Schools, Neighborhoods, and the Long-Run Effect of Crime-Prone Peers

Stephen B. Billings* Mark Hoekstra†

February 7, 2020

Abstract

This paper examines the effect of elementary-aged peers on adult crime and other outcomes by exploiting cohort variation in the proportion of peers with an arrested parent. Importantly, our data enable us to distinguish between the effect of school and neighborhood peers. Results indicate that a five percentage point increase in school and neighborhood crime-prone peers increases adult arrest rates by 6.4 and 2.6 percent, respectively. Additional evidence indicates that adult crime is primarily driven by interactions in schools rather than in neighborhoods. We also document how school and neighborhood peers affect cognitive and non-cognitive outcomes during adolescence.

* *stephen.billings@colorado.edu*, University of Colorado
† *markhoekstra@tamu.edu*, Texas A&M University, NBER, and IZA
Acknowledgments: We thank Amy Ellen Schwartz, Anna Piil Damm, Andrew Hanson & Steve Ross for helpful comments/discussions as well as seminar participants at the University of Colorado, Marquette University, 2018 Urban Economics Association and the 2018 NBER SI Children's Group. 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; Andy Baxter and Susan Freije from Charlotte-Mecklenburg Schools

1 Introduction

There is a large and growing literature on how childhood peers shape outcomes. This literature has primarily studied the effect of school peers, and in doing so has focused on academic and behavioral outcomes measured in school (e.g. Hoxby, 2000a; Deming, 2011; Imberman, Kugler, and Sacerdote, 2012; Lefgren, 2004; Lavy and Schlosser, 2011; Ohinata and Van Ours, 2013; Sacerdote, 2001; Carrell, Fullerton and West, 2009; Angrist and Lang, 2004; Bifulco, Fletcher, and Ross, 2011; Kristoffersen, Kraegpoth, Nielsen, and Simonsen, 2015; and Carrell and Hoekstra, 2010). At the same time, a separate literature has documented the effects of neighborhoods on later life outcomes (e.g., Damm, 2014; Damm and Dustmann, 2014; Kling, Liebman, and Katz, 2007; Chetty, Hendren, and Katz, 2016; Chetty and Hendren, 2016; Bayer, Ross, and Topa, 2008; Kling, Ludwig, and Katz, 2005; Ludwig, Duncan, and Hirschfield, 2001; and Ross, 2011). However, within this literature little is known about the effects of specific neighborhood factors, such as the relative importance of school versus neighborhood peers. The purpose of this paper is twofold. First, we document the impact of school and neighborhood peers on criminal behavior as adults, in addition to other cognitive and non-cognitive outcomes during adolescence. To our knowledge this is the first paper to examine how adult crime is shaped by exposure to early childhood peers outside the school context, and separate from other aspects of neighborhood quality. The second objective of this study is to distinguish between the effect of neighborhood and school peers. In doing so, we speak to the relative importance of living in a particular neighborhood versus attending schools with particular peers, the latter of which can be more easily affected by policy. A distinct advantage of our data is they enable us to estimate and compare the magnitudes of school and neighborhood peer effects. We are also able to test directly whether outcomes are shaped by peers in one's school but not neighborhood, peers in one's neighborhood but not school, or peers in one's school and neighborhood.

We do so using a rich data set in which administrative school records from Charlotte-Mecklenberg County are linked to juvenile and adult arrest data. We use these data to identify children whose parent had been arrested at least once during elementary school. It is well-established in the crime literature that children of criminal parents are significantly more likely to commit crimes themselves. For example, Hjalmarsson and Lindquist (2012) use data from Sweden to show that children with criminal fathers are more than twice as likely to have a criminal conviction themselves; Besemer and Farrington (2012) and Junger, Greene, Schipper, Hesper, and Estourgie (2013) find similar relationships using data from Great Britain and the Netherlands, respectively. Unsurprisingly, we document a similar relationship between the misbehavior and adult crime of a child and the criminality of the parent using our administrative data from North Carolina even after conditioning on school and neighborhood fixed effects and other controls.[2] We classify these children as crime-prone peers. Importantly, this measure of peers is unlikely to be due to reflection or a common shock that affects a given cohort of children (Manski, 1993). We ask whether exposure to these peers - who are at risk for future criminal activity for a reason exogenous to the other children in the school or neighborhood - affects outcomes. Importantly, while we assume that this measure of peers is exogenous to the other students in the school or neighborhood, we make no assumptions regarding the exact mechanism through which criminal tendencies are transmitted across generations (e.g., nature versus nurture), or how these children may affect their peers.

To distinguish the effects of school and neighborhood peers from confounding factors due to nonrandom selection, we exploit the natural population variation across cohorts. Intuitively, we compare children in a given school or neighborhood whose cohort has an idiosyncratically high or low proportion of peers linked to an arrested parent. Importantly, we show that this variation in peers is consistent with a random process, and is uncorrelated with other observed determinants of cognitive and non-cognitive outcomes. We also show that this measure of peers is uncorrelated with outcomes for the cohorts one-year-older or one-year younger. Collectively, this suggests that for a common shock to drive our effects, it would need to affect one school- or neighborhood-cohort but not adjacent cohorts, and would have to be uncorrelated with other observed student and family characteristics.

Results indicate that exposure to crime-prone peers has large and significant effects on adult crime, as well as other cognitive and non-cognitive outcomes. We estimate that a five percentage point increase in the share of crime-prone school peers results in a 6.4 percent increase in the probability of being arrested, and a 4.5 percent increase in days incarcerated. Both effects are statistically significant at the 5 percent level and are measured at ages 19 - 21, after the students leave school. This implies there are significant long-run implications of exposure to crime-prone peers during childhood. We also show this effect is driven primarily by exposure to crime-prone peers in school rather than in

[2] In using parental arrest as a signal of a child's propensity to misbehave and commit adult crime, this study differs from the objective and approach of the literature that examines the effect of parental incarceration on outcomes. These studies identify effects either by exploiting within-parent variation in incarceration over time (Billings, 2017) or variation across parents who are quasi-randomly assigned different penalties (Norris, Pecenco, and Weaver, 2018; Bhuller, Dahl, Loken, and Mogstad, 2018; Arteaga, 2018; Dobbie, Gronqvist, Niknami, Priks, and Palme, 2018). For example, Billings (2017) finds that parental incarceration leads to improved outcomes for children in Charlotte-Mecklenberg County, even though children whose parents are arrested and/or incarcerated have worse outcomes than their counterparts.

one's neighborhood. We conclude this based on a comparison of the overall magnitudes as well as results in which we simultaneously estimate the effect of peers with whom one shares only a school, or only a neighborhood, or both a school and neighborhood. Results also indicate effects on adult crime are evident only for black students, and not for white. This contrasts with Carrell, Hoekstra, and Kuka (2018), who find that the long-run effects of peers linked to domestic violence on labor market outcomes are present only for whites, and not blacks.

In addition, we also estimate that a five percentage point increase in crime-prone peers at school results in a performance reduction of 0.015 standard deviations, similar to previous work on the effect of peers linked to domestic violence (Carrell and Hoekstra, 2010). We also find evidence of effects on antisocial behavior during middle and high school. Results indicate that a five percentage point increase in one's share of crime-prone peers at school or in the neighborhood results in a 0.01 standard deviation in antisocial behavior, though only the neighborhood peer effect is significant at the 5 percent level. Effects on antisocial behavior at the school level are driven by increases in school crimes (9.9 percent increase), while effects of neighborhood peers are strongest on high school dropout (3.9 percent increase).

This paper makes two main contributions. The first is to document how exposure to crime-prone peers during childhood leads to long-run effects on adult crime. In doing so, it complements two other literatures. The first is the literature on the long-run effects of early childhood interventions. These studies have examined the long-run effects of factors such as kindergarten assignment and class size (Krueger and Whitmore, 2001; Chetty, Friedman, Hilger, Saez, Schanzenbach, and Yagan, 2011; Dynarski, Hyman and Schanzenbach, 2013), lead exposure (Aizer and Currie, 2017; Billings and Schnepel, 2017; Feigenbaum and Muller, 2016; Reyes, 2007); Head Start and the Perry Preschool programs (Garces, Thomas and Currie, 2002; Grosz, Ho, Kose, Marek, and Shenhav, 2017; Ludwig and Miller, 2007; Heckman, Pinto and Savelyev, 2013), and teacher value-added (Chetty, Friedman, and Rockoff, 2014). In assessing the effect of peer exposure, this paper is closely related to studies by Bifulco, Fletcher, Oh, and Ross (2014), Black, Devereux, and Salvanes (2013); Carrell, Hoekstra and Kuka (2018), and Gould, Lavy, and Paserman (2009). The difference between this paper and those is while those studies examine effects on educational achievement, attainment, and labor market performance, this study focuses on the role of peers in shaping criminal behavior in young adulthood.

Our paper also complements a second literature that shows how peers more generally affect crime. This includes papers on peer effects using interactions in juvenile corrections and prisons (Bayer, Hjalmarsson, and Pozen, 2009; Damm and Gorinas, forthcoming;

Stevenson, 2017) or exposure of 14 - 29 year-olds to adult drug users in public housing (Pons Rotger and Galster, 2019). This literature also examines the effect of neighborhood interactions with adult peers who are more disposed to criminal activity due to a prison release (Drago and Galbiati, 2012) or due to the birth of a first-born daughter rather than son (Dustmann and Landerso, 2018). Our paper uses a similar research design as Kim and Fletcher (2018), who use ADD Health survey data and cohort variation to examine the effect of peers with an incarcerated father on self-reported measures of criminal behavior. Our paper differs in that we examine effects on adult crime, separately estimate the effects of neighborhood and school peers, and do so using administrative data on parental arrest and child outcomes. Our paper is also similar to Larsen and Kristensen (2017) who examine the effect of exposure to peers with criminal records in upper secondary school on crime in the first 12 months of vocational school. Our paper differs in that we examine effects of earlier exposure during childhood, separately examine effects from school and neighborhood peers, and examine effects on a range of outcomes including adult crime after high school is completed.

The second contribution of this study is to assess the relative effects of school versus neighborhood peers. As alluded to above, previous work has clearly established both the effect of school peers and the effect of neighborhoods, the latter of which captures the effects of neighborhood peers as well as schools, school peers, and other neighborhood characteristics. However, to our knowledge, no study has directly estimated and compared the impact of school and neighborhood peers within a single setting. The most closely related paper is Billings, Deming & Ross (2018), who highlight the importance of schools in the formation of criminal partnerships for children living in the same neighborhood. But since identification in that study is based on discontinuities at school attendance boundaries, it is unable to estimate the direct impact of neighborhood on adult criminal activity. Similarly, Billings, Deming, and Rockoff (2013) examine the effects of changes in school racial composition (and other factors correlated with it) due to a 2002 rezoning in Charlotte-Mecklenburg.[3] Our paper differs in that we identify the effect of exposure to both neighborhood and school peers, and we do so using variation that is not accompanied by large-scale changes in other school and peer characteristics.

The limited literature on school versus neighborhood peers is due to the fact that school attendance zones often imply students attend schools with children who are also from their neighborhood. A unique feature of our data is that we observe both neighborhood

[3]In this study, we define schools and neighborhoods prior to the 2002 change, at which point students in our sample had completed the 3rd grade. As a result, the re-zoning is not used to identify effects in our study, and if anything attenuates our estimates due to reassignments of some students during middle school.

peers and school peers in the same setting and define the set of peers who are in one category but not the other.[4] This enables us to do two things. First, we can estimate effects separately, thereby replicating the approach and findings of previous papers that show effects of either neighborhood or school peers. We can also compare the magnitudes of the effects to each other within the same setting. Second, because we observe students who live in the same neighborhood but attend different schools, and vice versa, we can test directly which peers matter. Specifically, we show that while individuals are affected by crime-prone peers in their neighborhood, these effects seem primarily driven by those crime-prone peers who also attend the same school. To address concerns that the relative absence of neighborhood peer effects is because neighborhood peers are more difficult to identify than school peers, we perform several exercises. First, we show robustness of these findings to a range of neighborhood definitions. Second, we document that neighborhood peers from adjacent cohorts also have no effects. Finally, we show that while school peers affect outcomes, the effect of those peers is no larger when they also live in one's same neighborhood. This suggests that neighborhood exposure seems to matter little even for those peers who influence outcomes due to interactions at school.

Our results have important implications. First, these findings highlight the importance of childhood peers in shaping socially deviant behavior years later, even into adulthood. This is especially important given the persistence of criminal behavior in adulthood.[5] While our results only directly speak to effects in early adulthood, results from Billings and Schnepel (2018) show that rates of recidivism in Mecklenburg County are highest among a population of criminals who have prior incarcerations or who are arrested as a young adult.[6] In fact, Mecklenburg County criminal records indicate that of individuals who are arrested between ages 19 and 21, 54 percent of them are arrested again within 2 years and 31 percent are incarcerated at least once before age 30.[7] This suggests that the effects of crime-prone peers documented in this study impose significant costs on both the individual and broader society.

In addition, the results here demonstrate the relative importance of school peers, rather than neighborhood peers, in shaping long-run outcomes. The fact that this pattern

[4] The source of variation in school attended for students in the same neighborhood occurs due to attendance boundaries bisecting neighborhood boundaries as well as school choice away from assigned school.

[5] There is a substantial sociology and criminology literature that documents the persistence of criminal activity throughout life. Some examples include Sampson and Laub (1990), Sampson and Laub (2005), and Nagin and Farrington (1992).

[6] Mecklenburg county contains the entire city of Charlotte as well as a few bedroom communities adjacent to Charlotte, NC.

[7] Author's calculation using Mecklenburg County Sheriff's Department administrative records from 1998-2013.

persists even into adulthood, when the individuals are no longer in school, highlights the importance of schools as perhaps the primary context in which life-shaping peer effects occur.

2 Data

In order to identify the long-run effects of crime-prone peers on adult outcomes, we use four linked administrative data sets from Charlotte-Mecklenberg County, North Carolina. These include detailed pupil records from 1999-2011; detailed arrest records from 1998-2011; Mecklenburg County jail records from 1998-2011; North Carolina State Prison Records from 1998-2011 and NC state birth records for children born 1989-2001. In order to allow all individuals to be observed for at least two years in the public high school records as well as the arrest and incarceration records, we limit the sample to those children born between 1989 and 1994 in order to examine adult outcomes.[8]

The education data include student race, gender, and home address, as well as yearly end-of-grade test scores for grades 3 through 8 in math and reading, which we standardize at the state level by grade and year. In addition, the education data include days absent, days suspended from school, and number of incidents of school crime. Per NC State Statute 115C - 288(g), these data include any incident at school involving any violent or threats of violent behavior, property damage, theft or drug possession, all of which must officially be reported to the North Carolina school crimes division.[9]

The three databases from the criminal justice system include date of arrest, demographic information about the criminal including full name and date of birth, the home address of the arrestee, criminal charges, and all subsequent jail and prison periods of incarceration. Children are matched to their later criminal justice outcomes based on full name and date of birth.

In order to implement our study, we need to link children to the arrest records of their parents. This entails linking criminal justice records to school records. We match these records using last name and residential address.[10] Residential address is included in student records for each school year and residential (home) address is recorded in the

[8]We note that these children had all completed the 3rd grade prior to the 2002 rezoning studied by Billings, Deming, and Rockoff (2014). We also fix our definition of schools and neighborhood definitions prior to the 2002 rezoning.

[9]This statute ensures that this measure of school crime is consistently reported across schools and cannot be treated differently based on school administrators.

[10]One of the main assumptions is that a child is given the same last name as the parent. This cannot directly be verified for our sample, but for the universe of birth records in our sample, 65% of children share a last name with their birth mother and 85% with their birth father.

criminal arrest records when an adult is arrested as part of the arrest intake procedure. Nearly all students (98%) have geocodable addresses and 85% of adults have geocodable addresses for residence at the time of arrest.

The advantage of an address based method of parental matching is twofold. First, it leverages the detailed and well-populated address information available in the student and criminal justice records. Second, it allows this analysis to focus on parental figures that live with the child thus eliminating concerns of absentee fathers. The Appendix provides more detail on address-based matching, which has been used in previous work (Billings (2017)).[11]

Of course, we cannot directly verify parents, so in some cases this matching may capture other relatives with the same last name living in the home with the child. This is unlikely to be problematic for capturing parental environment since these other relatives also provide "parental" guidance to the child. In addition, in subsequent analyses we show robustness to alternative definitions of parental matching. However, there are other practical problems with using address and last name to match students. One concern is the uniqueness of last name and address in a given year. This potential problem is most likely for children with common last names living in large apartment complexes. This is due to the fact that apartment numbers are not provided in the student records since addresses are only used for school assignment in this database. Mailing addresses and contact information is not made available to outside researchers. We address this issue in several ways. First, in cases where student records match multiple arrested individuals that have the same address, gender and last name but different first names or dates-of-birth, we do not consider that individual to have a criminal parent. Second, we also limit matching criminal parents to students living in a larger apartment complex, which we define as having more than five units. Approximately 5% of arrests are duplicated in terms of names and addresses and 15% of arrests are linked to students living in large apartment complexes (> 5 units). This procedure will provide the most conservative estimates of kids with criminal parents. In Section 4.5 we show our findings are robust to expanding the definition of crime-prone kids to include duplicate parental matches and children living in large apartment complexes.

Using these four linked data sets, we are able to observe several outcomes for each student in our sample. Cognitive outcomes include the average math and reading test score from

[11] An alternative method would be to use birth records to link children to parents and then link parents to arrest and incarcerations records using full names and date-of-birth. However, this is problematic due to the large number of birth records that contain missing information on fathers as well as the overall low match rate between birth records and the student database. For example, we are only able to match 66% of student records to birth records, which is 20 percentage points lower than Figlio et al (2016)'s study in Florida in which data were matched using social security numbers.

grades 3 - 5 and from grades 6 - 8, as well as an indicator for whether the student repeated a grade between grades 6 and 8. We also have several non-cognitive outcomes and measures of antisocial behavior. These include days absent and days suspended during grades 6 - 10, as well as school crimes committed during grades 6 - 10. In addition, we observe if individuals dropped out of high school, and if they were arrested from ages 16 - 18. In addition to looking at these outcomes separately, we also use them to generate an education index and an antisocial behavior index using the method described by Anderson (2008). Finally, our main outcome of interest is an indicator for whether the individual was arrested as an adult, between ages 19 and 21. We also observe arrests by category of crime (violent, property, or alcohol/drug), and total days incarcerated.

The main sample for this analysis consists of between one and three observations per student based on cohort composition in 3rd-5th grades. In cases where we do not have a student observation in 5th grade, we drop all years for that observation. We weight observations by the inverse of the number of years a student appears in our dataset. We also drop observations where a student lives in a neighborhood with less than 5 peers or attends a school with less than 10 peers in order to limit the influence of unusual cohort sizes.[12] Finally, we exclude from the main sample all children linked to an arrested parent, in order to clearly delineate between the peers who are causing the peer effect and those who are affected (Angrist, 2014).

Summary statistics are shown in Table 1. The first column shows means for all students in the sample and the first panel shows outcomes. Test scores are normalized to have zero mean and standard deviation one, as are the education and antisocial indices. Twenty-six percent of the individuals in our sample repeat a grade sometime between 6th and 10th grades. The average number of total days absent and suspended in 6th through 10th grade are 36.5 and 7.8, respectively. Eight percent of our sample dropped out of high school by age 18, and eight percent were arrested between the ages of 16 and 18. Finally, 11 percent of our sample was arrested in early adulthood at ages 19 - 21.

Background characteristics are shown in the second panel of Table 1. Half of our sample is male, 43 percent are black, and 7 percent are Hispanic. Three-quarters live in a single family residence, and median neighborhood household income is $54,440, where neighborhood attributes are based on 2000 Census Block Groups (CBG).

The third panel of Table 1 shows information on peer groups. The average cohort size for school peers (defined at the school-by-grade-by-year level) is 121. Neighborhood cohort sizes (defined at the neighborhood-by-grade-by-year level) are somewhat smaller at 32 students, while the intersection of the two averages 16 students. For our main analysis,

[12] Results are similar when these observations are included.

in order to define neighborhoods to be as geographically small and contained as possible, we define them as the intersection of Census Block Groups and (future) elementary by middle school attendance boundaries (02-03). This gives us 491 neighborhoods in the Charlotte-Mecklenburg school district, while still ensuring that neighborhood and pre-2002 school boundaries that determined elementary school attendance for our sample are not collinear.[13] In Section 4.5 we show that our main findings are robust to alternative neighborhood definitions.

Our empirical approach requires that we identify children who are at risk for committing crimes in the future for reasons that are exogenous to their school or neighborhood peers. We do so by exploiting the well-documented finding in the crime literature that children of arrested or convicted parents are themselves more likely to be arrested or convicted (Hjalmarsson and Lindquist, 2012; Besemer and Farrington, 2012; and Junger, Greene, Schipper, Hesper, and Estourgie, 2013). Specifically, in our data we identify peers as those whose parent was arrested in elementary school.[14] For ease of exposition, we refer to these at-risk-of-future-misbehavior-and-crime-peers as crime-prone peers. We note, however, that we are agnostic about the mechanisms through which children linked to parental arrest may affect their peers. Instead, our focus is on estimating the reduced-form impact of exposure to these peers. In addition, we are also agnostic about whether the intergenerational transmission of crime is due to nature, nurture, or some combination or interaction of the two.[15]

Given the consensus in the literature on the high correlation between parent and child crime, it is unsurprising that we find similar intergenerational correlations in our data. Outcomes for children whose parents have not and have been arrested, respectively, are

[13] After 2002, these zones were redrawn as part of the court-ordered ending of desegregation and busing from satellite attendance zones. This reassignment is described in more detail by Billings, Deming, and Ross (2014).

[14] This is the earliest we can match parent name and address to student name and address, as we do not observe student address in the years prior to elementary school.

[15] There is some evidence that the reason for these children's future criminal activity is due at least in part to parental behavior. Hjalmarsson and Lindquist (2012) estimate that 60 to 80 percent of the intergenerational relationship can be explained by parental human capital and parental behaviors. Hjalmarsson and Lindquist (2013) show that while differences on the extensive margin are affected by both pre-birth and post-birth factors, differences on the intensive margin are primarily driven by post-birth factors. Similarly, Van de Weijer, Bijleveld, and Blokland (2014) show that in particular it is paternal violent offending during the son's childhood and adolescence, rather than before birth, that predicts future criminal behavior by the child. In Appendix Figures A2 and A3, we show how student educational performance and misbehavior are associated with the timing of the parental arrest. Figure A2 shows that educational performance drops one year prior to the arrest, then rises slightly in the years that follow. Figure A3 shows a spike in misbehavior in the year before and year of arrest, though misbehavior rates are also elevated before and after that. We interpret this as perhaps further evidence that parental behavior may be driving some of the behaviors that likely spill over onto peers. However, we emphasize that we see evidence of poor educational performance and misbehavior both well before and after the parental arrest, consistent with longer-term differences in these families.

shown in columns 2 and 3 of Table 1. The fourth column shows the difference. Overall, children linked to an arrested parent are significantly more likely to engage in antisocial behavior. Children with a parent who has been arrested are absent for 71 percent more days, suspended for 156 percent more days, are 129 percent more likely to drop out of high school, and are 157 percent more likely to be arrested between ages 16 and 18. In addition, children with an arrested parent are more than twice as likely to be arrested as an adult (23 versus 10 percent).

We also examine the extent to which these differences remain after controlling for neighborhood and school fixed effects. These conditional means for children linked and not linked to a parental arrest are shown graphically in Figure 1. Specifically, the dark bars represent mean residuals for each group after regressing the outcome on year-by-grade and school-by-grade-by-neighborhood fixed effects, where neighborhood is defined as 2000 Census Block Group and school is defined as the pre-2002 rezoning elementary school boundary. The three outcomes include a behavior index (mean=0; sd=1), an education index (mean =0; sd =1), and an indicator if one is ever arrested from age 19 to 21. As shown in Figure 1, the dark bars indicate that children linked to a parent arrest have much worse outcomes than their counterparts even after conditioning on school and neighborhood effects. Specifically, these children have behavioral and education indices that are over 0.25 standard deviations worse. They are also 13 percentage points (130 percent) more likely to be arrested as an adult. In short, Figure 1 shows that even after conditioning the effect of neighborhoods and assigned schools, children linked to arrested parents have much worse outcomes than their counterparts.

Figure 1 also shows the extent to which this conditional difference in outcomes can be explained by individual student characteristics such as gender, race, and single-family residence. These conditional means are shown in the light bars of Figure 1, which indicate that these differences persist even after controlling for individual characteristics. Collectively, the results shown in Figure 1 demonstrate that parental arrest is strongly predictive of poor academic performance, misbehavior, and adult crime even after controlling for neighborhood, school, and individual characteristics.

3 Empirical Strategy and Model

There are three major potential problems in estimating peer effects. The first is reflection, which refers to the problem that in addition to being affected by one's peers, one can also affect one's peers (Manski, 1993). We argue that our measure of crime-prone peers enables us to overcome this problem. Specifically, rather than defining crime-prone peers as those who show signs of antisocial behavior—which could be due at least in part to reflection—as described earlier we instead define them as children of a parent who has been arrested. While these children are systematically more likely to engage in antisocial behavior, we also argue that it is unlikely that a parent's arrest is due to her child's peers. Specifically, an identifying assumption of our research design is that whether a student is crime-prone—that is, whether a student has a parent who has been arrested in elementary school—is unaffected by her school-cohort or neighborhood-cohort peers. We view this assumption as reasonable.

The second potential problem is common shocks, which refer to the potential for another factor, such as a local economic shock, to affect both a student and the likelihood her peers are linked to a parental arrest. This could lead to a positive correlation even in the absence of a peer effect. We also argue that common shocks are unlikely to be a problem in our context. This is in part because we allow for grade-by-year fixed effects to control for common shocks across the district. Perhaps more importantly, we define a child as crime-prone if that child's parent was arrested at any time during elementary school, not just in a particular year. For example, suppose that a local neighborhood economic or drug shock were to cause an increase in parental arrests in a given year, while simultaneously also affecting child outcomes in that year. Because we define the peer group of interest as parents who were arrested at any time while the child was in elementary school, multiple cohorts (and possibly all cohorts) will be affected by that shock. In contrast, if we defined peer as someone whose parent was arrested in that year, and examined outcomes in that same year, our analysis would be much more susceptible to the common shock problem. For this reason, it is difficult for us to think of a shock that would generate an increase in arrest rates (as measured across years) for parents of one cohort, and also affect the children of that one cohort, without affecting other cohorts (including adjacent ones). In Section 4.5 we test for this directly by simultaneously estimating the (null) effect of exposure to peers one year older or younger.

The third problem in estimating peer effects is selection, or homophily. In our context, this means that children with parents who have been arrested—who have been shown in other contexts and in this particular one to have higher propensities for antisocial behavior and adult crime—are more likely to live near and attend school with other children with similar propensities. To overcome this problem, we borrow a methodology from recent papers in the peer effects literature in education that exploit cohort-to-cohort variation to identify effects.[16] The intuition of this approach is to compare the outcomes of otherwise similar individuals who are enrolled in the same school and grade in different

[16] This approach was pioneered by Hoxby (2000b) to identify the effect of class size, but has since by used by many other papers to identify peer effects in lower education.

years, and therefore are exposed to idiosyncratically more or fewer crime-prone peers. Similarly, to identify neighborhood peer effects, this approach compares individuals who grow up in the same neighborhood but are different ages, and therefore are exposed to more or fewer crime-prone peers of their same age in their neighborhood.

One potential limitation of this approach is while students must mechanically spend time with peers from their same age cohort while in school, the same is not necessarily true for neighborhood peers. As a result, one might be concerned that additional measurement error in defining neighborhood peers may attenuate estimates of neighborhood peer effects, at least compared to estimates of school peer effects. We do several different things to address this. First, in Appendix Table A3, we test for neighborhood peer effects from those peers who are one year older or younger. In addition, in Appendix Table A4 we show estimates for alternative neighborhood definitions larger and smaller than that used for our main analysis. Finally, we also estimate the additional marginal impact a school peer has when he lives in one's same neighborhood. We hypothesize that if neighborhood peer effects are large, then school peers from one's same neighborhood should have a larger impact than they otherwise would.

Table 2 contains a summary of the cohort variation we use to identify effects. As shown in the top panel, roughly eight percent of school and neighborhood peers have a parent linked to an arrest and are consequently defined as crime-prone. School peers are defined as those students who attend the same grade and school in the same year, and neighborhood peers are those who are the same age and live in the same Census Block Group and 02-03 elementary-middle school attendance zone. In addition, we also define three other groups: peers who share the same school but not neighborhood, peers who share the same neighborhood but not school, and peers who share the same school and neighborhood. The fraction of crime-prone peers in these three categories is 8.4, 8.8, and 6.3 percent, respectively.

In addition, the bottom panel of Table 2 shows the cohort variation in crime-prone peers over time. The across-cohort standard deviations in the school and neighborhood levels are 2.3 and 4.9 percentage points, respectively. By comparison, the across-cohort standard deviations in crime-prone peers at the school-but-not-neighborhood level, neighborhood-but-not-school level, and school-and-neighborhood level are 2.5, 10.3, and 8.8 percentage points, respectively.

The main model for estimating the impact of peer composition in terms of peers with parental arrest is based on Equation 1.

$$Y_{igsjt} = \alpha + \theta_1 \mathsf{PeerParentArrest}(\%)_{igsjt} + \\ + \beta \mathbf{X}_{igsjt} + \gamma_{gt} + \lambda_{sjg} + \epsilon_{igsjt} \tag{1}$$

where for any definition of peer, $\mathsf{PeerParentArrest}(\%)_{igsjt} = \frac{\Sigma^{k \neq i} \mathsf{ParentArrested}_{kgsjt}}{n_{gsjt}-1}$; $\mathbf{X}_{igsjt}$ represents a vector of student attributes and cohort fixed effects based on assigned school start year for kindergarten and normal grade progression (age 5 by September 1st);[17] γ_{gt} indicates a grade g by year t fixed effect and λ_{sjg} is a school s by neighborhood j by grade g fixed effect. In additional specifications we also include cohort controls for race, gender, and whether the student was living in a single-family residence, as well as cohort size. Standard errors are clustered by school and cohort and also by neighborhood and cohort. In addition, in Appendix Table A8 we report results when we two-way cluster at just the school and neighborhood levels, which are nearly identical.

We also perform an empirical test of whether year-to-year variation at the school, neighborhood, and school-by-neighborhood for a given grade is consistent with a random process. Following the resampling technique used in Carrell and West (2010), for each cohort in each school or neighborhood by grade combination, we first randomly draw 1,000 cohorts of equal size, drawn from the relevant school/neighborhood and grade. Secondly, for each of the random cohorts we compute the average proportion of peers with arrested parents. Thirdly, we compute empirical p-values for each of these random draws. Each empirical p-value is calculated as the proportion of simulated cohorts with a level of exposure to peers with arrested parents that is smaller than the average actually observed in that cohort. If the year-to-year variation for our different measures of peers is random, we expect the distribution of the p-value to be uniform. Hence, we use a Kolmogorov-Smirnov one sample equality of distribution test to test whether the distribution of p-values is uniform for our three main definition of peers. Using a standard 5% significance level, we reject uniformity only 6 times out of 95 for schools; 50 times out of 377 for neighborhoods and 78 times out of 617 for neighborhoods-by-schools.

In addition, we also perform a balancing test. Under the identifying assumption of our research design, all observed and unobserved determinants of antisocial behavior and adult crime are orthogonal to the across-cohort variation in exposure to crime-prone peers. We test the extent to which this is true by regressing our measures of exposure to crime-prone peers on exogenous individual characteristics including indicators for male,

[17] In assuming normal grade progression, we avoid potentially assigning treatment based on an outcome (grade retention) that could itself be affected by peer exposure.

black, Hispanic, stand-alone residence, limited english proficiency (lep) and academically gifted.[18] Each specification follows the same form as equation (1) above in that it includes year-by-grade and school-by-grade-by-neighborhood fixed effects.

Results are shown in Table 3. The first two columns show the correlation between school and neighborhood peers and other characteristics *before* we condition on school by grade by neighborhood fixed effects. As expected, we find strong correlations between these measures and demographic variables such as black, Hispanic, stand-alone residence, limited english proficiency (lep) and academically gifted.

However, in columns 3 - 7 we show balancing tests once we condition on school by grade by neighborhood effects. Specifically, we regress five measures of school and neighborhood peers on indicators for male, black, Hispanic, stand-alone residence, limited english proficiency (lep) and academically gifted. Of the 30 estimates shown in columns 3 - 7, three are significant at the 10 percent level and none are significant at the 5 percent level. In addition, we fail to reject the null hypothesis that the coefficients on these covariates are jointly equal to zero. This is consistent with our identifying assumption.

In summary, we identify effects by exploiting the within-neighborhood and within-school cohort-to-cohort variation in exposure to peers with arrested parents. We show that this variation is consistent with a random process. We also document that this variation is uncorrelated with observed exogenous student characteristics, consistent with the identifying assumption. Finally, we demonstrate that students classified as crime-prone are significantly more likely to engage in antisocial behavior during middle and high school, and to engage in criminal activity as a young adult.

4 Results

4.1 Cognitive outcomes

We begin by examining the effects on educational outcomes. To measure achievement, we use the average test score during 3rd – 5th grade and the average score from 6th to 8th grade. In addition, we also measure whether a student repeated a grade between the 6th and 10th grade. We also transform these three measures of educational performance into an indexed measure using the procedure outlined by Anderson (2008).

Results are shown in Table 4. In the top panel, we estimate the impact of school peers.

[18]Measures of limited english proficiency (lep) and academically gifted are based on state standardized assessments that usually occur when a student first enters the public school system.

It is important to note that in doing so, we are applying the approach of recent papers that have identified the impact of school peers. We note, however, that due to the large degree of overlap between school peers and neighborhood peers, it is difficult to infer from the results in the top panel whether it is school peers, neighborhood peers, or the intersection of both who drive any effects. Similarly, the middle panel of Table 4 shows results for neighborhood peers, who are defined as children who are of the same age and live in the same Census Block Group. However, we again note that due to the overlap of school attendance zones and neighborhoods, for these results it is difficult to distinguish the effect of neighborhood peers from the effect of school peers.

Results from the top two panels of Table 4 indicate that while crime-prone school peers have significant effects on elementary and middle school test scores, peers defined at the neighborhood level have no effect. Point estimates of -0.49 and -0.38 in the top panel indicate that a five percentage point increase in exposure to crime-prone peers results in test score reductions of 2.5 (0.05*0.49) and 1.9 (0.05*0.38) percent of a standard deviation on elementary and middle school test scores, respectively.[19] Effects on grade repetition are shown in column 3 of Table 4. While estimates are positive, neither effects of school or neighborhood peers are statistically significant at conventional levels.

Estimates in column 4 show the result using the education index. The estimates there indicate that while there is little effect of neighborhood peers, a five percentage point increase in crime-prone peers at school results in a statistically significant 0.015 (0.05*0.29) standard deviation reduction in academic performance. This reflects the overall pattern of results in the top two panels, which suggests that school peers, not neighborhood peers, seem to drive effects on cognitive outcomes.

While the results above suggest school peers matter more than neighborhood peers, it is difficult to know for sure given the overlap of school attendance zones and neighborhoods. For example, it is difficult to know whether all crime-prone peers at school matter, or if it is only those who also live in one's same neighborhood who have effects. In order to speak to this more directly, we simultaneously estimate the effect of peers from three groups: school peers from other neighborhoods; neighborhood peers who attend other schools; and neighborhood peers who attend the same school.

Results are shown in the bottom panel of Table 4. Consistent with the results above, we

[19]The effect during elementary school is similar to the effect of a similar increase in peers linked to domestic violence, estimated as reductions of 1.7 and 2.9 percent of a standard deviation for all peers and male peers, respectively (Carrell, Hoekstra, and Kuka, 2018). However, it is smaller than the (rescaled) estimated effect of exposure to a peer with a criminal parent reported by Kristoffersen, Kraegpoth, Nielsen, and Simonsen (2015). They estimate that adding one such student to a cohort averaging 50 students-a two percent increase in exposure—reduces reading scores by 1.68 to 2.89 percent of a standard deviation.

find no evidence that peers from the same neighborhood but who attend other schools affect cognitive outcomes. In contrast, we find significant evidence that peers from the same school (but different neighborhoods) lead to declines in cognitive outcomes. The estimate in column 4 indicates that a five percentage point increase in exposure leads to a statistically significant decline of 1.1 percent of a standard deviation in educational outcomes. Finally, we also find significant effects of peers from the same neighborhood and school. The estimate in column 4 indicates that a five percentage point increase in exposure results in a 0.4 percent of a standard deviation reduction in educational outcomes. It is perhaps surprising that this estimate is smaller than the estimated effect of crime-prone peers from the same school but different neighborhood. This difference could well be spurious, as the two coefficients for education index are not statistically different from each other. Alternatively, it could be that the difference is due to parental behavior. For example, while parents may know who the bad actors are in the neighborhood and encourage their children to avoid those children even at school, they may not know which school peers from other neighborhoods may have large negative effects on their children. Finally, this difference in magnitudes is at least partly due to specification combined with the difference in size of the two peer groups. This is evident by examining the marginal effects of a one standard deviation increase in exposure for all three groups, which are shown at the bottom of Table 4. The estimated effects of one standard deviation increases in exposure are 0.6 and 0.6 percent of a standard deviation declines in educational performance for just-school-peers and neighborhood-and-school peers, respectively. As a result, we are agnostic about the relative effect of own-school peers from the same versus different neighborhoods. Instead, our main conclusion from Table 4 is that effects on cognitive outcomes are driven by school exposure to crime-prone peers, rather than neighborhood exposure.

In summary, our results on the effect of crime-prone peers on cognitive outcomes indicate that a five percentage point increase in exposure to crime-prone peers reduces educational performance by just over one percent of a standard deviation. In addition, it is exposure to crime-prone peers from the same school that affect performance, rather than crime-prone peers from the same neighborhood who attend different schools.

4.2 Behavioral outcomes

We now turn to the effects of crime-prone peers on non-cognitive outcomes measuring antisocial behavior. Results are shown in Table 5, which includes five outcomes. Specifically, we observe days absent, days suspended, and school crimes, all of which are measured between the 6th and 10th grade. In addition, we also observe whether the individual dropped out of high school, and if he or she was ever arrested between the ages of 16 and 18. Finally, in column 6 we show results on the index of all five antisocial behavioral outcomes.

As in Table 4, the top and middle panels estimate the impact of crime-prone peers measured at the school and neighborhood levels, respectively. We are thus estimating effects as though we were identifying the effects of school peers or neighborhood peers, even though there is considerable overlap between the two.

Estimates in the top panel of Table 5 are all positive, suggesting that exposure to crime-prone school peers is associated with significant increases in antisocial behavior. However, only the effect on school crimes is significant at conventional levels, and indicates that a five percentage point increase in crime-prone peers is associated with a 9.3 percent increase in school crimes (0.05*2.08/1.06). Estimated effects of neighborhood peers are also positive, and estimates on high school drop out and ever arrested at age 16 - 18 are significant at the 5 percent level.[20] Estimated effects on the index of antisocial behavior, shown in column 5, are similar in magnitude. However, only the effect of neighborhood peers is significant at the 5 percent level. The estimate there implies a five percentage point increase in crime-prone neighborhood peers results in an increase in antisocial behavior of 8.8 percent of a standard deviation.

In the bottom panel of Table 5, we simultaneously estimate the effect of crime-prone peers from the school but not neighborhood, neighborhood but not school, and both the school and neighborhood. The strongest results are the negative effect of crime-prone peers from the school but not neighborhood on school crimes in column 4, which is significant at the 1 percent level. However, most estimates are imprecise. This imprecision could be do to the subjective nature of some of our behavioral outcomes, which may be influenced by peers in school or other types of measurement error. For example, the weaker effects for suspensions could arise if an increase in crime-prone peers makes an individual student look better-behaved and thus subject to less suspensions. Similarly, offenses for juveniles may go unreported even as similar offenses for adults might not.[21] Overall, our main takeaway from the results in Table 5 is that there is some suggestive evidence that exposure to crime-prone peers leads to increases in antisocial behavior during middle and high school.

[20] The overall pattern of neighborhood peer effects shown in the top panels of Tables 4 and 5 are consistent with Gibbons, Silva, and Weihardt (2013), who report effects of neighborhood peers on self-reported attitudes and misbehavior but not on test scores.

[21] Per NC State Statute 115C - 288(g), our measure of school crimes includes any incident at school involving any violent or threats of violent behavior, property damage, theft or drug possession, all of which must officially be reported to the North Carolina school crimes division.

4.3 Adult Crime

Next, we turn to the effect of crime-prone peers on adult crime, which is our main outcome of interest. We begin by showing Figures 2 and 3, which graph predicted and actual adult arrest rate against exposure to crime-prone peers at the school and neighborhood levels, respectively. Peer exposure is defined relative to the mean for that particular school-grade, or neighborhood-grade, and thus measures the extent to which the individual was exposed to an idiosyncratically low or high proportion of crime-prone peers.

Individuals are grouped into 25 equal-sized bins. The diamond symbols represent predicted arrest rates. To predict arrest rate, we first regress an indicator for whether an individual was arrested on year-by-grade and school-by-grade-by-neighborhood fixed effects, as well as all other exogenous characteristics such as gender, race, residential living status, limited english proficiency, and academically gifted status. We then used this equation to predict arrest rates. As a result, this measure captures a linear combination of exogenous individual-level determinants of crime, where the weights are chosen as to best predict adult crime. We then fit a dashed line to these underlying predicted arrest rate data.

As shown in Figure 2, predicted adult arrest rate is roughly flat. This indicates that the variation in crime-prone school peers we use to identify effects is uncorrelated with our best estimate of underlying propensity to commit crime as an adult. This is consistent with our identifying assumption and with the results of the balancing test shown in Table 3.

The solid circles represent actual arrest rates at age 19 - 21. Figure 2 shows there is a positive correlation between exposure to crime-prone school peers and actual adult arrest rate. While we turn to estimating effects more formally below, this highlights our central finding. In short, while underlying criminal propensity is uncorrelated with our measure of exposure to crime-prone school peers, actual adult criminality is positively correlated with it.

Figure 3 shows predicted and actual adult arrest rates for those with idiosyncratically low and high exposure to neighborhood peers linked to a parental arrest. Here, the estimated slope for predicted adult arrest is (slightly) upward sloping. By comparison, the actual adult arrest rate has a larger positive slope, suggesting that exposure to crime-prone neighborhood peers may also lead to higher arrest rates as an adult. We note that the upward slope of the predicted adult arrest rate gives us some cause for concern. In particular, one might worry that if this across-cohort exposure is correlated with negative observables, it could also be correlated with negative unobservables, implying we may overstate the effects of neighborhood peers. As alluded to earlier, however, the estimated effects of neighborhood peers turn out to be relatively small, suggesting that this issue leaves our qualitative conclusions unchanged. In addition, in the last three columns of Appendix Table A2 we show that controlling for observed own and peer characteristics does not change our neighborhood peer estimates in a meaningful way.

Estimates are shown in Table 6. Column 1 shows results for whether the individual was ever arrested as an adult aged 19 - 21. The top panel shows results for school peers, which are defined as the proportion of peers in one's school-grade-year linked to a parent who had been arrested during elementary school. The estimate is 0.132, and is significant at the 5 percent level. It indicates that a five percentage point increase in the share of crime-prone peers results in a 0.7 percentage point increase (0.05*0.132) in the likelihood of being arrested as an adult. This represents an increase of 6.4 percent relative to the mean rate of 10.6 percent. By comparison, the estimated increase in adult arrest rate due to exposure to neighborhood crime-prone peers is an increase of 2.6 percent (0.05*0.054/0.106), which is also significant at the 5 percent level.

Columns 2 through 4 show results for subcategories of arrests, including violent arrests, property arrests, and alcohol or drug-related arrests, respectively. Estimates are positive across all three subcategories, but are only estimated precisely for school peers and alcohol/drug arrests. Column 5 focuses only on arrests for which another person was involved in the crime that led to arrest.[22] This outcome provides a measure of criminal partnerships or group crimes which one would expect to be subject to greater influence from peer effects. Estimates are positive and precise for school peers. Columns 6 and 7 show results for ever incarcerated and days incarcerated. Estimates are positive, though the only estimates that are statistically significant at the five percent level are for days incarcerated. Estimates imply that a five percentage point increase in exposure to crime-prone school and neighborhood peers results in increases in days incarcerated of 0.2 and 0.1 days, respectively, both of which are small relative to the mean level of 4.6 days.

While the pattern of results in our top two panels of Table 6 suggest that effects are more likely to be driven by school peers than neighborhood peers, estimates in the bottom panel of Table 6 test this directly. Results in column 1 suggest it is indeed school peers who drive effects, though only the estimate for school and neighborhood peers are significant at conventional levels. Estimated coefficients for school-not-neighborhood peers and neighborhood-and-school peers are similar (0.060 and 0.045, respectively), and are both larger than the estimated coefficient of neighborhood-but-not-school crime-prone peers

[22] Beginning in 2005, Charlotte-Mecklenburg police department linked the registry of offenders 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. Crimes leading to multiple arrests are disproportionately burglaries, robberies, and drug offenses.

(0.016). This suggests that similar to the findings on the role of crime-prone peers in shaping educational outcomes, there are relatively few long-run effects of crime-prone peers from the neighborhood unless those peers also attended one's same school.

In summary, our results on the effect of peers on adult criminal outcomes yield two findings. The first is that exposure to crime-prone peers in elementary school leads to significant increases in adult criminality. We estimate that a five percentage point increase in crime-prone peers results in a 6.4 percent increase in the likelihood of being arrested as a young adult. Second, while we also estimate effects of neighborhood peers on adult criminality, a deeper analysis suggests that neighborhood peers only affect outcomes if those peers also attend one's same school.

4.4 Heterogeneous effects

In this section we examine the heterogeneity of effects by gender of the crime-prone peers and by the gender and race of the students.[23] Results are shown in Table 7, where we show results for three outcomes: the index of educational outcomes, the index of antisocial behavior, and whether the student was arrested as an adult. The first column for each outcome replicates our main estimates from Tables 4, 5, and 6. Results from columns 2, 6, and 10 indicate that the peer effects from male crime-prone students are similar to the overall effects. This is somewhat surprising, and suggests that boys and girls with arrested parents are similarly important in shaping the outcomes of their peers. However, while effects on antisocial behavior seem similar across male and female students (columns 7 and 8) it is clear that the effects on educational outcomes (columns 3 and 4) and adult arrest are (unsurprisingly) driven by male students.

In addition, in Table 8 we show results by the race of both the students linked to parental arrest and their peers. Results indicate that effects on educational outcomes are somewhat larger when we restrict to only black peers linked to parental arrest (column 2), though quite similar for antisocial behavior and adult arrests (columns 6 and 10). However, the main finding of Table 8 is while the effect of crime-prone school peers on educational outcomes is driven entirely by whites (column 4), effects on adult arrests are driven entirely by black students (column 11). We estimate that a five percentage point increase in exposure to crime-prone peers results in a 1.3 percentage point increase in adult criminality for blacks, compared to a small and insignificant 0.3 percentage points for whites. The finding that long-run effects on crime occur only for blacks, and not for

[23]The small number of Hispanic students in our population limits our ability to run a separate analysis for this group.

whites, contrasts with Carrell, Hoekstra, and Kuka (2018). They find that there is no long-run effect of exposure to peers linked to domestic violence on the *labor market* outcomes of black students, and instead find effects only on whites.

Table 9 shows results on the effect of own-group peers on outcomes. Results indicate that exposure to same-race neighborhood peers results in larger effects for education, misbehavior, and adult arrest outcomes. In contrast, same-gender effects are no larger, and sometimes smaller. Effects for same-race school peers are larger for misbehavior and adult arrest, but not for education outcomes.

One pattern that remains true across Tables 7 - 9 is that the impact of schools peers on adult arrest is consistently larger than the effect of neighborhood peers. This is reflected in part by the larger coefficient for school peers than neighborhood peers in the top two panels of each table. It is also reflected by the fact that estimated effects of those school peers who also live in one's neighborhood are no larger than those of peers who only attend one's same school. This suggests it is exposure to crime-prone peers in school, rather than in one's neighborhood, that drive effects on adult criminality.

In summary, these findings indicate there are relatively few differences in peer effects by gender, particularly when it comes to antisocial behavior and adult crime. However, the effects of crime-prone peers on adult arrests are driven entirely by effects on black students, and peers are most affected by those of their same race.

4.5 Robustness

One potential concern with our research design is that students who are exposed to an idiosyncratically high proportion of crime-prone peers may be more likely to be retained, which would lead us to understate effects. Alternatively, exposure could lead parents and students to leave the school or neighborhood. This would violate our identifying assumption, and would cause us to falsely attribute worse outcomes to peers. We note, however, that at least in the school context, there is another simpler avoidance behavior that is much less costly, but arguably nearly as effective. For example, parents may lobby teachers and administrators to place their child in a different classroom from certain peers. While the presence of this behavior would change the interpretation of our estimates, which can only be interpreted for the set of compliers, it would not undermine the internal validity of the estimates. This is because our analysis is conducted at the cohort level, rather than the classroom level. In addition, we suspect that extreme parental behavior to avoid certain peers—such as moving to a new neighborhood or school—is arguably less likely for in the types of families whose children are close to the margin of criminal behavior.

Nevertheless, we test empirically for whether exposure has effects on grade retention, a residential move within the school district, or departure from the school district. Results are shown in Figures 4 - 6, where each figure shows results by grade. Figure 4 shows that exposure to crime-prone peers in schools or neighborhoods has no effect on grade retention from grades 1 - 5. Figure 5 shows that peer exposure has no effect on moves across neighborhoods or schools within the district from grades 1 - 9, while Figure 6 shows the same for departures from the public school system. As a result, we conclude there is little evidence of grade retention, school or neighborhood mobility, or attrition that could bias our results.

We also perform a second test for whether changes in cohort demographics due to selection into or out of cohorts could affect our results. Specifically, we test how our estimates change with the inclusion of controls for student demographics and socioeconomic status. The intuition of the research design is that conditional on school, neighborhood, and year fixed effects, the across-cohort variation in exposure to crime-prone peers should be as good as random. This implies that the inclusion of individual and cohort controls should not affect the estimates. We demonstrate this in Appendix Table A2. The first three columns show results for the education index, columns 4 - 6 show results for the antisocial behavior index, and columns 7 - 9 show results for arrest as an adult. While the inclusion of cohort characteristics reduces the school peer effect estimate for the education index by just less than 30 percent, in all other cases, including adult arrest and the neighborhood peer effects, the estimates are unaffected by the inclusion of controls. This is consistent with the identifying assumption.

Another concern is the possibility that a common shock could lead to both a higher proportion of children linked to a parental arrest in a given cohort and worse outcomes for their peers. Given the inclusion of year-by-grade fixed effects, this shock would have to affect cohorts in some neighborhoods or schools but not others across the school district. While we cannot directly test this, we can ask if students are affected by peers one year older or younger than they are. This is because it is particularly difficult to think of a common shock that would lead to increased parental arrest (as measured across multiple years) and worse outcome for peers in one cohort, but not in adjacent cohorts. We note that this is an imperfect test in that it is possible—even probable—for one to be affected by peers who are one year older or younger. This is especially likely when peers are defined at the neighborhood level. As a result, we consider this to be primarily a test for school peer effects, which drive effects on adult crime. Results are shown in Appendix Table A3. Results for our main finding on adult arrests are shown in columns 7

- 9. The top two panels show that while we continue to estimate positive and significant effects of school and neighborhood peers in one's cohort on adult arrests, effects of peers in the cohort one year older or younger are statistically indistinguishable from zero. This is consistent with our identifying assumption.

One may also be concerned that our conclusion regarding the relative effects of school versus neighborhood peers is due to mis-measurement of neighborhood peer groups. For example, while children are always sorted into classrooms with others of the same age, students may associate with neighborhood peers of different ages. Similarly, it may be difficult to identify the proper geographic boundaries of the neighborhood in which peers matter. To test the robustness of our findings to these concerns, we perform several exercises. First, as alluded to above, in Appendix Table A3 we show neighborhood peer effects that if anything are smaller for peers who are one year older or younger. Thus, it is clear that measuring neighborhood peers in these alternative, reasonable ways do not result in larger effects. Second, in Appendix Table A4 we estimate effects for both larger and smaller alternative neighborhood definitions. We do so for three outcomes - our education index, antisocial behavior index, and adult arrest. The first column for each outcome in Table A4 shows our main estimates, in which neighborhood peers are defined as Census Block Group by 02-03 school boundaries. This definition resulted in 491 separate neighborhoods. The second shows estimates if we instead define neighborhood peers at only the Census Tract level (144 areas), while the third defines them at the Census Block Group level (365 areas). Finally, the fourth column for each outcome reports estimates if we define neighborhood peers as those who live on the same street and within 1000 street address numbers. This neighborhood definition results in the most tightly defined neighborhood peer groups (10,593). Results in the second panel of Table A4 indicate that the estimated effects of neighborhood peers are never qualitatively larger using these alternative peer definitions, and in many cases are smaller and less significant. As a result, we conclude that our main findings are not sensitive to alternative definitions of neighborhood peers. Finally, we also note that in general peers from the same school and neighborhood have no larger effects than those peers who share a school but not neighborhood. If anything, the combined effect is smaller, as shown for the education index (column 4, Table 4), the antisocial behavior index (column 6, Table 5), and adult arrest (column 1, Table 6). This suggests that school peers do not have additional influence even when they live in one's same neighborhood. Thus, we believe the most reasonable interpretation of our results is that outcomes are primarily affected by school peers, rather than neighborhood peers.

Another potential concern regards whether our definition of crime-prone peers is capturing anything beyond peer gender or race. To some extent this question is addressed indirectly by Appendix Table A2, which shows that estimates are robust to the inclusion of other peer variables such as cohort gender and race. However, in Appendix Table A5 we address this directly by showing results when we define the peer group of interest as the proportion male, proportion black, and proportion male and black. Estimates from those specifications are nearly all smaller and less significant than the estimated effects of our measure of crime-prone peers. This suggests that exposure to peers linked to criminal parents is meaningfully different than exposure to peers of a given race or sex, which we view as consistent with the literature documenting the intergenerational transmission of crime.

We also test the robustness of our results to alternative methods of matching students to parents. This linkage is important for our study given we define crime-prone peers as those linked to an arrested parent. In Appendix Table A6, we show our main results when we perform the match in a less restrictive way. Specifically, while the first column for each outcome reports our main results, in columns 2, 5, and 8 we show results when we include matches made to large apartment complexes where duplicate names are more common for a given street address. In columns 3, 6, and 9 we include matches of arrestees that share a last name and address with another arrestee. Changing the matching method results in very similar estimates. For example, our baseline estimate for the effect of school peers on adult arrest is 0.1316 as shown in column 7, and remains nearly identical in columns 8 (0.1334) and 9 (0.1337).

In addition, we test the robustness of our findings to our definition of crime-prone peers. We do so in Appendix Table A7, where we show results for more restrictive definitions of crime-prone peers. In the first column for each of our three outcomes (education index, antisocial behavior index, and adult arrest), we replicate our main results where we classify a student as crime-prone if his or her parent was arrested for any reason while the child was in elementary school. In the second column for each outcome, we classify students as crime-prone only if a parent was arrested for a property or violent crime, or if the parent was arrested multiple times. In the third column, we further restrict the definition to include only those with a parent who was incarcerated, which is 9 percent of the children in our sample. Results indicate that using more serious parental arrests results in similar estimates (see columns 2, 5, and 8). However, defining crime-prone peers only as those with a parent who was incarcerated results in estimated effects (and standard errors) that are larger. Importantly, however, our two main conclusions are unchanged: crime-prone peers have large negative effects on cognitive and non-cognitive outcomes, including adult arrest, and effects are driven largely by school peers rather than neighborhood peers.

Finally, we also report results using two-way clustered standard errors at the school and neighborhood levels, rather than at the school-cohort and neighborhood-cohort levels. Results are shown in Appendix Table A8, which otherwise replicates our main results. Standard errors are very similar. For example, the standard errors for the coefficients on school and neighborhood peers shown in the top two rows of Table 6 for adult arrest were 0.0561 and 0.0260, respectively. By comparison, the corresponding standard errors in Table A8 are 0.0584 and 0.0257.

5 Conclusion

In this paper, we examine the impact of exposure to peers during elementary school on educational outcomes and antisocial behavior during middle and high school, and crime as adults. Our findings suggest that childhood exposure to crime-prone peers—defined as children linked to an arrested parent—have important implications for medium and long-run outcomes. We estimate that a five percentage point increase in exposure to crime-prone peers results in a 0.015 standard deviation reduction in educational achievement. More importantly, we document that this childhood exposure has important implications for adult criminality. We estimate a similar increase in exposure results in a 6.4 percent increase in the likelihood of being arrested as an adult aged 19 - 21. We view this as a central finding of our study, as it indicates that school peers can affect non-cognitive outcomes even after leaving the school. In addition, while we are unable to examine criminal outcomes into individuals' mid- to late-20s, evidence elsewhere suggests that arrests in early adulthood are a strong predictor of future criminal activity. This suggests that the peers to whom individuals are exposed can lead to significantly worse outcomes for the individual as well as significant social costs due to additional criminal behavior.

In addition, a deeper analysis indicates that most of these effects are due to school peers. Specifically, we show that while exposure to crime-prone neighborhood peers matters, those effects seem to be caused by neighborhood peers who also attend one's same school. Consequently, the results here suggest that the neighborhood effects documented by studies like Move to Opportunity are likely due to a difference in school peers, rather than a difference in neighborhood peers. This suggests more emphasis should be put on schools when evaluating how neighborhoods can affect children's cognitive and non-cognitive outcomes.

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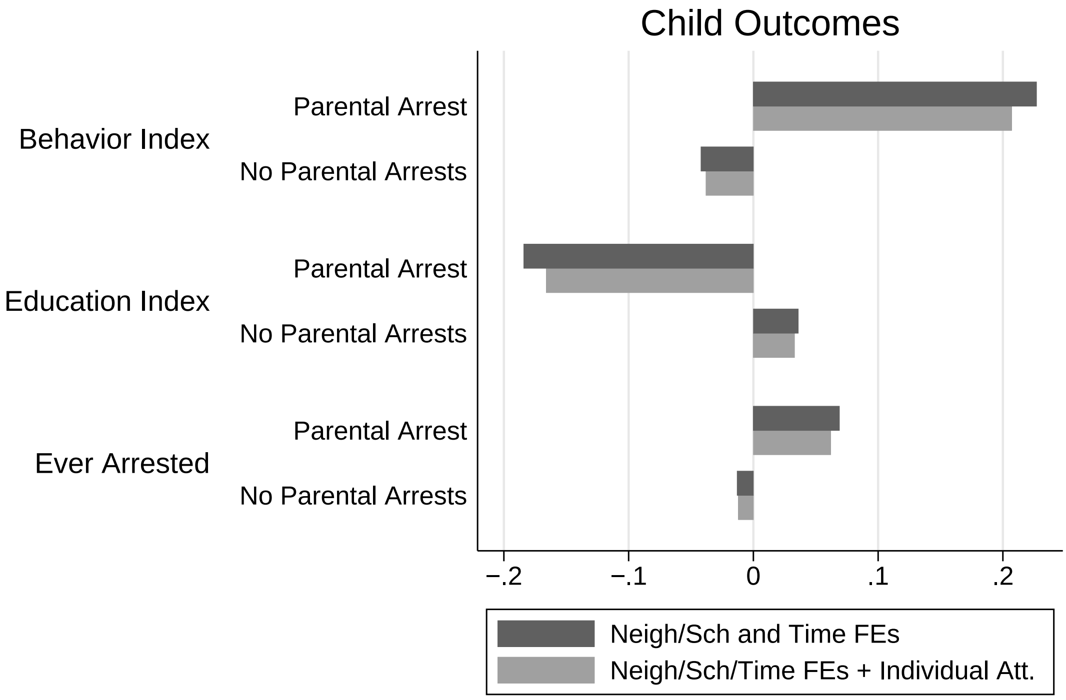
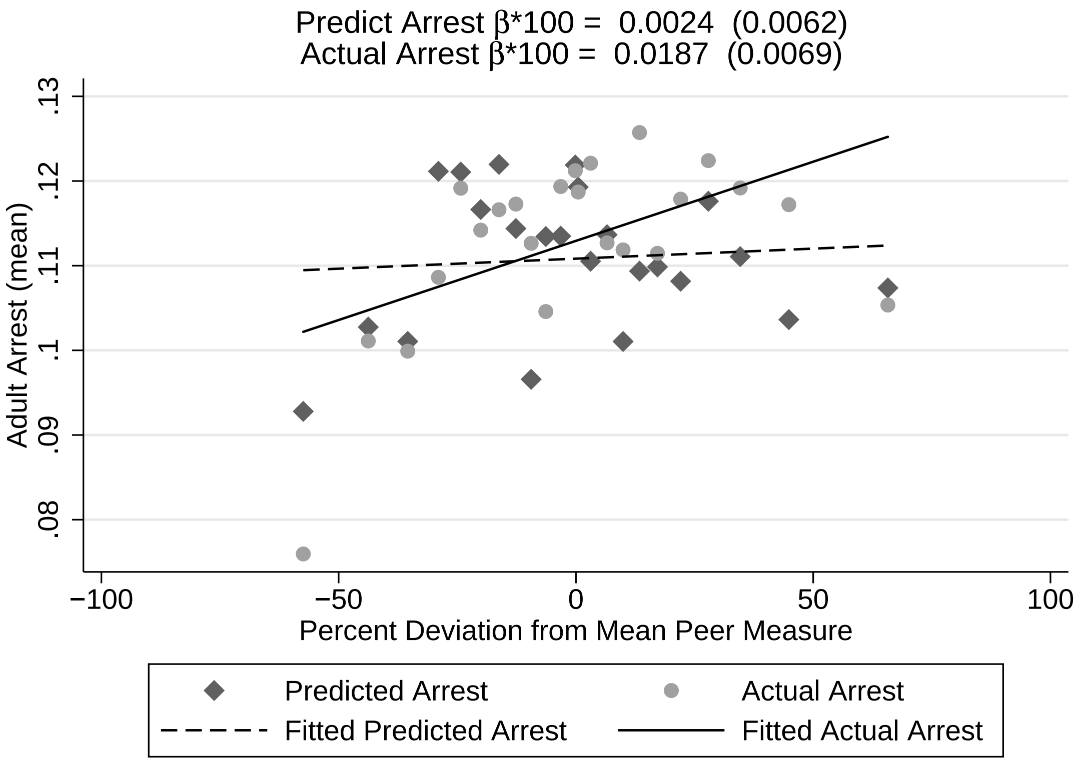
Figure 1: Parental Arrest & The Child's Outcomes



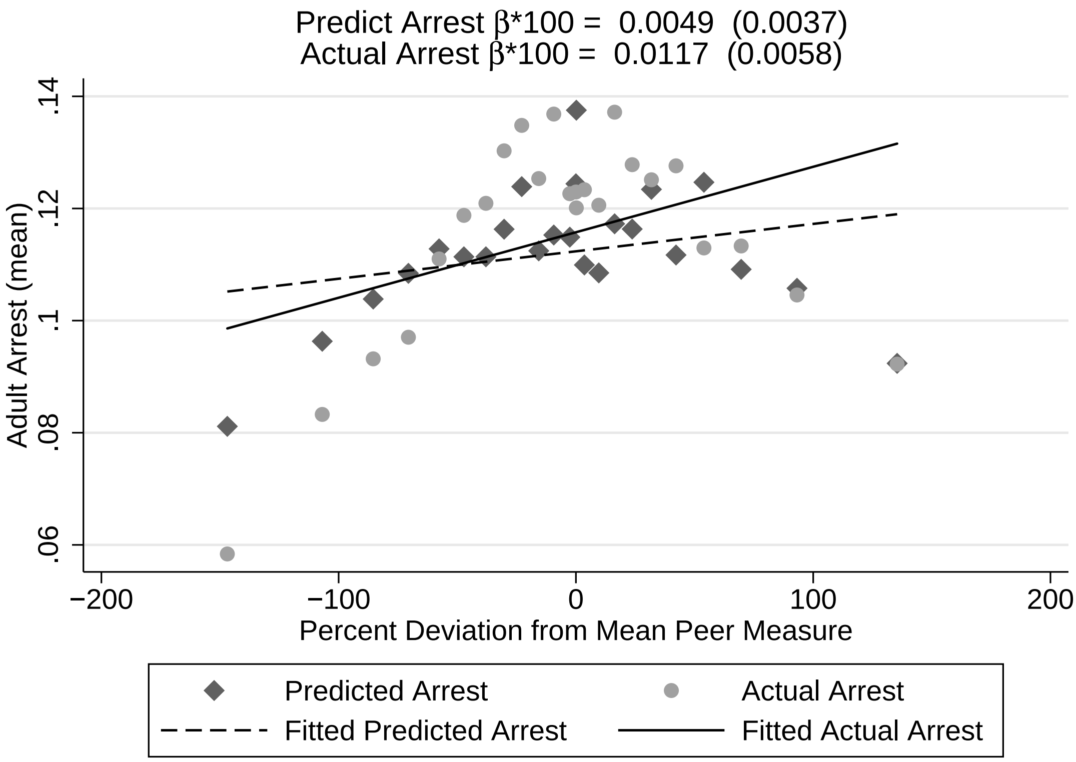
Notes: This figure depicts the intergenerational relationship between parental arrests and a child's average outcome across indices for middle and high school academic and behavior outcomes as well as adult arrest. Conditional values are based on a first stage regression residual that conditions on student demographics, cohort fixed effects , neighborhood fixed effects and school fixed effects.

Figure 2: Adult Arrests - School Peers



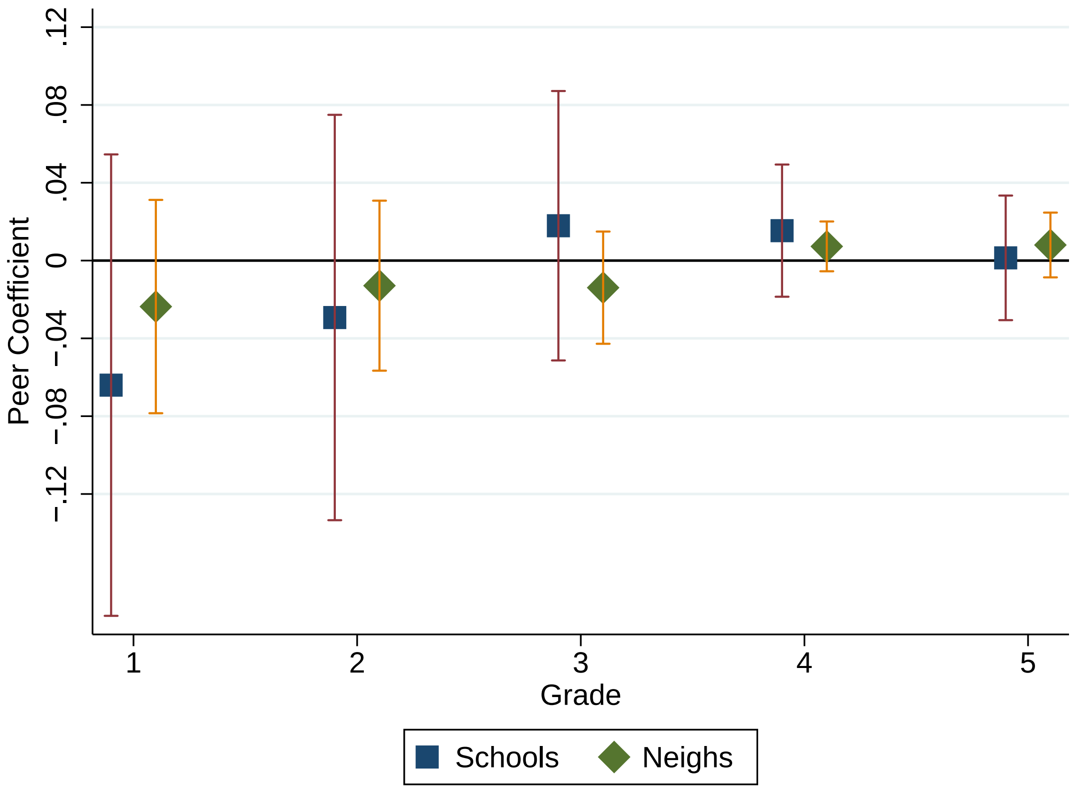
Notes: This figure depicts the relationship between share of school peers with arrested parents and adult arrest for our main sample of students without a parental arrest. We create the predicted arrest outcome by first running a regression that includes grade-year and school-neighborhood-grade fixed effects for grades third to fifth, as well as additional individual level controls. Individual controls include gender, race, living in a single-family home, limited english proficiency and academically gifted. The regression is weighted by the inverse of the number of times a student is observed in the sample. Second, we predict adult arrests using the estimated coefficients. Lastly, we collapse the data to 25 groups defined according to the percent change in residual exposure to peers with arrested parents (relative to the average peer exposure for that school) after controlling for school-neighborhood-grade and grade-year fixed effects.

Figure 3: Adult Arrests - Neigh Peers



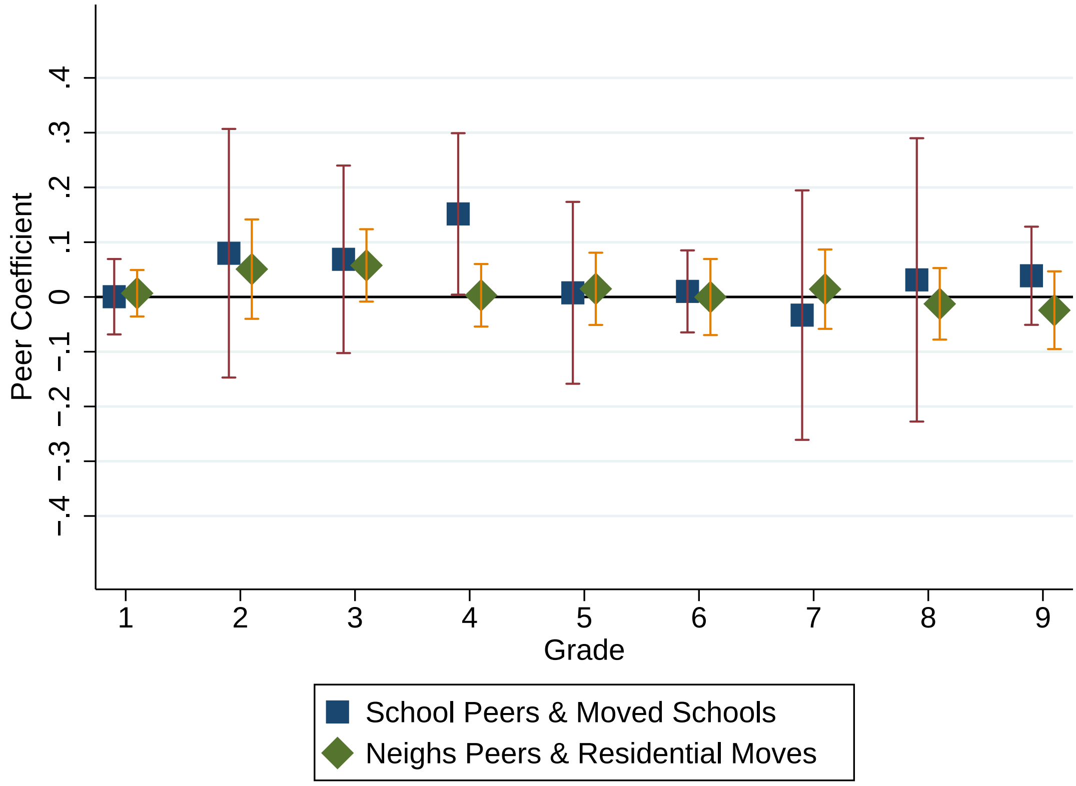
Notes: This figure depicts the relationship between share of neighborhood peers with arrested parents and adult arrest for our main sample of students without a parental arrest. We create the predicted arrest outcome by first running a regression that includes grade-year and school-neighborhood-grade fixed effects for grades third to fifth, as well as additional individual level controls. Individual controls include gender, race, living in a single-family home, limited english proficiency and academically gifted. The regression is weighted by the inverse of the number of times a student is observed in the sample. Second, we predict adult arrests using the estimated coefficients. Lastly, we collapse the data to 25 groups defined according to the percent change in residual exposure to peers with arrested parents (relative to the average peer exposure for that school) after controlling for school-neighborhood-grade and grade-year fixed effects.

Figure 4: Peer Composition and Grade Retention in Elementary School



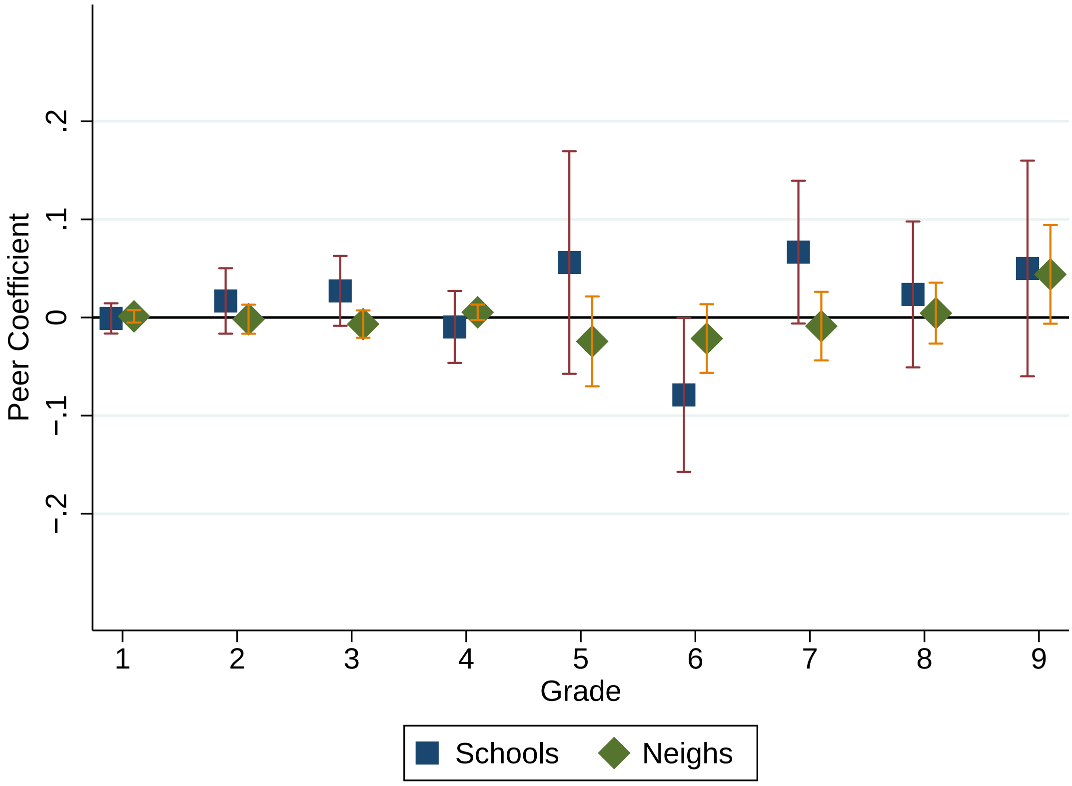
Notes: This figure depicts the coefficient on our main peer variable *PeerParentArrest* estimated using Equation 1 separately by Schools and Neighborhoods as it relates to an individual student being retained in a given grade.

Figure 5: Peer Composition and Changing Schools/Residential Neighborhoods



Notes: This figure depicts the coefficient on our main peer variable *PeerParentArrest* estimated using Equation 1 separately by Schools and Neighborhoods as it relates to an individual student moving schools or neighborhoods in a given grade.

Figure 6: Peer Composition and Leaving the Public School System



Notes: This figure depicts the coefficient on our main peer variable *PeerParentArrest* estimated using Equation 1 separately by Schools and Neighborhoods as it relates to an individual student leaving the public school system (i.e. private school or leaving the county) in a given grade.

Table 1: Summary Statistics

	(1) All Students	(2) Parents Not Arrested	(3) Parents Arrested	(2) -(3)
Student Outcomes				
Education Index	0.05	0.09	−0.37	0.46***
Avg. Test Score (3-5 grade)	−0.03	0.03	−0.49	0.52***
Avg. Test Score (6-8 grade)	0.03	0.08	−0.46	0.54***
Repeat Grade (6-10 grade)	0.26	0.24	0.47	−0.23***
Antisocial Behavior Index	−0.03	−0.07	0.39	−0.45***
Days Absent (6-10 grade)	36.46	34.27	58.75	−24.48***
Days Suspended (6-10 grade)	7.80	6.84	17.49	−10.65***
Drop Out of High School	0.08	0.07	0.16	−0.09***
Youth Arrest (16-18 yrs old)	0.08	0.07	0.18	−0.10***
Adult Arrest (19-21 yrs old)	0.11	0.10	0.23	−0.13***
Background Characteristics				
Male	0.51	0.51	0.49	0.02***
Black	0.43	0.40	0.70	−0.30***
Hispanic	0.07	0.08	0.05	0.02***
Limited English Proficiency	0.06	0.06	0.04	0.03***
Academically Gifted	0.15	0.16	0.06	0.09***
Single Family Residence	0.74	0.74	0.72	0.02***
People per sq mile (000s)	2.52	2.48	2.96	−0.49***
CBG Median HH Income (000s)	54.44	55.70	41.69	14.01***
Peer Characteristics				
Peers in School	120.76	121.85	109.65	12.20***
Peers in Neighborhood	32.31	32.70	28.35	4.35***
Peers in School & Neighborhood	15.95	16.33	12.10	4.23***
Sch. Peers w Arrested Parents (Share)	0.08	0.08	0.10	−0.02***
Neigh. Peers w Arrested Parents (Share)	0.08	0.08	0.12	−0.05***
Sch. & Neigh. Peers w Arrested Parents (Share)	0.06	0.06	0.11	−0.05***
Observations	126,390	115,606	10,784	

Means are reported above.

The data sample consists of an unbalanced panel of students observed during the 1998/1999-2010/2011 school years. We restrict the sample to only individuals born between 1989-1993 that attend a public school in 3rd, 4th or 5th grade in Mecklenburg County, NC between 1999-2011. We also only include students without a parental arrest for estimation.

Neighborhoods constructed as unique Census Block Group (CBG) 2000 by elementary/middle school attendance zones after redistricting in 2002-2003. This spatially narrows our definition of neighborhood and attendance zones do not impact our sample since they attend elementary school prior to redistricting. We include a peer as having a parental arrest if the child's parent was arrested during elementary school.

Table 2: Cohort Variation

	Mean	Std. Dev.	Min	Max
Raw Cohort Variables (Fraction w/ Criminal Parents)				
School Peers	0.081	0.046	0.000	0.362
Neigh Peers	0.079	0.083	0.000	1.000
School, Not Neigh Peers	0.084	0.049	0.000	0.419
Not School, Neigh Peers	0.088	0.137	0.000	1.000
School & Neigh Peers	0.063	0.121	0.000	1.000
Cohort Variation over Time (Fraction w/ Criminal Parents)				
School Peers	−0.000	0.023	−0.180	0.228
Neigh Peers	0.000	0.049	−0.377	0.626
School, Not Neigh Peers	−0.000	0.025	−0.204	0.248
Not School, Neigh Peers	0.000	0.103	−0.823	0.998
School & Neigh Peers	0.000	0.088	−0.747	0.971

The top panel contains descriptive statistics for raw values of peer definitions in row headings. The bottom panel contains descriptive statistics for row headings conditional on school by neighborhood by grade fixed effects as well as grade by year fixed effects. These conditional values are based on the residual of a first stage regression of the raw peer variable on fixed effects for school by neighborhood by grade and grade by year. We also only include students without a parental arrest for estimation.

Table 3: Arrested Parent Peer Model - Balance Test

	(1) School Peers (%)	(2) Neigh Peers (%)	(3) School Peers (%)	(4) Neigh Peers (%)	(5) School Not Neigh Peers (%)	(6) Neigh Not School Peers (%)	(7) School & Neigh Peers (%)
Male	−0.00041	−0.00159**	0.00026	−0.00014	0.00045*	0.00003	−0.00008
	(0.00040)	(0.00068)	(0.00023)	(0.00048)	(0.00026)	(0.00095)	(0.00082)
Black	0.02926***	0.05731***	0.00017	−0.00016	0.00031	0.00122	0.00158
	(0.00215)	(0.00238)	(0.00036)	(0.00067)	(0.00042)	(0.00133)	(0.00105)
Hispanic	0.01802***	0.02595***	0.00069	0.00092	0.00067	0.00075	0.00007
	(0.00182)	(0.00199)	(0.00057)	(0.00111)	(0.00065)	(0.00220)	(0.00180)
Stand-Alone Residence	−0.00099	0.00473**	0.00012	0.00072	−0.00010	0.00081	0.00102
	(0.00108)	(0.00198)	(0.00037)	(0.00075)	(0.00040)	(0.00152)	(0.00122)
Limited English Proficiency	0.01132***	0.01652***	−0.00087	−0.00007	−0.00081	0.00235	0.00006
	(0.00158)	(0.00196)	(0.00056)	(0.00120)	(0.00061)	(0.00267)	(0.00191)
Academically Gifted	−0.00706***	−0.01126***	−0.00059*	0.00063	−0.00061*	0.00041	0.00042
	(0.00119)	(0.00104)	(0.00032)	(0.00062)	(0.00035)	(0.00126)	(0.00097)
Observations	115,606	115,606	115,606	115,606	115,606	115,606	115,606
Dep. Var. (mean)	0.081	0.079	0.081	0.079	0.084	0.088	0.063
F-stat p-value	0.00	0.00	0.19	0.80	0.11	0.96	0.80
R^2	0.15	0.13	0.74	0.60	0.71	0.41	0.42
Year by Grade FE	✓	✓	✓	✓	✓	✓	✓
School by Grade by Neigh FE	-	-	✓	✓	✓	✓	✓

Neighborhoods constructed as unique Census Block Group (CBG) 2000 by elementary/middle school attendance zones after redistricting in 2002-2003. We include a peer as having a parental arrest if the child's parent was arrested during elementary school. We also only include students without a parental arrest for estimation.

All regressions include cohort fixed effects, grade by year fixed effects, school by neighborhood by grade fixed effects, student demographic variables and an indicator for single-family residence. * p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors robust to arbitrary correlation for school by cohort and neighborhood by cohort.

Table 4: Cognitive Outcomes

	(1) Avg. Test Score 3-5th grade	(2) Avg. Test Score 6-8th grade	(3) Repeat Grade 6-10th grade	(4) Education Index
School Peers (%)	-0.4949^{***}	-0.3574^{**}	0.0175	-0.2853^{**}
	(0.1631)	(0.1489)	(0.0859)	(0.1303)
Neigh Peers (%)	0.0282	−0.0080	0.0570	−0.0776
	(0.0621)	(0.0586)	(0.0358)	(0.0532)
Not Neigh, just School Peers (%)	-0.4013^{***}	-0.2797^{**}	0.0096	-0.2201^{*}
	(0.1444)	(0.1281)	(0.0757)	(0.1124)
Neigh, not School Peers (%)	0.0324	0.0265	0.0069	0.0052
	(0.0272)	(0.0255)	(0.0160)	(0.0227)
Neigh & School Peers (%)	−0.0398	-0.0648^{**}	0.0370^{*}	-0.0716^{**}
	(0.0327)	(0.0306)	(0.0189)	(0.0277)
Marginal Impacts (+5 p.p.)				
Not Neigh, just School Peers	-0.020^{***}	-0.014^{**}	0.000	-0.011^{*}
Neigh, not School Peers	0.002	0.001	0.000	0.000
Neigh & School Peers	−0.002	-0.003^{**}	0.002^{*}	-0.004^{**}
Marginal Impacts (+1 std. dev)				
Not Neigh, just School Peers	-0.010^{***}	-0.007^{**}	0.000	-0.006^{*}
Neigh, not School Peers	0.003	0.003	0.001	0.001
Neigh & School Peers	−0.004	-0.006^{**}	0.003^{*}	-0.006^{**}
Observations	90,668	88,531	115,585	115,606
Dep. Var. (mean)	-0.005	0.038	0.255	0.035

All regressions include cohort fixed effects, grade by year fixed effects, school by neighborhood by grade fixed effects, student demographic variables and an indicator for single-family residence. * p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors robust to arbitrary correlation for school by cohort and neighborhood by cohort.

Education Index is scaled to be mean zero and standard deviation one and represents a composite of the outcomes given in other columns. We also only include students without a parental arrest for estimation.

Table 5: Behavioral Outcomes

	(1) Days Absent	(2) Days Suspended	(3) Drop Out of High School	(4) School Crimes	(5) Ever Arrested 16-18 yr old	(6) Antisocial Behavior Index
School Peers (%)	7.5001	5.5644	0.0168	2.0878***	0.0746	0.1927
	(7.5675)	(3.5660)	(0.0531)	(0.5767)	(0.0508)	(0.1424)
Neigh Peers (%)	5.9073*	1.3224	0.0610***	0.2517	0.0609**	0.1760***
	(3.3269)	(1.5180)	(0.0227)	(0.2874)	(0.0251)	(0.0599)
Not Neigh, just School Peers (%)	4.5900	4.7314	−0.0119	1.8406***	0.0386	0.0986
	(6.5048)	(3.1729)	(0.0460)	(0.5025)	(0.0457)	(0.1264)
Neigh, not School Peers (%)	0.7433	−0.1962	0.0201*	−0.1068	0.0151	0.0410
	(1.5771)	(0.6868)	(0.0107)	(0.1170)	(0.0105)	(0.0270)
Neigh & School Peers (%)	1.9017	0.1434	0.0278**	−0.0083	0.0065	0.0470
	(1.7634)	(0.8518)	(0.0128)	(0.1430)	(0.0129)	(0.0313)
Marginal Impacts (+5 p.p.)						
Not Neigh, just School Peers	0.229	0.237	−0.001	0.092***	0.002	0.005
Neigh, not School Peers	0.037	−0.010	0.001*	−0.005	0.001	0.002
Neigh & School Peers	0.095	0.007	0.001**	−0.000	0.000	0.002
Marginal Impacts (+1 std. dev.)						
Not Neigh, just School Peers	0.115	0.119	−0.000	0.046***	0.001	0.002
Neigh, not School Peers	0.076	−0.020	0.002*	−0.011	0.002	0.004
Neigh & School Peers	0.167	0.013	0.002**	−0.001	0.001	0.004
Observations	115,585	115,585	115,606	115,585	115,606	115,606
Dep. Var. (mean)	36.097	7.600	0.078	1.060	0.080	-0.032

All regressions include cohort fixed effects, grade by year fixed effects, school by neighborhood by grade fixed effects, student demographic variables and an indicator for single-family residence. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary correlation for school by cohort and neighborhood by cohort.

Behavioral Index is scaled to be mean zero and standard deviation one and represents a composite of the outcomes given in other columns. We also only include students without a parental arrest for estimation.

Table 6: Adult Outcomes

	(1) Adult Ever Arrest	(2) Adult Vio Arrest	(3) Adult Prop Arrest	(4) Adult Alc-Drug Arrest	(5) Adult Partner Crime Arrest	(6) Adult Ever Incarc	(7) Days Incarc
School Peers (%)	0.1316**	0.0362	0.0311	0.0717**	0.1114***	0.0173	3.9189**
	(0.0561)	(0.0262)	(0.0360)	(0.0337)	(0.0369)	(0.0422)	(1.9474)
Neigh Peers (%)	0.0554**	0.0094	−0.0060	0.0056	0.0256	0.0329*	1.8849**
	(0.0260)	(0.0125)	(0.0192)	(0.0163)	(0.0193)	(0.0196)	(0.7967)
Not Neigh, just School Peers (%)	0.0602	0.0085	0.0206	0.0419	0.0651**	−0.0184	2.2380
	(0.0489)	(0.0221)	(0.0314)	(0.0298)	(0.0307)	(0.0370)	(1.5948)
Neigh, not School Peers (%)	0.0156	−0.0025	−0.0097	0.0040	−0.0028	0.0033	0.3489
	(0.0120)	(0.0057)	(0.0080)	(0.0076)	(0.0076)	(0.0081)	(0.3231)
Neigh & School Peers (%)	0.0451***	0.0197**	0.0121	0.0041	0.0250**	0.0282**	0.6451*
	(0.0147)	(0.0079)	(0.0101)	(0.0093)	(0.0112)	(0.0111)	(0.3464)
Marginal Impacts (+5 p.p.)							
Not Neigh, just School Peers (%)	0.003	0.000	0.001	0.002	0.003**	−0.001	0.112
Neigh, not School Peers (%)	0.001	−0.000	−0.000	0.000	−0.000	0.000	0.017
Neigh & School Peers (%)	0.002***	0.001**	0.001	0.000	0.001**	0.001**	0.032*
Marginal Impacts (+1 std. dev.)							
Not Neigh, just School Peers (%)	0.002	0.000	0.001	0.001	0.002**	−0.000	0.056
Neigh, not School Peers (%)	0.002	−0.000	−0.001	0.000	−0.000	0.000	0.036
Neigh & School Peers (%)	0.004***	0.002**	0.001	0.000	0.002**	0.002**	0.057*
Observations	115,606	115,606	115,606	115,606	115,606	115,606	115,606
Dep. Var. (mean)	0.106	0.019	0.043	0.031	0.041	0.043	4.6

All regressions include cohort fixed effects, grade by year fixed effects, school by neighborhood by grade fixed effects, student demographic variables and an indicator for single-family residence. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary correlation for school by cohort and neighborhood by cohort.

All arrest and incarceration variables based on age 19-21. Crime types indicate the criminal charge at the time of arrest with property indicating burglary, auto theft, larceny or fraud; violent indicating murder, rape, assault or robbery; alcohol and drug arrests include any drug charge as well as DUIs, public drunkenness and related charges. Column 6 estimates a Poisson count model on the number of days incarcerated ages 19-21. We also only include students without a parental arrest for estimation.

Table 7: Heterogeneous Effects I

	Education Index				Antisocial Behavior Index				Adult Arrest			
	(1) *Main*	(2) *Male Peers Only*	(3) *Male Students Only*	(4) *Female Students Only*	(5) *Main*	(6) *Male Peers Only*	(7) *Male Students Only*	(8) *Female Students Only*	(9) *Main*	(10) *Male Peers Only*	(11) *Male Students Only*	(12) *Female Students Only*
School Peers	−0.2853**	−0.3778**	−0.3762**	−0.1354	0.1927	0.1494	0.2176	0.1522	0.1316**	0.1573*	0.2344***	0.0402
	(0.1303)	(0.1803)	(0.1712)	(0.1717)	(0.1424)	(0.2051)	(0.2302)	(0.1521)	(0.0561)	(0.0820)	(0.0902)	(0.0640)
Neigh Peers (%)	−0.0776	−0.1411**	−0.0210	−0.1329*	0.1760***	0.2089**	0.1591	0.1683**	0.0554**	0.1107***	0.0913**	0.0258
	(0.0532)	(0.0708)	(0.0752)	(0.0767)	(0.0599)	(0.0863)	(0.0986)	(0.0737)	(0.0260)	(0.0358)	(0.0449)	(0.0312)
Not Neigh, just School Peers (%)	−0.2201*	−0.2522*	−0.3391**	−0.0404	0.0986	0.0476	0.1229	0.0464	0.0602	0.0704	0.1412*	−0.0219
	(0.1124)	(0.1527)	(0.1525)	(0.1527)	(0.1264)	(0.1811)	(0.2006)	(0.1431)	(0.0489)	(0.0715)	(0.0821)	(0.0537)
Neigh, not School Peers (%)	0.0052	−0.0310	−0.0063	0.0107	0.0410	0.0960**	0.0518	0.0365	0.0156	0.0381**	0.0452**	−0.0056
	(0.0227)	(0.0300)	(0.0319)	(0.0314)	(0.0270)	(0.0378)	(0.0420)	(0.0299)	(0.0120)	(0.0168)	(0.0203)	(0.0133)
Neigh & School Peers (%)	−0.0716**	−0.0814**	−0.0398	−0.1176***	0.0470	0.0564	0.0417	0.0368	0.0451***	0.0482**	0.0526**	0.0302*
	(0.0277)	(0.0365)	(0.0404)	(0.0428)	(0.0313)	(0.0495)	(0.0558)	(0.0428)	(0.0147)	(0.0204)	(0.0235)	(0.0176)
Observations	115,606	115,606	59,189	56,417	115,606	115,606	59,189	56,417	115,606	115,606	59,189	56,417
Dep. Var. (mean)	0.035	0.035	-0.041	0.115	-0.032	-0.032	0.056	-0.125	0.106	0.106	0.153	0.057

All regressions include cohort fixed effects, grade by year fixed effects, school by neighborhood by grade fixed effects, student demographic variables and an indicator for single-family residence. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary correlation for school by cohort and neighborhood by cohort.

We estimate our main model where we define peers simply based on demographics by column headings to show that peers with parental arrest have different influences than simply peers defined by demographic dimensions that correlate with adult arrest. We also only include students without a parental arrest for estimation.

Table 8: Heterogeneous Effects II

	Education Index				Antisocial Behavior Index				Adult Arrest			
	(1) *Main*	(2) *Black Peers Only*	(3) *Black Students Only*	(4) *White Students Only*	(5) *Main*	(6) *Black Peers Only*	(7) *Black Students Only*	(8) *White Students Only*	(9) *Main*	(10) *Black Peers Only*	(11) *Black Students Only*	(12) *White Students Only*
School Peers	-0.2853^{**}	-0.4618^{***}	-0.0192	-0.3043^{*}	0.1927	0.1478	0.1715	0.2100	0.1316^{**}	0.1265^{**}	0.2573^{***}	0.0487
	(0.1303)	(0.1323)	(0.1790)	(0.1744)	(0.1424)	(0.1546)	(0.2269)	(0.1563)	(0.0561)	(0.0596)	(0.0911)	(0.0669)
Neigh Peers (%)	-0.0776	-0.1251^{**}	-0.1091	-0.0013	0.1760^{***}	0.2458^{***}	0.2037^{**}	0.0494	0.0554^{**}	0.0612^{*}	0.0603	0.0347
	(0.0532)	(0.0612)	(0.0711)	(0.0791)	(0.0599)	(0.0739)	(0.0989)	(0.0733)	(0.0260)	(0.0324)	(0.0393)	(0.0321)
Not Neigh, just School Peers (%)	-0.2201^{*}	-0.3602^{***}	0.0543	-0.2697^{*}	0.0986	0.0434	0.0693	0.1441	0.0602	0.0545	0.1292	0.0320
	(0.1124)	(0.1149)	(0.1616)	(0.1481)	(0.1264)	(0.1348)	(0.2108)	(0.1311)	(0.0489)	(0.0516)	(0.0838)	(0.0540)
Neigh, not School Peers (%)	0.0052	-0.0247	-0.0357	0.0239	0.0410	0.0563^{*}	0.0885^{*}	-0.0043	0.0156	0.0130	0.0070	0.0227^{*}
	(0.0227)	(0.0276)	(0.0379)	(0.0280)	(0.0270)	(0.0337)	(0.0516)	(0.0295)	(0.0120)	(0.0145)	(0.0214)	(0.0135)
Neigh & School Peers (%)	-0.0716^{**}	-0.0892^{***}	-0.0643^{*}	-0.0285	0.0470	0.0818^{**}	0.0416	0.0295	0.0451^{***}	0.0615^{***}	0.0648^{***}	-0.0011
	(0.0277)	(0.0325)	(0.0367)	(0.0451)	(0.0313)	(0.0364)	(0.0468)	(0.0460)	(0.0147)	(0.0186)	(0.0220)	(0.0186)
Observations	115,606	115,606	48,022	67,584	115,606	115,606	48,022	67,584	115,606	115,606	48,022	67,584
Dep. Var. (mean)	0.035	0.035	-0.348	0.307	-0.032	-0.032	0.181	-0.184	0.106	0.106	0.182	0.052

All regressions include cohort fixed effects, grade by year fixed effects, school by neighborhood by grade fixed effects, student demographic variables and an indicator for single-family residence. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary correlation for school by cohort and neighborhood by cohort.

We estimate our main model where we define peers simply based on demographics by column headings to show that peers with parental arrest have different influences than simply peers defined by demographic dimensions that correlate with adult arrest. We also only include students without a parental arrest for estimation.

Table 9: Heterogeneous Effects III

	Education Index				Antisocial Behavior Index				Adult Arrest			
	(1) *Main*	(2) *Same Gender Peers*	(3) *Same Race Peers*	(4) *Same Gender-Race Peers*	(5) *Main*	(6) *Same Gender Peers*	(7) *Same Race Peers*	(8) *Same Gender-Race Peers*	(9) *Main*	(10) *Same Gender Peers*	(11) *Same Race Peers*	(12) *Same Gender-Race Peers*
School Peers	−0.2853**	−0.2600	−0.2317*	−0.2266	0.1927	0.1372	0.2782*	0.3323	0.1316**	0.0677	0.2463***	0.2712***
	(0.1303)	(0.1683)	(0.1316)	(0.2017)	(0.1424)	(0.1906)	(0.1622)	(0.2399)	(0.0561)	(0.0770)	(0.0619)	(0.0966)
Neigh Peers (%)	−0.0776	−0.0562	−0.1977***	−0.1680**	0.1760***	0.0943	0.3593***	0.2981***	0.0554**	0.0070	0.1342***	0.0883**
	(0.0532)	(0.0639)	(0.0577)	(0.0710)	(0.0599)	(0.0799)	(0.0709)	(0.0946)	(0.0260)	(0.0314)	(0.0298)	(0.0392)
Not Neigh, just School Peers (%)	−0.2201*	−0.1164	0.0022	−0.0075	0.0986	0.0109	−0.0332	0.0510	0.0602	0.0030	0.0812	0.1065
	(0.1124)	(0.1514)	(0.1266)	(0.1900)	(0.1264)	(0.1764)	(0.1549)	(0.2179)	(0.0489)	(0.0706)	(0.0587)	(0.0904)
Neigh, not School Peers (%)	0.0052	0.0016	−0.0346	−0.0422	0.0410	0.0457	0.1221***	0.1217***	0.0156	−0.0062	0.0326**	0.0153
	(0.0227)	(0.0306)	(0.0288)	(0.0380)	(0.0270)	(0.0364)	(0.0342)	(0.0459)	(0.0120)	(0.0150)	(0.0148)	(0.0195)
Neigh & School Peers (%)	−0.0716**	−0.0818**	−0.0987***	−0.0810*	0.0470	0.0244	0.0745*	0.0361	0.0451***	0.0333	0.0904***	0.0736***
	(0.0277)	(0.0385)	(0.0355)	(0.0423)	(0.0313)	(0.0503)	(0.0411)	(0.0583)	(0.0147)	(0.0213)	(0.0188)	(0.0258)
Observations	115,606	115,606	115,606	115,606	115,606	115,606	115,606	115,606	115,606	115,606	115,606	115,606
Dep. Var. (mean)	0.035	0.035	0.035	0.035	-0.032	-0.032	-0.032	-0.032	0.106	0.106	0.106	0.106

All regressions include cohort fixed effects, grade by year fixed effects, school by neighborhood by grade fixed effects, student demographic variables and an indicator for single-family residence. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary correlation for school by cohort and neighborhood by cohort.

We estimate our main model but allow a more narrow definition of peers based on race and gender as given in column headings.

A For Online Publication

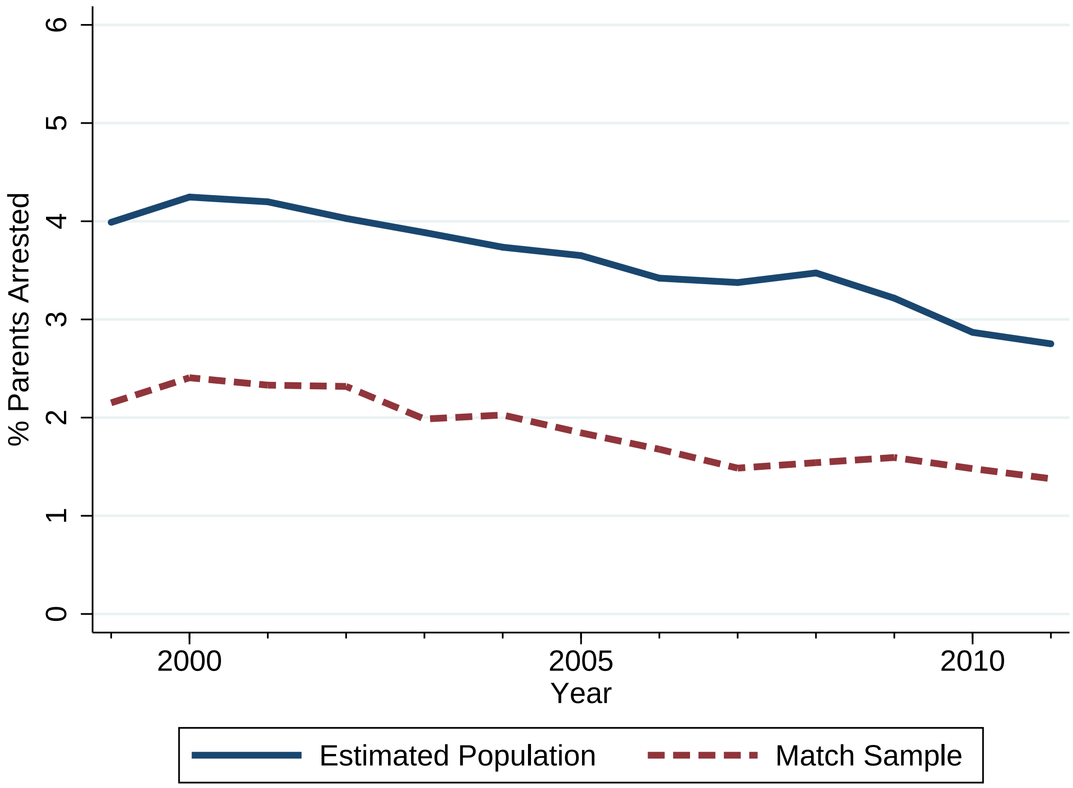
A.1 Parental Matching based on Addresses and Last Name

Appendix Table 1 provides an example of the structure of the data and what potential matches look like. In this example, we observe a student John Williams for five years in our dataset and he lives in the same residence the entire time. In the arrest records, we have arrests of people with the same last name and in some cases the same address. For all matching, we require that same last name and addresses must match between student records and an arrestee's home address at the time of booking. We also only consider adults of parental age which includes almost all adults in the arrest records given that age profile of most criminals. If an adult matches a student uniquely and the student is in elementary school, the student is considered to have a criminal parent (i.e. crime-prone).

In order to get a sense of the portion of likely criminal parents we are able to match to the student database, we created Appendix Figure 1. Appendix Figure 1 provides match rates between arrest and student records for each year relative to the estimated population of children with arrested parents. This figure highlights that we are able to match about 55% of the estimated population to the student records.[24] The dotted line provides the share of students that had a parent arrested in a given year. To create the solid line, we estimate the population of arrestees with children using Census data from the American Community Survey for the study area of Mecklenburg County, NC. The main assumption is that adult arrestees have similar number of children as the overall population. Based on Census 2000 data, we assume that 16.8% of households have children age 6-17 and multiply this times the population of adult arrestees of parental age (age 15-42 for women; 16-48 for men) from the arrest records. we then divided this estimate of parental arrestees by the number of students in the population of student records. we conduct this for each year of overlapping student and arrest records 1999-2011 and present this share in this figure as the dotted line. One would not expect address matching to capture anything close to 100% of the estimated population because of the large prevalence of absentee fathers in this population of incarcerated parents. Furthermore, the estimated population may even be too low if parents involved in the criminal justice system have above average number of children.

[24] This calculation is based on excluding children matched to more than one arrestee as well as children in large apartment complexes. Including these types of matches would bring the average closer to 75%.

Figure A1: Quality of Parental Arrest Match to Student Records



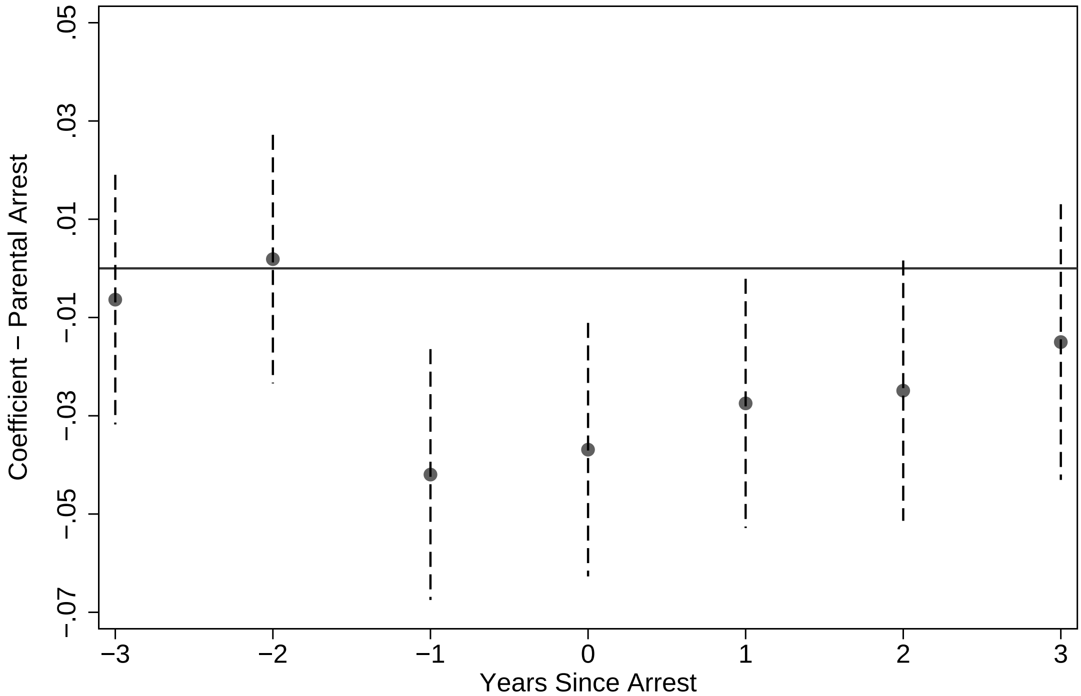
This figure provides my sample match rates of arrest and student records for each year relative to the estimated population of children with criminal parents.

Table A1: Parental Matching

Student Records				
First Name	Last Name	Student Address	School Year	Criminal Parent
John	Williams	200 E 43rd Street	2001	0
John	Williams	200 E 43rd Street	2002	0
John	Williams	200 E 43rd Street	2003	1
John	Williams	200 E 43rd Street	2004	0
John	Williams	200 E 43rd Street	2005	1
Arrest Records				
First Name	Last Name	Address at Arrest	Arrest Year	
Sam	**Williams**	**200 E 43rd Street**	**2003**	
Sam	Williams	200 E 43rd Street	2007	
John	Williams	100 N Broadway Ave.	2004	
Mary	**Williams**	**200 E 43rd Street**	**2005**	

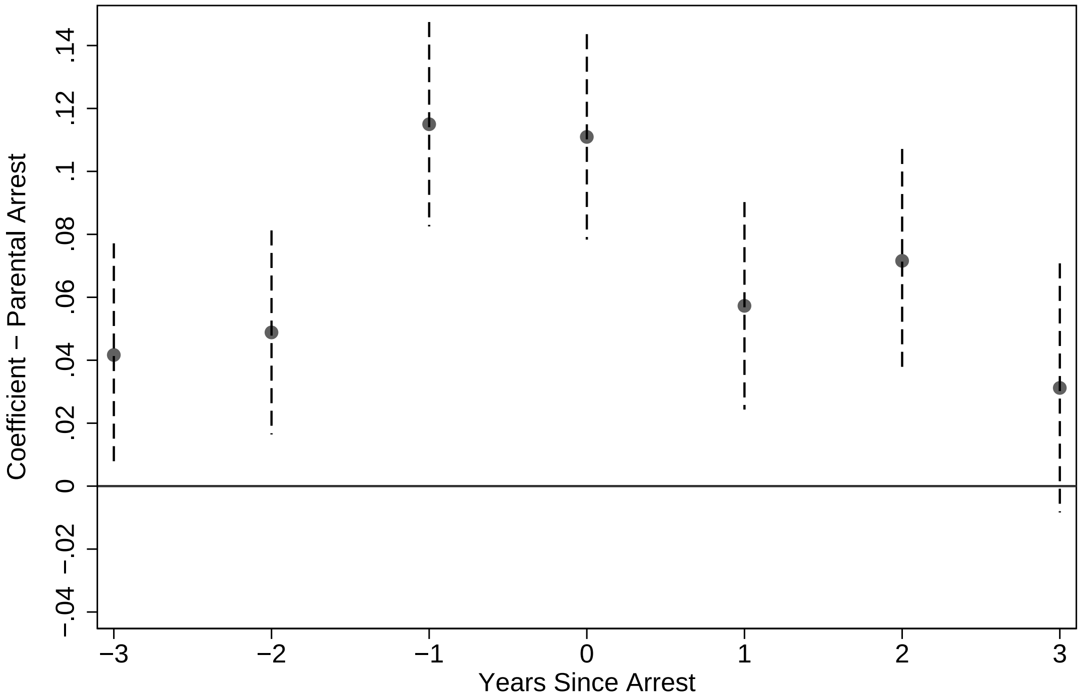
A.2 Appendix Figures and Tables

Figure A2: Own Parent Arrest and Student's Education Index



This figure provides the coefficients and 95% confidence intervals for a panel model that estimates the impact of the yearly education index on contemporaneous parental arrest as well as lagged and lead time periods of parental arrest. The model includes seven dummies estimated in the same model with outcome measured in time period t and dummies for parental arrest for up to three years prior or post the year of the outcome. The model also include fixed effects for birth year by school year by age. Coefficients are relative to arrests more than 3 years before/after a parental arrest. This figure is only based on a sample of kids with birth years between 1989 and 1993 who had a parent that was arrested. N = 58,563 since we observe 10,784 students in Table 1 for an average of 5.8 years.

Figure A3: Own Parent Arrest and Student's Misbehavior Index



This figure provides the coefficients and 95% confidence intervals for a panel model that estimates the impact of the behavior index on contemporaneous parental arrest as well as lagged and lead time periods of parental arrest. The model includes seven dummies estimated in the same model with outcome measured in time period t and dummies for parental arrest for up to three years prior or post the year of the outcome. Coefficients are relative to arrests more than 3 years before/after a parental arrest. The model also include fixed effects for birth year by school year by age. This figure is only based on a sample of kids with birth years between 1989 and 1993 who had a parent that was arrested. N = 58,563 since we observe 10,784 students in Table 1 for an average of 5.8 years.

Table A2: Robustness

	Education Index			Antisocial Behavior Index			Adult Arrest		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
School Peers (%)	-0.3585^{**}	-0.2853^{**}	-0.2304^{*}	0.2311	0.1927	0.2101	0.1510^{***}	0.1316^{**}	0.1204^{**}
	(0.1511)	(0.1303)	(0.1377)	(0.1467)	(0.1424)	(0.1536)	(0.0581)	(0.0561)	(0.0580)
Neigh Peers (%)	−0.0638	−0.0776	−0.0755	0.1733^{***}	0.1760^{***}	0.1688^{***}	0.0531^{**}	0.0554^{**}	0.0551^{**}
	(0.0598)	(0.0532)	(0.0539)	(0.0610)	(0.0599)	(0.0601)	(0.0268)	(0.0260)	(0.0260)
Not Neigh, just School Peers (%)	-0.3055^{**}	-0.2201^{*}	−0.1691	0.1476	0.0986	0.1090	0.0841^{*}	0.0602	0.0449
	(0.1309)	(0.1124)	(0.1177)	(0.1291)	(0.1264)	(0.1365)	(0.0508)	(0.0489)	(0.0504)
Neigh, not School Peers (%)	−0.0007	0.0052	0.0046	0.0427	0.0410	0.0376	0.0157	0.0156	0.0160
	(0.0256)	(0.0227)	(0.0230)	(0.0274)	(0.0270)	(0.0270)	(0.0123)	(0.0120)	(0.0119)
Neigh & School Peers (%)	-0.0800^{**}	-0.0716^{**}	-0.0699^{**}	0.0531^{*}	0.0470	0.0458	0.0475^{***}	0.0451^{***}	0.0447^{***}
	(0.0310)	(0.0277)	(0.0279)	(0.0317)	(0.0313)	(0.0315)	(0.0151)	(0.0147)	(0.0148)
Observations	115,606	115,606	115,606	115,606	115,606	115,606	115,606	115,606	115,606
Dep. Var. (mean)	0.035	0.035	0.035	-0.032	-0.032	-0.032	0.106	0.106	0.106
Individual Controls	-	✓	✓	-	✓	✓	-	✓	✓
Cohort Controls	-	-	✓	-	-	✓	-	-	✓

All regressions include cohort fixed effects, grade by year fixed effects, school by neighborhood by grade fixed effects, student demographic variables and an indicator for single-family residence. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary correlation for school by cohort and neighborhood by cohort.

We also only include students without a parental arrest for estimation. Individual control include demographic variables for race, gender and if living in a single-family residence. Cohort controls include share of peers that are male, minority and the total size of a given peer groups for both school and neighborhood definitions.

Table A3: Lag/Lead Cohort Effects

	Education Index			Antisocial Behavior Index			Adult Arrest		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
School Peers (t-1) (%)	−0.2207*		−0.2470*	0.1532		0.1539	0.0384		0.0470
	(0.1249)		(0.1270)	(0.1346)		(0.1394)	(0.0550)		(0.0562)
School Peers (t+1)(%)		−0.0857	−0.1440		0.0300	0.0619		0.0519	0.0790
		(0.1223)	(0.1340)		(0.1329)	(0.1444)		(0.0574)	(0.0593)
School Peers (%)			−0.3128**			0.2066			0.1534***
			(0.1405)			(0.1493)			(0.0581)
Neigh Peers (t-1) (%)	−0.0298		−0.0468	−0.0435		−0.0007	−0.0252		−0.0105
	(0.0482)		(0.0517)	(0.0543)		(0.0583)	(0.0247)		(0.0267)
Neigh Peers (t+1)(%)		0.0257	0.0055		−0.0177	0.0379		−0.0005	0.0161
		(0.0453)	(0.0500)		(0.0576)	(0.0626)		(0.0234)	(0.0260)
Neigh Peers (%)			−0.0957			0.1924***			0.0629**
			(0.0586)			(0.0627)			(0.0283)
Not Neigh, just School Peers (t-1) (%)	−0.1392		−0.1492	0.0205		0.0557	0.0037		0.0371
	(0.1225)		(0.1312)	(0.1241)		(0.1335)	(0.0522)		(0.0539)
Neigh, not School Peers (t-1) (%)	0.0129		0.0212	−0.0696***		−0.0586**	−0.0129		−0.0113
	(0.0246)		(0.0262)	(0.0266)		(0.0270)	(0.0130)		(0.0137)
Neigh & School Peers (t-1)(%)	−0.0356		−0.0570*	0.0325		0.0429	−0.0127		−0.0074
	(0.0278)		(0.0319)	(0.0317)		(0.0362)	(0.0141)		(0.0154)
Not Neigh, just School Peers (t+1) (%)		−0.0115	0.0584		0.0181	−0.0229		0.0961*	0.0807
		(0.1128)	(0.1274)		(0.1209)	(0.1316)		(0.0523)	(0.0553)
Neigh, not School Peers (t+1) (%)		−0.0050	0.0092		0.0224	0.0052		0.0178	0.0165
		(0.0249)	(0.0271)		(0.0302)	(0.0312)		(0.0113)	(0.0116)
Neigh & School Peers (t+1)(%)		0.0238	−0.0126		−0.0416	0.0204		−0.0378	−0.0260
		(0.0554)	(0.0626)		(0.0699)	(0.0773)		(0.0288)	(0.0312)
Not Neigh, just School Peers (%)			−0.2024			0.0829			0.0574
			(0.1368)			(0.1479)			(0.0536)
Neigh, not School Peers (%)			0.0274			−0.0013			0.0174
			(0.0263)			(0.0286)			(0.0134)
Neigh & School Peers (%)			−0.0794**			0.0452			0.0347*
			(0.0378)			(0.0403)			(0.0181)
Observations	115,606	115,606	115,606	115,606	115,606	115,606	115,606	115,606	115,606
Dep. Var. (mean)	0.035	0.035	0.035	-0.032	-0.032	-0.032	0.106	0.106	0.106

All regressions include cohort fixed effects, grade by year fixed effects, school by neighborhood by grade fixed effects, student demographic variables and an indicator for single-family residence. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary correlation for school by cohort and neighborhood by cohort.

We also only include students without a parental arrest for estimation. For each outcome, the first model is our main specification where we substitute the cohort attributes of a cohort a year older into our measure of peers. The second model uses a cohort that is a year younger and the third column for each outcome provides a model where we jointly estimate the effects of same age, one year older and one year younger cohorts.

Table A4: Robustness to Definition of Neighborhood

	Education Index				Antisocial Behavior Index				Adult Arrest			
	Neigh = Main	*Neigh = Tract*	*Neigh = Just CBG*	*Neigh = same street*	*Neigh = Main*	*Neigh = Tract*	*Neigh = Just CBG*	*Neigh = same street*	*Neigh = Main*	*Neigh = Tract*	*Neigh = Just CBG*	*Neigh = same street*
School Peers (%)	−0.2853**	−0.3278***	−0.3249**	−0.1952	0.1927	0.2500*	0.2265	0.2445	0.1316**	0.1102**	0.1246**	0.1496**
	(0.1303)	(0.1239)	(0.1287)	(0.1553)	(0.1424)	(0.1350)	(0.1408)	(0.1677)	(0.0561)	(0.0521)	(0.0552)	(0.0724)
Neigh Peers (%)	−0.0776	−0.2270**	−0.1008	−0.0458*	0.1760***	0.1516	0.1722**	0.0813***	0.0554**	0.0028	0.0582*	0.0201
	(0.0532)	(0.1013)	(0.0618)	(0.0237)	(0.0599)	(0.1005)	(0.0693)	(0.0278)	(0.0260)	(0.0463)	(0.0300)	(0.0130)
Not Neigh, just School Peers (%)	−0.2201*	−0.1875**	−0.2477**	−0.1423	0.0986	0.1166	0.1332	0.0784	0.0602	0.0382	0.0598	0.0719
	(0.1124)	(0.0942)	(0.1107)	(0.1311)	(0.1264)	(0.1053)	(0.1229)	(0.1466)	(0.0489)	(0.0411)	(0.0481)	(0.0633)
Neigh, not School Peers (%)	0.0052	−0.0435	−0.0138	−0.0288	0.0410	−0.0106	0.0562*	0.0330	0.0156	−0.0351	0.0289**	0.0393**
	(0.0227)	(0.0565)	(0.0293)	(0.0319)	(0.0270)	(0.0582)	(0.0323)	(0.0412)	(0.0120)	(0.0261)	(0.0131)	(0.0168)
Neigh & School Peers (%)	−0.0716**	−0.0631**	−0.0728**	−0.0656*	0.0470	0.0425	0.0387	0.0949**	0.0451***	0.0378**	0.0409***	0.0502**
	(0.0277)	(0.0288)	(0.0283)	(0.0391)	(0.0313)	(0.0366)	(0.0319)	(0.0457)	(0.0147)	(0.0155)	(0.0151)	(0.0211)
Observations	115,606	115,606	115,606	115,606	115,606	115,606	115,606	115,606	115,606	115,606	115,606	115,606
Dep. Var. (mean)	0.035	0.035	0.035	0.035	-0.032	-0.032	-0.032	-0.032	0.106	0.106	0.106	0.106

All regressions include cohort fixed effects, grade by year fixed effects, school by neighborhood by grade fixed effects, student demographic variables and an indicator for single-family residence. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary correlation for school by cohort and neighborhood by cohort.

Column headings highlight versions of our main model where we define neighborhood differently. The main model based neighborhoods off of unique CBG by elementary and middle school attendance boundaries (2002-2003) (N=491). Census Tract definition of neighborhoods only uses Census Tract 2000 boundaries (N=144). Just CBG definition of neighborhoods only uses Census Block Group 2000 boundaries (N=365). Same street are based on defining a neighborhood as students living on the same street (N=10,593). In order to break up longer streets, every 1000 street numbers is grouped together to create a unique neighborhood. We also only include students without a parental arrest for estimation.

Table A5: Standard Peer Model

	Education Index				Antisocial Behavior Index				Adult Arrest			
	Main	*Peers = % male*	*Peers = % black*	*Peers = % male & black*	*Main*	*Peers = % male*	Peers = % black	*Peers = % male & black*	*Main*	*Peers = % male*	*Peers = % black*	*Peers = % male & black*
School Peers	−0.2853**	−0.0745	−0.0988*	−0.1145	0.1927	0.0347	0.0157	−0.0224	0.1316**	0.0478	0.0266	0.0161
	(0.1303)	(0.0788)	(0.0533)	(0.0745)	(0.1424)	(0.0748)	(0.0563)	(0.0806)	(0.0561)	(0.0325)	(0.0251)	(0.0355)
Neigh Peers (%)	−0.0776	−0.0378	0.0003	0.0233	0.1760***	−0.0112	0.0643*	0.0126	0.0554**	0.0190	−0.0025	0.0083
	(0.0532)	(0.0267)	(0.0344)	(0.0383)	(0.0599)	(0.0287)	(0.0331)	(0.0412)	(0.0260)	(0.0136)	(0.0139)	(0.0184)
Not Neigh, just School Peers (%)	−0.2201*	−0.0337	−0.0753	−0.0584	0.0986	0.0283	−0.0305	−0.0990	0.0602	0.0318	0.0155	−0.0163
	(0.1124)	(0.0692)	(0.0488)	(0.0669)	(0.1264)	(0.0645)	(0.0517)	(0.0703)	(0.0489)	(0.0280)	(0.0225)	(0.0311)
Neigh, not School Peers (%)	0.0052	−0.0113	−0.0040	0.0261*	0.0410	0.0093	−0.0017	−0.0295**	0.0156	0.0048	−0.0034	−0.0163**
	(0.0227)	(0.0087)	(0.0122)	(0.0133)	(0.0270)	(0.0088)	(0.0117)	(0.0142)	(0.0120)	(0.0041)	(0.0054)	(0.0063)
Neigh & School Peers (%)	−0.0716**	−0.0061	−0.0385**	−0.0086	0.0470	−0.0085	0.0486***	−0.0188	0.0451***	0.0046	0.0072	−0.0115
	(0.0277)	(0.0119)	(0.0152)	(0.0172)	(0.0313)	(0.0144)	(0.0152)	(0.0207)	(0.0147)	(0.0064)	(0.0073)	(0.0091)
Observations	115,606	115,606	115,606	115,606	115,606	115,606	115,606	115,606	115,606	115,606	115,606	115,606
Dep. Var. (mean)	0.035	0.035	0.035	0.035	-0.032	-0.032	-0.032	-0.032	0.106	0.106	0.106	0.106

All regressions include cohort fixed effects, grade by year fixed effects, school by neighborhood by grade fixed effects, student demographic variables and an indicator for single-family residence. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary correlation for school by cohort and neighborhood by cohort.

We estimate our main model where we define peers simply based on demographics by column headings to show that peers with parental arrest have different influences than simply peers defined by demographic dimensions that correlate with adult arrest. We also only include students without a parental arrest for estimation.

Table A6: Robustness to Parent-Student Matching

	Education Index			Antisocial Behavior Index			Adult Arrest		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
School Peers (%)	-0.3188^{**}	-0.3242^{**}	-0.3318^{**}	0.2022	0.1990	0.2051	0.1316^{**}	0.1334^{**}	0.1337^{**}
	(0.1417)	(0.1417)	(0.1417)	(0.1438)	(0.1438)	(0.1437)	(0.0556)	(0.0558)	(0.0559)
Neigh Peers (%)	−0.0702	−0.0711	−0.0726	0.1781^{***}	0.1777^{***}	0.1816^{***}	0.0554^{**}	0.0558^{**}	0.0562^{**}
	(0.0575)	(0.0576)	(0.0576)	(0.0601)	(0.0603)	(0.0602)	(0.0260)	(0.0262)	(0.0262)
Not Neigh, just School Peers (%)	-0.2504^{**}	-0.2552^{**}	-0.2606^{**}	0.1065	0.1045	0.1057	0.0602	0.0605	0.0594
	(0.1233)	(0.1232)	(0.1236)	(0.1270)	(0.1269)	(0.1273)	(0.0489)	(0.0488)	(0.0490)
Neigh, not School Peers (%)	0.0031	0.0022	0.0018	0.0412	0.0421	0.0439	0.0156	0.0153	0.0152
	(0.0246)	(0.0249)	(0.0249)	(0.0272)	(0.0273)	(0.0273)	(0.0120)	(0.0120)	(0.0120)
Neigh & School Peers (%)	-0.0741^{**}	-0.0740^{**}	-0.0748^{**}	0.0499	0.0496	0.0504	0.0451^{***}	0.0450^{***}	0.0453^{***}
	(0.0298)	(0.0298)	(0.0298)	(0.0315)	(0.0315)	(0.0315)	(0.0147)	(0.0148)	(0.0148)
Observations	115,606	115,606	115,606	115,606	115,606	115,606	115,606	115,606	115,606
Dep. Var. (mean)	0.036	0.036	0.036	-0.032	-0.032	-0.032	0.106	0.106	0.106
Include Lg. Apts	-	✓		-	✓		-	✓	
Include Multiple Matches	-	-	✓	-	-	✓	-	-	✓

All regressions include cohort fixed effects, grade by year fixed effects, school by neighborhood by grade fixed effects, student demographic variables and an indicator for single-family residence. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary correlation for school by cohort and neighborhood by cohort.

Residential Address based matching is based on using only homes address at the time of arrest for the parent-aged individual and school assignment based address for pupil records. Including all duplicates includes all criminal parents defined in cases of larger apartment complexes where duplicates names are more common for a given street address as well as cases where two arrestees with the same last name and gender are at the same address.

Table A7: Robustness to Alternative Definitions of Criminal Parent

	Education Index			Antisocial Behavior Index			Adult Arrest		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
School Peers (%)	−0.2853**	−0.2106	−0.7192*	0.1927	0.1323	0.9126**	0.1316**	0.1158*	0.3170*
	(0.1303)	(0.1433)	(0.3729)	(0.1424)	(0.1555)	(0.4275)	(0.0561)	(0.0628)	(0.1698)
Neigh Peers (%)	−0.0776	−0.0477	0.0511	0.1760***	0.1380*	0.3158	0.0554**	0.0369	0.1543*
	(0.0532)	(0.0627)	(0.1554)	(0.0599)	(0.0716)	(0.2017)	(0.0260)	(0.0324)	(0.0791)
Not Neigh, just School Peers (%)	−0.2201*	−0.1542	−0.6627**	0.0986	0.0699	0.5314	0.0602	0.0520	0.1245
	(0.1124)	(0.1232)	(0.3355)	(0.1264)	(0.1353)	(0.3766)	(0.0489)	(0.0543)	(0.1488)
Neigh, not School Peers (%)	0.0052	0.0102	0.0524	0.0410	0.0431	−0.0245	0.0156	0.0095	0.0101
	(0.0227)	(0.0291)	(0.0717)	(0.0270)	(0.0334)	(0.1008)	(0.0120)	(0.0140)	(0.0409)
Neigh & School Peers (%)	−0.0716**	−0.0750**	−0.0467	0.0470	0.0585	0.1319	0.0451***	0.0378**	0.0912*
	(0.0277)	(0.0333)	(0.0900)	(0.0313)	(0.0382)	(0.1018)	(0.0147)	(0.0179)	(0.0473)
Observations	115,606	115,606	115,606	115,606	115,606	115,606	115,606	115,606	115,606
Dep. Var. (mean)	0.035	0.035	0.035	-0.032	-0.032	-0.032	0.106	0.106	0.106
Only Serious Crime Parents	-	✓	-	-	✓	-	-	✓	-
Only Parents w/ Incarcerations	-	-	✓	-	-	✓	-	-	✓

All regressions include cohort fixed effects, grade by year fixed effects, school by neighborhood by grade fixed effects, student demographic variables and an indicator for single-family residence. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary correlation for school by cohort and neighborhood by cohort.

To define a serious criminal parent, we only include crimes that are indexed property or violent crimes or parents with multiple arrests. Incarceration defined as any incarceration of at least 2 days that would coincide with an arrest. Approximately, 72% of all criminal parents are considered serious and 9% of all criminal parents are incarcerated.

Table A8: Robustness to Alternative Clustering of Standard Errors

	Education Index			Antisocial Behavior Index			Adult Arrest		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
School Peers (%)	-0.3585^{**}	-0.2853^{**}	-0.2304^{*}	0.2311	0.1927	0.2101	0.1510^{**}	0.1316^{**}	0.1204^{*}
	(0.1374)	(0.1222)	(0.1319)	(0.1439)	(0.1399)	(0.1529)	(0.0608)	(0.0584)	(0.0610)
Neigh Peers (%)	−0.0638	−0.0776	−0.0755	0.1733^{**}	0.1760^{***}	0.1688^{***}	0.0531^{*}	0.0554^{**}	0.0551^{**}
	(0.0616)	(0.0540)	(0.0553)	(0.0666)	(0.0635)	(0.0631)	(0.0271)	(0.0257)	(0.0258)
Not Neigh, just School Peers (%)	-0.3055^{**}	-0.2201^{**}	−0.1691	0.1476	0.0986	0.1090	0.0841	0.0602	0.0449
	(0.1259)	(0.1099)	(0.1159)	(0.1317)	(0.1285)	(0.1402)	(0.0509)	(0.0480)	(0.0498)
Neigh, not School Peers (%)	−0.0007	0.0052	0.0046	0.0427	0.0410	0.0376	0.0157	0.0156	0.0160
	(0.0250)	(0.0207)	(0.0210)	(0.0280)	(0.0279)	(0.0278)	(0.0120)	(0.0118)	(0.0116)
Neigh & School Peers (%)	-0.0800^{***}	-0.0716^{***}	-0.0699^{***}	0.0531	0.0470	0.0458	0.0475^{***}	0.0451^{***}	0.0447^{***}
	(0.0282)	(0.0259)	(0.0265)	(0.0387)	(0.0379)	(0.0385)	(0.0168)	(0.0161)	(0.0162)
Observations	115,606	115,606	115,606	115,606	115,606	115,606	115,606	115,606	115,606
Dep. Var. (mean)	0.035	0.035	0.035	-0.032	-0.032	-0.032	0.106	0.106	0.106
Individual Controls	-	✓	✓	-	✓	✓	-	✓	✓
Cohort Controls	-	-	✓	-	-	✓	-	-	✓

All regressions include cohort fixed effects, grade by year fixed effects, school by neighborhood by grade fixed effects, student demographic variables and an indicator for single-family residence. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors robust to arbitrary correlation for school by cohort and neighborhood by cohort.

We estimate our main model but cluster standard errors two ways based on school and neighborhood (instead of school by cohort and neighborhood by cohort). We also only include students without a parental arrest for estimation.