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PARTNERS IN CRIME:
SCHOOLS, NEIGHBORHOODS AND THE FORMATION OF CRIMINAL NETWORKS

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ABSTRACT

Why do crime rates differ greatly across neighborhoods and schools? Comparing youth who were assigned to opposite sides of newly drawn school boundaries, we show that concentrating disadvantaged youth together in the same schools and neighborhoods increases total crime. We then show that these youth are more likely to be arrested for committing crimes together – to be "partners in crime". Our results suggest that direct peer interaction is a key mechanism for social multipliers in criminal behavior. As a result, policies that increase residential and school segregation will – all else equal – increase crime through the formation of denser criminal networks.

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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, 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. When an individual's propensity to commit a crime varies causally with the behavior of the group, so-called "endogenous" peer effects generate social multipliers for interventions that target individuals.[3] Thus, the nature of endogenous peer effects is important for policy prescriptions. Proposed mechanisms for social multipliers in crime include strained monitoring resources (Levitt 1997, Jacobson 2004), shifting norms of behavior and reputation (Anderson 1999, Silverman 2004), learning about criminal opportunities (Sah 1991, Calvo-Armengol and Zenou 2004) and criminal network formation (Bayer, Hjalmarsson and Pozen 2009). Each of these mechanisms implies distinct strategies to reduce crime. For example, if social multipliers result from youth endogenously increasing criminal activity because they know that a fixed police presence is now spread across more potential offenders,

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

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

[3] Manski (1993) distinguishes endogenous peer effects from "exogenous" or correlated peer effects, where behavior is affected by pre-determined characteristics or the propensity for similar individuals to self-segregate. Manski (1993) shows that strong cross-sectional correlations between crime patterns and neighborhood characteristics need not be evidence of social multipliers – they can be driven by exogenous peer effects or sorting.

the policy solution is to directly target increased monitoring resources to 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 2015).

In this paper, we present direct evidence of the importance of criminal network formation for generating social multipliers in youth criminal activity. We first demonstrate strong evidence of agglomeration externalities in crime by showing that increases in the number of similar peers living nearby and attending the same school makes youth more likely to commit a crime. Second, we show using newly available data that similar individuals who live near each other and attend the same school are more likely to be arrested for having committed the *same crime together*. Notably, the general empirical patterns are very similar across both findings. Agglomeration and partnership effects are only found for individuals who reside very near each other, within a kilometer, and are assigned to the same school. For both models, estimated effects are largest when individuals are assigned to the same grade and of the same race and gender.

For both analyses, we use school administrative data from Charlotte-Mecklenburg schools (CMS) linked to arrest records from the Charlotte-Mecklenburg Police Department (CMPD), and we obtain exogenous variation in school and grade attendance using newly drawn school boundaries following Billings, Deming and Rockoff (2014) and birth date required for kindergarten attendance, respectively. In testing for agglomeration, we calculate the number of same assigned grade-gender-race youth within a kilometer overall and who are assigned to the same school, and make comparisons across attendance zone boundaries. Concentrations of nearby youth only increase arrests if they are assigned to the same school – neighborhood proximity alone is not sufficient. In the partnership model, we pair each youth offender residing in the neighborhoods that were bifurcated by newly drawn attendance boundaries with all other youth offenders, and examine how the likelihood of criminal partnership varies with distance. We find that the likelihood of partnership declines rapidly with the distance between residences, and as with the agglomeration model we find no relationship unless the offenders were assigned to the same school. Consistent with the peer interaction mechanisms, we find that the partnership impacts are strongly increasing in length of neighborhood and school residence. Taken together, these two sets of results suggest that the formation of criminal networks – "partners in crime" - is an important mechanism underlying social multipliers in criminal activity.

A number of recent papers shed light on the mechanisms for endogenous 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 criminal networks, they cannot demonstrate directly that youth in juvenile facilities 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 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 criminal network formation.[4] While several other studies find important impacts of schools on crime, they are unable to pin

[4] Also see Patacchini and Zenou (2009) for a more structural approach where they find that conforming to the criminal activity of self-reported friends contributes to a youth's own criminal activity. They instrument for the criminal activity of friends using the criminal activity of the individuals who the friends report as friends.

down peers as the causal mechanism rather than school context or changes in the expected return to schooling investments (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 criminal networks 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 why attending a more segregated school might increase crime (Kling, Ludwig and Katz 2005, Billings, Deming and Rockoff 2014). In the case of Billings, Deming and Rockoff (2014), our findings suggest that the large estimated effects of segregation might have in part been driven by the specific policy, where students in Charlotte NC were redistricted both to more segregated schools and to schools that contained more of their residential neighbors. However, our findings do not imply that criminal network formation is the *only* mechanism for social multipliers in crime – only that they play an important role holding other factors constant.

2. Data

Our sample is comprised of administrative records from Charlotte-Mecklenburg Schools (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. 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 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.

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. 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. Crimes committed by partners are disproportionately burglaries, robberies, and drug offenses.[7]

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. Our primary analysis involves the sample of students who attended public school at age

[5] Our match rate between student and arrest records is 94% 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] See Appendix Figure A1 for the distribution of crime categories for all arrests as well as partnership arrests.

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. 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 sample is quite noisy due to the high rates of residential mobility among our sample of student offenders. Estimates based on this second sample are shown in the appendix and are consistent with later results.[8]

Table 1 provides descriptive statistics for our sample of students in bisected block groups at age 14. Panel 1 presents arrest data for ages 16-21, panel 2 presents basic individual demographics, education outcomes, and school and neighborhood attributes, and panel 3 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, and the final column presents means for all offenders who were arrested for committing a crime with one of more partners. 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.[9] Among those students ever arrested, the incidence of violent crime is 19 percent, but jumps to 34 percent for offenders who commit crimes 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 1.23 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 age and same age-same school peers than all

[8] For the partnership analysis, see Figure A2 and Table A1, which are comparable to Figures 2 and Table A6 using our primary sample. Estimates for the agglomeration analysis using this sample are too noisy to be informative.
[9] Based on the 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).

students. Other attributes are broadly similar between all offenders and those offenders involved in criminal partnerships. Finally, panel 3 illustrates that the block groups that contain our sample are relatively densely populated with on average two to three hundred similar aged peers within a kilometer of our sample students.

We then use our linked student and arrest data to match student offenders in our sample of bisected block groups with all offenders in the county, and then identify whether they were ever criminal partners during our sample period. Specifically, for each sample offender in a bisected block group, we create a unique observation of offender pairs based on all offenders within three years of age who were ever arrested during our sample period.[10] We exclude pairs less than 130 feet apart since this is the smallest distance upon which we find different school pairs and to remove the influence of siblings and criminals within a single housing or apartment building.[11] Then, we create an indicator variable for a criminal partnership if both individuals were arrested for the same crime. Since our data uniquely links each individual's arrest back to the Charlotte-Mecklenburg Police Department (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, as well as rule out situations where individuals were arrested at the same time, but were not acting together to commit a crime.

Given the scale for which we later observe a relationship between distance and probability of partnership, most of our analysis focuses on pairs of offenders who live within 1 kilometer of

[10] We limit analysis to individuals within 3 years of age since less than 5% of criminal partnerships involve individuals more than 3 year apart. Even with this restriction, the size of this dataset is substantial (over 30 million observations) and thus we limit our analysis to pairs of individuals within certain distance thresholds. For computational ease, some models limit our sample to only non-partner observations that were ever arrested age 16-18. Results are unchanged if we limit non-partner observations to only individuals ever arrested age 19-21.
[11] Our main results are slightly larger in magnitude if we include these pairs.

each other. [12] For this sample of offender pairs, we observe 366 unique partners out of our possible 123,982 pairwise combination of offenders within 1km of each other. Our key analysis focuses on the relative share of partners for offender pairs assigned to the same school versus different schools. Unconditionally, 85% of offender pairs are assigned to the same school, but 94% of criminal partners were assigned to the same school. This relatively higher concentration of criminal partnerships for offender pairs assigned to the same school is the main insight from the partnership analysis. Appendix Table A2 present descriptive statistics for the sample of matched pairs based on assigned school.[13]

3. Empirical Strategy

3.1 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. Specifically, the likelihood of individual *i* in school *s* and neighborhood *n* committing a crime (C_{isn}) is allowed to depend upon the number of students *N* who share student *i*'s attributes (X_{isn}) and reside within a specified distance; the number of students *S* who share these same attributes including residential proximity and are also assigned to the same school; neighborhood fixed effects δ_n; assigned school fixed effects; and individual controls X_{isn}.

$$C_{isn} = \alpha_1 N\big(X_{jt,v-w} = X_{is,v-w} | d_{ij} < \bar{d}\big) + \alpha_2 S\big(X_{jt,v-w} = X_{is,v-w} | d_{ij} < \bar{d}, s = t\big) + \delta_n + \gamma_S + \beta X_{isn} + \mu_{isn} \quad (1)$$

[12] One kilometer is identified as the threshold for which the relationship between distance and probability of partnership approaches zero in our dataset.
[13] We do observe some differences in the distribution of students attributes between partners and non-partners. Partners are more likely to be similar in age, both male, same race and both reside in single-family residences than non-partners.

where N and S are constructed so that they contain the number of students j who match student i on attributes v through w in the vector X_j and satisfy the distance threshold for N and both distance threshold and same school criteria for S. We start with a simple model just requiring same assigned grade and then we condition on same grade, race and gender, student type. In models that condition on student type, we expand our vector of fixed effects to neighborhood by student type fixed effects. Finally, we restrict N and S only to student's whose predicted likelihood of committing a crime falls in the top quintile, as well as allow effects to vary by racial and ethnic group.

The inclusion of neighborhood or neighborhood by student type fixed effects yields models that are identified from differences in proximate same school potential peers for similar individuals in the same general neighborhood simply due to school attendance boundary configuration. While variation in potential same school peers exists within attendance zones based on exact residential location, the control for overall number of proximate peers captures any variation in neighborhood peer concentration for similar students in similar residential locations. The fact that we focus on school assignment – rather than actual attendance – also further limits the effects of individuals changing schools due to disciplinary problems or early criminal behavior.

3.2 Criminal Partnership

We then 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? Concretely, suppose that the probability of criminal partnership depends upon both spatial proximity and school attended:

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

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 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 offenders who are assigned to different schools, and our function of interest g captures the effect of school assignment on this relationship. Intuitively, our identification strategy 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 and controls X_{jt} for the individual *j* who is being paired with each individual *i* in neighborhood *n*, where X_{jt} also includes a school *t* fixed effect. Specifically,

$$P_{insjt} = f(d_{ij}) + g(d_{ij})D(s = t) + \delta_n + \beta X_{jt} + \gamma_t + \varepsilon_{insjt} \quad \text{if } d_{ij} < \bar{d} \tag{3}$$

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 and so could not both be assigned to the same school as individual *j*.

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 offenders in the same school and pairs of offenders in different schools. In our follow-up analyses, we restrict our sample to *j*'s who reside within a specified distance threshold $\bar{d}$ of an individual *i* and so *f* and *f*+g are specified implicitly as step functions, where we specify $\bar{d}$ based on the distribution of criminal partnership over pairwise distances d_{ij}. Standard errors for this model and for generalizations of this model are clustered at the neighborhood n level of individual i in each pair.[14]

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 school and birth date implied grade assignment (which are arguably exogenous),[15] school attended, current grade and assignment to classrooms is potentially biased by sorting and so we treat these results as suggestive.

3.3 Initial Robustness Checks

The use of the newly drawn boundaries under court-ordered end of 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. Based on the model given by Equation 1, we formally show that student attributes do not explain school peer concentration in Table 2.[16]

Table 2 highlights four measures of potential same school peers: number of students assigned to the same school, number of students assigned to the same school and grade (based on age) residing within one kilometer, number of students assigned to the same school and grade

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[14] 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.

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

[16] The fact that Billings, Deming and Rockoff (2014) find minimal residential relocation after redistricting based on changes in school peer composition minimizes some of these concerns too.

who share the individuals' gender and race, and number of high risk students assigned to the same school and grade who share the individuals' gender and race, where high risk is based on a simple regression of ever arrested on student attributes of rising 9th graders prior to the 2002 redistricting. We then regress these measures of peers on student attributes, neighborhood peer counts and block group of residence by student type fixed effects. 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 a joint significance of 10 percent. Our preferred 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.

In order to test the validity of our school attendance boundary discontinuity approach for our partnership analysis, we provide a second balancing test for assigned to same school for our sample of bisected block group offenders who are paired to all other similar age offenders. Table 3 presents the results of a regression of whether paired offenders are assigned to the same school (column 1) or assigned to the same school and grade (column 2) on variables for demographic attributes, test scores, suspensions and absences for individual *j*, while controlling for census block group fixed effects associated with the bisected block group. Both assignment measures pass the balancing tests and coefficients are small given that 85% of our sample is in the same school and 16% of our sample is in the same school and grade.

Since early adolescent behavioral or academic problems may be predictive of adult criminal activity and thus may influence residential location, we also examine models that focus on individuals with longer neighborhood tenures. If our results were driven by movers into these neighborhoods, we would expect larger effects for the recent residents, and we find no evidence of larger effects for this group in any of our later models.[17]

4. Results

4.1 Effects of Exposure to Potential Peers

Table 4 presents models that estimate the impact of peer agglomeration on the likelihood of individual youths committing a crime. 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 respectively on both the number of potential peers overall and the number of potential peers in the same school. We define a student's potential peers in four ways: 1) nearby (within 1 kilometer); 2) nearby, same assigned grade; 3) nearby, same assigned grade, same race and same gender; and 4) nearby, same assigned grade, same race, same gender, and at high risk of arrest (predicted values of ever arrested place student in the top quintile). The results for these four measures are shown in panels 1 through 4.

Column 1 of Table 4 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 1 presents the results for number of same school peers, and the estimates are small and not significantly different from zero. Panel 2 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 3), we find large positive effects of same school potential partners on the likelihood of a student committing any crime, a violent crime or a property crime.

[17] See Tables A3 and A7.

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, which indicates a 23% increase in the probability of arrest relative to the average student. A standard deviation increase in the number of 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. This effect represents a relatively larger increase of 67% and 41% in the probability of arrest relative to the average student. When focusing only on high risk potential peers, we see a substantial increase in the absolute effect on violent crimes consistent with the effects for violent crimes being disproportionately driven by these high risk students.

The last panel of table 4 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 quite sizable on ever arrested for males, 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 one 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 0-1km, 1-2km, 2-3km, 3-4km and 4-5km. Figure 1 presents results for models using the outcome of ever arrested and indicates that we only observe any effects from peers for our less than 1km definition of neighborhood. Results for larger distance bands are all closer 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. In Appendix Table A4, we conduct a counterfactual analysis based on randomly shifting school attendance boundaries by between 1 and 2km 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 actually 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 actually have approximately the same number of actual same school peers. 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 4 but report the mean and standard deviation of our coefficient across these 100 replications. The estimates on "falsely assigned same school" potential peers are statistically insignificant, and always substantially smaller in magnitude than the significant estimates in Table 4.

Overall, the results in Table 4 and Figure 1 show that youth who happen to have larger concentrations of peers living in close physical proximity and attending the same school are more likely to be arrested. The results are strongest for same peers of the same grade, race and gender, and are concentrated among minority males. In the next section, we use criminal partnership data to show that this increase in crime is driven in part by youth committing crimes together.

4.2 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 (same assigned grade), are concentrated among youth that match on gender and race, and finally are largest among minority males. Having established these key empirical patterns in our causal estimates, we now turn to our partnership analysis in order to establish whether similar patterns arise in the likelihood of being arrest together for committing a crime

We begin our criminal partnership analysis with graphical results that display the relationship between distance and the probability of criminal partnerships. Figure 2 plots the probability of a pair of offenders committing a crime together as a function of the distance between the offenders.[18] We show results separately for two samples – students assigned to same school and grade, and students assigned to different schools and the same grade. [19] 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*. The figure shows that the probability of partnership is high when the offenders are within a few hundred feet of each other and are assigned to the same school, and declines quickly towards zero and is very near to zero once the offenders are 1km away or more. The probability of partnership for offenders who attend different schools is small and does not demonstrate any significant relationship with distance.[20] This constitutes strong *prima facie* evidence that attending the same school increases the

[18] The distribution of offenders and partners (all as well as subsamples for same/different school) is presented in Appendix Figures A.5, A.6 and A.7. These figures highlight that the sample size for different school pairs increases substantially at larger distances, and for distances within 1km about 15% of our observations are assigned different schools.

[19] Appendix Figure A.4 shows similar results for the same school/different school comparison.

[20] The relatively higher probability of partnerships for different school/same grade at 1km is due to a few partnerships at similar distances and is not statistically different from same school/grade partnerships.

likelihood of criminal partnership, even for students who live in the same neighborhoods. There are negligible differences between Figure 2 and a similar figure that does not control for observed covariates.

Figure 3 presents the difference between our conditional probabilities of partnerships for the two samples. The 95% 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 and differences are almost significant at distances closer to 1 km.[21] Overall, these results highlight a strong positive relationship between shared school assignment and criminal partnerships for individuals that live very close together. As we found in our agglomeration analysis, the effect of spatial proximity on partnerships is entirely associated with proximity to potential peers who are assigned to the same school.

In order to verify that these results are due to school assignment boundaries and not the spatial distribution of offenders, we conduct a second falsification test in Appendix Figure A3 where we randomly shifted all attendance boundaries by between 1 and 2 kms. We construct a new version of Figure 3 where we assign our main sample of 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% confidence interval as the shaded area in Figure A3. This figure does not demonstrate any relationship between falsely assigned same school and partnership based on spatially relevant, artificial attendance zone boundaries.

Table 5 presents estimates of equation (3) based on pairs who are within ½, 1 or 2 kilometers of each other, with the same dependent variable as the figures above: being arrested together for at least one crime. The right hand side variables in the model are assigned to same

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

school and assigned to same school and same grade based on birth date. All models include block group fixed effects for offenders *i* in the bisected block groups and controls for the observable attributes of the paired offenders *j*. Column 1 presents estimates of the partnership model for these subsamples. The coefficients on offenders being assigned to the same school and assigned to the same school and grade are both positive and highly statistically significant for all distance thresholds.

Consistent with our figures, the strongest effects occur for individuals residing within ½ km and effects weaken as we extend distance thresholds out to 2 km. Given the limited number of observations in different schools within ½ km, we focus our analysis on results for 1km. Results at 1 km are more precise than the ½ km results and have quantitatively similar magnitudes relative to mean partnership rates. For column 1, being assigned to the same school increases the probability of being criminal partners by 0.21 percentage points and being assigned to the same grade increases this probability by an additional 0.33 percentage points. Overall, being assigned to the same school and grade makes two individuals six times more likely to form a criminal partnership, increasing from a mean probability of 0.0011 for different school pairs to 0.0065 for same school and grade pairs.

The rest of the columns in Table 5 present the model using the number of crimes committed together, if individuals were partners at age 16-18, partners at age 19-21, and finally partnerships for specific crime classifications. [22] Column 2 examines if schools have an effect on the number of partnerships between two offenders. Results for both same school and same school/grade are positive and the magnitudes are larger than other estimates relative to the mean number of partnerships. Partnership effects for both same school and same grade persist except

[22] Appendix Table A5 shows results for assigned school by further disaggregated types of crime with assault and burglary generating the largest effects.

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. Results for crime categories show a consistent role of schools in increasing criminal partnerships for a variety of criminal activities.

4.3 Robustness Checks and Additional Results

Since offenders may form partnerships based on students attributes, Table 6 presents results from models where we examine heterogeneity in the likelihood of two offenders partnering together based on same assigned school. We re-estimate our main model from Table 5 including additional controls for the socio-economic/demographic match between the two offenders in any pair, and interact these controls with the assigned to the same school dummy variable. These variables include whether the offenders are assigned to the same grade (as in Table 5) and four dummies for whether they match on minority status and gender: both male-minority etc.[23]

We find strong effects of increased partnership when the offenders in the pair are in the same grade, are both minority males and more modest effects for both minority female. These results are robust for both the 16-18 and the 19-21 age group subsamples and for minority males robust for violent and property crimes. These patterns are very similar to the patterns uncovered in the agglomeration analysis.

[23] These variables are essentially triple interactions. Our models also include variables for all of the associated two way interactions.

Appendix Table A6 provides a series of robustness checks for our partnership models.[24] Results are largely consistent across these models with both distance bin and individual fixed effects generating similar results. The only noticeable difference across these specifications is the smaller magnitude when we base school assignment solely on high school attendance zone boundaries. Smaller effects for models where school assignment only based on high school is consistent with the loss of same school partners that interacted in middle school only.

In addition, as discussed above, we ran a series of models that interact our school variables with a dummy for individual *i* living in the same address since 2001. If our results are driven by individuals sorting into these neighborhoods, we might expect stronger results for the most recent residents who presumably selected into a particular side of the boundary based on across boundary differences. On the other hand, if our results arise from social interactions, we might expect the strongest results to arise between individuals who have resided in this neighborhood the longest and so have the strongest connections to the neighborhood. Table A7 shows estimated effects for the partnership model. The estimates suggest stronger effects on pre-2002 residents for both school variables, which is consistent with our main results not being driven by new residents sorting to schools after redistricting, and thus limits concern that post redistricting sorting is generating our results.[25]

[24] Appendix Table A6 presents a series of robustness tests where the first column presents the main results for the within 1 kilometer sample. The second column presents the model for the same sample, but including fixed effects based on bins for different distances between the individuals in the pair. The third and fourth columns present the model for the ½ kilometer sample, without and with the distance bin fixed effects, respectively. Column 5 presents results for the 1 kilometer sample replacing offender j's observable attributes with an individual offender fixed effect. Columns 6 and 7 presents the results for the 1 kilometer sample without and with individual fixed effects where same school assignment is based only on high school. The final column presents estimates controlling for residential block group of offender i by individual offender j fixed effects.

[25] In addition, we tested our main results for our sample of students using a student's residential address prior to 2002 and provide results in the top panel of Appendix Table A1 and Appendix Figure A2. Our conclusions regarding the role of schools is still consistent in these models, but represents a mix of attenuated results due to the large amount of residential movement coupled with a greater presence of pre-2002 address information for longer-term neighborhood residents. For example, only 35% of our main sample of offenders live in the same address at

Appendix Table A8 provides a version of our main partnership results (Table 5) for the 1 kilometer sample using whether partners actually attended the same school, the same school and same grade, and attended at least two classes together.[26] These models all include fixed effects for individual j since individuals likely attended schools and specific courses based on a number of unobserved attributes.[27] Most of the effects are concentrated among pairs who attended class together, but partnership effects also arise at both the grade and school level. The magnitudes of our effects are larger in these models even though in some cases we lose statistical significance. For column 1, being in the same course, grade and school increase the probability of partnership by 0.0097 percentage points over the mean partnership probability of 0.0016 for different school partners. Again, we find that the effects on 19-21 year old partners are substantially smaller than for 16-18 year old partners.[28] Similar to results for individuals with longer tenures in a home, Appendix Table A14 examines whether partnerships are more likely for pairs that attended the same elementary school and we find stronger effects for this subgroup.

5. Conclusion

In this paper we study the influence of schools and neighborhoods on criminal partnership. We find evidence for neighborhood spillovers in crime based on exposure to same race and gender peers. These effects only arise when the students reside in close proximity to each other and are assigned to the same school, and the effects are strongest when the students

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age 14 as they did in 2001. Coupled with the fact that we are using pairs of offenders leads to only about 10% of our observations having the same residential information in 2001 as well as at age 14.

[26] Defining same course as at least two classes together provides more precise estimates than other definitions. Summary statistics for our partnership bisected by school attended is provided in Appendix Table A9

[27] To further test if sorting to specific courses is problematic to our results, we created Appendix Table A10 where we only define same course based on courses required of all students in english, math, science and social studies.

[28] Appendix Tables A11, A12 and A13 provide results for different crime types, other model specifications and interactions with student attributes for models based on school attended and generate similar conclusions as our school assigned results.

are also assigned to the same grade. These effects are driven primarily by males, especially minority males, and are stronger when the potential peer is especially likely to ever have been arrested based on observables. Given strong existing evidence of homophily in friendship patterns by race and grade/age (Weinberg 2007; Fletcher, Ross and Zhang 2013), the patterns of findings are clearly consistent with social interactions contributing to the probability of committing a crime.

Further, our analysis of criminal partnership is consistent with the role of social interactions. 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 assigned to the same grade. The effects are strongest when the offenders share the same race and gender too, and the largest effects are for minority males.

Our results have important implications for understanding the determinants of criminal activity. School assignment policies can have unintended effects on neighborhood crime in that drawing boundaries that keep together cohesive neighborhoods with clusters of similar students may contribute to higher rates of criminal activity among youth, greater frequency of criminal partnerships among young offenders, and larger criminal networks facilitating future partnerships and crimes. Our results also have implications for the literature on the endogenous effects of social interactions. We demonstrate that the social context of the school affects a key behavior related to criminal activity, criminal partnerships, and show that the relationship between the availability of local peers and criminal activity follows a pattern that is consistent with what we would expect if peers were influencing criminal activity by presenting partnership opportunities. This suggests that interventions which reduce crime among a subset of youth within a school will have a spillover effect on other potential criminals by reducing the opportunities for criminal partnership available to other students who live nearby, leading to a social multiplier that would not exist if the mechanism behind the peer effects were purely social learning or school context.

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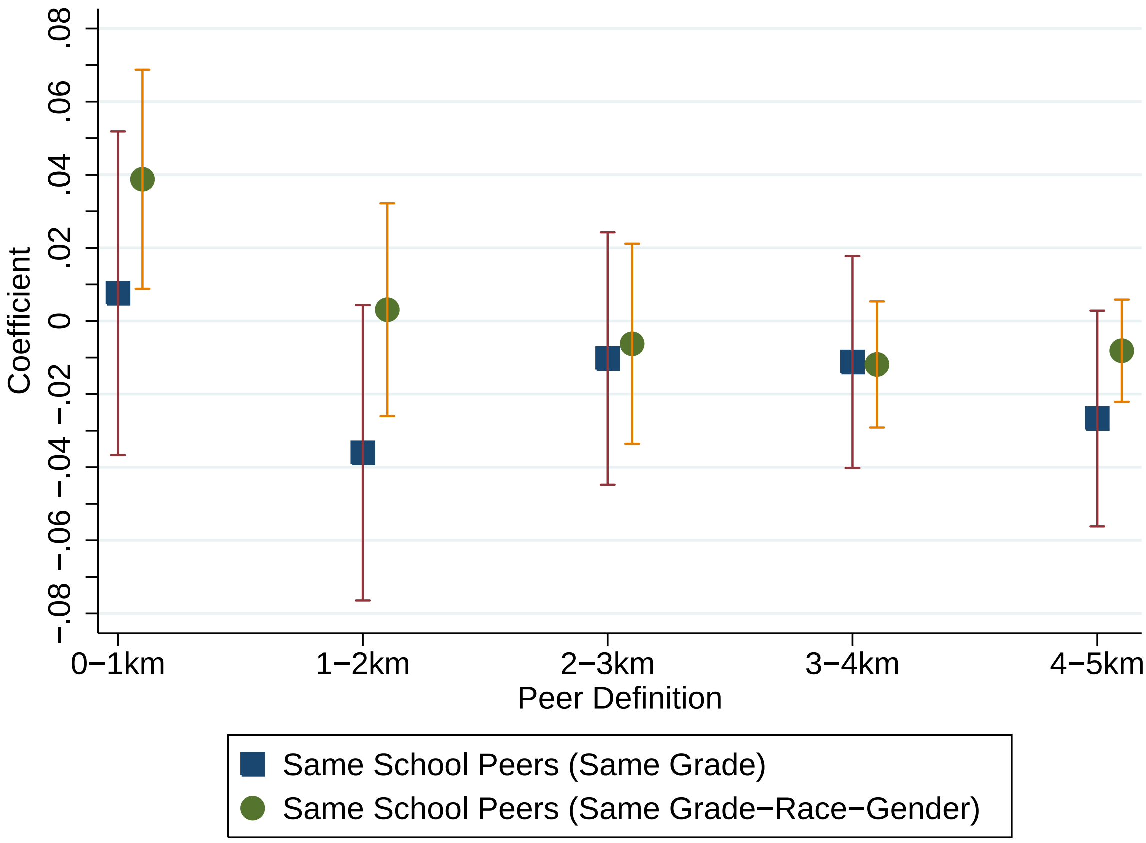
Figure 1: Crime Agglomeration by Distance Bands

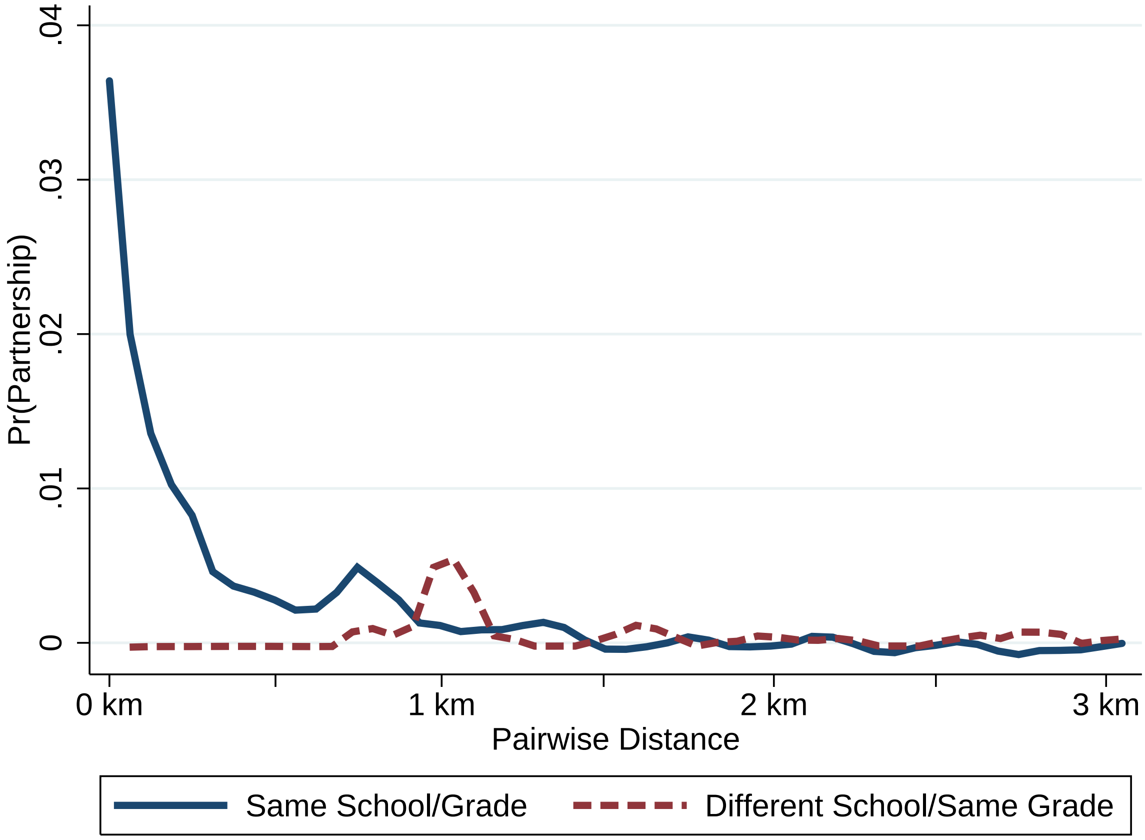


This figure provides the estimated coefficient of a standard deviation increase in same school and neighborhood peer counts and 95% confidence interval for a series of estimates of Equation 1 where we vary definitions of peers based on student attributes as well as distance bands away from an individual upon which to define peers.

All regressions include controls for gender, race, 5th grade reading and math test scores, indicator if missing a test score or other 5th grade information, days suspended (5th grade) , total days absent (5th grade), single family home indicator, 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).

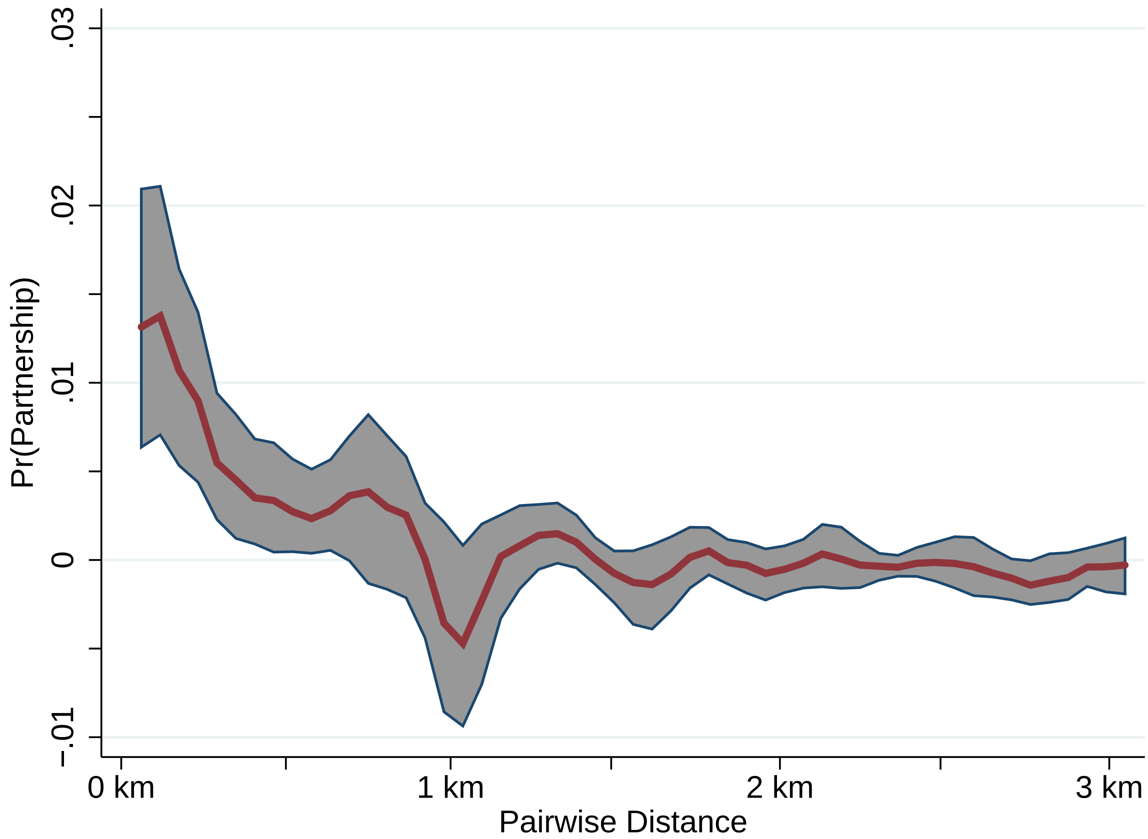
Figure 2: Conditional Probabilities of Partnership (Same School/Grade vs. Different School/Same Grade)



This figure provides the distribution of partnership probabilities conditional on individual and neighborhood attributes for our sample of offender 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 which controls for individual attributes of person j (gender, race, 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 used to construct this figure includes all pairs of arrested individuals (age 16-21) who are three years or less apart in age (less than 5% of criminal partners are more than 3 year apart), live within 3 km of each other based on school age 14 address and at least one offender resides in a Census Block Group (CBG) bisected by a new middle or high school attendance zone boundary. For computational ease, we limit non-partner pairs to only those ever arrested age 16-18, but results are similar with the use of non-partner pairs arrested age 19-21.

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



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% 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.

Table 1: Summary Statistics - Individuals

	All Students	Ever Arrested	Criminal Partners
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	1.70	1.70	1.70
	(1.11)	(1.11)	(1.11)
Number of People per Crime	1.44	1.44	2.71
	(0.73)	(0.73)	(0.91)
Background Characteristics			
Male	0.50	0.69	0.76
	(0.50)	(0.46)	(0.43)
Minority	0.59	0.78	0.87
	(0.49)	(0.41)	(0.33)
Single Family Residence	0.79	0.75	0.75
	(0.41)	(0.44)	(0.43)
Math Test Score	−0.05	−0.53	−0.66
	(0.97)	(0.85)	(0.79)
Read Test Score	−0.04	−0.54	−0.67
	(0.98)	(0.91)	(0.89)
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)
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 & 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 & School Peers within 1 km	10.57	12.53	13.63
	(8.30)	(8.78)	(8.96)
Observations	34,958	5,867	1,625

Means and standard deviations are reported above. All information regarding housing or Census Block Group (CBG) 2000 neighborhood is based on address at school age 14. School grade determined by Charlotte-Mecklenburg Schools (CMS) matriculation policy of starting kindergarten if age 5 by September 1st and we assume 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. The column for Ever Arrested and all crime outcomes based on arrests in Mecklenburg County of CMS students age 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. We restrict the calculation of number of people per crime to only crimes with partners for our sample of Criminal Partners in column 3.

Table 2: Balancing Test - Crime Agglomeration Models

	(1) Same School Peers (1 km)	(2) Same School & Grade Peers (1 km)	(3) Same School & Grade-Race-Gender Peers (1 km)	(4) Same School & Grade-Race-Gender High Risk Peers (1 km)
Male	−0.0026	−0.0000		
	(0.0023)	(0.0011)		
Hispanic	−0.0226**	−0.0009		
	(0.0114)	(0.0022)		
Black	−0.0135*	−0.0002		
	(0.0071)	(0.0014)		
Single Family Residence	0.0358	−0.0031	0.0144	−0.0022
	(0.0344)	(0.0028)	(0.0284)	(0.0079)
Math Test Score	0.0057	−0.0001	0.0024	−0.0009
	(0.0038)	(0.0010)	(0.0034)	(0.0036)
Read Test Score	−0.0055	0.0012	−0.0031	0.0030
	(0.0036)	(0.0011)	(0.0036)	(0.0042)
Total Days Suspended from School	0.0005	0.0005*	0.0004	0.0002
	(0.0006)	(0.0003)	(0.0008)	(0.0007)
Total Days Absent	−0.0002	0.0000	−0.0002	−0.0001
	(0.0003)	(0.0001)	(0.0003)	(0.0002)
Observations	34,958	34,958	34,958	34,958
F-Stat (p-value)	0.10	0.49	0.88	0.93
R^2	0.93	0.99	0.96	0.98

* p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors 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 one indicates the number of peers that live within 1 km and are assigned to the same middle or high school. Column two restricts the definition of peers in column one 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 9th 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 attending CMS at school age 14 at any time from 2003-2009. Each column includes but does not report coefficients for same neighborhood ($\leqslant 1km$) 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 assigned to the same school and neighborhood.

All reported covariates for test scores, suspension days and days absent are based on 5th grade and all models include but do not report an indicator if missing a test score or other 5th grade information and assigned school fixed effects. Column one includes Census Block Group 2000 (CBG), column two includes CBG by grade fixed effects, column 3 includes includes CBG by grade, gender and race fixed effects. Column five includes CBG by grade, gender, race and quintile of predicted arrest fixed effects.

Table 3: Balancing Test - Partnership Models

	(1) Assigned Same School	(2) Assigned Same School & Grade
Male	0.0023	−0.0002
	(0.0051)	(0.0036)
Minority	−0.0046	−0.0031
	(0.0075)	(0.0063)
Single Family Residence	−0.0031	−0.0040
	(0.0196)	(0.0136)
Math Test Score	0.0036	0.0073*
	(0.0047)	(0.0043)
Read Test Score	−0.0027	−0.0035
	(0.0042)	(0.0038)
Total Days Suspended from School	−0.0002	−0.0002
	(0.0006)	(0.0006)
Total Days Absent	−0.0000	−0.0001
	(0.0003)	(0.0002)
Observations	123,982	38,560
F-Stat (p-value)	0.98	0.45

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary correlation within CBG. 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 5 as of 9/1, assigned middle and high school fixed effects for j and CBG fixed effects for person i.

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 5 and normal grade progression. Column 2 excludes same school, different grade pairs and includes 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.

Table 4: Crime Agglomeration Models

	(1) Ever Arrested	(2) Ever Arrested Violent	(3) Ever Arrested Property
Peers = All			
Same School Peers	−0.0014	0.0008	0.0006
	(0.0095)	(0.0036)	(0.0066)
Peers = Same Grade			
Same School Peers	0.0076	0.0165	0.0358***
	(0.0226)	(0.0122)	(0.0131)
Peers = Same Grade-Race-Gender			
Same School Peers	0.0388**	0.0220***	0.0286***
	(0.0153)	(0.0084)	(0.0103)
Peers = Same Grade-Race-Gender High Risk			
Same School Peers	0.0337*	0.0301***	0.0241*
	(0.0192)	(0.0092)	(0.0137)
Peers = Same Grade-Race-Gender			
Same School Peers*Minority*Male	0.0611***	0.0328***	0.0422***
	(0.0229)	(0.0109)	(0.0153)
Same School Peers*Minority*Female	0.0240	0.0203**	0.0241**
	(0.0203)	(0.0100)	(0.0107)
Same School Peers*White*Male	0.0358	0.0009	0.0103
	(0.0380)	(0.0120)	(0.0186)
Same School Peers*White*Female	0.0236	0.0100	0.0148
	(0.0209)	(0.0075)	(0.0143)
Dep. Var (mean)	0.1678	0.0326	0.0713
Observations	34,958	34,958	34,958

* p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors 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 km, second panel expands to those students that are the same grade, 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 9th graders prior to 2002 and not involved in criminal partnerships. We define high risk based on those individuals that fall in 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 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, 5th grade reading and math test scores, indicator if missing a test score or other 5th grade information, days suspended (5th grade) , total days absent (5th grade), single family home indicator, assigned school fixed effects.

The top panel includes Census Block Group 2000 (CBG), second panel includes CBG by grade, 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 bottom set of results provide interaction effects for minority and gender for our model using peers defined as same age-race-gender.

Table 5: Impact of School Assignment on Criminal Partnerships

	(1) Any Crime Partner	(2) Number of Partner Crimes	(3) 16-18 yr old Partnership	(4) 19-21 yr old Partnership	(5) Violent Crime Partners	(6) Property Crime Partners	(7) Felony Partners	(8) Misdemeanor Partners
Pairs $\leqslant$ 1 km								
Assigned Same	0.0033***	0.0050***	0.0027***	0.0006	0.0014***	0.0017**	0.0023***	0.0011**
School & Grade	(0.0007)	(0.0011)	(0.0007)	(0.0005)	(0.0004)	(0.0007)	(0.0007)	(0.0005)
Assigned	0.0021***	0.0025**	0.0013**	0.0011***	0.0005	0.0015***	0.0016***	0.0007**
Same School	(0.0007)	(0.0010)	(0.0006)	(0.0004)	(0.0004)	(0.0005)	(0.0006)	(0.0003)
Dep. Var (mean) for Diff. School	0.00112	0.00138	0.00106	0.00037	0.00037	0.00053	0.00085	0.00043
Observations	123,982	123,982	123,982	123,982	123,982	123,982	123,982	123,982
Pairs $\leqslant$ 1/2 km								
Assigned Same	0.0034**	0.0055**	0.0029*	−0.0002	0.0011	0.0025*	0.0030*	0.0002
School & Grade	(0.0016)	(0.0022)	(0.0016)	(0.0007)	(0.0008)	(0.0014)	(0.0015)	(0.0007)
Assigned	0.0045***	0.0056*	0.0025*	0.0027***	0.0010	0.0035***	0.0035**	0.0018***
Same School	(0.0017)	(0.0029)	(0.0014)	(0.0009)	(0.0008)	(0.0013)	(0.0014)	(0.0006)
Dep. Var (mean) for Diff. School	0.00102	0.00136	0.00102	0.00000	0.00034	0.00068	0.00102	0.00000
Observations	42,601	42,601	42,601	42,601	42,601	42,601	42,601	42,601
Pairs $\leqslant$ 2 km								
Assigned Same	0.0010**	0.0016***	0.0007*	0.0002	0.0002	0.0006*	0.0009***	−0.0000
School & Grade	(0.0004)	(0.0006)	(0.0004)	(0.0002)	(0.0002)	(0.0003)	(0.0003)	(0.0003)
Assigned	0.0009***	0.0011***	0.0006***	0.0004***	0.0003***	0.0005***	0.0007***	0.0003***
Same School	(0.0002)	(0.0003)	(0.0002)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0001)
Dep. Var (mean) for Diff. School	0.00060	0.00069	0.00048	0.00027	0.00017	0.00030	0.00034	0.00034
Observations	397,687	397,687	397,687	397,687	397,687	397,687	397,687	397,687

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary within-CBG correlation in parentheses.

All regressions include controls for gender, race, 5th grade reading and math test scores, indicator if missing a test score or other 5th grade information, days suspended (5th grade) , total days absent (5th grade), single family home indicator, indicator for year individual j turned age 5 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 column heading. Number of partner crimes indicates the number of times a pair of individuals were arrested for the same crime. 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. Felony and Misdemeanor based on the severity of the charge at arrest and coded accordingly by the Mecklenburg County Sheriff's Department.

Table 6: Interaction Effects of School Assigned on Criminal Partnerships

	(1) Any Crime Partner	(2) 16-18 yr old Partnership	(3) 19-21 yr old Partnership	(4) Violent Crime Partners	(5) Property Crime Partners
Assigned to Same School	0.0021***	0.0013**	0.0012***	0.0005	0.0015***
	(0.0007)	(0.0006)	(0.0004)	(0.0004)	(0.0005)
*Same Age	−0.0005	−0.0001	−0.0006	0.0002	−0.0006
	(0.0007)	(0.0006)	(0.0004)	(0.0003)	(0.0005)
*Same Age-Race-Gender*Minority Male	0.0098***	0.0065***	0.0031***	0.0031***	0.0063***
	(0.0020)	(0.0017)	(0.0010)	(0.0011)	(0.0017)
*Same Age-Race-Gender*Minority Female	0.0043***	0.0043***	0.0001	0.0027*	0.0015
	(0.0016)	(0.0016)	(0.0003)	(0.0014)	(0.0009)
*Same Age-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 Age-Race-Gender*NonMinority Female	0.0250	0.0249	0.0004	−0.0003	0.0251
	(0.0253)	(0.0252)	(0.0006)	(0.0004)	(0.0253)
Observations	123,982	123,982	123,982	123,982	123,982

All regressions include controls for gender, race, 5th grade reading and math test scores, indicator if missing a test score or other 5th grade information, days suspended (5th grade) , total days absent (5th grade), single family home indicator, indicator for year individual j turned age 5 as of 9/1, assigned middle and high school fixed effects, and CBG fixed effects for person i.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary within-CBG correlation in parentheses.

All regressions include, but do not report, indicators for all variables used as an interaction with the assigned to same school variable.