American Economic Journal: Applied Economics 2019, 11(1): 126–150
https://doi.org/10.1257/app.20170249

Partners in Crime[†]

By Stephen B. Billings, David J. Deming, and Stephen L. Ross*

Social interactions may explain the large variance in criminal activity across neighborhoods and time. We present direct evidence of social spillovers in crime using random variation in neighborhood residence along opposite sides of a newly drawn school boundary. We first show evidence for agglomeration effects—within small neighborhood areas, grouping more disadvantaged students together in the same school increases total crime. We then show that these youths are more likely to be arrested for committing crimes together—to be "partners in crime." Our results show that neighborhood and school segregation increase crime by fostering social interactions between at-risk youth. (*JEL* I24, I28, J13, K42, R11, R23, Z13)

Crime is an inherently social activity. Patterns of crime across neighborhoods and over time display strong evidence of social interactions (Glaeser, Sacerdote, and Scheinkman 1996).[1] Peer effects in criminal activity have been found within neighborhoods, schools, and juvenile corrections facilities (Ludwig, Duncan, and Hirshfield 2001; Kling, Ludwig, and Katz 2005; Ludwig and Kling 2007; Bayer, Hjalmarsson, and Pozen 2009; Patacchini and Zenou 2009; Deming 2011; Billings, Deming, and Rockoff 2014).[2] The available evidence suggests that concentrating disadvantaged youth together in the same environment leads to more total crime (Jacobson 2004; Cook and Ludwig 2005; Carrell and Hoekstra 2010; Deming 2011; Imberman, Kugler, and Sacerdote 2012; Billings, Deming, and Rockoff 2014; Damm and Dustmann 2014).

While there is strong evidence of agglomeration externalities for criminal behavior, the exact mechanism remains unclear. Proposed mechanisms for peer and spillover effects in crime include strained monitoring resources (Levitt 1997, Jacobson

*Billings: Leeds School of Business, University of Colorado, 995 Regents Drive, Boulder, CO 80309 (email: stephen.billings@colorado.edu); Deming: Harvard Kennedy School, Harvard Graduate School of Education, Harvard University, Gutman 411, Appian Way, Cambridge (email: david_deming@harvard.edu); Ross: Department of Economics, University of Connecticut, 341 Mansfield Rd, University of Connecticut, Storrs, CT 06269 (email: stephen.l.ross@uconn.edu). We thank Phil Cook, Jon Guryan, Mark Hoekstra, Edward Kung, Jason Lindo, and Justin McCrary for helpful comments/discussions as well as seminar participants at the Richmond Federal Reserve Regional Workshop 2015, University of Houston, University of Rochester, Wake Forest University, 2015 Southern Economic Association, 2015 Urban Economics Association Meetings, and the 2015 NBER SI Economics of Crime and Labor Studies Groups. We would also like to thank Brian Cunningham, Mike Humphrey, and Monica Nguyen of the Charlotte-Mecklenburg Police Department; Julia Rush of the Mecklenburg County Sheriff's Department; and Andy Baxter and Susan Freije from Charlotte-Mecklenburg Schools.

[†]Go to https://doi.org/10.1257/app.20170249 to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

[1]For examples of social interactions in other markets, see Bertrand, Luttmer, and Mullainathan (2000) on welfare program participation, Bayer, Ross, and Topa (2008) on labor referrals, Grinblatt, Keloharju, and Ikäheimo (2008) on automobile consumption, and Fletcher and Ross (2012) on health behaviors.

[2] See Ross (2011) for a recent review of the peer and neighborhood effects literatures more generally.

2004), shifting norms of behavior and reputation (Anderson 1999, Silverman 2004), learning about criminal opportunities (Sah 1991, Calvó-Armengol and Zenou 2004), conformism (Patacchini and Zenou 2009), and criminal network formation (Bayer, Hjalmarsson, and Pozen 2009). Each of these mechanisms implies distinct strategies to reduce crime. For example, if agglomeration externalities result from youth increasing criminal activity because they know that a fixed police presence is now spread across more potential offenders, the policy solution is to increase monitoring resources in those areas. On the other hand, if concentrating crime-prone youth together leads to the formation of denser and more active criminal networks, it may be necessary to directly reduce segregation through neighborhood mobility or school assignment policies (Billings, Deming, and Rockoff 2014; Damm and Dustmann 2014; Chetty, Hendren, and Katz 2016).

In this paper, we present direct evidence of the importance of school-based social interactions for generating youth criminal activity within neighborhoods. We first show that youth are more likely to be arrested when their neighborhood has a greater density of peers with similar characteristics. We then show, using quasi-random variation in school boundaries, that this agglomeration externality in crime is much greater when neighborhood peers also attend the same school.

We explore the role of direct peer interactions using a unique dataset of arrests involving criminal partnerships. Criminal partnerships are relatively common. Among offenders age 16–21, 28 percent of all arrests are for crimes with a partner, and the figure is higher for violent crimes. We first show that neighborhood peers are more likely to be arrested for criminal partnerships if they were assigned to the same school. The impacts of being assigned to the same school on neighborhood spillovers and on criminal partnership are found only for individuals who reside very near each other, within 1 kilometer. Moreover, in both cases, the estimates are largest when individuals are in the same school, grade, and of the same race and gender.

Our analysis uses school administrative data from Charlotte-Mecklenburg Schools (CMS) linked to arrest records from the Charlotte-Mecklenburg Police Department (CMPD). To identify the causal role of schools, we use quasi-random variation in school assignment that arises from a large and sudden change in the attendance zone boundaries. CMS redrew their attendance boundaries in 2002 in response to a court order that ended decades of race-based busing. We compare students who live in the same neighborhoods, but are on opposite sides of a newly drawn attendance boundary. We find that a 1 standard deviation increase in the number of same school-grade-race-gender students living within 1 kilometer increases the probability that a student will be arrested by 23 percent, with increases of 67 percent and 41 percent for violent and property crime, respectively.

We study criminal partnerships by pairing each youth within a neighborhood that was bisected by a new boundary to all other youth in CMS, and examining how the probability of criminal partnership varies with distance and school assignment. We find that criminal partnership is much more likely for students who live very close to each other, but only for youth who are also assigned to the same school. Our estimates suggest that two students residing within 1 kilometer of each other are six times more likely to be arrested together for a crime if they are assigned to the same school. Consistent with social interactions as a mechanism, we find that the partnership impacts are strongly increasing in length of time residing in both neighborhood and school. Taken together, these results suggest that the formation of criminal partnerships—"partners in crime"—through social interactions in school is an important mechanism underlying neighborhood clusters of youth criminal activity.

Our work follows Billings, Deming, and Rockoff (2014), who study the impact of the end of race-based busing in CMS using a similar design that compares students residing on opposite sides of the newly drawn school boundary. They find that minority males who attend a school with higher shares of minority students are more likely to be arrested and incarcerated. However, data limitations preclude them from understanding the mechanisms—in particular, whether increases in crime are due to social interactions between peers. Our results build on the findings of Billings, Deming, and Rockoff (2014) by showing that concentrations of minority youth in the same schools also increases arrests at the neighborhood level and that these arrests are more likely to involve same school neighbors being arrested *together*.[3]

A number of recent papers shed light on the mechanisms for peer effects in criminal activity. The most similar paper to ours is Bayer, Hjalmarsson, and Pozen (2009), who show that criminal peer effects are stronger when juveniles who have similar criminal expertise are grouped together in correctional facilities. While the evidence in Bayer, Hjalmarsson, and Pozen (2009) is strongly suggestive of the importance of social interactions between criminals, they do not observe direct evidence of joint activities and so, for example, cannot demonstrate directly that youth in juvenile facilities either learn from each other or subsequently commit crimes together. Another closely related paper is Damm and Dustmann (2014), who find that growing up in a high-crime neighborhood increases adult crime and that the impacts are driven by the share of criminals, presumably potential peers, rather than the number of crimes committed.

Our findings contribute to this literature by directly measuring peer interactions and by demonstrating that schools are an important social setting for explaining criminal partnership formation among neighborhood youth. While several other studies find important impacts of schools on crime, these studies primarily document the causal impact of the school on overall criminal activity, whereas we condition on school effects and highlight an additional role of schools in mediating the impact of neighborhoods on crime (Jacob and Lefgren 2003; Cullen, Jacob, and Levitt 2006; Deming 2011; Billings, Deming, and Rockoff 2014). Our findings suggest that neighborhood and school segregation itself may be partially responsible for high crime rates in disadvantaged urban areas. If concentrating disadvantaged youth together increases total crime, and if at least part of the mechanism is through the formation of social relationships and criminal partnerships in school, then the only way to disrupt this endogenous process is to manipulate the location or school assignment of youth across settings. Our results help explain why housing vouchers might reduce violent crime as in the Moving to Opportunity (MTO) experiment, or

[3] Billings, Deming, and Rockoff (2014) examines peer effects in crime that arise at the school or grade level. Our analysis examines neighborhood effects in crime showing that neighborhood spillovers are larger when students are assigned to the same school. It is notable that our models include school fixed effects so our results cannot be driven by the effects identified in Billings, Deming, and Rockoff (2014).

why attending a more segregated school might increase crime (Kling, Ludwig, and Katz 2005; Billings, Deming, and Rockoff 2014).

I. Data

Our sample is comprised of administrative records from CMS for all individual students that attended public school in the county. We limit the sample to students that we observe at age 14 between the 2002–2003 and 2008–2009 school years, as well as students for which we observe a residential address during this period.[4] The data include student gender, race, yearly end-of-grade (EOG) test scores, days absent, and days suspended from school. The EOG tests are standardized and administered across the state of North Carolina from 1993 to the present.

This sample allows us to identify the residential location of students two years prior to age 16, which is the age at which criminal offenders in North Carolina are included in the registry of all adult arrests. We link CMS data to arrest registry data for Mecklenburg County from 1998 to 2013 using first and last name as well as date of birth.[5] The arrest data includes individual names and identifiers, and information on the number and nature of charges.[6] We define "offenders" as students who were arrested by the Charlotte-Mecklenburg Police Department (CMPD) during our sample period between the ages of 16 and 21. While this data allow us to observe the future criminal behavior of CMS students, regardless of whether they transfer or drop out of school, they are limited to crimes committed within Mecklenburg County.[7]

Beginning in 2005, the registry of offenders was linked to records of all criminal incidents, so that officers could better understand crime patterns among repeat offenders. This data allows us to identify individuals that were arrested for the same crime, who we refer to as "partners." Approximately 22 percent of all reported crimes from 2005–2013 that led to an arrest of a 16- to 21-year-old were committed with one or more partners. Individuals ever arrested for a crime committed with one or more partners have 50 percent more arrests than offenders overall and are 79 percent more likely to have ever been arrested for a violent crime. Crimes committed by partners are disproportionately burglaries, robberies, and drug offenses.[8] The nature of partnerships for such crimes as burglary or robbery are clearer as these crimes often entail the use of force or the transport of goods quickly. Other partnerships, such as drug crimes, are more complicated and range from two drug users consuming together, to the sale of drugs, to the larger distribution of drugs between dealers.

[4] We lose approximately 2 percent of our sample due to missing or non-geocodable residential addresses.

[5] Our match rate between student and arrest records is 94 percent, and this same matching procedure has been incorporated and verified in Deming (2011) and Billings, Deming, and Rockoff (2014) for these two datasets.

[6] The Mecklenburg County Sheriff (MCS) tracks arrests across individuals using a unique identifier that is established with fingerprinting.

[7] Mecklenburg County contains Charlotte and the surrounding, relatively affluent suburbs. Most arrests are concentrated in and around the urban center. Further, the surrounding counties are lower density, have lower crime rates, and do not have any urban centers near the boundary with Mecklenburg County. As a result, it is unlikely that there are significant numbers of arrests of CMS youth in the surrounding counties, as evidenced by the fact that there are very few young adult arrestees in Mecklenburg County who were not observed in CMS schools.

[8] See online Appendix Figure A1 for the distribution of crime categories for all arrests as well as partnership arrests.

We define residential neighborhoods within Mecklenburg County using 373 block groups from the 2000 census. We identify 129 block groups that were bisected by middle and high school attendance zone boundaries that were newly drawn under redistricting in the summer of 2002.[9] The nature of this redistricting is important given our assumption of exogenous boundaries bisecting neighborhoods. Prior to 2003, CMS operated under a court-ordered desegregation plan that used busing to achieve racial integration in schools. A court case challenging this busing policy led to the end of court-ordered busing in the summer of 2002.[10] In order to adapt to the court order to end race-based busing, CMS dramatically redrew school attendance zone boundaries. Starting with the 2002–2003 academic year, school attendance boundaries were redrawn under a court order that prohibited the use of race in student assignment. Decisions about where to draw the boundaries were based on school capacity and the geographical concentration of students around a school (Smith 2004; Mickelson et al. 2009; Billings, Deming, and Rockoff 2014). This mechanical redistricting process rarely took advantage of environmental features such as streams and major roads, and was controversial because it often bisected existing neighborhoods.[11] The end of court-based busing led to approximately 50 percent of students being reassigned to a new school over the summer of 2002.

Our primary analysis involves the sample of students who attended public school at age 14 and resided in one of these bisected block groups between the 2002–2003 and 2008–2009 school years, and examining the criminal behavior of students in this sample. The sample contains one observation per student, and arrest outcomes are defined based on ever arrested or number of times arrested for specific crimes or during specific age ranges. Students are organized into same assigned grade cohorts based on comparing their birth date to the date of first eligibility for attending kindergarten. Our oldest cohort is 14 during the 2002–2003 academic year, and turns 16 during 2004–2005, coinciding with the beginning of our partnership data in 2005. We include arrest records for that cohort as late as age 21, which would be during the 2009–2010 school year. Our youngest cohort turned 14 during the 2008–2009 academic year, and we observe arrests for them through 2013, when they would be age 19. We also consider a sample based on all individual students who reside in one of those block groups prior to the fall of 2002 at any age and are age 14 or older sometime between 2002–2003 and 2008–2009. However, the assigned school in this second sample is quite noisy due to the high rates of residential mobility among our sample of student offenders.[12]

[9] See online Appendix Figure A2 for a map showing the new boundaries and bisected block groups. The bisected block groups are spread throughout the county, and the new boundaries often bisect multiple block groups.

[10] In 1997, a CMS parent whose child was denied entrance to a magnet school program based on race filed a lawsuit against the district (*Capacchione versus Charlotte-Mecklenburg Schools*). This case later escalated to a direct challenge of the busing policy in CMS.

[11] See Billings, Deming, and Rockoff (2014) for a detailed discussion. At a November 9, 1999 meeting of the CMS Board, Superintendent Eric Smith described the redistricting as "a mechanical process, not a human process. It simply draws [maps] based on capacity and numbers of children, it doesn't make any sense in terms of where children play, associations children naturally make as they are growing up, and it doesn't make any sense in terms of how families relate and interact."

[12] For example, only 35 percent of our main sample of offenders live at the same address at age 14 as they did in 2001, which translates to about 10 percent of pairs with unchanged addresses since 2001. Even with these high rates of mobility, we see partnership results using this prior to 2002 sample that are consistent with later results. See

Table 1 provides descriptive statistics for our sample of students residing in bisected block groups at age 14. Panel A presents arrest data for ages 16–21, panel B presents basic individual demographics, education outcomes, and school and neighborhood attributes, and panel C presents the number of peers within 1 kilometer under various restrictions. The first column shows means for the full sample of students, the second column presents means for all offenders (youth ever arrested by CMPD), and the final column presents means for all offenders who were ever arrested for committing a crime together with another offender in our sample. The arrest rates among our sample of students are high, with 17 percent of the sample ever being arrested and 3 percent of the sample being arrested for violent crimes.[13] Among those students ever arrested, the incidence of violent crime is 19 percent, but jumps to 34 percent for offenders who were arrested with partners. The overall rate of individuals ever involved in a criminal partnership for our sample of offenders is 28 percent, with offenders involved in criminal partnerships averaging 4.26 arrests and 1.70 unique partners. Offenders are more likely to be male, black, have low test scores, more absences and suspensions, reside in poorer neighborhoods, and reside near more same grade and same grade-same school peers than all students. Similar patterns arise comparing offenders to those offenders involved in criminal partnerships, more likely to be black, male, etc. Finally, panel C illustrates that the block groups that contain our sample are relatively densely populated with on average 200-300 similar aged peers within a kilometer of our sample students.

In order to create our sample of potential partnerships, we use our linked student and arrest data to draw a sample of all students or all offenders (ever arrested) living in bisected block groups. These student and offender-level samples are then turned into pairs of students by matching each student/offender in the sample with all students/offenders in the county whose age places them within three grades of the sample student/offender's assigned grade. We identify whether each resulting pair of students or offenders were ever arrested for the same crime, i.e., entered into a detected criminal partnership.[14] We exclude pairs less than 130 feet apart since this is the smallest distance upon which we find pairs assigned to different schools.[15] Since our data uniquely links each individual's arrest back to the CMPD's reported crime database, it allows us to determine if two individuals were arrested for the same crime even if each member of the partnership was arrested at different times.

Given the scale for which we later observe a relationship between distance and probability of partnership, most of our analysis focuses on pairs of students or

online Appendix Table A1 and Appendix Figures A3 and A4. For details on the construction of the table and the two figures, see discussions later in the text concerning Appendix Table A10 and Figures 2 and 3.

[13] Based on FBI Uniform Crime Reporting, we classify violent crimes as assault, kidnapping, rape, and robbery, while property crimes are auto theft, burglary, fraud/forgery, larceny, and criminal trespassing (attempted burglary).

[14] We limit analysis to individuals within 3 assigned grade levels since less than 5 percent of criminal partnerships involve individuals more than 3 years apart. Even with this restriction, the size of this dataset is substantial (over 30 million observations for even the smaller sample of just offenders), and thus we limit our analysis to pairs of individuals within certain distance thresholds.

[15] Our main results are slightly larger in magnitude if we include these pairs.

Table 1—Summary Statistics–Individuals

	All students	Ever arrested	Criminal partners
Panel A. Crime outcomes			
Ever arrested	0.17	1.00	1.00
	(0.37)	(0.00)	(0.00)
Ever arrested violent	0.03	0.19	0.34
	(0.18)	(0.40)	(0.47)
Ever arrested property	0.07	0.42	0.61
	(0.26)	(0.49)	(0.49)
Ever in crime partnership	0.05	0.28	1.00
	(0.21)	(0.45)	(0.00)
Number of arrests	0.48	2.83	4.26
	(1.60)	(2.91)	(3.78)
Number of unique partners	n/a	n/a	1.70
			(1.11)
Number of people per crime	n/a	1.44	2.71
		(0.73)	(0.91)
Panel B. Background characteristics			
Male	0.50	0.69	0.76
	(0.50)	(0.46)	(0.43)
Black	0.48	0.70	0.81
	(0.50)	(0.46)	(0.40)
Hispanic	0.11	0.08	0.07
	(0.31)	(0.27)	(0.25)
Single family residence	0.79	0.75	0.75
	(0.41)	(0.44)	(0.43)
Limited English proficiency	0.09	0.06	0.05
	(0.29)	(0.24)	(0.22)
Avg. test score	−0.05	−0.54	−0.66
	(0.98)	(0.87)	(0.84)
Total days absent	5.35	7.45	8.29
	(6.98)	(9.00)	(8.89)
Total days suspended from school	0.47	1.30	1.63
	(2.45)	(4.20)	(4.69)
CBG median HH income (000s)	56.90	48.73	45.91
	(20.79)	(18.22)	(16.83)
People per sq mile (000s)	2.11	2.39	2.57
	(1.82)	(1.94)	(1.99)
Panel C. Peer characteristics			
All peers (age +/− 3 years) within 1 km	238.80	259.98	280.23
	(120.96)	(125.41)	(124.69)
Same grade peers within 1 km	44.97	48.44	50.78
	(21.76)	(22.44)	(21.72)
Same grade and school peers within 1 km	40.36	42.55	44.20
	(20.27)	(20.84)	(20.70)
Same grade-race-gender peers within 1 km	11.86	14.37	15.77
	(9.20)	(9.82)	(9.90)
Same grade-race-gender and school peers within 1 km	10.57	12.53	13.63
	(8.30)	(8.78)	(8.96)
Observations	34,958	5,867	1,625

Notes: Means and standard deviations are reported above. All information regarding housing or neighborhoods is based on address at school age 14 under the assumption of starting kindergarten if age 5 by September 1 and normal grade progression. The sample of all students is based on students attending CMS at school age 14 at any time from 2003–2009 and living in a CBG bisected by a new 2002 middle or high school boundary. All crime outcomes are based on arrests in Mecklenburg County at ages 16 to 21. The column for "criminal partners" is based on those students that were "ever arrested" for a crime for which another student was also arrested for that crime.

offenders who live within 1 kilometer of each other.[16] For these samples of pairs, we observe 366 unique partners out of our possible 8,372,921 pairwise combinations of students or 123,982 pairwise combinations of offenders.[17] Our key analysis focuses on the relative share of observed partnerships for pairs assigned to the same school versus different schools. By construction, we expect that pairs in close proximity are more likely to be assigned to the same school, simply because it is less likely that a school attendance boundary was randomly drawn between two offenders when they live close together. Unconditionally, 85 percent of offender pairs are assigned to the same school, but 94 percent of criminal partners were assigned to the same school.[18] This relatively higher concentration of criminal partnerships for offender pairs assigned to the same school is the main insight from the partnership analysis. Online Appendix Tables A2 and A3 presents descriptive statistics for the sample of student and offender pairs based on assigned school.[19]

II. Empirical Strategy

A. *Peer Exposure and Crime Agglomeration*

First, we test whether increased opportunities for local interactions with same school peers affect the likelihood of committing a crime. We allow the probability that individual i in school s and neighborhood n is arrested for a crime (C_{isn}) to depend upon the number of students S who share student i's attributes (X_{isn}), reside within a specified distance (1 km of the student's precise residential location) and are also assigned to the same school. As a result, S can vary over space within the same neighborhood and attendance zone for identical students. Therefore, in addition to the traditional boundary analysis controls for neighborhood fixed effects (e.g., census geography) δ_n, we also include a control N for the total number of students sharing i's attributes within the specified distance regardless of school assignment.

[16] One kilometer is identified as the threshold for which the relationship between distance and probability of partnership approaches zero in our dataset.

[17] In order to put these numbers into context, we divide the number of offenders (5,867) by the number of bisected block groups (129) and find approximately 45 offenders per block group on average. The number of pairs from a block group with 45 offenders is $45 \times 44/2$ or 990, and if there were 129 block groups of this size, we would have 127,710 pairs, which is close to the actual sample size of 123,982. In the actual data, we have a partnership link rate of 0.3 percent, which when applied to this representative block group is consistent with 3 of the 45 offenders having partnership links with individuals within a kilometer or 6.6 percent. The reason that this number is much lower than the 28 percent partnership rate in the individual sample is that the potential partner sample only considers partnerships between individuals within 1 km of each other. On average, each offender resides within 1 km of approximately 44 offenders compared to 5,867 offenders within the sample (133 times more), and so even with the very low baseline rate of partnership outside the proximity threshold of 1 km, most of the partnerships lie outside that threshold.

[18] The high percentage of students assigned to the same school is expected even with quasi-random assignment across the attendance zone sides. Random variation in location will lead to more students being on one side of a boundary than on the other side. That random variation always increases the fraction of same school potential offenders because most of the students are on the attendance zone side with more students. If we extend the distance between pairs of students and thereby reduce the spatial sampling variation, this percentage decreases in value.

[19] We do observe some differences in the distribution of attributes between student/offender pairs that become partners and those that do not. Partners are more likely to be similar in age, both male and same race than non-partners. Online Appendix Tables A4 and A5 presents descriptive statistics for school attended.

Specifically, we estimate

$$(1)\quad C_{isn} = \alpha S\big(X_{jt,v-w} = X_{is,v-w} \,|\, d_{ij} < \bar{d}, s = t\big) + \beta_1 N\big(X_{jt,v-w} = X_{is,v-w} | d_{ij} < \bar{d}\big) + \delta_n + \gamma_s + \beta_X X_{isn} + \mu_{isn},$$

where S is the number of students j who are assigned to the same school and match student i on attributes v through w in the vector X_j. We also include fixed effects for the student's original neighborhood school prior to rezoning as well as neighborhood school after rezoning γ_s, and individual controls X_{isn}. We use assigned school and grade (based on birth date) in an "intent-to-treat" framework rather than actual school attended because some students attend magnet schools, are enrolled in other special programs, or have repeated a grade.

The key parameter in equation (1) is α, the coefficient on the number of same school potential peers. The parameter α is identified by comparing students in the same neighborhood who had the same original school assignment and similar overall number of potential peers, but who received different school assignments and so different exposure to same school peers under the new policy because they reside on opposite sides of a newly drawn attendance boundary. Standard errors are clustered at the neighborhood n level.

We start by estimating equation (1) for all nearby students. We then restrict the number of students counted in the construction of S to students in the same grade and cohort, and then to students in the same grade and demographic type (gender and race), and finally to students in the same grade and same risk category according to a constructed arrest risk index.[20] For each of these analyses, we also both modify N and expand the vector of neighborhood fixed effects to incorporate the identical same grade and similar student type (e.g., by race and gender, or additionally by quintiles of the composite measure of arrest risk) restrictions, i.e., number of proximate students of that cohort and type only and student cohort by type by neighborhood fixed effects, respectively.

B. *Criminal Partnership*

Next, we test for direct evidence of behaviors consistent with social interactions in crime. Specifically, we ask the following question: is an individual more likely to commit a crime with an individual who resides nearby if they are also assigned to the same school? We first examine this question for the full sample of all students, and then we restrict our consideration of criminal partnerships to individuals who were arrested for a crime with or without a partner. By conditioning on being arrested for a crime in our follow-up model, C_{isn}, we attempt to detect the effect of proximity on partnership above and beyond the direct effects on total criminal

[20] The two categories are high risk, students in the top quintile of the index, and all other students. We estimate arrest risk by regressing an indicator for ever being arrested on student attributes, using a sample of students that were rising ninth graders prior to 2002 and not involved in criminal partnerships.

activity from the analysis in Section IIIA that would naturally lead to additional arrests including partnership arrests.[21]

Concretely, let the probability of criminal partnership between individual i in neighborhood n and other individuals j in the sample (P_{isnjt}) depends upon both spatial proximity (d_{ij}) and assigned schools s and t, respectively:

$$P_{isnjt} = f(d_{ij}) + g(d_{ij})D(s = t) + \varepsilon_{isnjt}, \tag{2}$$

where f and g are functions that describe the relationship between the probability of partnership and distance (d_{ij}) between the two individuals; D is an indicator for whether the two individuals are assigned to the same school or, for example, the same school and grade; and ε_{isnkt} is an idiosyncratic error. The function f captures the reduced-form relationship over distance for pairs of students or offenders who are assigned to different schools, and our function of interest g captures the effect of school assignment on this relationship. Intuitively, equation (2) asks whether the probability of criminal partnership between any two offenders who live the same distance apart is greater when they also attend the same school.

Then, we extend the model by adding a neighborhood fixed effect δ_n based on i's neighborhood, controls X_{jt} for the individual j who is being paired with each individual i in neighborhood n, and fixed effects for j's assigned school t. Specifically,

$$P_{insjt} = f(d_{ij}) + g(d_{ij})D(s = t) + \delta_n + \beta X_{jt} + \gamma_t + \varepsilon_{insjt}. \tag{3}$$

The neighborhood fixed effect implies that g is identified by differences in the frequency of criminal partnership for two offenders who reside in the same neighborhood, but are on opposite sides of the school attendance boundary. We calculate standard errors with two-way clustering at both the neighborhood n of individual i in each pair level and the individual j level.

Our initial analyses estimate f and $f + g$ by creating a histogram of the distribution of criminal partnership frequency over distance separately for pairs of students or offenders in the same school and pairs in different schools. In our follow-up analyses, we restrict our sample to potential partners j who reside within a specified distance threshold $\overline{d}$ (again 1 kilometer for most of our analyses) of an individual i, and so f and $f + g$ are specified implicitly as step functions.[22]

In addition, we examine criminal partnership by type of crime, and whether criminal partnership is greater when students are assigned to the same grade or share similar characteristics, i.e., race and gender. Finally, we examine the association of partnership with actually attending the same school, and explore whether the association is greater when partners are in the same grade, as well as whether they shared a classroom together. However, unlike our results for school assignment and

[21] In order to further demonstrate that the findings on our pair results are not driven simply by the large number of arrests associated with the same school crime agglomeration effects, we also re-estimate these models adding a control for total number of arrests for both individuals in each pair. This has no impact on our estimated effects of being assigned to the same school.

[22] As robustness tests, we later estimate models that include distance bin fixed effects, individual fixed effects for each individual j, as well as models that control for individual j by neighborhood n fixed effects.

school and birth date implied grade assignment (which are arguably exogenous),[23] school attended, current grade, and assignment to classrooms is potentially biased by sorting, and so we treat these results as suggestive.

C. *Balancing Tests and Policing Zone Analysis*

The use of the newly drawn boundaries under the end of court-ordered racial-based busing in 2002 helps address concerns about boundaries correlating with neighborhood attributes, and the presence of both school and neighborhood fixed effects help address concerns about post-busing sorting based on neighborhood and school quality. To test for balance, we estimate equation (1) using same school potential peers S as the dependent variable and conduct an F-test for whether student attributes such as demographics, test scores, and school suspensions can explain S. We use four measures of potential same school peers S: number of students assigned to the same school, number of students assigned to the same school and grade (based on age), number of students assigned to the same school and grade who share the individuals' gender and race, and number of high-risk students assigned to the same school and grade (with risk estimated as described above).

The results are in Table 2. Each column regresses one of our four measures of same school peers on student attributes while controlling for overall proximate peer counts N for the same type of students and block group of residence by student-type fixed effects, wherein each specification the student type is based on the peer definitions above. Standard errors are clustered at the block group level. Only our first measure of peers—which looks across school grades—shows any relationship between student attributes and the number of same school peers with coefficients on Black and Hispanic being significant, and the joint significance of all coefficients at the 15 percent level. The within-grade models in columns 2, 3, and 4 all show that student attributes do not explain same school peers and those coefficients are small in magnitude with demographic dummies generating marginal effects of at most 0.01 standard deviations in same school peers.

We also conduct a balancing test for our analysis of criminal partnerships. The results are in Table 3. We regress whether individual i is assigned to the same school as individual j with which they are paired (columns 1 or 3) or assigned to the same school and grade (column 2 or 4) on attributes for individual j, while controlling for census block group fixed effects associated with the bisected block group. Columns 1 and 2 use the full sample of pairs, and columns 3 and 4 use pairs of offenders.

While the balancing tests for the full sample approach the 10 percent level of significance based on a sample of over 8 million pairs, the magnitudes are incredibly small. Moreover, the only significant coefficient in those models is the black indicator, and those estimates are very similar to the estimates for the offender sample. We find no evidence of imbalance in the sample of offender pairs. Overall, there is little evidence of meaningful sample imbalance across the attendance zone boundaries.

[23] Same grade in the school assigned models is based on starting kindergarten when an individual is age 5 by September 1 and normal grade progression.

Table 2—Balancing Test–Crime Agglomeration Models

	Same school peers (1)	Same school and grade peers (2)	Same school and grade-race-gender peers (3)	Same school and grade-race-gender high-risk peers (4)
Male	−0.0022	−0.0001		
	(0.0023)	(0.0011)		
Black	−0.0138	−0.0002		
	(0.0069)	(0.0013)		
Hispanic	−0.0231	−0.0009		
	(0.0108)	(0.0021)		
Single family residence	0.0359	−0.0032	0.0141	−0.0023
	(0.0345)	(0.0029)	(0.0284)	(0.0080)
Limited English proficiency	0.0011	−0.0000	−0.0045	−0.0040
	(0.0096)	(0.0021)	(0.0098)	(0.0079)
Avg. test scores	0.0001	0.0012	−0.0009	0.0016
	(0.0029)	(0.0008)	(0.0027)	(0.0021)
Total days absent	−0.0003	0.0000	−0.0002	−0.0001
	(0.0003)	(0.0001)	(0.0003)	(0.0002)
Total days suspended from school	0.0005	0.0005	0.0004	0.0002
	(0.0007)	(0.0003)	(0.0008)	(0.0007)
Observations	34,958	34,958	34,958	34,958
F-stat (p-value)	0.15	0.46	0.89	0.87
R^2	0.93	0.99	0.96	0.98

Notes: Standard errors are robust to arbitrary correlation within CBG. All dependent variables in column headings have been standardized to a mean of zero and a standard deviation of one. The dependent variable in column 1 indicates the number of peers that live within 1 kilometer and are assigned to the same middle or high school. Column 2 restricts the definition of peers in column 1 to be the same grade, column 3 further restricts to same gender and same race also. Column 4 defines peers based on same grade-race-gender peers that are also identified as high risk for arrest. To determine arrest risk, we conduct a first-stage regression of ever being arrested on student attributes for a sample of students that were rising ninth graders prior to 2002 and not involved in criminal partnerships. We define high risk based on those individuals that fall in the top quintile of predicted arrest using the first-stage estimated coefficients. The sample used for determining the number of peers is based on all students within three grade levels and attending CMS at school age 14 at any time from 2003–2009. Each column includes but does not report coffiecients for same neighborhood ($\leq$ 1 km) peer counts based on each column's definition of peers. For columns 2–4, we also include, but do not report, variables for total number of peers in neighborhood, as well as the total number of students assigned to the same school and neighborhood. All reported covariates for test scores, limited English proficiency, suspension days, and days absent are based on fifth grade and all models include, but do not report, an indicator if missing a test score or other fifth grade information and assigned school fixed effects. Column 1 includes Census Block Group 2000 (CBG), column 2 includes CBG by grade fixed effects, column 3 includes CBG by grade, gender, and race fixed effects. Column 4 includes CBG by grade, gender, race, and quintile of predicted arrest fixed effects.

An additional concern is that police may respond differently to students based on their assigned school. In that case, our results might pick up differences in police treatment rather than actual criminal behavior. In Charlotte, policing is determined by 13 policing divisions drawn based on 911 call volume. These boundaries were unchanged from prior to the 2002 redistricting until 2007. In 2007, policing divisions were completely redrawn and decreased to 12 divisions. This process lead to about 25 percent of block groups being served by a different division after 2007. In online Appendix Table A6, we test if being in a neighborhood that was bisected by a new school attendance boundary in 2002 explains these 2007 changes in policing assignments. We find no relationship between the drawing of a new school boundary and policing division assignments.

Table 3—Balancing Test–Partnership Models

	All students		Offenders only	
	Same school	Same school and grade	Same school	Same school and grade
	(1)	(2)	(3)	(4)
Male	−0.0008	−0.0002	0.0016	0.0001
	(0.0009)	(0.0002)	(0.0046)	(0.0010)
Black	−0.0093	−0.0016	−0.0074	−0.0005
	(0.0039)	(0.0008)	(0.0086)	(0.0020)
Hispanic	−0.0090	−0.0015	−0.0015	−0.0030
	(0.0061)	(0.0011)	(0.0148)	(0.0034)
Single family residence	0.0077	0.0012	0.0011	−0.0008
	(0.0141)	(0.0027)	(0.0193)	(0.0035)
Limited English proficiency	−0.0013	−0.0006	0.0112	0.0039
	(0.0047)	(0.0009)	(0.0095)	(0.0030)
Avg. test scores	−0.0000	0.0000	−0.0010	0.0011
	(0.0008)	(0.0002)	(0.0055)	(0.0012)
Total days absent	−0.0001	−0.0000	−0.0000	−0.0001
	(0.0001)	(0.0000)	(0.0003)	(0.0001)
Total days suspended from school	0.0002	0.0001	−0.0001	0.0001
	(0.0002)	(0.0000)	(0.0007)	(0.0001)
Observations	8,372,921	8,372,921	123,982	123,982
F-stat (p-value)	0.11	0.08	0.81	0.85
Dependent variable (mean)	0.90	0.17	0.85	0.16

Notes: Standard errors are robust to arbitrary correlation within CBG i and within student j. We include a total of 129 unique CBGs in our sample. All regressions include, but do not report, an indicator for missing a test score/absences, dummies for year individual j turned age five as of 9/1, assigned middle and high school fixed effects for j and CBG fixed effects for person i. Test scores, absences, limited English proficiency, and suspensions are based on fifth grade. We define assigned to the same school as two individuals being assigned to the same middle or high school based on 2002–2003 school attendance boundaries. Same grade is based on starting kindergarten at age five and normal grade progression. Columns 2 and 4 include an indicator if individual i and j are the same grade. F-statistics reports p-value that all reported covariates are jointly equal to zero.

III. Results

A. *Effects of Exposure to Potential Peers*

We begin our analysis by estimating equation (1), but without controlling for the number of same school potential peers or the block group fixed effects. Table 4 presents results for three measures of potential peers: all youth residing within 1 kilometer, all youth assigned to the same grade, and all youth assigned to the same grade and of the same gender and race, shown in columns 1–3, respectively. Turning to column 3, we find a strong positive correlation between criminal activity and potential neighborhood peers when those peers are the same age, gender, and race. The estimates in columns 1 and 2 are much smaller and mostly statistically insignificant.

Next, Table 5 presents models that estimate the impact of being assigned to the same school on the relationship between peer agglomeration and the likelihood of individual youths committing a crime (α). Again, using our sample of students from blocks that are bisected by a newly drawn school boundary, we regress indicators for ever having been arrested for any crime and for violent and property crimes

Table 4—Relationship between Neighborhood Peer Concentration and Arrests

	Ever arrested (1)	Ever arrested violent (2)	Ever arrested property (3)
Total peers	0.0027	0.0028	0.0015
	(0.0031)	(0.0015)	(0.0024)
Total same grade peers	0.0020	−0.0001	0.0009
	(0.0061)	(0.0030)	(0.0046)
Total same grade-race-gender peers	0.0110	0.0076	0.0037
	(0.0032)	(0.0015)	(0.0020)
Dependent variable (mean)	0.1678	0.0326	0.0713
Observations	34,958	34,958	34,958

Notes: Standard errors are robust to arbitrary correlation within CBG. All coefficients indicate the marginal effect of a standard deviation increase in the number of neighborhood peers on arrest outcomes. The top row of results is based on defining an individual's number of peers as all students within 1 kilometer, the second row expands to those students that are the same grade; the third row defines peers based on same grade, same gender, and same race. The sample used for determining the number of peers is based on all students within 3 grade levels and attending CMS at school age 14 at any time from 2003–2009. Each cell indicates a separate regression, and we include but do not report coefficients for total students in the same neighborhood for rows 2 and 3. All regressions include controls for gender, race, fifth grade reading and math test scores, learning English proficiency, indicator if missing a test score or other fifth grade information, days suspended (fifth grade), total days absent (fifth grade), single family home indicator, and assigned school fixed effects. "Ever arrested property" indicates that an individual was arrested for auto theft, burglary, fraud/forgery, or larceny between ages 16–21. "Ever arrested violent" indicates that an individual was arrested for aggravated/sexual/simple assault, rape, or robbery between ages 16–21.

respectively on the number of potential peers in the same school. These models include neighborhood fixed effects as well as controls for the number of potential peers, and thus the same school effect is identified by variation in potential peers on either side of a newly drawn attendance zone boundary.

As above, we define a student's potential same school peers in three ways: (i) nearby (within 1 kilometer); (ii) nearby, same assigned grade; and (iii) nearby, same assigned grade, same race, and same gender. Model 1 uses block group fixed effects, the same grade model uses block group by cohort fixed effects, model 3 uses block group by cohort by gender by race fixed effects, and the final model uses block group by cohort by race by gender by quintile over predicted likelihood of ever arrested fixed effects. The results for these four measures are shown in columns 1 through 3.

Column 1 of Table 5 presents results for ever arrested, and the next two columns present the results for ever arrested for a violent crime and ever arrested for a property crime. Panel A presents the results for number of same school peers, and the estimates are small and not significantly different from zero. Panel B presents results for same school-grade peers, and the estimates are larger overall and statistically significant for property crimes. When potential partners are defined based on same grade, race, and gender (panel C), we find large positive effects of same school potential partners on the likelihood of a student being arrested for any crime, a violent crime or a property crime.

Table 5—Crime Agglomeration Models

	Ever arrested (1)	Ever arrested violent (2)	Ever arrested property (3)
Panel A. Peers = All			
Same school peers	−0.0014	0.0008	0.0006
	(0.0095)	(0.0036)	(0.0066)
Panel B. Peers = same grade			
Same school peers	0.0076	0.0165	0.0358
	(0.0225)	(0.0122)	(0.0131)
Panel C. Peers = same grade-race-gender			
Same school peers	0.0389	0.0220	0.0286
	(0.0150)	(0.0083)	(0.0103)
Panel D. Peers = same grade-race-gender high risk			
Same school peers	0.0333	0.0300	0.0240
	(0.0189)	(0.0091)	(0.0137)
Panel E. Peers = same grade-race-gender			
Same school peers × minority × male	0.0608	0.0327	0.0420
	(0.0225)	(0.0109)	(0.0153)
Same school peers × minority × female	0.0246	0.0205	0.0244
	(0.0202)	(0.0100)	(0.0107)
Same school peers × white × male	0.0361	0.0010	0.0104
	(0.0381)	(0.0120)	(0.0186)
Same school peers × white × female	0.0234	0.0100	0.0147
	(0.0207)	(0.0075)	(0.0143)
Dependent variable (mean)	0.168	0.033	0.071
Dependent variable (mean)—minority male	0.304	0.074	0.138
Dependent variable (mean)—minority female	0.138	0.026	0.060
Dependent variable (mean)—white male	0.126	0.012	0.041
Dependent variable (mean)—white female	0.054	0.003	0.020
Observations	34,958	34,958	34,958

Notes: Standard errors are robust to arbitrary correlation within CBG. All coefficients indicate the marginal effect of a standard deviation increase in the number of peers on arrest outcomes. The top panel of results is based on defining an individual's number of peers as all students within 1 kilometer. The second panel expands to those students that are the same grade; and the third panel defines peers based on same grade, same gender, and same race. The fourth panel includes peer counts based on same grade-race-gender peers that are also identified as high risk for arrest. To determine arrest risk, we conduct a first-stage regression of ever being arrested on student attributes for a sample of students that were rising ninth graders prior to 2002 and not involved in criminal partnerships. We define high risk based on those individuals that fall in the top quintile of predicted arrest using the first-stage estimated coefficients. The sample used for determining the number of peers is based on all students within 3 grade levels and attending CMS at school age 14 at any time from 2003–2009. Each cell indicates a separate regression, and we include but do not report coefficients for total students in same school-neighborhood, total students in the same neighborhood, and same neighborhood counts for each peer definition. All regressions include controls for gender, race, fifth grade reading and math test scores, learning English proficiency, indicator if missing a test score or other fifth grade information, days suspended (fifth grade), total days absent (fifth grade), single family home indicator, and assigned school fixed effects. The top panel includes Census Block Group 2000 (CBG), the second panel includes CBG by grade, and the third panel includes CBG by grade, gender, and race fixed effects. The fourth panel includes CBG by grade, gender, race, and quintile of predicted arrest fixed effects. "Ever arrested property" indicates that an individual was arrested for auto theft, burglary, fraud/forgery, or larceny between ages 16–21. "Ever arrested violent" indicates that an individual was arrested for aggravated/sexual/simple assault, rape, or robbery between ages 16–21.

The coefficients on same school, grade, race, and gender peers indicate that a standard deviation increase (8.3 students) in same school peers increases the probability of ever being arrested by 3.9 percentage points, a 23 percent increase in the probability of arrest relative to the average student. A standard deviation increase in the number of same school peers that are the same age, race, and gender increases the probability of violent or property crime arrest by 2.2 and 2.9 percentage points, respectively. These effects represent relatively large increases of 67 percent and 41 percent in the probability of arrest for violent or property crime, respectively. We also find very large impacts on violent crime for high risk students.[24]

Panel F of Table 5 presents estimates separately for four gender and race groups—minority males, minority females, white males, and white females—using all four groups separately to define peers. All estimates are positive and sizable on ever arrested, but the estimates are largest and statistically significant for minority males. Minority males are over 6 percentage points more likely to be arrested for any crime if they have a 1 standard deviation increase in same school, minority male peers in their neighborhood, and about 3 percentage points more likely to be arrested for violent crimes. For violent and property crimes, we also find sizable effects for minority females.

In order to examine the spatial scale of the agglomeration results, we ran a series of regressions that redefined our peers as individuals within varying distance bands of 0–1 kilometers (km), 1–2 km, 2–3 km, 3–4 km, and 4–5 km. We re-estimated our main results while defining peers based on distance bands of 0–1 km, 1–2 km, 2–3 km, 3–4 km, and 4–5 km. Figure 1 presents results for models using the outcome of ever arrested and indicates that we only observe effects from peers for our less than 1 km definition of neighborhood. Results for larger distance bands are all close to zero and are typically more precisely estimated due to the larger number of different school peers. Results for other arrest outcomes are similar to this figure.

B. *Robustness Checks for Exposure Results*

One additional concern is that our results are biased by recent movers. However, online Appendix Table A8 presents suggestive evidence of a positive interaction between exposure to same school peers and having lived in the block group since 2001, which implies larger effects for individuals who have resided in the neighborhood longer. If our results were biased by residential relocation, we would expect larger effects for more recent movers. Instead, we find the opposite.

Finally, in online Appendix Table A9 we conduct a counterfactual analysis based on randomly shifting school attendance zone boundaries by between 1 km and 2 km in every direction. The random boundaries will bisect neighborhoods in the middle of attendance zones so that students on either side of the boundary are assigned to the same school. We would expect there to be no relationship between an individual's arrest outcomes and nearby peer same school concentration based on these artificial boundaries conditional on overall peers because students on either side of the false boundary have approximately the same number of actual same school peers.

[24] Online Appendix Table A7 shows that the coefficient on same school, grade, race, and gender peers for number of arrests is 0.21 arrests per student. Given that the average number of arrests among students ever arrested is 2.83, the increase in the rate of ever arrested from Table 5 can in principle explain 0.11 arrests per person in the full sample of all students, or about half of the same school exposure effect on number of arrests.

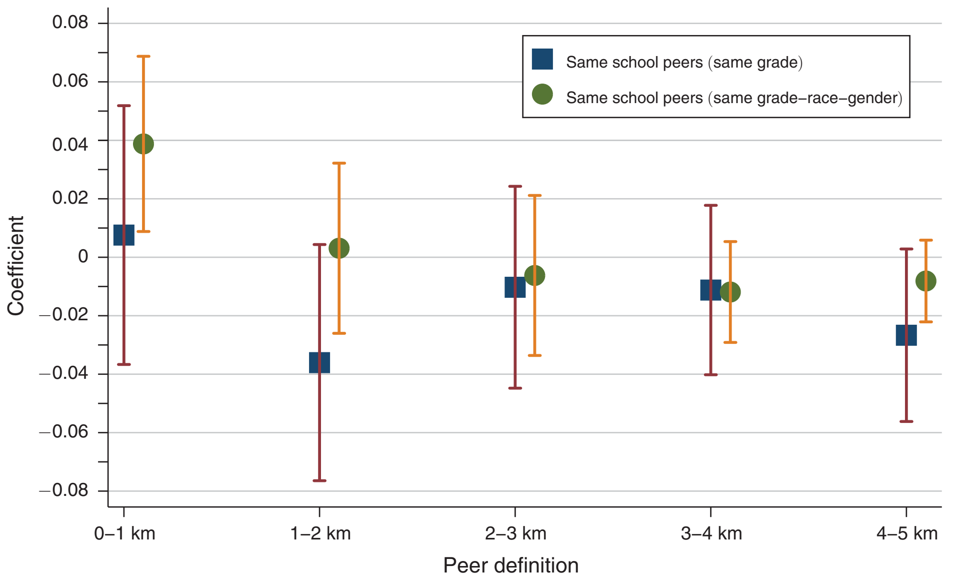

Figure 1. Crime Agglomeration by Distance Bands

Notes: This figure provides the estimated coefficient of a standard deviation increase in same school and neighborhood peer counts and 95 percent confidence interval for a series of estimates of equation (1), where we vary definitions of peers based on student attributes as well as distance bands away from an individual upon which to define peers. All regressions include controls for gender, race, fifth grade reading and math test scores, indicator if missing a test score or other fifth grade information, days suspended (fifth grade), total days absent (fifth grade), single family home indicator, limited English proficiency, assigned school fixed effects, total number of students within a distance interval, total number of students within a distance interval and assigned to the same school, and the number of same age peers within a given distance band for a given peer attribute definition. All models include Census Block Group 2000 (CBG) by peer attribute definition fixed effects. Standard errors robust to arbitrary correlation within CBG. Dependent Variable is an indicator for Ever Arrested (16–21).

Since we want to conduct this falsification for a number of boundary shifts, we randomly shift attendance boundaries and recalculate school assignment 100 times. We then re-estimate our main results in Table 5 but report the mean and standard deviation of our coefficient across these 100 replications. The estimates on "falsely assigned same school" potential peers are always substantially smaller in magnitude than the significant estimates in Table 5.[25]

C. *Criminal Partnerships*

Our agglomeration analysis establishes key facts about residential location spillovers arising from the concentration of youth. Location spillovers are very proximate, happening within 1 kilometer, and concentrated among youth who are assigned to the same school. Further, the spillovers arise entirely for youth that are similar in age, and are concentrated among youth that match on gender and race, and finally

[25] Across peer definitions and our three arrest types, 0 out of our 100 replications using false boundaries exceed our statistically significant main results in Table 5. For insignificant results, we do find a nonzero portion of replications that exceed our main results.

are largest among minority males. Having established these key empirical patterns, we now turn to our partnership analysis in order to establish whether similar patterns arise in the likelihood of being arrested for committing a crime with a partner.

We begin with graphical results that display the relationship between distance and the probability of criminal partnerships. Figure 2 plots the probability of a pair of students being arrested together for at least one crime as a function of the distance between the students. We show results separately for two groups—students assigned to same school and grade, and students assigned to different schools and the same grade. In both models, we condition on block group fixed effects associated with the residence of offender i, and covariates for the observed attributes of offender j including school fixed effects.

Figure 2 shows that the probability of partnership is high when the students reside within a few hundred feet of each other and are assigned to the same school, but declines quickly toward zero once potential partners are almost 1 kilometer distant or more. The probability of partnership for offenders who are assigned different schools is small and does not demonstrate any significant relationship with distance. This constitutes strong prima facie evidence that attending the same school increases the likelihood of criminal partnership for students who live in the same neighborhoods. The results are not sensitive to controlling for additional covariates. Online Appendix Figure A5 presents results using the sample of offenders or ever arrested students, and the results are very similar.

Figure 3 presents the difference between our conditional probabilities of partnerships for the two groups, or our estimate of the function g from equation (3). The 95 percent confidence intervals are represented by the shaded area. The differences are statistically significant for pairs who are located within about 2/3 km of each other.[26] Overall, these results highlight a strong positive relationship between shared residential proximity and criminal partnerships for individuals who are assigned to the same school. Online Appendix Figure A6 presents analogous results for the sample of offenders.

Table 6 presents estimates of equation (3) based on a step function that only includes pairs who are within 1 kilometer of each other, with being arrested together for at least one crime as the outcome. Panel 1 presents the results for the full sample of student pairs, and panel 2 presents results for pairs of offenders. The estimates and means in panel 1 are scaled up by a factor of 100 to represent percentage points. All models include block group fixed effects, individual and pair-level covariates, and fixed effects for student j's assigned school.

For column 1 in panel 1, being assigned to the same school increases the probability of being criminal partners by 0.0037 percentage points and being assigned to the same grade increases this probability by an additional 0.0051 percentage points.[27] Overall, being assigned to the same school and grade makes two individuals 6.5 times more likely to be arrested as part of a criminal partnership, increasing from

[26] The standard errors are bootstrapped based on resampling from the data 500 times.

[27] The probabilities for the all student sample are substantially smaller than the offender sample because of the differences in samples sizes, but the same total number of partnerships. We scale (00s) the all student sample to make it more readable, but the relevant interpretation is the size of the coefficient relative to the mean of the dependent variable.

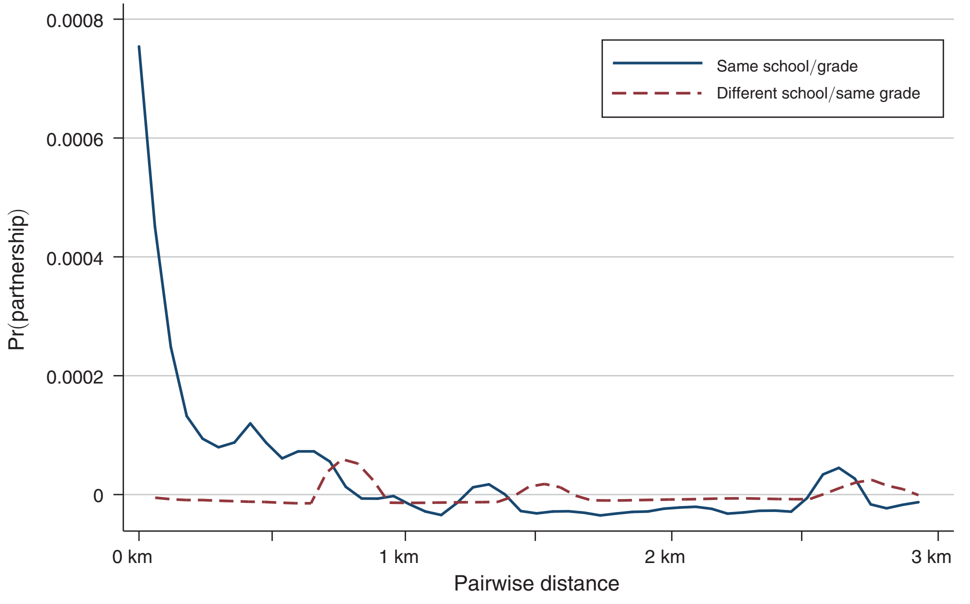

Figure 2. Conditional Probabilities of Partnership (*Same School/Grade versus Different School/Same Grade*)

Notes: This figure provides the distribution of partnership probabilities conditional on individual and neighborhood attributes for our sample of student pairs. The solid line represents pairs assigned to the same middle or high school and the same grade, while the dotted line represents pairs assigned to different schools and the same grade. The x-axis indicates the pairwise distance between each individual's home address (while in school) and conditional probabilities are based on the residuals from a first-stage regression that controls for individual attributes of person j (gender, race, lep, test scores, absences, suspensions, assigned school fixed effects), school year born fixed effects for k, and CBG fixed effects for i. We also implement kernel-weighted local polynomial smoothing in order to generate a continuous distribution of conditional probabilities. The sample included in this figure represents all pairs of students who are 3 years or less apart in age (less than 5 percent of criminal partners are more than 3 years apart), live within 3 kilometers of each other based on school age 14 address (2003–2009 school years), and at least one student resides in a Census Block Group (CBG) bisected by a new middle or high school attendance zone boundary.

a mean probability of 0.0016 percentage points for different school pairs to 0.0104 percentage points for same school and grade pairs. For the sample of offenders, the basic results are the same with the likelihood of partnership for pairs in the same school and grade being six times larger than the baseline probability. The estimates above are based on students' assigned schools—treatment on the treated estimates that use assigned school as an instrument for actual school attended yield estimates that are 10 and 11 times larger than the baseline probabilities for all students and for offenders, respectively.[28]

The rest of the columns in Table 6 present the model for whether individuals were partners at age 16–18, partners at age 19–21, and finally partnerships for specific crime classifications.[29] Partnership effects for both same school and same grade

[28] For the sample of all students, 64 percent attend their assigned school and 47 percent attend their assigned school and grade. Using just students that were arrested, 57 percent attend their assigned school and 38 percent attend their assigned school and grade. These figures are used to scale the coefficient estimates from Table 6 for the TOT results reported above.

[29] Online Appendix Table A10 shows results for assigned school by further disaggregated types of crime with assault and burglary generating the largest effects.

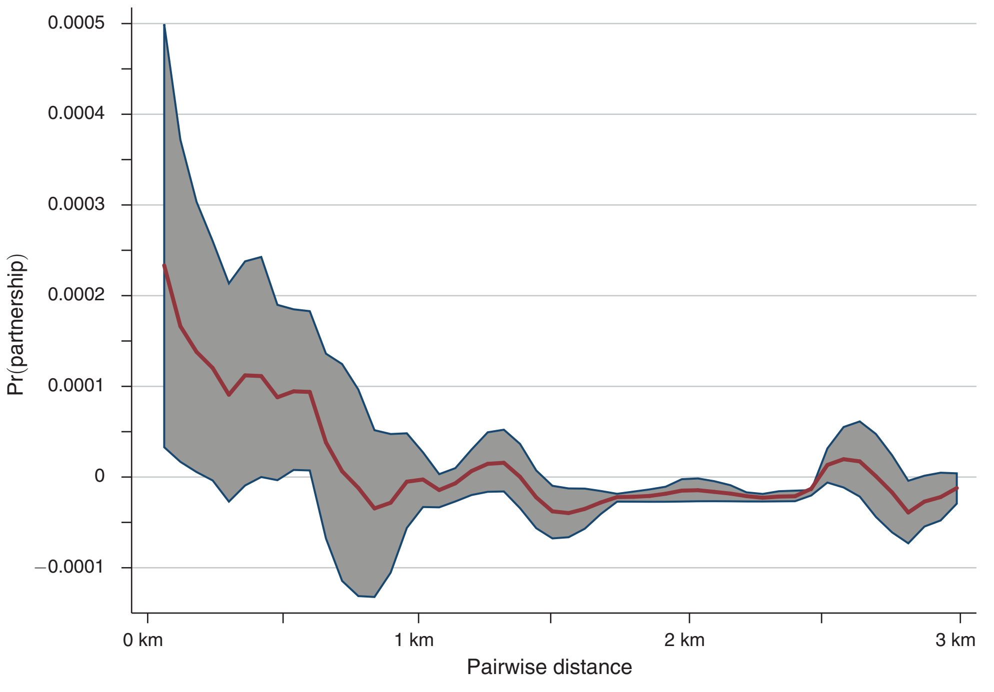

Figure 3. Difference in Conditional Probabilities of Partnership
(*Same School/Grade versus Different School/Same Grade*)

Notes: This figure provides the difference in conditional probability (residuals) of partnership between same school and grade and different school and grade pairs from Figure 2. The solid line indicates same school/grade minus different school partnership probabilities; 95 percent confidence intervals are given by the shaded area and we derive confidence intervals based on resampling with replacement and recalculating partnership probabilities for each 200 foot distance interval using 500 replications. We also implement kernel-weighted local polynomial smoothing in order to generate a continuous distribution of differences in conditional probabilities.

persist except for the 19–21 age samples where effects are primarily at the same school level. The fact that age 16–18 and age 19–21 results differ for same school and grade coefficients is consistent with partnerships within grade being more likely during years that individuals are still in school or recently dropped out of school. We find relatively similar results for both violent and property crimes.

Online Appendix Figures A7 and A8 present falsification tests that randomly shift all attendance boundaries by between 1 and 2 kms for the two samples. Specifically, we construct new versions of Figure 3 where we assign our main sample of students or offender pairs to same or different schools based on these false boundaries. We repeated this exercise 500 times in order to create a distribution of false boundary discontinuities and present the average results as a solid line and a 95 percent confidence interval as the shaded area in Figure 3. This figure does not demonstrate any relationship between falsely assigned same school and partnership based on spatially relevant, artificial attendance zone boundaries. Finally, we also demonstrate the robustness of our estimates by repeating the analysis in Figure 2 using the 2001 address of students and offenders, see online Appendix Figures A3 and A4, respectively.

Table 6—Impact of School Assignment on Criminal Partnerships

	Any crime partner (1)	16–18 yr. old partners (2)	19–21 yr. old partners (3)	Violent crime partners (4)	Property crime partners (5)
Panel A. All students ($\beta \times 100$)					
Assigned same school and grade	0.0051	0.0043	0.0007	0.0018	0.0030
	(0.0013)	(0.0012)	(0.0007)	(0.0007)	(0.0010)
Assigned same school	0.0037	0.0023	0.0019	0.0009	0.0026
	(0.0011)	(0.0009)	(0.0007)	(0.0005)	(0.0010)
Dependent variable (mean) for diff. school (00s)	0.0016	0.0016	0.0005	0.0005	0.0009
Observations	8,372,921	8,372,921	8,372,921	8,372,921	8,372,921
Panel B. Just offenders					
Assigned same school and grade	0.0033	0.0027	0.0006	0.0013	0.0018
	(0.0007)	(0.0007)	(0.0004)	(0.0004)	(0.0007)
Assigned same school	0.0021	0.0013	0.0012	0.0005	0.0015
	(0.0007)	(0.0006)	(0.0004)	(0.0003)	(0.0005)
Dependent variable (mean) for diff. school	0.0011	0.0011	0.0003	0.0004	0.0006
Observations	123,982	123,982	123,982	123,982	123,982

Notes: Standard errors are robust to arbitrary correlation within CBG i and within student j. The samples used in this table only include pairs where individuals are within 1 kilometer of each other. All regressions include controls for gender, race, learning English proficiency, fifth grade reading and math test scores, indicator if missing a test score or other fifth grade information, days suspended (fifth grade), total days absent (fifth grade), single family home indicator, indicator for year individual j turned age five as of 9/1, assigned middle and high school fixed effects, and CBG fixed effects for person i. We also include an indicator if individuals i and j are the same assigned grade. Dependent variable is an indicator based on the column heading. Given the large number of pairs in the top panel, we scale partner outcomes to be percentage point units ($\beta \times 100$). Number of partner crimes indicates the number of times a pair of individuals were arrested for the same crime. Columns for 16–18 and 19–21 yr. old indicates the age group for which one of the partners belonged at the time of arrest. Property crime partnerships include partnerships where at least one individual was arrested for auto theft, burglary, fraud/forgery, or larceny. Violent crime partnerships include partnerships where at least one individual was arrested for aggravated/sexual/simple assault, rape, or robbery.

D. *Heterogeneity Results and Comparison to Agglomeration Results*

Table 7 presents results from models for same school and grade partnerships, both overall and by race and gender. We re-estimate our main model from Table 6 and add interactions between same grade and gender controls and dummy variables for whether individual i is a minority male, a minority female, a non-minority male, or a non-minority female. We also include all the associated two-way interaction terms, although these are not reported in the table.

We find strong effects of increased partnership when the offenders in the pair are in the same school and grade, are both minority males and more modest effects for both minority females. These results are robust for the offender sample, the 16–18 age group subsample, and for minority males robust for 19–21 year olds and both violent and property crimes. The same school-grade-race-gender effects are noticeably larger than the same school-grade effects in Table 6. Calculating a weighted average of the point estimates in panels A and B of Table 7 yields likelihoods of partnership that are 10 and 12 times larger than the baseline probability, respectively. These patterns are very similar to the patterns uncovered in the agglomeration analysis. Interestingly, the positive main effect coefficients for same school assignment imply a significant increase in cross-race partnerships as well.

Table 7—Interaction Effects of School Assigned on Criminal Partnerships

	Any crime partner (1)	16–18 yr. old partners (2)	19–21 yr. old partners (3)	Violent crime partners (4)	Property crime partners (5)
Panel A. All students ($\beta \times 100$)					
Assigned to same school	0.0036	0.0022	0.0019	0.0009	0.0025
	(0.0010)	(0.0009)	(0.0007)	(0.0005)	(0.0009)
× Same grade	0.0001	0.0007	−0.0008	0.0002	−0.0001
	(0.0012)	(0.0012)	(0.0006)	(0.0005)	(0.0008)
× Same grade-race-gender × minority male	0.0332	0.0220	0.0100	0.0106	0.0225
	(0.0075)	(0.0063)	(0.0035)	(0.0040)	(0.0065)
× Same grade-race-gender × minority female	0.0031	0.0031	0.0000	0.0020	0.0011
	(0.0014)	(0.0013)	(0.0004)	(0.0012)	(0.0008)
× Same grade-race-gender × nonminority male	0.0063	0.0065	0.0023	−0.0002	0.0019
	(0.0056)	(0.0056)	(0.0026)	(0.0003)	(0.0025)
× Same grade-race-gender × nonminority female	0.0027	0.0016	0.0019	0.0006	0.0017
	(0.0007)	(0.0005)	(0.0005)	(0.0003)	(0.0004)
Dependent variable (mean) for diff. school (00s)	0.0016	0.0016	0.0005	0.0005	0.0009
Observations	8,372,921	8,372,921	8,372,921	8,372,921	8,372,921
Panel B. Just offenders					
Assigned to same school	0.0023	0.0015	0.0013	0.0005	0.0016
	(0.0007)	(0.0006)	(0.0004)	(0.0003)	(0.0005)
× Same grade	−0.0015	−0.0010	−0.0010	−0.0003	−0.0009
	(0.0005)	(0.0005)	(0.0003)	(0.0003)	(0.0003)
× Same grade-race-gender × minority male	0.0098	0.0065	0.0032	0.0030	0.0066
	(0.0020)	(0.0018)	(0.0010)	(0.0011)	(0.0018)
× Same grade-race-gender × minority female	0.0043	0.0043	0.0001	0.0027	0.0014
	(0.0016)	(0.0016)	(0.0003)	(0.0014)	(0.0009)
× Same grade-race-gender × nonminority male	0.0332	0.0336	0.0151	−0.0004	0.0095
	(0.0231)	(0.0232)	(0.0117)	(0.0003)	(0.0099)
× Same grade-race-gender × nonminority female	0.0250	0.0249	0.0004	−0.0003	0.0250
	(0.0253)	(0.0252)	(0.0006)	(0.0004)	(0.0253)
Dependent variable (mean) for diff. school	0.0011	0.0011	0.0003	0.0004	0.0006
Observations	123,982	123,982	123,982	123,982	123,982

Notes: All regressions include controls for gender, race, learning English proficiency, fifth grade reading and math test scores, indicator if missing a test score or other fifth grade information, days suspended (fifth grade), total days absent (fifth grade), single family home indicator, indicator for year individual j turned age five as of 9/1, assigned middle and high school fixed effects, and CBG fixed effects for person i. Standard errors are robust to arbitrary correlation within CBG i and within student j. All regressions include, but do not report, indicators for all variables used as an interaction with the assigned to same school variable.

To compare the magnitude of partnership effects to the exposure effects estimated in Section IVA, online Appendix Table A11 presents results from exposure models for ever arrested for a crime where the student's partner in that crime was also arrested. While the incidence of partnership within the paired sample of students is very small, the aggregate partnership effects are comparable in magnitude to the exposure effects estimated in the previous section.[30]

[30] Focusing on same school-grade-race-gender peers, the same school exposure estimate for ever arrested in Table 5 is 0.389 and for ever arrested with a partner is 0.202 or 52 percent of the ever arrested effect. The effects

E. *Robustness Tests*

Online Appendix Table A12 examines the sensitivity of our main estimates to a variety of different assumptions.[31] The only noticeable difference across these specifications is that we find smaller impacts when we base school assignment only on high school attendance zone boundaries. This is consistent with middle school being an important setting for forming and developing criminal partnerships.

Online Appendix Tables A13 presents results that interact our main independent variables with indicators for whether a student has lived at the same address since 2001. In general, we find larger effects for longer term residents, which limits concerns that our results are explained by sorting after the boundary change. Online Appendix Table A1 also shows that our main results are robust to using each student's Fall 2001 address.

Online Appendix Tables A14 present results using actual school attended, rather than assigned school and includes student *j* fixed effects. We define potential partners based on same school, same school and same grade, and then whether they attended at least two classes together. We find very large impacts of same course attendance in our samples of students and offenders. Online Appendix Table A15 shows further that partnerships are more likely for pairs that also attended the same elementary school. Overall, these results are consistent with the importance of repeated exposure to potential criminal partners.

IV. Conclusion

In this paper, we study the influence of schools and neighborhoods on criminal partnership. We find evidence that neighborhood spillovers in crime based on exposure to same race and gender peers are larger when those peers are assigned to the same school. These effects only arise when the students reside in close proximity to each other, and the effects are strongest when the students are also assigned to the same grade. These effects are driven primarily by minority males, and are strongest for peers with high estimated arrest risk. Given strong homophily in friendship patterns (Weinberg 2007; Fletcher, Ross, and Zhang 2013), these findings are consistent with social interactions being a significant contributor to youth crime.

Our analysis of criminal partnership demonstrates the importance of social interactions for determining criminal activity. We find very similar patterns on the likelihood of individual offenders being arrested for the same crime, i.e., partnering together to commit a crime. The effects of proximity on partnership are strong and decay rapidly over space. These effects are only observed when the offenders were assigned to the same school, and again are stronger when the offenders were also

for violent crime partnership and property crime partnership are 38 percent and 52 percent, respectively, relative to ever arrested for such crimes. This ever partner effect is also four times larger than the base rate of partnership arrests of 0.05.

[31] Inclusion of distance bin fixed effects every 200 feet, only pairs within 1/2 km sample with distance bin fixed effects, student *j* fixed effects, only pairs across high school attendance zone boundaries, high school sample with student fixed effects, and neighborhood by student fixed effects.

assigned to the same grade. The effects are strongest when the offenders share the same race and gender, and the largest effects are for minority males.

Our findings demonstrate that there is an important social cost to neighborhood and school segregation. School assignment policies can have unintended effects on neighborhood crime. Clustering similar students together may contribute to higher rates of criminal activity among youth, greater frequency of criminal partnerships among young offenders, and larger criminal networks that facilitate future partnerships and crimes. These potential costs must be weighed against potential advantages of assigning cohesive neighborhoods to the same school, such as shared supervision of youths among neighbors and stronger social relationships among families.

REFERENCES

Anderson, David A. 1999. "The Aggregate Burden of Crime." *Journal of Law and Economics* 42 (2): 611–42.

Bayer, Patrick, Randi Hjalmarsson, and David Pozen. 2009. "Building Criminal Capital behind Bars: Peer Effects in Juvenile Corrections." *Quarterly Journal of Economics* 124 (1): 105–47.

Bayer, Patrick, Stephen L. Ross, and Giorgio Topa. 2008. "Place of Work and Place of Residence: Informal Hiring Networks and Labor Market Outcomes." *Journal of Political Economy* 116 (6): 1150–96.

Bertrand, Marianne, Erzo F. P. Luttmer, and Sendhil Mullainathan. 2000. "Network Effects and Welfare Cultures." *Quarterly Journal of Economics* 115 (3): 1019–55.

Billings, Stephen B., David J. Deming, and Jonah Rockoff. 2014. "School Segregation, Educational Attainment, and Crime: Evidence from the End of Busing in Charlotte-Mecklenburg." *Quarterly Journal of Economics* 129 (1): 435–76.

Brock, William A., and Steven N. Durlauf. 2001. "Interactions-Based Models." In *Handbook of Econometrics*, Vol. 5, edited by J. J. Heckman and E. Leamer, 3297–3380. Amsterdam: North-Holland.

Calvó-Armengol, Antoni, and Yves Zenou. 2004. "Social networks and Crime Decisions: The Role of Social Structure in Facilitating Delinquent Behavior." *International Economic Review* 45 (3): 939–58.

Carrell, Scott E., and Mark L. Hoekstra. 2010. "Externalities in the Classroom: How Children Exposed to Domestic Violence Affect Everyone's Kids." *American Economic Journal: Applied Economics* 2 (1): 211–28.

Chetty, Raj, Nathaniel Hendren, and Lawrence F. Katz. 2016. "The Effects of Exposure to Better Neighborhoods on Children: New Evidence from the Moving to Opportunity Experiment." *American Economic Review* 106 (4): 855–902.

Cook, Philip J., and Jens Ludwig. 2005. "Assigning Deviant Youths to Minimize Total Harm." National Bureau of Economic Research (NBER) Working Paper 11390.

Cullen, Julie Berry, Brian A. Jacob, and Steven Levitt. 2006. "The Effect of School Choice on Participants: Evidence from Randomized Lotteries." *Econometrica* 74 (5): 1191–1230.

Damm, Anna Piil, and Christian Dustmann. 2014. "Does Growing Up in a High Crime Neighborhood Affect Youth Criminal Behavior?" *American Economic Review* 104 (6): 1806–32.

Deming, David J. 2011. "Better Schools, Less Crime?" *Quarterly Journal of Economics* 126 (4): 2063–2115.

Fletcher, Jason M., and Stephen L. Ross. 2012. "Estimating the Effects of Friendship Networks on Health Behaviors of Adolescents." National Bureau of Economic Research (NBER) Working Paper 18253.

Fletcher, Jason M., Stephen L. Ross, and Yuxiu Zhang. 2013. "The Determinants and Consequences of Friendship Composition." National Bureau of Economic Research (NBER) Working Paper 19215.

Glaeser, Edward L., Bruce Sacerdote, and José A. Scheinkman. 1996. "Crime and Social Interactions." *Quarterly Journal of Economics* 111 (2): 507–48.

Grinblatt, Mark, Matti Keloharju, and Seppo Ikäheimo. 2008. "Social Influence and Consumption: Evidence from the Automobile Purchases of Neighbors." *Review of Economics and Statistics* 90 (4): 735–53.

Imberman, Scott A., Adriana D. Kugler, and Bruce I. Sacerdote. 2012. "Katrina's Children: Evidence on the Structure of Peer Effects from Hurricane Evacuees." *American Economic Review* 102 (5): 2048–82.

Jackson, Matthew O. 2010. *Social and Economic Networks*. Princeton: Princeton University Press.

Jacob, Brian A., and Lars Lefgren. 2003. "Are Idle Hands the Devil's Workshop? Incapacitation, Concentration, and Juvenile Crime." *American Economic Review* 93 (5): 1560–77.

Jacobson, Mireille. 2004. "Baby Booms and Drug Busts: Trends in Youth Drug use in the United States, 1975–2000." *Quarterly Journal of Economics* 119 (4): 1481–1512.

Kling, Jeffrey R., Jens Ludwig, and Lawrence F. Katz. 2005. "Neighborhood Effects on Crime for Female and Male Youth: Evidence from a Randomized Housing Voucher Experiment." *Quarterly Journal of Economics* 120 (1): 87–130.

Levitt, Steven D. 1997. "Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime." *American Economic Review* 87 (3): 270–90.

Ludwig, Jens, Greg J. Duncan, and Paul Hirschfield. 2001. "Urban Poverty and Juvenile Crime: Evidence from a Randomized Housing-Mobility Experiment." *Quarterly Journal of Economics* 116 (2): 655–79.

Ludwig, Jens, and Jeffrey R. Kling. 2007. "Is Crime Contagious?" *Journal of Law and Economics* 50 (3): 491–518.

Patacchini, Eleanora, and Yves Zenou. 2009. "Juvenile Delinquency and Conformism." *Journal of Law, Economics, and Organization* 28 (1): 1–31.

Ross, Stephen L. 2011. "Social Interactions within Cities: Neighborhood Environments and Peer Relationships." In *Oxford Handbook of Urban Economics and Planning*, edited by Nancy Brooks, Kieran Donaghy, and Gerrit-Jan Knapp, 203–29. Oxford, UK: Oxford University Press.

Sah, Raaj K. 1991. "Social Osmosis and Patterns of Crime." *Journal of Political Economy* 99 (6): 1272–95.

Silverman, Dan. 2004. "Street Crime and Street Culture." *International Economic Review* 45 (3): 761–86.

Smith, Stephen Samuel. 2004. *Boom for Whom? Education, Desegregation, and Development in Charlotte*. Albany: State Press of New York.

Weinberg, Bruce A. 2007. "Social Interactions with Endogenous Associations." National Bureau of Economic Research (NBER) Working Paper 13038.

This article has been cited by:

1. Nathan Deutscher. 2020. Place, Peers, and the Teenage Years: Long-Run Neighborhood Effects in Australia. *American Economic Journal: Applied Economics* **12**:2, 220-249. [Abstract] [View PDF article] [PDF with links]
2. Darwin CORTÉS, Guido FRIEBEL, Darío MALDONADO. 2020. CRIME AND EDUCATION IN A MODEL OF INFORMATION TRANSMISSION. *Annals of Public and Cooperative Economics* **91**:1, 71-93. [Crossref]
3. Jason M. Fletcher, Stephen L. Ross, Yuxiu Zhang. 2020. The consequences of friendships: Evidence on the effect of social relationships in school on academic achievement. *Journal of Urban Economics* **116**, 103241. [Crossref]
4. Matthew P. Steinberg, Benjamin Ukert, John M. MacDonald. 2019. Schools as places of crime? Evidence from closing chronically underperforming schools. *Regional Science and Urban Economics* **77**, 125-140. [Crossref]
5. Manudeep Bhuller, Gordon B. Dahl, Katrine Vellesen Løken, Magne Mogstad. 2018. Incarceration Spillovers in Criminal and Family Networks. *SSRN Electronic Journal* . [Crossref]