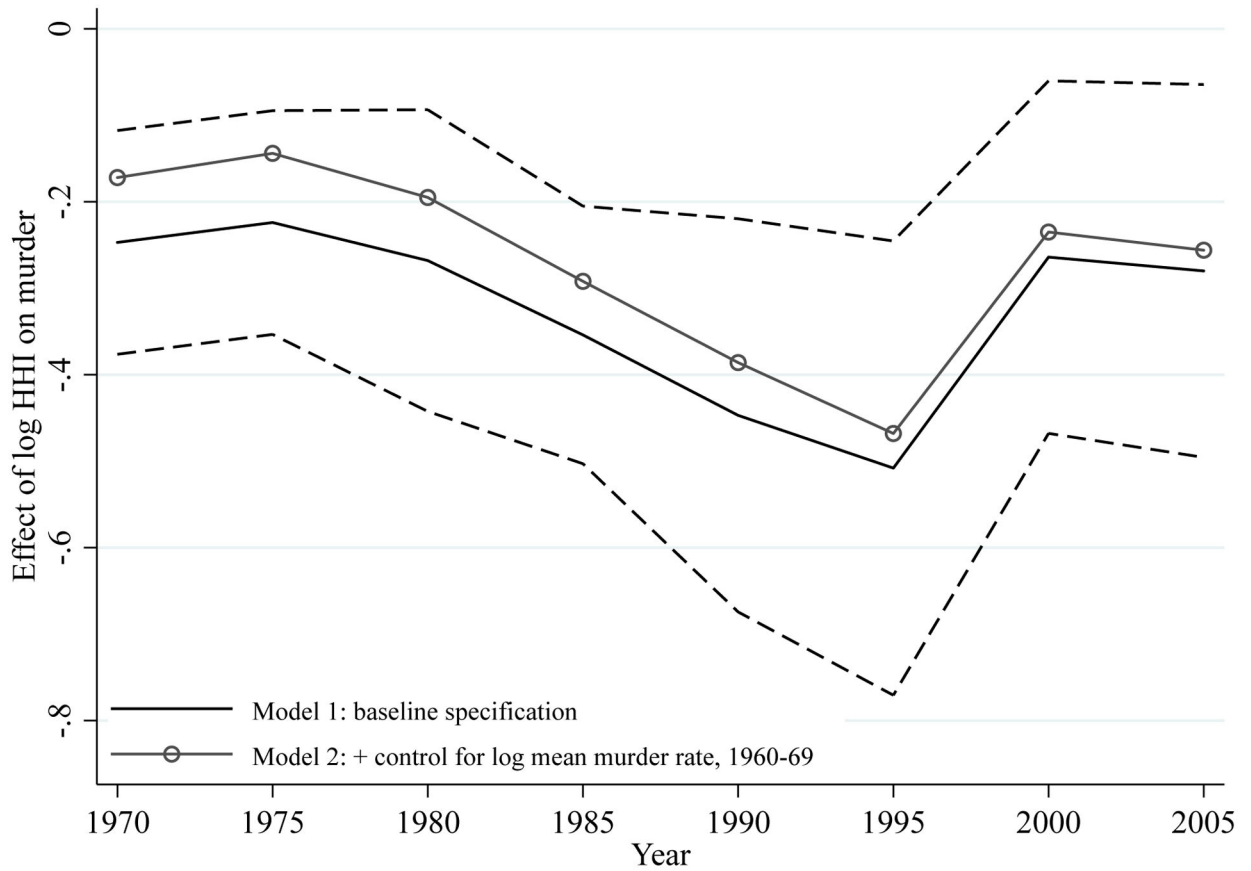
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**Figure 1:**

The Top Sending Town Accounts for Most of the Variation in Social Connectedness

Notes: The leading term of log HHI equals the log squared percent of migrants from the top sending town. Figure contains 224 cities. Source: Duke SSA/Medicare data



**Figure 2:**  
 The Effect of Social Connectedness on Murder, Assessing Threats to Empirical Strategy by Controlling for 1960–1969 Murder Rate  
 Notes: Figure shows point estimates and 95-percent confidence intervals from estimating equation (1) separately for years 1970–74, 1975–79, and so on. Model 1 includes the same covariates used in Table 2, and model 2 additionally controls for the log mean murder rate from 1960–69. Sources: See Table 2.

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**Table 1:**

## Key Correlates of Social Connectedness

Dependent variable: Log HHI, Southern black migrants			
	(1)	(2)	(3)
Log number Southern black migrants	-0.412 (0.020)	-0.456 (0.031)	-0.452 (0.033)
Log population, 1940		-0.150 (0.094)	-0.154 (0.095)
Percent black, 1940		0.577 (1.067)	-0.051 (1.445)
Log manufacturing employment, 1940		0.255 (0.065)	0.249 (0.069)
Log mean murder rate, 1936–1939			0.051 (0.064)
State fixed effects	x	x	x
N (cities)	195	195	195
R2	0.731	0.751	0.752

Notes: Sample contains cities in the North, Midwest, and West Census regions with at least 25,000 residents from 1940 to 2000 for which the mean murder rate from 1936–1939 is available. Log HHI and log number of migrants are measured between 1976 and 2001. Heteroskedastic-robust standard errors in parentheses. Sources: Duke SSA/Medicare data, Haines and ICPSR (2010), United States Bureau of the Census (2008), United States Department of Justice, Federal Bureau of Investigation (2005)



The Effect of Social Connectedness on Crime, 1970–2009

Table 2:

	Dependent variable: Number of offenses reported to police						
	Murder (1)	Rape (2)	Robbery (3)	Assault (4)	Burglary (5)	Larceny (6)	Motor Vehicle Theft (7)
Log HHI, Southern black migrants	-0.245 (0.064)	-0.105 (0.048)	-0.234 (0.045)	-0.221 (0.047)	-0.149 (0.032)	-0.069 (0.043)	-0.227 (0.083)
Log population and log land area	x	x	x	x	x	x	x
Log number, Southern black migrants	x	x	x	x	x	x	x
1920–1960 covariates	x	x	x	x	x	x	x
State-year fixed effects	x	x	x	x	x	x	x
Pseudo R2	0.823	0.871	0.947	0.914	0.952	0.945	0.935
N (city-years)	8,345	8,345	8,345	8,345	8,345	8,345	8,345
Cities	224	224	224	224	224	224	224

Notes: Table displays estimates of equation (1). 1920–1960 covariates are log population, percent black, and log manufacturing employment. Standard errors, clustered at the city level, are in parentheses. Sources: Duke SSA/Medicare data, Haines and ICPSR (2010), United States Bureau of the Census (2008), United States Department of Justice, Federal Bureau of Investigation (2005)

**Table 3:** The Effect of Social Connectedness on Murder, 1970–2009, Addressing Threats to Empirical Strategy

	Dependent variable: Number of murders reported to police								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Log HHI, Southern black migrants	-0.245 (0.064)	-0.239 (0.063)	-0.207 (0.056)	-0.247 (0.057)	-0.256 (0.061)	-0.209 (0.059)	-0.223 (0.065)	-0.212 (0.069)	-0.180 (0.055)
Log population and log land area	x	x	x	x	x	x	x	x	x
Log number, Southern black migrants	x	x	x	x	x	x	x	x	x
1920–1960 covariates	x	x	x	x	x	x	x	x	x
State-year fixed effects	x	x	x	x	x	x	x	x	x
Percent black and percent female									
Age and education covariates		x							x
Economic covariates				x					x
Log HHI, Southern white migrants					x				x
Log number, Southern white migrants					x				x
Log HHI, immigrants					x				x
Log number, immigrants					x				x
Racial fragmentation, percent Hispanic, percent foreign, English language skills						x			x
Birth county covariates							x		x
Share of Southern black migrants influenced by birth town migration network								x	x
Pseudo R2	0.823	0.825	0.836	0.832	0.827	0.832	0.824	0.823	0.840
N (city-years)	8,345	8,345	8,345	8,345	8,345	8,345	8,345	8,345	8,345
Cities	224	224	224	224	224	224	224	224	224

Notes: Table displays estimates of equation (1). 1920–1960 covariates are log population, percent black, and log manufacturing employment. Age and education covariates are percent age 5–17, 18–64, and 65+, percent with high school degree, and percent with college degree. Economic covariates are log median family income, unemployment rate, labor force participation rate, and log manufacturing employment. Racial fragmentation is one minus an HHI of racial population shares. English language skills are the share of people age 5+ who speak only English at home and the share who speak English well or very well (including those who only speak English). Birth county covariates are migrant-weighted averages of the black farm ownership rate, black literacy rate, black population density, percent black, and percent rural, all measured in the 1920 Census, plus Rosenwald school exposure. Column 8 includes an estimate of the share of migrants that chose their destination because of their birth town migration network. We estimate this variable using a structural model of location decisions, as described in the text. Standard errors, clustered at the city level, are in parentheses. Sources: Aaronson and Mazumder (2011), Duke SSA/Medicare data, Haines and ICPSR (2010), United States Bureau of the Census (2008), United States Department of Justice, Federal Bureau of Investigation (2005)

**Table 4:**

The Effect of Social Connectedness on Murder, 1980–1989, Possible Mechanisms

	Dependent variable: Number of murders reported to police				
	(1)	(2)	(3)	(4)	(5)
Log HHI, Southern black migrants	−0.232 (0.063)	−0.249 (0.065)	−0.232 (0.061)	−0.244 (0.064)	−0.243 (0.059)
Log population and log land area	x	x	x	x	x
Log number, Southern black migrants	x	x	x	x	x
1920–1960 covariates	x	x	x	x	x
State-year fixed effects	x	x	x	x	x
Black demographic and economic covariates		x			x
Black homeownership rate			x		x
Share of black households headed by single woman				x	x
Pseudo R2	0.829	0.833	0.829	0.830	0.834
N (city-years)	2,202	2,202	2,202	2,202	2,202
Cities	222	222	222	222	222

Notes: Table displays estimates of equation (1). 1920–1960 covariates are log population, percent black, and log manufacturing employment. Black demographic and economic covariates are percent age 5–17, 18–64, and 65+, percent female, percent with a high school degree, percent with a college degree, and unemployment rate. Standard errors, clustered at the city level, are in parentheses. Sources: Duke SSA/Medicare data, Haines and ICPSR (2010), United States Bureau of the Census (2008), United States Department of Justice, Federal Bureau of Investigation (2005)

**Table 5:**

The Effect of Social Connectedness on Murder, 1976–2009, By Circumstance and Victim Characteristic

		Share of all victims	Coefficient on Log HHI, Southern black migrants
(1)	All victims	1.00	−0.286 (0.071)
	Circumstance		
(2)	Gang and drug activity	0.10	−0.653 (0.171)
(3)	Felony	0.18	−0.328 (0.094)
(4)	Argument	0.31	−0.192 (0.077)
(5)	Other	0.13	−0.150 (0.078)
(6)	Unknown	0.28	−0.347 (0.136)
	Weapon		
(7)	Gun	0.63	−0.399 (0.104)
(8)	Other	0.33	−0.138 (0.045)
(9)	Unknown	0.04	−0.218 (0.112)
	Age of victim		
(10)	0–9	0.04	−0.181 (0.066)
(11)	10–17	0.06	−0.410 (0.110)
(12)	18–25	0.30	−0.357 (0.091)
(13)	26–35	0.27	−0.296 (0.070)
(14)	36+	0.32	−0.222 (0.064)
	Relationship between victim and offender		
(15)	Romantic partner	0.09	−0.123 (0.049)
(16)	Family	0.06	−0.117 (0.070)
(17)	Known, not family	0.30	−0.177 (0.067)
(18)	Stranger	0.16	−0.284 (0.101)
(19)	Unknown	0.40	−0.483 (0.142)

Notes: Table displays estimates of equation (1), using the same specification as Table 2. The dependent variable is the number of murders, by the indicated circumstance or victim characteristic. Standard errors, clustered at the city level, are in parentheses. Sources: Duke SSA/Medicare data, Haines and ICPSR (2010), United States Bureau of the Census (2008), United States Department of Justice, Federal Bureau of Investigation (2006)

**Table 6:**

The Effect of Social Connectedness on Murder, 1976–2009, By Offender Race, Victim Race, and Circumstance

		Black offenders		Non-black offenders	
		Share of all victims (1)	Coefficient on Log HHI, S. black migrants (2)	Share of all victims (3)	Coefficient on Log HHI, S. black migrants (4)
(1)	All victims	0.35	−0.201 (0.065)	0.28	−0.214 (0.077)
(2)	Black victims	0.29	−0.183 (0.070)	0.03	−0.361 (0.130)
(3)	Gang and drug activity	0.03	−0.412 (0.188)	<0.01	−0.917 (0.365)
(4)	Felony	0.03	−0.388 (0.110)	0.01	−0.344 (0.241)
(5)	Argument	0.14	−0.174 (0.085)	0.01	−0.446 (0.123)
(6)	Other	0.05	−0.110 (0.091)	<0.01	−0.062 (0.184)
(7)	Unknown	0.05	−0.076 (0.206)	<0.01	−0.152 (0.222)
(8)	Non-black victims	0.06	−0.301 (0.068)	0.25	−0.192 (0.078)
(9)	Gang and drug activity	0.01	−0.421 (0.214)	0.02	−0.781(0.231)
(10)	Felony	0.02	−0.392 (0.108)	0.04	−0.320 (0.112)
(11)	Argument	0.02	−0.275 (0.097)	0.11	−0.154 (0.105)
(12)	Other	0.01	−0.128 (0.097)	0.06	−0.118 (0.063)
(13)	Unknown	0.01	−0.261 (0.197)	0.03	−0.092 (0.099)

Notes: Table displays estimates of equation (1), using the same specification as Table 2. The dependent variable is the number of murders, by the indicated offender race, victim race, and circumstance. To ensure that all estimates in Table 6 can be interpreted as elasticities of the crime rate, we add log black population to the models in rows 2–7 and constrain the coefficient to equal one. In rows 8–13, we do the same for the log non-black population. We construct an annual black population estimate by combining Census annual population estimates with a linear extrapolation of the African American population share from decennial census data. Standard errors, clustered at the city level, are in parentheses. Sources: Duke SSA/Medicare data, Haines and ICPSR (2010), United States Bureau of the Census (2008), United States Department of Justice, Federal Bureau of Investigation (2006)