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DOES SCHOOL CHOICE INCREASE CRIME?

Andrew Bibler
Stephen B. Billings
Stephen Ross

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ABSTRACT

While prior evidence suggests that school choice lottery winners have lower adult criminality, there is little evidence on how school choice lotteries impact non-applicants. We leverage variation in actual lottery winners, conditional on expected lottery winners, to link the displacement of middle school peers to adult criminal outcomes. We find that non-applicant, lower-risk boys are more likely to be arrested as adults when lottery applicants from their neighborhood do not attend their assigned middle school. Our estimated effects of an applicant's departure on students left behind are similar in magnitude to the positive effects experienced by a lottery winner.

Andrew Bibler
University of Nevada, Las Vegas
andrew.bibler@unlv.edu

Stephen B. Billings
University of Colorado Boulder
Department of Economics
stephen.billings@colorado.edu

Stephen Ross
University of Connecticut
Department of Economics
and NBER
stephen.l.ross@uconn.edu

1 Introduction

School choice is an increasingly popular tool for public school districts to better compete with private and charter schools, and stem the loss of students to neighboring school districts (Brunner et al., 2012; Tuttle et al., 2012). School choice lotteries are the primary mechanism for allocating seats to oversubscribed schools, and several scholars find mixed evidence on the effects of winning a lottery on academic outcomes (Abdulkadiroğlu et al., 2011, 2018; Cullen et al., 2006; Dobbie and Fryer Jr, 2015; Hastings et al., 2006; Hastings and Weinstein, 2008; Imberman, 2011; Deming et al., 2014; Mills and Wolf, 2017). In contrast, scholars consistently find benefits in terms of lower self-reported disciplinary issues, arrests, and incarceration for lottery winners (Cullen et al., 2006; Deming, 2011; Imberman, 2011; Dobbie and Fryer Jr, 2015). However, there is no evidence on the subsequent school discipline and criminal justice outcomes of the non-lottery students left behind. The studies that come closest to addressing this question examine the aggregate effects of school choice systems by exploiting the introduction or expansion of school choice policies These studies find mixed impacts on academic outcomes (Gilraine et al., 2021; Mumma, 2022; Hsieh and Urquiola, 2006; Muralidharan and Sundararaman, 2015) and a net reduction in violence and classroom disruption (Lavy, 2010).

We provide novel evidence on the adult criminality of male students who experience the loss of a lottery winner who resides in their small neighborhood and belongs to their cohort of rising middle school students.[1] While innumerable studies have examined the effects of exposure to the attributes of a broad set of peers (e.g., Hoxby (2000); Bifulco et al. (2011); Billings and Hoekstra (2023)), we examine the impact of removing neighborhood peers from a student's middle school, which potentially disrupts pre-existing relationships or prevents future peer connections. In this context, our analysis is more comparable to a small number of studies on the effect of friendships (Fletcher and Ross, 2018; Fletcher et al., 2020; Lavy and Sand, 2019). Finally, while our study cannot capture general equilibrium effects on behavior system-wide as in (Lavy, 2010), we do estimate the direct benefits to lottery winners as a point of comparison.

While prior literature suggests that winning a lottery decreases adult criminality, the expected effect on non-applicants is less clear. If lottery winners are positive neighborhood and school peers, and/or their families offer positive parental inputs to either their neighborhoods or schools, non-applicants may be worse off in terms of criminality. These negative effects might be especially large if lottery wins weaken or break existing relationships between chil-

[1]We focus on male students given their overwhelming presence in the criminal justice system, but also provide results for female students and peers. The use of the same neighborhood highlights the importance of residential proximity in determining social relationships (Bayer et al., 2008; Billings et al., 2019)

dren and/or families. Further, the departure of winners may lead to behavioral responses from parents or the re-sorting of teachers (Pop-Eleches and Urquiola, 2013). Alternatively, a number of papers highlight the negative influence of peers on adult crime (e.g., Glaeser et al. (1996); Bayer et al. (2009); Billings et al. (2019); Kim and Fletcher (2018); Billings and Hoekstra (2023)). In fact, the literature on friendships highlights a range of potential impacts, including negative spillovers on substance abuse and criminality from having more neighborhood peers in the same high school (Fletcher and Ross, 2018; Billings et al., 2019), positive effects on GPA from having friends from educationally advantaged families (Fletcher et al., 2020), and positive test scores and behavioral outcomes from being placed in the same classroom as one's friends (Lavy and Sand, 2019).

To isolate the effects of losing a neighborhood peer on non-applicants, and consistent with literature that highlights homophily among peer groups, we focus our analysis of spillovers from lottery winners onto students who live in the same neighborhood (census block group), are the same gender, are similar in age (rising 6th graders), and are assigned to attend the same middle school. Our main empirical analysis includes a sample of three cohorts of male 5th grade students in Charlotte-Mecklenburg County, NC in the 2005-2006 to 2007-2008 school years. The school district operates a centralized school choice system where a random lottery is used in conjunction with partial priorities to award seats to individuals.

We estimate the effect of the number of lottery applicants who depart their assigned middle school on non-applicant outcomes using the number of 5th grade lottery applicants in the same cohort and residing in both the same Census Block Group (CBG) and the same middle school attendance zone who won their first choice in the lottery as an instrument for the number of applicant departures from this cohort and location. Following Abdulkadiroğlu et al. (2017) and Borusyak and Hull (2023), we isolate the random portion of exposure to lottery winners by conditioning on the expected number of wins constructed from individual applicant probabilities of winning their first choice lottery. Similar to Abdulkadiroğlu et al. (2017), who condition on the probability of winning the lottery to account for individual student application choices and priorities, our analysis isolates the variation arising from randomization by extracting the conditional expectation of the number of peer applicant winners, based on the assignment mechanism.[2]

We find that male students whose same-grade neighbors win the lottery experience greater departures of lottery applicants from their home middle school and neighborhood and are then more likely to be arrested between the ages of 16 and 22. An increase of one applicant departing from their home school implies an imprecise 1.3 percentage point increase in the

[2]We also include block group fixed effects since the instrument—the number of lottery wins—has weak explanatory power without them.

probability of arrest from ages 16-22, which is a 10.7% increase over the sample average. The effects are more precisely estimated and sometimes larger for students with below median arrest risk and considerably larger in percentage terms, given lower rates of arrest in this population. Among those with low predicted risk of future arrest, an increase of one winner leads to a 2.3 percentage point increase in the probability of arrest, a 2.0 percentage point increase in the probability of incarceration, and 0.028 more violent arrests, or 43%, 105%, and 72% of the respective means. It is important to note that we cannot rule out similarly sized absolute effects for the high predicted risk of arrest sample, but the point estimates for this sample are either negative or consistent with much smaller percentage increases in the criminal justice outcomes.

While one possible explanation for the large percentage effects could simply be estimation error, these effects appear more reasonable when recognizing that our estimates may be capturing endogenous peer effects.[3] For example, Billings et al. (2019) also studies the Charlotte-Mecklenburg County school district and finds that being assigned to the same school implies a likelihood of being arrested with a potential neighborhood peer (residing within 1 kilometer) that is 6.5 times larger than the likelihood of being arrested together if the potential peer was assigned to a different school, and Glaeser et al. (1996) documents across city variation in crime that is dramatically larger than can be explained by variation in observable city attributes, attributing the larger variation to social interactions. Even in education where peer effects are notoriously small (Gibbons and Telhaj, 2016), Ross and Shi (2022) estimate a sizable lower bound for endogenous peer effects on college test scores, utilizing partially overlapping peer groups.[4] As an alternative to comparing treatment effect sizes with the mean outcomes of low-risk applicants, we conduct back-of-the-envelope calculations to estimate the gap between the expected outcomes of non-applicants if untreated and the equilibrium outcomes of the middle school peers they are exposed to when treated. We find that losing one same-neighborhood applicant from their middle school cohort shifts the outcomes of these low-risk non-applicants 51% and 61% of the way toward the outcomes of their middle school peers in terms of the likelihood of future arrest and incarceration, respectively.

Lottery applicants have substantially higher test scores than non-applicants in 5th grade, and lottery losers who remain at the neighborhood school outperform non-applicants, exhibiting faster test score growth throughout middle school. Thus, the loss of high-achieving

[3]The 95% confidence lower bound effects for the likelihood of arrest and incarceration are 0.5 and 0.6 percentage points, which imply 9% and 32% increases, respectively.

[4]Ross and Shi (2022) estimates a lower bound because they must correct for the weak instrument problem that arises from the small contextual effects. Small contextual effects of peers on academic outcomes in higher education may explain why most reduced form models have been unable to detect peer effects in that context.

neighborhood peers from the school environment appears to negatively affect students' later life outcomes. This effect is more pronounced among students at low a priori risk of arrest who may have been more likely to associate with high-achieving peers. This finding is consistent with Lavy and Sand (2019) who find larger academic gains when friends in the classroom are stronger academically and with Fletcher et al. (2020) who find that friendship opportunities with students from educationally advantaged families lead to higher grade point averages. This arrest effect persists and increases in magnitude after age 19, when most youths are no longer in high school.[5]

Consistent with a mechanism where lottery wins disrupt pre-existing relationships or prevent new relationships from forming, increases in adult criminality are stronger for those with smaller neighborhood peer groups and for non-applicants who attended elementary school with lottery winners. However, sizable effects persist within the subsample of non-applicants who did not attend the same elementary school as the applicant who departed, suggesting that a substantial portion of the effects documented may arise from the disruption of future relationships, as opposed to the disruption of pre-existing elementary school relationships as in Lavy and Sand (2019).

Further, we rule out broader effects of lottery wins on school peer composition or quality by showing that the effects are not related to underlying changes in assigned middle school characteristics and that estimates are robust to the inclusion of attendance zone by cohort fixed effects. Therefore, these results point to residential proximity, an important determinant of social and peer relationships, as the key factor driving the results (Billings et al., 2019). In addition, we find little evidence of effects on academic outcomes, which suggests that effects on criminal justice outcomes are not simply a function of worse labor market or education options as young adults. Notably, we find no effects on crime for girls.[6] Together, our results support the importance of peer influences among boys, which are often solidified in middle school, and show that the loss of positive role models and friendships when entering 6th grade may lead to anti-social behavior that persists into high school and lasts beyond K-12 education.

To confirm the conditional randomness of peer wins, we show that, conditional on the expected lottery wins, actual wins are uncorrelated with observable student attributes. We also provide a series of robustness tests in which we alter the combinations of neighbor-

[5]Larger effects for adults are consistent with Deming (2009), who finds larger effects for young adults than school-aged children, and with Deming (2009); Kim and Fletcher (2018); Billings and Hoekstra (2023), who find that school-age peer effects persist and are often stronger in adulthood.

[6]These gender differences are consistent with Lavy and Sand (2019) who find that, in Israel, being separated from elementary school friends in middle school leads to large declines in social and school satisfaction and increases in violent behaviors for boys. The effects for girls, however, are smaller in magnitude and focused on feelings of safety and social satisfaction.

hood and school fixed effects, all of which produce similar results. Finally, as a falsification test, we show that using wins of younger cohorts produces insignificant estimates that are substantially smaller in magnitude than our main estimates.

Since lottery applicants tend to be positively selected based on academics, the concentration of effects among low crime risk (and higher achievement) students is consistent with relationships forming between similar students (Billings et al., 2019; Fletcher et al., 2020) and lottery wins disrupting existing or potential relationships among similar peers. The exact nature of this peer relationship is unclear and could vary from direct friendships to academic role models or a more indirect form of social capital. Our findings speak to a broader literature on school segregation and criminal activity among youth; most notably, Billings et al. (2014); Weiner et al. (2009); and Johnson (2011) show that racial segregation contributes to African-American youth involvement in the criminal justice system. However, unlike Billings et al. (2019), our estimated effects are concentrated among low-risk students, and we do not find evidence of larger effects when the winner and the low-risk students left behind are of the same race. This result suggests that the social processes involved in the exit of positively selected peers may be quite different from, for example, rezoning, which could lead to increased concentrations of at-risk students (Billings et al., 2014, 2019).

To examine the benefits to lottery winners, we estimate the direct effects of winning the lottery on adult criminality for lottery applicants to oversubscribed schools. While Deming (2011) also estimates the effects of winning the lottery on criminality in Mecklenburg County, our sample contains more recent cohorts and a different mix of schools. Like Deming (2011), we find that winning the lottery leads to declines in the probability of being incarcerated for students in the top quintile of predicted arrest risk. However, we also show that winning the lottery reduced criminality among students at median as well as below median risk of arrest in our sample.

While school choice in the U.S. has grown dramatically and researchers document positive behavioral benefits for lottery winners, the literature on the broader impacts of school choice on student behavior is limited. Furthermore, the context matters, and our finding of worse outcomes for peers in a large U.S. school district is consistent with recent theoretical work by Barseghyan et al. (2019) showing that, in the presence of strong peer preferences, school choice can be welfare decreasing since aggregate peer characteristics are fixed. Consistent with prior work, we find weaker and less conclusive results on academic outcomes.

Our findings are especially important given the popularity of school choice among local voters (Cookson and Schneider, 2014; Hochschild and Scovronick, 2003). School choice programs that more strongly promote choice among lower-achieving students may lead to a different type of selection than documented here, as well as by Hastings et al. (2006); Burgess et al. (2015); and Barseghyan et al. (2019). A more representative composition of lottery winners would likely generate lower costs for non-applicants. For example, Rucinski and Goodman (2022) recommends providing lower-achieving students higher priorities in school choice lotteries, as well as removing academic admissions exams for certain choice schools. Notably, many state financing systems shift funding away from residentially assigned schools when students opt out of their assigned schools, limiting resources to address the negative effects experienced by students left behind. The gains from school choice of retaining high-achieving students in public school districts likely come at a significant cost to non-school choice students in the form of increased arrests and incarceration.

2 School Choice In Charlotte-Mecklenburg Schools

We use data from Charlotte-Mecklenburg Schools (CMS) which is a large and diverse urban school district that currently serves about 150,000 students. The CMS school choice system started in 2002 and shares a number of attributes with other commonly studied US school districts (e.g., Boston, NYC, Chicago, etc.). By default, all CMS students are assigned to a neighborhood (home) school based on their residential address. Through the school choice system, students can apply to other traditional public schools and several magnet school options. CMS uses a centralized lottery system to ration seats in oversubscribed programs, with siblings of current students guaranteed admission. In the lottery, students can submit up to three program choices in order of preference.[7] Non-guaranteed seats are assigned in three rounds, considering only first choices in the first round of assignment. If there are more seats available than applicants for a program, all applicants to that program will be assigned to their first choice.

When a choice is oversubscribed, i.e., the number of applicants is greater than the number of available seats, assignment is conditionally random. Seat assignment is not completely random because the probability of winning for a particular student depends on both their choices and their priority group. "Priority groups" refer to sets of students who meet some prespecified individual criteria, such as economic disadvantage and the Title I choice status of their neighborhood school.[8] If a student is not assigned to their first choice, they remain in the unassigned pool and may win a seat in another choice in the following rounds. If a

[7]We refer to programs rather than schools because students apply for specific grades as well as special magnet programs that may include only some classrooms in a school.

[8]A school is designated as a Title I choice school after failing to meet adequate yearly progress in the same subject for two consecutive years. No Child Left Behind (NCLB) mandated that students have the opportunity to attend a non–Title I choice school, but not necessarily to choose the school they attend. Title I choice students who are unassigned in the lottery may receive an assignment to a different non-Title I Choice School.

student does not win any of their lottery choices, they are assigned to their neighborhood school based on pre-specified school attendance zones. Students are most likely to win a choice by making it their first choice, and most seats are awarded in the first round. The share of applications that won their first choice is about 35% of the sample of applicants entering sixth grade in the 2006-2007 to 2008-2009 school years.

In addition to the lottery rules, some magnet programs have specific entrance requirements that are often based on end-of-grade exams from the previous year. For example, to enter one of the STEM programs in sixth grade, students must score at grade level in reading, math, and science on their fifth-grade exams. When the requirements are based on test scores, we can observe whether a student met the stated requirements for their program of choice when determining the probability of admission.

To examine the effects of the lottery on both students who forego the lottery, as well as individual winners, our analysis focuses on two distinct samples: lottery applicants and non-applicants. To construct the sample of lottery applicants, we use any student who applied to a school (other than their residentially assigned neighborhood school) and was not guaranteed a seat, and who had a nonzero chance of admission; e.g., we drop sibling placements and limit this to oversubscribed programs. We also restrict ourselves to students who met the requirements for their first-choice program.[9] The sample of non-applicants includes students who did not specify any choice in the lottery and are therefore initially assigned to their neighborhood school based on their 5th grade residence. Most non-applicants (64%) have exposure to lottery applicants, meaning that at least one same grade, would-be peer from their Census 2000 Block Group (CBG) and neighborhood school zone applied to the lottery. The remaining 36% of non-applicants have no exposure to lottery applicants from their CBG, neighborhood school, and cohort group, but they do share a CBG or neighborhood school with other students in the non-applicant sample.

3 Data

Our main sample is composed of administrative records from Charlotte-Mecklenburg Schools (CMS) for 24,883 5th grade students who attended public school in the county from the 2005-2006 to 2007-2008 school years (North Carolina Education Research Data Center, 2021).[10] The sample includes applicants to 32 oversubscribed programs across 27 schools (out of 28 middle schools in CMS), which come from 59 distinct lotteries (program by cohort combi-

[9] We drop applicants to arts programs and leadership programs from our analysis because we do not observe whether the students met the entrance requirements, e.g., auditions, portfolio assessments, leadership school interviews.

[10] See Bibler et al. (2026) for data and replication details.

nations). We focus on 6th grade (entering middle school) lotteries for these cohorts. The data include student gender, race, an indicator for economic disadvantage, yearly end-of-grade (EOG) test scores, days absent, and days suspended from school. EOG tests are standardized and administered statewide from 1993 to the present.

The North Carolina Education Research Data Center (NCERDC) linked CMS administrative data to arrest registry data for Mecklenburg County using first and last names, as well as date of birth. The arrest data includes individual names and identifiers, information on the number and nature of charges, and any spells of incarceration (Mecklenburg County Sheriff's Office and Charlotte-Mecklenburg Police Department, 2021).The incarceration data provides the time span in which an individual spent time in a county or state facility in North Carolina (Mecklenburg County Sheriff's Office, 2021; North Carolina Department of Adult Correction, 2021). The NCERDC used a sequential matching algorithm that first matches individuals on an exact full name (including middle name) and date of birth. After the first round, matching proceeds by excluding middle names, and finally, by fuzzy matching on full name. Per NCERDC rules, the resulting data only includes unique matches, disallowing probabilistic matching or multiple matches.

The arrest rates in the matched dataset are comparable to other papers using administrative data in Mecklenburg County (Deming, 2011; Billings et al., 2014, 2019). An arrest rate of 13% for males aged 16-22 in the merged data is in line with the 10-16% found in the literature, which varies somewhat based on the age range and years examined. We define "offenders" as students who were arrested by the Charlotte-Mecklenburg Police Department (CMPD) during our sample period between the ages of 16 and 22.[11] While we observe the future criminal records of CMS students, regardless of whether they transfer or drop out of school, our arrest outcomes are limited to Mecklenburg County. While we do not observe arrests outside of Mecklenburg County, 99% of students who are ever incarcerated based on statewide prison records also have at least one observed arrest within our Mecklenburg County data.[12]

Because arrests are overwhelmingly male (80%), we limit our sample to 13,491 boys observed in CMS in these cohorts, including 4,165 who specified some choice in the lottery. Although in the appendix, we will present a limited set of estimates for girls. We exclude 2,363 applicants because they were either guaranteed admission, specified their first choice

[11] Individuals arrested at ages 16 or 17 were automatically charged in the adult criminal justice system in North Carolina until Dec 1, 2019.

[12] Mecklenburg County contains Charlotte and the surrounding, relatively affluent suburbs. Most arrests are concentrated in and around the urban center. Surrounding counties have lower population density, lower crime rates, and do not have any urban centers near Mecklenburg County. As a result, most young adult arrestees in Mecklenburg County are observed in CMS schools, and over 90% of age comparable arrestees are matched to student records.

as their neighborhood school, were in lottery groups for which either all or no applicants won their first choice, applied to a magnet program with subjective admission criteria, or did not meet the admission requirements for the magnet program to which they applied.

The non-applicant sample includes male students observed in CMS in 5th grade in the 2005-2006 through 2007-2008 cohorts who did not apply to the school choice lottery for their sixth-grade year[13] and were assigned to attend their neighborhood school for 6th grade. Of the 9,052 students who did not specify any non-neighborhood school choice in the lottery and were assigned to their neighborhood school, we remove students who did not have at least one other same-grade, male student in their CBG-School-Cohort group. The treatment variable described below is undefined for these students because they have no same CBG-School-Cohort peers. This process leaves a sample of 8,572 students. Finally, 9 students are dropped because they would be the only student left from their CBG in the sample after imposing the restrictions above. After these sample restrictions, the analysis samples include 1,802 6th grade lottery applicants and 8,563 non-applicants, of which 5,481 had at least one applicant in their neighborhood and cohort. The main sample includes applicants from 28 neighborhood schools, 296 CBGs, and 456 neighborhood school by CBG combinations.

The sample of non-applicants exhibits arrest and incarceration frequencies that are similar to district averages, and most non-applicants have an applicant in the same cohort and neighborhood. Appendix Table A1 provides descriptive statistics of criminality measures, as well as other student outcomes. The three columns include descriptive statistics for all CMS students, the lottery applicant sample, and the non-applicant sample, respectively. We find that 13% of CMS students had at least one arrest and 8% were incarcerated at least once from ages 16 to 22, which suggests substantial involvement with the criminal justice system. Table 1 includes descriptive statistics for the number of male applications and the total number of same neighborhood male students, as well as student level attributes including race, ethnicity, and test scores. Importantly, the average neighborhood group (CBG-School-Cohort) for non-applicants includes 21 students, which reflects a reasonable scale for likely peer groups. About 2 of these potential peers are lottery applicants, on average.

Non-applicants have more peers in their local neighborhood and cohort relative to lottery applicants. Table 1 also highlights higher test scores for lottery applicants and shows that applicants are more likely to be Black rather than White or Hispanic, suggesting substantial selection into lotteries for oversubscribed schools. The pattern of more Black students selecting into the lottery may relate to an over-representation of lower-performing schools in neighborhoods with a higher proportion of Black students.

Table 2 provides descriptive statistics for the applicant sample, which is the source of the

[13] Or submitted an application to their neighborhood school.

identifying variation. The first two columns contain means and standard deviations among lottery winners and losers. Lottery winners are less likely to be economically disadvantaged and have higher test scores. Therefore, not only are lottery applicants positively selected based on test scores, but the sample of actual winners is more positively selected due to the programs they list as their first choice, which vary in the degree of oversubscription. The last column of Table 1 and the last two columns of Table 2 report results of balancing tests that are discussed below.

Finally, it is important to recognize that our population is heterogeneous in students' likelihood of future involvement with the criminal justice system. For example, the likelihood of future arrest among non-applicants varies substantially by race, past academic performance, and assigned middle school, and the data exhibit substantial skewness in continuous outcomes like the number of arrests or days incarcerated. Therefore, similar to Deming (2011), we predict arrest probabilities and divide the non-applicant sample into above and below median likelihood of arrest so that we can allow the effect of neighborhood peer wins to vary with arrest risk. Specifically, we estimate a logistic regression using an indicator for any arrests between ages 16-22 on individual, CBG, and neighborhood school level covariates observed prior to 6th grade. The crime risk estimation results are provided in Appendix Table B1. The individual level predictors are a set of race/ethnicity dummy variables, 5th grade math and reading scores, an indicator for economically disadvantaged, as well as a continuous variable for age. For missing values, we use the mean value and include a dummy variable for missing. We also include means of each race/ethnicity, 5th grade test scores, and economically disadvantaged at both the residential CBG and neighborhood school levels. The model has strong predictive power, with a mean predicted arrest probability for the high-risk subsample of 0.20 versus 0.05 for the low-risk sample. The mean number of arrests in the high- and low-risk samples is 0.86 and 0.13, respectively.

4 Methodology

To begin, we consider how lotteries generate variation in school of attendance since it is the lottery applicants' decisions to leave the neighborhood school that drive the variation in our main analysis. We then use variation in lottery wins to estimate the impact of the number of neighborhood applicant peers that win the lottery on the number of applicant peers that depart the assigned school. Finally, we use the number of wins as an instrument to estimate the effect of peer departures on non-applicant outcomes. Similar to Deming (2011), we focus on the first-choice lottery wins for two main reasons. First, almost all oversubscribed schools are filled with first choice winners, thus limiting random variation in later rounds.

Specifically, 38% of applicants won in the first round, while only 9% won in the second and 6% won in the third. Second, prior literature of lottery effects on crime focused on first choice winners. Thus, adopting the same structure facilitates a better comparison with prior work.

4.1 Estimating Effects on Lottery Winners

Prior to presenting our estimation model, we briefly review the approach developed by Abdulkadiroğlu et al. (2017) for estimating the effect of a student i winning their first choice lottery l (w_{il}) and attending the lottery or application school (t_{il}) on applicant outcomes (y_{il}). We start with a two-stage model of y_{il} as a function of whether the applicant takes up the lottery offer when winning the lottery, where δ_1 and α_1 represent the causal effects of the applicant winning the lottery on attending their first choice school and the applicant attending the choice school on future outcomes, respectively.

$$t_{il} = \delta_1 w_{il} + \lambda_{il} \tag{1}$$

$$y_{il} = \alpha_1 t_{il} + \phi_{il}. \tag{2}$$

Estimation of these equations will yield biased results because individual attributes influence the application school choice and likely correlate with priority group membership, both of which can affect the likelihood of winning the school choice lottery and are correlated with student outcomes, so that $E[\phi_{il}|w_{il}] \neq 0$.

Abdulkadiroğlu et al. (2017) develop an approach to address this bias by including a control for the likelihood of winning the lottery, which we will refer to as a risk control.[14] The intuition is that individual unobservables can only affect the lottery outcome through choices that influence the probability of winning. By conditioning on the conditional expectation of treatment, i.e., the probability of winning, the estimated effect of winning the lottery is identified only based on the randomly drawn lottery number. Specifically, Abdulkadiroğlu et al. (2017) show that for school choice systems with Equal Treatment of Equals, i.e., lottery tie breakers for individuals with the same application school and priority group membership (θ_{il}), the win probability (P_{il}) is conditionally independent of individual attributes. That is,

[14]This differs from Cullen et al. (2006) and Deming (2011), who use lottery by priority group fixed effects. These fixed effects have the potential to subsume the risk control and thus also deliver consistent estimates, but this lottery fixed effect approach is much more data intensive and may not be feasible for many applications.

$$P_{il} \equiv Pr[w_{il}|\phi_{il}, \theta_{il}] = Pr[w_{il}|\theta_{il}]. \tag{3}$$

As a result, precisely controlling for P_{il}, the risk control for treatment is sufficient to obtain an unbiased estimator of both δ_1 and α_1. So, our estimation equations condition on the estimated win probability (P_{il}).

$$t_{il} = \delta_1 w_{il} + \delta_2 P_{il} + \tilde{\lambda}_{il} \tag{4}$$

$$y_{il} = \alpha_1 t_{il} + \alpha_2 P_{il} + \tilde{\phi}_{il}. \tag{5}$$

where δ_2 and α_2 represent linear projections of take-up and outcomes on the probability of winning the lottery. As a result, the unobservables $\tilde{\lambda}_{il}$ and $\tilde{\phi}_{il}$ are orthogonal to treatment w_{il}, and w_{il} serves as a valid instrument for t_{il} in the second stage equation.

Accordingly, instrumental variables estimation of equations (4) and (5) provides a consistent estimator of the causal effect of lottery applicants attending their first choice school (α_1). As part of supplemental analyses, this specification is estimated using the applicant sample to capture the effect of attending lottery schools on applicants.

4.2 Estimating Lottery Spillovers onto Non-Applicants

For our main analysis, we estimate the impact of the total number of neighborhood departures from the assigned school among lottery applicants on the outcomes of non-applicants j from that neighborhood and attendance zone by isolating random variation in the number of lottery wins for a given census block group (CBG) (b) - middle school attendance zone (s) - 5th grade cohort (t) cluster of students. We change the subscript index on an applicant lottery win to reflect this change in the relevant application population to w_{ibst}.

We define total wins (W_{bst}) from the CBG-School-Cohort bst as the sum of the wins w_{ibst} over all applicants in bst where n_{bst} is the number of applicants in bst.

$$W_{bst} \equiv \sum_{i=1}^{n_{bst}} w_{ibst}. \tag{6}$$

We define total departures (T_{bst}) based on the decisions of individual lottery winners and losers to depart from their assigned school ($\tilde{t}_{ibst}$), again among applicants within bst, as

$$T_{bst} \equiv \sum_{i=1}^{n_{bst}} \tilde{t}_{ibst}. \tag{7}$$

We model total departures (T_{bst}) among applicants as a function of total wins (W_{bst}) and model non-applicants' (j) outcomes (y_{jbst}) as a function of the total number of departures, where β_1 captures the causal effect of applicant departures on non-applicant outcomes. However, as discussed below, γ_1 represents a linear projection because interactions between lottery applicants may create non-linearities in the true model of aggregate departures.

$$T_{bst} = \gamma_1 W_{bst} + \mu_{bst} \tag{8}$$

$$y_{jbst} = \beta_1 T_{bst} + \epsilon_{jbst}. \tag{9}$$

As above, estimation of this model, even using wins as an instrument for departures, is biased because non-applicants have sorted into neighborhoods that contain applicants to different schools and in different priority groups. Therefore, non-applicant unobservables are likely correlated with both the number of applicants and the likelihood of those applicants winning access to their first choice school. We calculate the "risk control" for the number of neighborhood-school lottery wins as the expected number of wins, which is a function of the application schools and priority groups (θ_{il}) of all applicants il from location and cohort bst (Θ_{bst}). As in the case of the lottery winners above, the random assignment of each applicant to oversubscribed schools assures independence of the number of wins conditional on the information that determines the expected number of wins.

$$D_{bst} \equiv E[W_{bst}|\epsilon_{il}, \Theta_{bst}] = E[W_{bst}|\Theta_{bst}]. \tag{10}$$

Therefore, conditional on D_{bst}, the unobservable in non-applicant outcomes is orthogonal to the number of departures among lottery applicants, and the resulting estimation equations are.[15]

$$T_{bst} = \gamma_1 W_{bst} + \gamma_2 D_{bst} + \tilde{\mu}_{bst} \tag{11}$$

$$y_{jbst} = \beta_1 T_{bst} + \beta_2 D_{bst} + \tilde{\epsilon}_{jbst}. \tag{12}$$

where γ_2 and β_2 represent the linear projections of applicant departures and non-applicant outcomes on the expected number of wins among applicants. As a result, the unobservables

[15] One might notice a similarity between our control D_{bst} and the types of controls identified by Angrist (2014) in Section 2.2, Greek Peers. The difference between this model and the models criticized by Angrist is that those models included a variable like D_{bst} as the right hand side variable of interest and so implicitly instrumented for mean outcomes using a high dimensional vector of fixed effects as instruments, while in our model D_{bst} is included as an incidental control in both the first and second stage equations.

$\tilde{\mu}_{bst}$ and $\tilde{\epsilon}_{jbst}$ are orthogonal to treatment (W_{bst}), and the number of wins serves as an instrument for the number of departures from the home school among applicants in the second stage equation for the outcomes of non-applicants.

Accordingly, instrumental variables estimation of equations (11) and (12) provides a consistent estimator of the causal effect of peer departures (same neighborhood and assigned middle school lottery applicants) on non-applicants (β_1). Even though lottery applicant information is used to create the controls at the level of the neighborhood and assigned middle school cohort bst, this specification is estimated using the non-applicant sample to capture the effect of lottery applicant departures on non-applicants.

4.3 Consistency of Spillover Estimates

4.3.1 Asymptotic Consistency

In applying the approaches in Abdulkadiroğlu et al. (2017) to peer effect models or more general models of spillovers, several issues arise. The first issue that arises is a change in the necessary assumptions for asymptotic consistency. In Abdulkadiroğlu et al. (2017), estimates are consistent as the number of individuals in each lottery by priority group subsample becomes large. Essentially, their sample estimates pool individually consistent estimates from many individual lottery by priority group subsamples. However, in our design, asymptotics in the size of each CBG-School-Cohort subsample would imply that the number of wins and the expected number of wins should become infinite as each subsample becomes large. In models using shares, such as the fraction of lottery applicants departing the home school, the number of wins asymptotically approaches the expected number of wins, and the model is unidentified. This problem does not arise for traditional lottery effect applications of Abdulkadiroğlu et al. (2017), since, unlike peer effect models, those models exploit variation in treatment within subgroups.

Therefore, we must rely on the asymptotic results provided in Borusyak and Hull (2023). Specifically, Borusyak and Hull (2023) shows that for a general function (f_j) of a vector of random shocks (g) that interacts with individual and potentially endogenous observables, in our case Θ_{bst}, such that $W_{bst} \equiv f(g, \Theta_{bst})$, conditioning on the expected value of the instrument over the distribution of potential shocks (the risk control) assures that the resulting function value is conditionally orthogonal to the error. The intuition is very similar to Abdulkadiroğlu et al. (2017). The relevant function combines information from both individual attributes or choices that likely correlate with individual unobservables and random shocks that are orthogonal to any individual attributes. Conditional on the expected value of the function, the estimated effect of the function on individual outcomes is identified entirely based on the shocks and is therefore orthogonal to the individual unobservables.[16]

Notably, Borusyak and Hull (2023) prove consistency, allowing the subgroup size to be finite while the number of subgroups becomes large. For the reasons described above, these asymptotic results are critical in order to apply the strategies in Abdulkadiroğlu et al. (2017) to studies of peer effects, or more general models of spillovers that rely on random shocks to peer composition or the surrounding environment. However, orthogonality of the error is not sufficient for consistency. Specifically, as described in more detail by Borusyak and Hull (2023), the unobservable, $\tilde{\epsilon}_{jbst}$, may be correlated across individuals within the sample. As a result, the variance of $(W_{bst} - D_{bst})\tilde{\epsilon}_{jbst}$, which captures the variation of the estimator around the true value of the parameter, will contain a quadratic number of non-zero, off-diagonal covariance terms and may not converge to zero as the number of bst clusters becomes large. Accordingly, the estimator may not converge to the true value as the sample becomes large. Borusyak and Hull (2023) shows that the variance converges to zero after imposing one assumption. We present that assumption utilizing our notation as

$$E\left[\frac{1}{N^2}\sum_{l=1}^{N_{bst}}\sum_{j=1}^{n_{bst}}\left(\sum_{m=1}^{N_{bst}}\sum_{k=1}^{n_{bst}} Cov\left[(W_l - D_l)(W_m - D_m)_k|\Theta_l), \Theta_m)\right]\right)\right] \to 0 \tag{13}$$

where N_{bst} is the number of unique neighborhoods by attendance zone by cohort clusters and $(W_l - D_l)$ is the recentered treatment in the cluster l, which takes the same value for all non-applicants j in that cluster.

In our context, this assumption holds, and the estimator is asymptotically consistent if the number of observations within a school by neighborhood by cohort cluster bst remains finite, while the total number of clusters and observations in the sample grows large. This assumption appears reasonable in a context where both neighborhoods and schools are size-constrained as a region grows. Even if cluster size increases with the school district population, such as residential density increasing with the size of the school district, it is still reasonable to assume that an upper limit exists to the size of these neighborhood by school clusters. In fact, census block groups are limited in population by administrative rules, and many practical issues limit growth in school size. See Appendix C for a more detailed discussion.

[16]While the exposition in Borusyak and Hull (2023) focuses on what they refer to as recentering, i.e. subtracting the expected value from the function, they prove consistency for both models with re-centered variables and models that control for the expected value as an additional regressor, similar to Abdulkadiroğlu et al. (2017).

4.3.2 Bias in First Stage and Reduced Form Estimates

The second way in which our model differs from traditional applications of Abdulkadiroğlu et al. (2017) is that treatment in the second stage depends on the aggregation of several first-stage or take up decisions, rather than a single individual's decision to take up the school choice opportunity. In our case, the number of individuals departing the assigned middle school depends both on the number of lottery winners choosing to take up the option and the number of lottery losers who choose not to attend the assigned school by moving residence or by attending a second or third choice school.

In Appendix C, we develop two specific examples where a model involving treatment based on the aggregation of individual choices can affect the estimate of the number of wins in the first stage model. In the first example, we allow the departure decision of both lottery winners and losers to depend on the number of winners in the same attendance zone and block group, possibly because the departures implied by the overall number of wins affect the attractiveness of remaining at the assigned school for both lottery winners and losers. In this case, the first stage individual departure decision model is:

$$t_{ibst} = \delta_1 w_{ibst} + \delta_{2w_i} W_{bst} + \lambda_{ibst} \tag{14}$$

where the parameters on W_{bst} and δ_{2w_i} vary based on whether the individual wins or loses the lottery.

Summing this equation over all applicants in location and cohort bst yields a model for the number of departures T_{bst} that depends on both W_{bst} and the square of W_{bst}, except for the unlikely case in which the effect of the number of winners on individual departure decisions is identical for lottery winners and losers. The presence of this quadratic term in the error of equation (14) implies that the estimate on the number of wins is not equal to δ_1 in the first stage model described by equation (11). This "bias" also influences the reduced form estimates of the effect of the number of wins on non-applicant outcomes. Notably, however, the "bias" in the first stage and reduced form estimates cancel out, and the instrumental variable estimates of the effect of the number of departures on non-applicant outcomes are unaffected by this phenomenon.[17]

Our reliance on the aggregation of potentially complex take-up decisions has implications

[17] The second example involves a situation where the effect of unobservable attributes, such as family resources, attachment to the specific first choice school, or other motivations behind lottery applications, has different effects on home school departure based on whether the individual wins or loses their first choice lottery. In this case, the number of departures depends on both the number of wins and the interaction of the number of wins and the expected number of wins. In Appendix C, we provide empirical evidence that these non-linear terms involving the number of wins have predictive power for explaining the number of departures.

for both the interpretation of balancing tests and the parameter stability of reduced form estimates. First, this "bias" can occur even in apparently well-identified models that pass balance and have stable reduced form estimates as linear controls are added. Specifically, after conditioning on the expected shock, in our case the number of wins, any variable that varies across the entire sample will be orthogonal to the actual shock, since conditioning on the expected shock assures that the estimate is based solely on the random component of the shock. Further, adding an additional control that is conditionally uncorrelated with the variable of interest, i.e., the treatment shock, will have no effect on the relevant parameter estimate. Therefore, while balancing tests are valuable for assessing whether the expected value of the treatment shock is well specified, these tests cannot rule out the type of aggregation bias in the first stage and reduced form models described above. As a result, we strongly recommend that studies of peer and spillover effects using the strategies in Abdulkadiroğlu et al. (2017) and Borusyak and Hull (2023) not rely on reduced form estimation. Rather, researchers should model the first stage of peer or spillover exposure and provide instrumental variable estimates.

A second feature of these types of models is that first stage and reduced form estimates may be unstable in magnitude when high dimensional fixed effects are added as controls, even if conditioning on the expected treatment shock successfully assures that these estimates are based only on the random component of the shock. This parameter instability arises due to the need to rely on asymptotic consistency over the number of clusters in peer effect and spillover models, while allowing the number of individuals in each cluster to be finite. In our case, we require a finite number of applicants in the block groups and attendance zones that make up our block group by attendance zone by cohort clusters. Unlike balancing test variables that are conditionally orthogonal to the random component of the treatment shock, high dimensional fixed effects will be estimated based on finite samples and thus capture part of the random lottery shocks, changing the "bias" in the first stage and leading to instability in the magnitude of reduced form estimates. Therefore, when assessing parameter stability, it is critical to focus on the stability of the instrumental variable estimates, not the reduced form estimates, and examine stability by adding high dimensional fixed effects in addition to traditional balancing test controls. Again, see Appendix C for details on the results described above.

4.4 Expected Number of Wins, Empirical Power and Balancing Tests

Following Abdulkadiroğlu et al. (2017), we construct the probability of winning the lottery for each applicant, our risk control, using an application program by priority group specific lottery win rates and conditioning on these applicant-specific win probabilities. In our context, we observe the number of seats available in all oversubscribed lotteries, the specific rules for determining lottery priority, and the number of applicants and winners within each observed priority group. Specifically, the prediction is based on observed group level wins by program and cohort, using characteristics that determine lottery priorities: whether their neighborhood school or magnet continuation was a Title I Choice school, whether the student is economically disadvantaged, and whether the student scored at grade level in reading in 4th grade (with a separate group for missing 4th grade scores). Therefore, unlike Abdulkadiroğlu et al. (2017) and Borusyak and Hull (2023), who rely on simulations, we directly estimate first choice lottery win probabilities using observed wins within priority group bins. See Appendix B for a more detailed discussion of estimating win probabilities.

In order to highlight the random nature of lottery wins versus expected lottery wins, we provide two results that show expected wins strongly predict wins and that deviations from expected wins are unrelated to neighborhood attributes. Specifically, we expect a one-to-one relationship between expected wins and the number of wins. First, Appendix Figure A1 provides a binned scatter plot of the number of expected wins versus wins for each of our peer groups (neighborhood by cohort), and the slope of the fitted line is very close to one. Second, Appendix Table A3 provides the formal regression of wins on expected wins with different fixed effects. Again, all of these estimates are very close to one, with all estimates ranging between 0.93 and 0.95.

We next examine the power of the random component of the number of lottery wins in explaining the number of departures by estimating equation (11). We estimate the effects for the entire sample of non-applicants and then separately by risk type, i.e., whether the non-applicant is considered to have a high (or low) risk of arrest at age 16 or older based on pre-lottery student and neighborhood attributes. Table 3 column 1 presents the simple first stage results conditional only on the expected number of wins, and unfortunately, the first stage estimate is relatively weak, with a t-statistic of 2.54, or equivalently an F-statistic of only 6.4. The second column presents estimates after adding CBG fixed effects, and those results indicate substantial instability in the first stage estimates, consistent with the aggregation bias discussed above.[18] Including CBG fixed effects increases the point estimate by 60% and decreases the standard error, and the associated F-statistic is 22.5. Columns 3 and 4 show the first stage estimates separately for the sample of high and low risk non-applicants, conditional only on the expected number of wins, and columns 5 and 6 show estimates also conditioning on block group FEs. Similar to columns 1 and 2, columns 3 and 4 imply relatively weak instruments without block group FEs, while columns 5 and 6 imply reasonable instrument power. Specifically, focusing on the diagonal elements, the implied F-statistics in a model with block group fixed effects are 16.7 and 17.6 for the high and low arrest risk non-applicants, respectively. We include Census Block Group fixed effects in our main results in order to increase the power of our first stage estimates, but later show robustness across other specification choices.

We also conduct a power analysis to provide insights into the effect size that our analysis can potentially detect. Specifically, we use our data and estimation equations to simulate outcomes under various alternative hypotheses. Appendix Table A4 includes the results and shows rejection rates for the null hypothesis of no effect, with effect sizes equal to 20% (Panel A) and 30% (Panel B) of the sample mean of the corresponding outcome for both one- and two-sided tests in the pooled, high-risk, and low-risk samples. The power analyses suggest that we have considerable power to reject the null hypothesis for non-applicants at low risk of offending for effect sizes of approximately 20% of the sample mean and some power to reject the null for the high-risk sample for effect sizes of 30% of the sample mean, especially for binary outcomes, like whether ever arrested or incarcerated.

However, the power to reject the null hypothesis for high risk applicants is much more limited when considering the absolute size of the effects. The sample means for the high risk sample are much larger (between 4 and 18 times larger) than the means for the low risk sample. Therefore, in the high risk sample, we do not have the ability to detect absolute effect sizes that would be quite sizable for the low risk sample and easily detected in that sample. Further, while the power appears substantial for the pooled sample, especially at effect sizes of 30% of the mean, the differences in the control group means between high- and low risk non-applicants imply that the effects for low risk applicants would need to be very large as a share of the control group mean to be detected in the pooled sample. For example, the control group mean on any arrest for low risk individuals is 0.05 versus 0.21 for high risk individuals, so the mean for the pooled sample is 2.3 times the mean for the

[18]Another possible explanation is that we do not observe whether the student lived within the small radius (1/3 mile) walk zone of a first choice magnet school, which is also a priority factor. While the number of students affected by these walk zones is small, census block groups might capture residential location within a walk zone. However, the stability of our instrumental variable estimates, as additional controls are added (shown below), works against walk zones as an explanation.

low risk sample.

We conduct two sets of balancing tests to verify that our calculated win probabilities capture the non-random component of lottery wins. We first regress an indicator for winning the first choice in the lottery on individual applicant attributes (X_{il}^k), conditional on the probability of winning, and test for joint significance of the attributes in explaining lottery wins.

$$w_{il} = \delta_{1k} X_{il}^k + \delta_{2k} P_{il} + \lambda_{ilk} \tag{15}$$

Second, we regress the number of peer wins on individual attributes in the sample of non-applicants, conditional on the expected number of wins. Given the limited power of our instrument when only conditioning on the expected number of wins, we also conduct balancing tests after adding CBG fixed effects to the model.

$$W_{bst} = \gamma_{1k} X_{bst}^k + \gamma_{2k} E_{bst} + \mu_{bstk} \tag{16}$$

Returning to our descriptive statistics in Tables 1 and 2, the final two columns of these tables present balancing tests for non-applicants and applicants, respectively. Table 1 column 4 presents estimates of γ_{1k} from Equation (16) for the non-applicant attributes identified in each row, and column 5 presents estimates from a model that also adds CBG fixed effects to increase the first stage power. We fail to reject the null hypothesis of $\gamma_{1k} = 0$ in every case, and the p-values from the tests for joint significance in each column are reasonably far from significance at 0.78 and 0.41, respectively. Similarly, Table 2 column 4 presents the balancing tests for applicant attributes from Equation (15). Again, all estimates are insignificant, and the joint test is well away from significance levels, with a p-value of 0.14. Column 3 presents the balancing test without conditioning on the risk control, i.e., the win probability. Again, all individual estimates are insignificant, although the joint test has a p-value that approaches significance, reinforcing the need to condition on the non-random portion of the treatment shock. In both tables, we also present a balancing test on the predicted likelihood of arrest that we use for separating the sample into above and below the median risk of arrest. The estimates for the likelihood of arrest are far from significance, except for column 3 in Table 2, which presents estimates from a model that did not include the risk control.

5 Results

First, we formally test whether lottery winners are less likely to attend their neighborhood school. Table 4 presents estimates of Equation 1 from regressing several attendance and movement-related outcomes on a dummy variable for winning their first choice in the lottery. Panel A provides results for all lottery applicants, and Panel B provides risk-specific estimates, using interactions with indicators for above and below median crime risk. The outcome in column one is an indicator for having won the lottery for any of their choice schools. Those who win their first choice are about 76 percentage points more likely to have won any choice. The estimate is less than one because some who lose their first choice win their second or third choice. Columns 2 and 3 indicate that first choice lottery winners are almost 70 percentage points more likely to attend their first choice school in 6th grade and 36 percentage points less likely to attend their neighborhood school in 6th grade, relative to lottery losers. This estimated effect for attending neighborhood school is less than the first stage estimates in Table 3 because the total number of winners likely has aggregate effects on departures. Given the lack of effects on non-applicants below, we hypothesize that more wins from a neighborhood increase the likelihood that winners leave their neighborhood school.[19] Furthermore, Appendix Table A2 highlights that the 17% of lottery winners who do not comply with their assigned school are more economically disadvantaged and have lower average test scores than students who win and attend their first choice school. These results suggest that lottery winners who comply with their assignment are even more positively selected than non-complying lottery winners.

Returning to Table 4, the outcomes in columns 4 - 6 are dummy variables for whether the student changed their assigned neighborhood school (Change NS), a sign of residential movement, and dummies for exiting the district (Exit) by grade 6 and grade 9, respectively. These results indicate lower attrition of lottery winners, relative to losers, in terms of residential relocation and exit from the school district. Similar to Bibler and Billings (2020), the negative effect on residential moves (Change NS) indicates that lottery losers are more likely to move. Columns 7 and 8 provide evidence that lottery winners attend schools with higher average test scores and lower predicted arrest rates. Panel B indicates limited heterogeneity by crime risk, with the exception that lower risk students generate most of the effects on residential movement and district exit.

Similarly, an unexpectedly high number of neighborhood lottery winners may impact the decision of non-applicants to attend their neighborhood school or lead to exit from the

[19]Note that 83% of winners attend their first choice school. This is greater than the estimate in Table 4 because some losers eventually gain access to their first choice school through waiting lists, relocation, or subsequent lottery rounds.

district. In Appendix Table A5, we present the estimated effects of a higher number of peer wins (W_{bst}) on residential mobility and district exit in the sample of non-applicants, which are based on estimating Equation 8. We find no evidence that non-applicants respond to peer lottery wins by exiting CMS, overall or in the arrest risk subsamples. We also find small and insignificant effects on residential movement, i.e., changing neighborhood schools by moving to a new attendance zone and exiting the school district. These limited effects exist for both high and low-risk non-applicants. These results limit any concern about sample attrition due to exiting the district or that the loss of positive peers increases the probability that non-applicants remain in the same neighborhood school.

Table 5 provides the main estimates for the impact of lottery winners on those left behind (non-applicants). The first three columns present extensive margin outcomes for any arrest, arrest for violent crime, and incarceration by age 22, while the remaining columns present the analogous intensive margin outcomes. Panel A includes estimates from the pooled sample of non-applicants. Results in panel A are positive and insignificant for all arrests and marginally significant for ever being incarcerated. The loss of one additional peer increases the probability of incarceration by 15%. Turning to Panel B, estimates for students with below median arrest risk are more precise and considerably larger in percentage terms. Among the low-risk non-applicants, the loss of one additional peer generates a 2.3 percentage point increase in the probability of arrest, a 2.0 percentage point increase in the probability of incarceration, and 0.028 more violent arrests. Given lower rates of arrest and incarceration for this subsample, the estimates imply increases of 43%, 105%, and 72% of the respective means, which are much larger than the percentage effects for high-risk students.[20] We see insignificant and small to modest effects for the high-risk group, with effect sizes close to zero, with the exception of days incarcerated, which generates a noisy increase of about 14%.

An alternative way to evaluate the treatment effect for students at low risk of future arrest is to compare the increase in arrest likelihood to the outcomes of the peers to whom they are exposed in their middle school cohort. If treatment is interpreted as losing a positively selected member of the incoming middle school cohort who lives nearby, it may increase the likelihood that the student forms social relationships with other students in the sixth-grade cohort, including peers at relatively high risk of arrest. Although this exercise is

[20] Appendix Tables A6 and A7 show that results are consistent when using a variety of non-linear specifications that better address the intensive margin outcomes, which have a large share of zeros and, in the case of days incarcerated, can generate large values. In fact, we observe substantial gains in precision for days incarcerated, and the positive effects on this outcome for low risk non-applicants are statistically significant. The estimates in these appendix tables are in reduced form to avoid the complexities of non-linear IV. However, we preserve the comparability of the reduced form estimates by retaining the block group fixed effects since the instability of the first stage only arises from changes in the FE structure.

descriptive, we conduct a back-of-the-envelope calculation beginning with the mean outcomes of male, low-risk non-applicants who are not exposed to any same-neighborhood lottery applicant departures (untreated), which are 5.0 and 1.7 percentage points for arrest and incarceration, respectively. We then compare these to the average outcomes of the males in the middle school cohorts of low-risk non-applicants who experience at least one lottery applicant departure (treated). These cohort exposure means are 9.5 and 5.0 percentage points, respectively.

The difference between the outcomes of untreated low-risk non-applicants and the average outcomes of the middle school peers of treated low-risk non-applicants is intended to capture the intensity of exposure. For example, treatment effects equal to this difference would imply that the loss of an applicant peer causes low-risk non-applicants to fully converge to the average equilibrium behavior of their middle school peers. The estimated effects above imply that treated low-risk non-applicants move 51% and 61% of the way toward the behavior of their middle school peers in terms of arrest and incarceration, respectively. While sizable, these effects remain well below the implied doubling of incarceration risk when sample means are used as the benchmark.

Finding strong effects among low-risk non-applicants is consistent with a greater likelihood of social interactions between low-risk students and lottery applicants, which supports the strong positive selection of applicants. The presence of stronger peer effects between individuals with similar attributes is well established in the literature (Billings et al., 2019; Black et al., 2013; Lavy and Schlosser, 2011). The positive selection into the lottery is evident in Table 1, as applicants have higher average test scores in elementary school. Appendix Table A8 provides additional evidence for positive selection, showing that lottery losers who attend their home school score substantially higher than same school non-applicants on middle school tests, both unconditionally and conditionally on lagged test scores. Unfortunately, we cannot rule out sizable absolute effects for the high-risk sample that are similar in magnitude to our effects for the low-risk sample, but half of the estimates have the opposite sign for the high-risk sample, and the percentage effects associated with the positive insignificant estimates are much smaller than the effects for the low-risk sample.

5.1 Lottery Winners and Adult Crime

To assess the relative magnitude of these non-applicant effects, we replicate existing work and examine the direct effect of winning the first choice in the lottery on the arrest and incarceration outcomes for the lottery winners themselves.[21] Our analysis includes the sample

[21] Our results will differ from existing work due to differences in the years of data incorporated as well as some modifications to estimation based on recent econometric developments.

of lottery applicants, focusing on comparing winners and losers. We initially mirror the sample split in our analyses above by using above versus below median arrest risk, but we also provide splits by quintile of arrest risk consistent with Deming (2011).

The estimates are based on Equation 5, which modifies a standard lottery model by incorporating a control for the probability of winning, suggested by Abdulkadiroğlu et al. (2017) to address lottery priorities and choices that determine the probability of winning. The results for our criminal justice outcomes are included in Table 6. While the estimated effects of winning the lottery on arrest and incarceration outcomes are all negative, suggesting decreases in arrests and incarceration, most estimates in the pooled sample (Panel A) are statistically insignificant. However, the magnitudes of the effects are quite large. Lottery winners are an estimated 3.9 percentage points less likely to be incarcerated at any time between the ages of 16 and 22, a 63% decrease off the mean incarceration rate. In the sample of low-risk lottery applicants (Panel B), we find significant declines in both the probability of any violent arrests and any incarceration. Results for the low-risk subsample are more precisely estimated, while estimates for the high-risk subsample are often sizable and negative but with much larger standard errors. Focusing on the low-risk applicant sample, we observe reductions in criminal activity for a student who attends their first choice school that are similar in magnitude to the effect of an applicant departing from the home school on one non-applicant who was left behind.

Following Deming (2011), we also estimate the effects of winning the lottery on arrests and incarceration for those with the highest ex-ante crime risk (top quintile). Appendix Table A9 provides results for 5 groups of lottery applicants based on quintiles of arrest risk. Similar to Deming (2011), we find the largest declines in the highest quintile on the intensive margin, and all three intensive margin estimates for the highest risk quintile are statistically significant. The gains for these high-risk applicants from attending the first choice school are substantially larger than the point estimates for losses experienced by a high-risk non-applicant who was left behind. However, we also estimate statistically significant declines on the extensive and intensive margins for all arrests in the second quintile, similar to the estimates for low-risk students in Table 6. Appendix Table A10 presents similar results for non-applicants exposed to a lottery winner by risk quintile, identifying statistically significant increases in criminal activity from being left behind by an applicant for the lowest three quintiles on non-applicant arrest risk.

5.2 Robustness and Validation

A number of additional results highlight the validity of our identification strategy. First, we show that the main results (which only include CBG fixed effects) are robust to different combinations of neighborhood (CBG) and assigned school fixed effects. Table 7 includes estimates for the effect of the number of exits on our criminal justice outcomes for four different specifications. The inclusion of various fixed effects and additional controls has little effect on the results, confirming the effectiveness of the Borusyak and Hull (2023) approach for IV estimation. Notably, panel A of Table 7 includes school attendance zone by neighborhood fixed effects, which is equivalent to a cohort variation estimator across the three cohorts of students within a given geography. The cohort variation estimates are almost identical to our main estimates.

Next, we implement a falsification test in which we replicate the main results in Table 5, but assign treatment based on the rising 6th graders in the same neighborhood one year later. Specifically, for each student, we estimate our main model using the cohort in the following year in the same CBG and neighborhood school zone. Under the assumption that the behavior of students in a lower grade (one year younger) is unlikely to have a substantial influence on higher grade students, we expect outcomes to be unrelated to these temporally lagged lottery wins. The results are displayed in Table 8. All estimates are statistically insignificant and, in most cases, substantially smaller in magnitude than the main results.

5.3 Academic Outcomes and Effects by Age

Next, we test whether these results extend to non-criminal justice behavioral outcomes and whether these effects arise in middle school prior to the period of observed criminal justice outcomes. Table 9 provides estimated effects of exits on non-applicant absences and suspensions over 5 post-lottery years (typically grades 6-10) based on Equation 8. As with arrests and incarceration, the results suggest that low-risk non-applicants who are left behind by lottery winners experience worse outcomes in these school-related measures; however, these are noisy and substantially smaller effects. Only for low-risk non-applicants do we see any significant increases, including marginally significant 6% and 11% increases in absences and the probability of having any suspension for middle school, respectively.

We also look at other academic outcomes in Appendix Table A11 and find small and mostly insignificant effects on test scores and high school graduation. The estimates for low-risk non-applicants imply that an additional peer exit reduces math and reading scores by 0.034 standard deviations, but these estimates are only marginally significant.

The main estimates for arrests and incarceration use outcomes aggregated over ages 16 to 22. To test how arrest and incarceration effects evolve over time, Table 10 includes the estimated effects of exits on low-risk non-applicant arrest and incarceration outcomes separately over ages 16 to 18 and ages 19 to 22. Coefficients indicate that an additional exit insignificantly increases the probability of any arrest at ages 16-18 by 0.5 percentage points, or 19% over the sample average. At ages 19-22, we estimate an even larger effect on the probability of any arrest of 2.9 percentage points, which, despite the higher incidence of arrest, represents a larger increase in percentage terms of 78%. We find a similar pattern across the other arrest and incarceration outcomes in Table 10.

To illustrate the larger effects post-high school, Figure 1 displays estimated effects at different ages for the sample of low crime risk individuals. Each point corresponds to an estimated effect of an additional peer win on some outcome as a percentage of the average outcome. We include estimated effects on suspensions and absences over three approximate age ranges following the lottery, as well as arrests, violent arrests, and days incarcerated at ages 16-18, 19-20, and 21-22. The estimated effects on suspensions start in middle school and are relatively consistent in magnitude through age 16 and comparable to the effects on arrest and incarceration at ages 16-18. However, we observe substantially larger effects in adulthood as captured by arrests and incarceration. This age trend provides evidence consistent with several papers in the education literature (Deming, 2009; Jacob et al., 2010; Carrell and Hoekstra, 2010; Chetty et al., 2014) that educational treatments (e.g., Head Start, high-quality teachers, and, in our case, peers) can have effects that fade-out initially but grow in young adulthood.

5.4 Heterogeneity

Two key correlates of arrest rates are race and academic performance. Therefore, we estimate heterogeneous effects by race/ethnicity and by above/below median test scores, and present the results in Appendix Table A12. To estimate the heterogeneous effects, we use specifications analogous to our main results based on Equation 12, but we include a set of indicator variables interacted with wins, exits, and expected wins. Panel A reports results for race and ethnicity. The results for White students are the main significant results and closely parallel the results for students with a low risk of arrest in significance and magnitude. Panel B of Appendix Table A12 includes separate estimates for students with above and below median 4th grade test scores, and we find imprecise estimates for each group, indicating that test scores are not as meaningful a dimension as predicted arrest in highlighting the effects of losing a peer. However, in percentage terms, effect sizes across all outcomes are larger for above median test score students. The imprecision in estimates does limit our ability to rule out statistical differences between the various groups.

To this point, we have focused only on male students. To test whether the same dynamics occur among female students, we include estimated effects of departures by arrest risk in the sample of female students in Appendix Table A13. In order to obtain sufficient first stage power, we focus on a model that includes incidental controls for the lottery attributes of applicants and school by cohort FE's, which yields an F-statistic in the first stage of 24. In general, estimates are smaller in magnitude and have larger standard errors than the effects for boys. Although we cannot rule out sizable positive effects relative to the mean for some outcomes, none of the estimates approach statistical significance, and some estimates are negative. While noisy, these results suggest that the nature of peer effects may differ between boys and girls, with stronger influences on male criminality. This result is consistent with Lavy and Sand (2019) where the loss of a peer entering middle school only negatively impacts behavior for boys and has a larger negative impact on school and social satisfaction for boys than for girls.

5.5 Mechanisms

The larger effects on low-risk non-applicants, combined with the positive selection of applicants based on academic performance, suggest two potential mechanisms: 1) a decline in neighborhood school quality and 2) the potential disruption of pre-existing peer relationships and, more generally, the loss of positive peer influences when same-neighborhood students win the lottery.

Lottery applicants and their families may provide positive inputs (e.g., student performance and behavior, as well as parental involvement) to the neighborhood school. To explore this mechanism, Figure 2 includes a series of figures that show the correlation between school-cohort wins and school attributes 3 years post-lottery: average test scores, the proportion of students who are white, and the proportion of students who are economically disadvantaged. Each dot represents a school-cohort level number of wins and characteristic value pair. The estimated slopes and standard errors are presented in the upper right-hand corner of each panel. The panels on the left-hand side of Figure 2 measure actual wins on the x-axis, showing a negative and statistically significant correlation with average test scores and the proportion of students who are White, and a positive and significant correlation with the proportion of economically disadvantaged students. The correlations are consistent with higher lottery participation from more disadvantaged schools and the positive selection of applicants. However, at the school level, wins are aggregated over several small within-attendance zone neighborhoods, and random wins in one neighborhood may not have a disproportionate effect on the school. The right-hand side of Figure 2 includes the analogous correlation between each characteristic and wins minus expected wins. All three correlations change sign and become statistically insignificant when controlling for the expected wins. These results suggest that observable characteristics of neighborhood schools are uncorrelated with unexpected lottery wins.

Additionally, our results in Table 7 Panel B using School by Cohort fixed effects absorb the effect of school-wide departures of lottery applicants, comparing only students in the same school and cohort with different neighborhood group wins. The estimates in Panel B are very similar to our main estimates, and therefore our results cannot be explained by changes in the school experience of a specific cohort of students. Admittedly, changes in the school environment due to a lost neighborhood peer or due to less parental involvement from an immediate neighbor may have a disproportionate effect on the particular students from that neighborhood, which would not be subsumed by the attendance zone by cohort FE's. However, it seems very unlikely that teachers or administrators would be identifying students based on whether they lost same neighborhood peers prior to entering the school. In which case, changes in resources or teacher behavior must be arising from changes in either student or parental behavior to which teachers or administrators are responding. From our perspective, we would classify such neighborhood-specific effects of missing students or parental resources as part of the broad set of effects that arise from social interactions among students and their families at the same school when those students and families live near each other.

Further, several pieces of evidence are consistent with a channel arising from lost relationships with peers. First, the increase in arrests and incarcerations among non-applicants is primarily driven by low-risk non-applicants. This finding is consistent with an expectation that low-risk students would be more likely to interact with the positively selected applicants, and our focus on outcomes that are primarily behavioral in nature, i.e., arrests, incarcerations, and middle school absences and suspensions, rather than academic outcomes, which is consistent with research on school-based peer effects (Billings and Hoekstra, 2023; Kim and Fletcher, 2018; Billings et al., 2014).

Second, previous work on friendships (Billings et al., 2019; Fletcher et al., 2020; Lavy and Sand, 2019) has documented the importance of same-gender peer relationships. Meanwhile, the direct effect of applicant departures on schooling might be expected to operate for both genders as schools lose positively selected students. Therefore, we re-estimate our main results using the departures, wins, and expected wins of girl applicants to estimate the effect of same-neighborhood girls' departures on non-applicant boys, again using a model with school by cohort fixed effects to ensure that the first stage has sufficient power. As shown in

Appendix Table A14, all coefficients are statistically insignificant and always much smaller in magnitude than the estimated effect of boys' departures. This result is consistent with effects being driven by social interactions, given the importance of gender homophily in peer-based relationships, especially for middle school boys.

Next, we split our sample into groups with a large versus small number of same cohort peers in the neighborhood. A small peer group implies closer social connections, which may lead to larger negative effects of losing peers.[22] These results are shown in Appendix Table A15. As hypothesized, larger effects are observed for non-applicants in neighborhoods with fewer peers, both overall and when we stratify by non-applicant arrest risk. Notably, the outcome means for the low-risk group are very similar between the small and large group size subsamples.

We also test whether the response of non-applicants is related to the proportion of same-neighborhood lottery winners who also attended their same elementary school and present the results in Appendix Table A16. This analysis serves two purposes: providing additional evidence that social relationships are important and testing how important the disruption of pre-existing elementary school relationships might be relative to preventing future relationships from forming or broad peer effects such as role models. We first simply include an interaction of the same elementary school control with the number of wins by risk type, plus a similar interaction with the expected number of wins.[23] Notably, we continue to find strong effects of the number of wins for low risk non-applicants even when none of the winning applicants attended the same elementary schools, suggesting that the disruption of pre-existing school relationships is not the dominant factor in explaining the effects of losing peers in our sample. Unfortunately, the estimates of the same elementary school interaction terms are relatively noisy. However, when including school by block group by cohort fixed effects that subsume the direct effect of applicant departures, the interaction estimates imply a substantially larger effect of departures on the likelihood of arrest for low risk non-applicants, an estimate that is statistically significant. Together, these estimates are again suggestive of social relationships as a mechanism but do not support a conclusion that most of the effects are driven by the disruption of pre-existing relationships (at least relationships formed within the elementary school).

Finally, we examine whether the effects on those left behind are concentrated among same-race relationships. Appendix Table A17 tests whether the number of same-race students winning the lottery as a share of total same-race, same-cohort students in the neighbor-

[22]The sample of non-applicants is split evenly between large and small groups within risk types. Large groups average 33 students and 3 applicants, while the small group averages are 8 students and 1 applicant.

[23]We, of course, make the analogous changes to the first stage model of applicant departures.

hood has an additional effect on arrests and incarcerations of non-applicants. The estimates have relatively large standard errors, and for the low-risk sample, they are consistently negative, suggesting smaller effects of losing same-race peers. Overall, while the estimates are noisy, the pattern of evidence works against the effect of lost peers operating along racial lines. We also estimate models allowing for heterogeneity in effects associated with the arrest risk of applicants and the elementary school academic performance of applicants. The estimates from those models are simply too noisy to be informative.

While we cannot rule out absolute effects for the high arrest risk subsample, our findings of large percentage increases in arrests for low risk non-applicants are broadly consistent with Fletcher et al. (2020) and Lavy and Sand (2019) who find positive effects from access to friends. Both Fletcher et al. (2020) and Lavy and Sand (2019) find effects associated with "higher quality" friends, consistent with the fact that lottery applicants are very positively selected. However, unlike Lavy and Sand (2019), our elementary school analysis suggests that a substantial portion of our effects arises from preventing future relationships, as opposed to disrupting pre-existing relationships that formed in elementary school. Our results are inconsistent with the findings of Billings et al. (2019) on arrests and Fletcher and Ross (2018) on substance abuse, especially the strong effects of race identified in Billings et al. (2019). However, in those papers, the estimated effects arose from potential peers at high risk of arrest or, in the case of substance abuse, from friends who used illicit substances, and the role of social relationships in those contexts may be very different from the effects of losing positively selected potential peers.

6 Conclusions

We estimate the effects of neighborhood exposure to male peer lottery winners on arrest and incarceration using three cohorts of 5th grade boys in Charlotte, NC. We find large negative effects on boys who did not apply to the school choice lottery, including an increased probability of arrest, more total arrests, and increased days of incarceration between ages 16 and 22, with the largest effects occurring between ages 19 and 22. These negative effects are concentrated among students with below median arrest risk, which is consistent with peer departure from the home school having a stronger impact on students who would have been likely to interact socially with the positively selected lottery applicants.

Given that applicants have higher test scores in elementary school and lottery losers who remain at the assigned middle school have larger test score gains than their classmates, we examine two potential mechanisms for these effects: the removal of positively selected peers from the same neighborhood and middle school cohort, and broader effects of losing positively selected students on middle school characteristics. The fact that our effects are generated only on low-risk non-applicants and are stronger for groups with fewer same-neighborhood peers suggests the importance of closer social relationships. On the other hand, we do not find any relationship between lottery wins and middle school characteristics. In addition, estimates are robust to the inclusion of middle school attendance zone by cohort fixed effects, which capture unobserved school-level changes due to applicant departures.

Estimating the direct effects of winning the lottery on adult criminality shows that lottery winners experience lower arrest and incarceration rates than those who lost the lottery. For non-applicants at low risk of future arrest, the effect of an applicant departing their home school on adult criminality is comparable in magnitude to the reduced crime outcomes for a low risk applicant attending their first choice school. In terms of numbers, we have over 4,000 low-risk non-applicants exposed to, on average, about 0.6 home school departures, while we have around 900 low-risk applicants, of which around 40% attended their first choice school.[24] Taken together, our estimates and the number of impacted students suggest substantial potential harm for non-applicants left behind in their neighborhood schools relative to the benefits for lottery winners.

These results raise important questions about the overall impact of school choice programs in the key domain where school choice has consistently been shown to have positive impacts on lottery winners. The potential costs of school choice that arise from students who are left behind represent a significant cost that might be considered when deciding whether, or how, to expand school choice opportunities, especially given the heterogeneous costs and benefits across non-applicants and applicants, respectively, based on the likelihood of future arrests. Our results have important implications for public school systems as the popularity of school choice grows (Brunner et al., 2012; Tuttle et al., 2012) and scholars continue to study and refine the lottery mechanisms used to implement school choice (Abdulkadiroğlu et al., 2017; Abdulkadiroğlu and Andersson, 2023).

[24] Admittedly, applicants in the top quintile of arrest risk experience substantially larger declines in criminal activity when they attend their first choice school, but those very high-risk applicants represent a relatively small portion of the applicant pool.

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Tables and Figures

Table 1: Characteristics Summary and Tests

	Summary of Characteristics			Tests (Non-Applicants)	
	CMS	Lottery Apps	Non-Applicants	E[Wins]	+ CBG FE
Arrest Risk	0.13	0.11	0.12	-0.160	0.015
	(0.10)	(0.09)	(0.10)	(0.196)	(0.105)
1[High Risk]	0.50	0.47	0.45	-0.029	-0.022
	(0.50)	(0.50)	(0.50)	(0.045)	(0.019)
Num. Apps	2.37	4.18	2.15		
	(2.98)	(3.32)	(2.86)		
Group N	18.65	16.74	21.34		
	(16.85)	(15.04)	(17.34)		
Num. Wins	0.88	1.53	0.82		
	(1.32)	(1.66)	(1.25)		
Black	0.42	0.52	0.36	-0.058	-0.007
	(0.49)	(0.50)	(0.48)	(0.042)	(0.029)
White	0.36	0.29	0.43	-0.034	-0.006
	(0.48)	(0.45)	(0.49)	(0.052)	(0.020)
Hispanic	0.14	0.11	0.13	0.004	0.036
	(0.35)	(0.31)	(0.34)	(0.036)	(0.022)
Ec. Disadvantage	0.48	0.46	0.44	-0.020	-0.004
	(0.50)	(0.50)	(0.50)	(0.033)	(0.014)
Math 5	0.13	0.38	0.14	0.007	0.004
	(1.01)	(0.94)	(1.02)	(0.017)	(0.010)
Read 5	-0.01	0.27	0.01	-0.005	-0.009
	(1.01)	(0.94)	(1.02)	(0.013)	(0.009)
Math 4	0.08	0.31	0.10	-0.027	-0.011
	(0.99)	(0.97)	(0.99)	(0.021)	(0.012)
Read 4	-0.03	0.23	-0.01	-0.000	-0.001
	(0.98)	(0.91)	(0.98)	(0.014)	(0.010)
Missing Test 5	0.05	0.01	0.04	0.000	0.010
	(0.23)	(0.10)	(0.21)	(0.055)	(0.034)
Missing Test 4	0.13	0.04	0.13	0.002	-0.007
	(0.33)	(0.20)	(0.33)	(0.027)	(0.017)
ELL	0.11	0.05	0.10	-0.012	-0.003
	(0.31)	(0.22)	(0.30)	(0.036)	(0.024)
Days Absent 5	5.90	5.10	5.94	0.001	0.000
	(5.94)	(4.99)	(5.83)	(0.001)	(0.001)
Missing Days Absent	0.00	0.00	0.00	-0.131	-0.166
	(0.05)	(0.00)	(0.05)	(0.118)	(0.110)
Observations	13,491	1,802	8,563	8,563	8,563
Clusters				1154	1154
P-value				0.784	0.410

Notes: Summary of student and group level attributes. The first three columns show the mean and standard deviation for CMS, lottery applicants, and non-applicants, respectively. *Ec. Disadvantage* = economically disadvantaged. *Num. Apps* = the number of other male applicants in student's cohort-school-CBG group. *Num. Wins* = the number of other male lottery winners from the applicant sample in the student's cohort-school-CBG group. *Group N* = the number of other male students in student's cohort-school-CBG group. *ELL* = English Language Learner. The last two columns display estimated coefficients from regressing the number of wins on the listed characteristics. The column labeled $E[Wins]$ conditions on expected wins, and the column labeled + *CBG FE* also includes CBG fixed effects. Standard errors are clustered at the CBG-Neighborhood School-Cohort level. P-value refers to the joint test on the individual characteristics and test scores from regressing the number of wins on the characteristics, conditional on expected wins (and CBG fixed effects in the last column).

Table 2: Characteristics Summary and Tests (Applicant Sample)

			Tests	
	Won	Lost	Unconditional	Conditional
Arrest Risk	0.10	0.12	-0.765***	-0.153
	(0.08)	(0.09)	(0.250)	(0.100)
1[High Risk]	0.39	0.52	-0.119**	-0.014
	(0.49)	(0.50)	(0.047)	(0.022)
Made 2nd Application Choice	0.80	0.80	0.016	-0.032
	(0.40)	(0.40)	(0.049)	(0.036)
Made 3rd Application Choice	0.54	0.55	0.025	0.027
	(0.50)	(0.50)	(0.033)	(0.025)
Black	0.45	0.57	-0.071	-0.015
	(0.50)	(0.50)	(0.052)	(0.040)
White	0.36	0.24	0.049	0.071
	(0.48)	(0.43)	(0.063)	(0.048)
Hispanic	0.09	0.12	-0.055	-0.016
	(0.29)	(0.32)	(0.078)	(0.059)
Ec. Disadvantage	0.40	0.50	-0.040	0.024
	(0.49)	(0.50)	(0.034)	(0.020)
Math 4	0.45	0.23	0.024	-0.001
	(0.98)	(0.95)	(0.020)	(0.015)
Read 4	0.34	0.15	0.003	-0.002
	(0.91)	(0.91)	(0.022)	(0.017)
Missing Test 4	0.04	0.04	0.025	-0.072
	(0.20)	(0.20)	(0.065)	(0.065)
ELL	0.04	0.06	-0.031	-0.042
	(0.20)	(0.23)	(0.055)	(0.052)
Days Absent 4	5.13	5.10	0.000	0.000
	(4.06)	(4.11)	(0.003)	(0.003)
Missing Days Absent 4	0.02	0.02	-0.013	0.010
	(0.14)	(0.14)	(0.091)	(0.083)
Observations	685	1,117	1,802	1,802
Clusters			59	59
P-value			0.066	0.142

Notes: Summary of characteristics in the lottery sample. *ELL* is an indicator for English Language Learner. *Ec. Disadvantage* is an indicator for economically disadvantaged. The first two columns show means and standard deviations for lottery winners and losers. The last two columns display estimated coefficients from regressing an indicator for winning the first choice in the lottery on the listed characteristics. Standard errors are clustered at the application choice by year level. P-values refer to the joint test on the listed characteristics from the regression of winning on the characteristics. The column labeled *Unconditional* reports coefficients without conditioning. The *Conditional* column conditions on the estimated win probability.

Table 3: Effect of Peer Wins on Exits (First Stage)

	N Exit	N Exit	N Exit × HR	N Exit × LR	N Exit × HR	N Exit × LR
Panel A: Without Group or Individual Controls						
Wins	0.469**	0.750***				
	(0.185)	(0.158)				
Win × HR			0.389***	0.000***	0.458***	0.103**
			(0.106)	(0.000)	(0.112)	(0.052)
Win × LR			-0.000	0.530**	0.045	0.870***
			(0.000)	(0.255)	(0.040)	(0.207)
Observations	8,568	8,563	8,568	8,568	8,563	8,563
CBG FE		✓			✓	✓
Panel B: With Group and Individual Controls						
Wins	0.782***	0.782***				
	(0.111)	(0.111)				
Win × HR			0.571***	0.061*	0.572***	0.060*
			(0.095)	(0.032)	(0.095)	(0.032)
Win × LR			0.044	0.840***	0.044	0.840***
			(0.031)	(0.121)	(0.031)	(0.121)
Observations	8,563	8,563	8,563	8,563	8,563	8,563
CBG FE	✓	✓	✓	✓	✓	✓
Group Controls	✓	✓	✓	✓	✓	✓
Individual Controls		✓			✓	✓

Notes: First stage estimated effects of peer wins on exits. Panel A reports estimates that condition on expected wins (columns 1 and 3-4) or expected wins with CBG fixed effects (columns 3 and 5-6). The first two columns report the effect of the number of students from their neighborhood group who won their first choice in the lottery on the number of applicants who attended a different school (other than the assigned neighborhood school at the time of the lottery) in the following year. Neighborhood groups include male students in the same cohort, initially assigned neighborhood school, and CBG. The last four columns report results using interactions with the risk indicators of the focal student, based on the estimated arrest probabilities. *N Exit* × *HR* and *N Exit* × *LR* refer to the number of exits interacted with indicators for higher than median arrest risk and lower than median arrest risk. *Wins* × *HR* and *Wins* × *LR* refer to the number of wins interacted with the high- and low-risk indicators, respectively. Panel B reports the analogous estimates while including lottery-related group-level covariates (number of 2nd and 3rd choice wins, number of other wins, and number of other applications) and individual controls. Standard errors are clustered at the CBG-Neighborhood School-Cohort level.

Table 4: Effects of Winning Lottery on Own Attendance and Movement

	Attendance				Exit		Att. School Mean	
	Won Any	App School	Neighb. Sch.	Change NS	Grade 6	Grade 9	Math	Pr(Arrest)
Panel A: Pooled								
Win	0.756***	0.683***	-0.357***	-0.053*	-0.017	-0.051*	0.257***	-0.013***
	(0.018)	(0.029)	(0.029)	(0.032)	(0.027)	(0.029)	(0.045)	(0.004)
Sample Mean	.532	.402	.254	.271	.120	.233	.202	.124
Observations	1802	1802	1802	1802	1802	1802	1577	1577
Panel B: Effect Win, by Risk								
Win X HR	0.788***	0.705***	-0.330***	-0.003	0.004	-0.030	0.275***	-0.021***
	(0.024)	(0.030)	(0.033)	(0.035)	(0.024)	(0.034)	(0.058)	(0.006)
Win X LR	0.733***	0.667***	-0.377***	-0.093**	-0.033	-0.069	0.241***	-0.007*
	(0.031)	(0.042)	(0.042)	(0.038)	(0.037)	(0.043)	(0.055)	(0.004)
Dep Var Mean, HR	.473	.321	.267	.282	.112	.190	.037	.146
Dep Var Mean, LR	.586	.475	.243	.262	.129	.271	.353	.104
Observations	1802	1802	1802	1802	1802	1802	1577	1577

Notes: Estimated effect of winning first choice on attendance and movement of applicants. Each estimate is conditional on the set of student characteristics, and the estimated win probability. Panel A includes estimates for all applicants. Panel B reports the same set of estimates for the high- and low-risk students, based on the estimated probability of arrest. *Wins* $\times$ *HR* refers to estimates for the group with higher than median estimated risk, and *Wins* $\times$ *LR* shows estimates for the group with lower than median estimated risk. Each column in Panel B is estimated in a single regression using interaction terms. *Won Any* is an indicator for winning any choice in the lottery (first, second, or third). *App School* is an indicator for attending the middle school they applied to with their first choice in the lottery. *Neighb. Sch.* indicates whether the student attended their initially assigned neighborhood school in 6th grade. *Change NS* = Designated neighborhood school changes from year of lottery to the next year, indicating likely residential movement. *Exit* = Missing sixth grade school of attendance in CMS data, and missing 9th grade school in CMS data, each of which indicate likely district exit. The last two columns refer to average characteristics in the 6th grade school of attendance. *Math* is the average 6th grade standardized test score from the prior year, and *Pr(Arrest)* is the mean predicted arrest risk among the students attending that school in the same cohort. Standard errors are clustered at the application choice by year level.

Table 5: Effect of Peer Exits on Arrest Outcomes

	Pr(Arrest)	Pr(Violent)	Pr(Incarc.)	All Arrests	Violent Arrests	Days Incarc.
Panel A: Pooled						
N Exit	0.013	0.004	0.011*	0.068	0.023	1.268
	(0.009)	(0.007)	(0.007)	(0.059)	(0.024)	(1.194)
Dep Var Mean	.122	.072	.071	.453	.181	7.501
Observations	8563	8563	8563	8563	8563	8563
Panel B: By Risk						
N Exit × HR	-0.006	-0.001	-0.006	0.040	0.018	2.209
	(0.021)	(0.017)	(0.018)	(0.152)	(0.064)	(3.466)
N Exit × LR	0.023**	0.007	0.020***	0.087**	0.028*	0.909
	(0.009)	(0.006)	(0.007)	(0.038)	(0.015)	(0.623)
Dep Var Mean, HR	.208	.136	.134	.856	.357	15.676
Dep Var Mean, LR	.053	.021	.019	.126	.039	.868
Observations	8563	8563	8563	8563	8563	8563

Notes: Estimated effects of peer exits on non-applicant outcomes. Panel A reports estimates for the full sample. Each estimate in Panel A is for the effect of peer lottery applicants who exit the neighborhood school on the arrest or incarceration outcome indicated by the column heading. The number of applicants who win their first choice in the lottery is used as an instrument for the number of peer exits. Neighborhood groups include male students in the same cohort, initially assigned neighborhood school, and CBG. All regressions condition on the expected number of wins and include CBG fixed effects. Panel B reports the same set of estimates for the high- and low-risk students, based on the estimated probability of arrest. *N Exit* × *HR* shows estimates for the group with higher than median estimated risk, and *N Exit* × *LR* shows estimates for the group with lower than median estimated risk. These are estimated in a single regression using interaction terms. Panel B regressions use two instruments, the number of wins interacted with high- and low-risk indicators. Panel B includes interaction terms between risk indicators and expected wins, as well as a risk indicator. Standard errors are clustered at the CBG-Neighborhood School-Cohort level.

Table 6: Effects of Winning Lottery on Own Arrest Outcomes

	Pr(Arrest)	Pr(Violent)	Pr(Incarc.)	All Arrests	Violent Arrests	Days Incarc.
Panel A: Pooled						
Att 1st Choice School	-0.016	-0.015	-0.039**	-0.237	-0.102	-5.145
	(0.020)	(0.020)	(0.018)	(0.178)	(0.100)	(4.307)
Sample Mean	.11	.065	.062	.398	.177	6.407
Observations	1802	1802	1802	1802	1802	1802
Panel B: By Risk						
Att 1st Choice School × HR	-0.018	0.005	-0.060*	-0.502	-0.203	-17.344*
	(0.040)	(0.039)	(0.033)	(0.388)	(0.224)	(10.038)
Att 1st Choice School × LR	-0.015	-0.032**	-0.022**	-0.035	-0.027	4.363
	(0.016)	(0.013)	(0.010)	(0.040)	(0.024)	(4.262)
Dep Var Mean, HR	.186	.113	.11	.742	.334	11.939
Dep Var Mean, LR	.044	.023	.021	.093	.038	1.491
Observations	1802	1802	1802	1802	1802	1802

Notes: Estimated effect of the attending their first choice school on own arrests and incarceration using first choice wins as an instrument. Each is conditional on the set of student characteristics, and the estimated win probability. Panel A includes estimates for all applicants. Panel B reports the same set of estimates for the high- and low-risk students, based on the estimated probability of arrest. *Att 1st Choice School* × *HR* shows estimates for the group with higher than median estimated risk, and *Att 1st Choice School* × *LR* shows estimates for the group with lower than median estimated risk. Each column in Panel B is estimated in a single regression using interaction terms. Standard errors are clustered at the application choice by year level.

Table 7: Effect of Peer Exits on Arrest Outcomes, Robustness

	Pr(Arrest)	Pr(Violent)	Pr(Incarc.)	All Arrests	Violent Arrests	Days Incarc.
Panel A: CBG-School Fixed Effects						
N Exit × HR	-0.013	-0.003	-0.008	-0.025	0.002	1.339
	(0.019)	(0.016)	(0.017)	(0.135)	(0.056)	(3.151)
N Exit × LR	0.023**	0.007	0.021***	0.078**	0.029*	0.975
	(0.010)	(0.006)	(0.008)	(0.037)	(0.015)	(0.633)
Observations	8535	8535	8535	8535	8535	8535
Panel B: CBG and School-Cohort Fixed Effects						
N Exit × HR	-0.019	-0.004	-0.010	0.002	0.018	1.176
	(0.021)	(0.017)	(0.017)	(0.132)	(0.058)	(3.406)
N Exit × LR	0.022**	0.007	0.021**	0.105**	0.039**	0.997
	(0.011)	(0.007)	(0.009)	(0.050)	(0.020)	(0.893)
Observations	8563	8563	8563	8563	8563	8563
Panel C: CBG Fixed Effects and Group Level Controls						
N Exit × HR	-0.010	-0.007	-0.012	-0.028	-0.005	0.696
	(0.017)	(0.015)	(0.014)	(0.119)	(0.050)	(2.703)
N Exit × LR	0.025***	0.008	0.022***	0.088**	0.030**	1.173*
	(0.009)	(0.006)	(0.007)	(0.039)	(0.015)	(0.707)
Observations	8563	8563	8563	8563	8563	8563
Panel D: CBG FE with Group and Individual Controls						
N Exit × HR	-0.013	-0.010	-0.014	-0.047	-0.015	0.305
	(0.017)	(0.015)	(0.014)	(0.119)	(0.050)	(2.683)
N Exit × LR	0.026***	0.009	0.023***	0.096**	0.036**	1.406*
	(0.009)	(0.006)	(0.007)	(0.039)	(0.016)	(0.733)
Observations	8563	8563	8563	8563	8563	8563

Notes: Estimated effects of peer exits on non-applicant outcomes in alternative specifications. Each panel reports the risk-specific estimates using the number of peer wins interacted with risk indicators as instruments for the number of peer exits interacted with risk indicators (comparable with Table 5, Panel B). All panels include the controls indicated in the panel heading plus controls for the number of expected wins interacted with risk indicators and a risk indicator. Panel A includes CBG by School fixed effects, Panel B includes CBG and School by Cohort fixed effects, and Panel C includes CBG fixed effects and group-level lottery controls (number of 2nd and 3rd choice wins, number of other wins, and number of other applications). Panel D includes CBG fixed effects, group level lottery controls, and individual level controls of the focal (non-applicant) students. Neighborhood groups include male students in the same cohort, initially assigned neighborhood school, and CBG. Each panel is estimated in a single regression using interaction terms. Standard errors are clustered at the CBG-Neighborhood School-Cohort level.

Table 8: Falsification Test, Younger Cohort Wins on Arrest Outcomes

	Pr(Arrest)	Pr(Violent)	Pr(Incarc.)	All Arrests	Violent Arrests	Days Incarc.
Panel A: Pooled						
Lead N Wins	-0.001	-0.002	-0.001	0.008	0.002	0.342
	(0.007)	(0.005)	(0.005)	(0.042)	(0.018)	(0.935)
N	7913	7913	7913	7913	7913	7913
Panel B: By Risk						
Lead N Wins × HR	-0.002	-0.003	-0.003	0.038	0.016	0.979
	(0.010)	(0.009)	(0.008)	(0.069)	(0.032)	(1.841)
Lead N Wins × LR	0.001	-0.000	0.002	-0.013	-0.007	-0.152
	(0.007)	(0.004)	(0.004)	(0.028)	(0.011)	(0.578)
Observations	7913	7913	7913	7913	7913	7913

Notes: Estimated effect of the number of lottery wins from the same school-cbg in the following year on outcomes. Panel A reports estimates for the full sample. Each estimate in Panel A is for the effect of the number wins from the same neighborhood school and CBG in the *following* cohort (one year younger) on the arrest or incarceration outcome indicated by the column heading. Each estimate is conditional on expected wins from the following year and CBG fixed effects. Panel B reports the same set of estimates for the high- and low-risk students, based on the estimated probability of arrest. *Lead Wins* × *HR* shows estimates for the group with higher than median estimated risk, and *Lead Wins* × *LR* shows estimates for the group with lower than median estimated risk. These are estimated in a single regression using interaction terms. Panel B regressions also includes interaction terms between risk indicators and expected wins, as well as a risk indicator. Standard errors are clustered at the CBG-Neighborhood School-Cohort level.

Table 9: Peer Effects on Other Outcomes

		Suspensions			
	Absences	Total Days	Days ISS	Days OSS	Any Susp.
Panel A: Pooled					
Num. Exit	0.249	-0.120	-0.061	-0.059	0.003
	(0.172)	(0.155)	(0.043)	(0.122)	(0.009)
Dep Var Mean	7.557	2.763	.705	2.058	.295
Observations	36092	34386	34386	34386	34386
Panel B: Effect Win, by Risk					
Num. Exit × HR	0.117	-0.665	-0.225*	-0.439	-0.005
	(0.487)	(0.477)	(0.135)	(0.373)	(0.023)
Num. Exit × LR	0.338*	0.179	0.028	0.152	0.011
	(0.177)	(0.158)	(0.044)	(0.118)	(0.009)
Dep Var Mean, HR	9.664	4.981	1.213	3.768	.462
Dep Var Mean, LR	5.807	.877	.273	.604	.152
Observations	36092	34386	34386	34386	34386
Panel C: Grades6 to 8, Low Risk					
Num. Exit × LR	0.315*	0.187	0.045	0.142	0.018*
	(0.162)	(0.158)	(0.045)	(0.117)	(0.010)
Dep Var Mean, LR	5.446	.919	.299	.621	.16
LR Observations	12428	11243	11243	11243	11243
Observations	22739	20779	20779	20779	20779

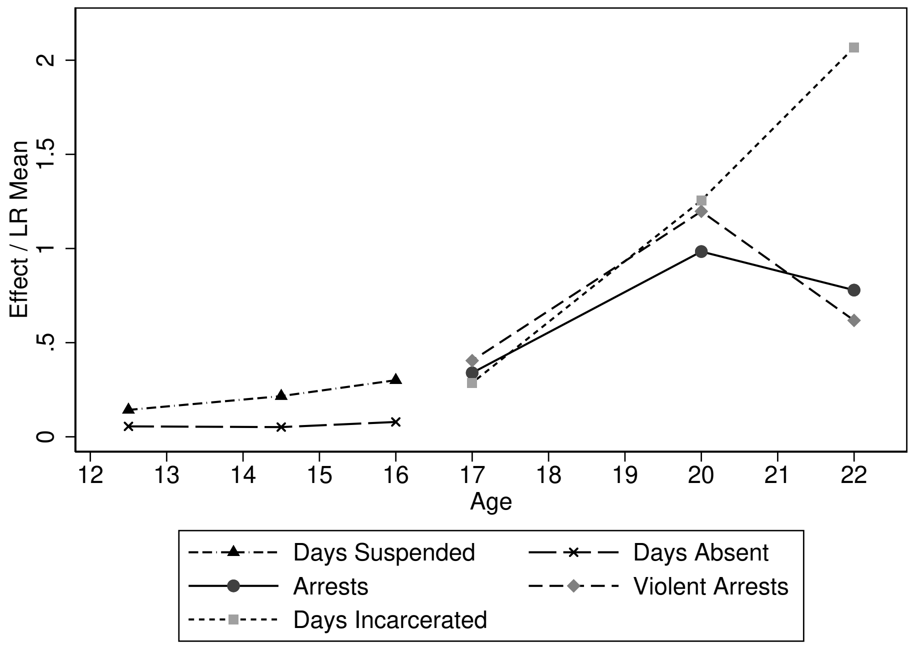
Notes: Estimated effect of peer exits on non-applicant outcomes. Each is conditional on CBG fixed effects and expected wins. ISS = In School Suspension; OSS = Out of School Suspension. Panels A and B include up to 5 years post lottery, or 6th through 10th grade years. Panel A includes estimates from the full sample and uses the number of peer wins as an instrument for the number of exits. Panel B reports the same set of estimates for the high- and low-risk students, based on the estimated probability of arrest. *N Exit × HR* shows estimates for the group with higher than median estimated risk, and *N Exit × LR* shows estimates for the group with lower than median estimated risk. Panel C reports the estimated effects on the subsample with below median risk for the three years post-lottery, or 6th through 8th grade years. Panel B and Panel C are estimated using interaction terms between the number of exits and high- and low-risk, using the number of wins interacted with risk indicators as instruments. Panels B and C regressions also include interaction terms between risk indicators and expected wins, as well as a risk indicator. Standard errors are clustered at the CBG-Neighborhood School-Cohort level.

Table 10: Effect of Peer Wins on Arrest Outcomes (Low-Risk, by Age)

	Pr(Arrest)	Pr(Violent)	Pr(Incarc.)	All Arrests	Violent Arrests	Days Incarc.
Panel A: Low-Risk, Age 16 to 18						
N Exit × LR	0.005	0.001	0.011***	0.016	0.006	0.099
	(0.005)	(0.003)	(0.004)	(0.013)	(0.006)	(0.174)
Dep Var Mean, LR	.027	.011	.01	.047	.016	.349
Observations	8563	8563	8563	8563	8563	8563
Panel B: Age 19 to 22						
N Exit × LR	0.029***	0.010**	0.014**	0.071**	0.021*	0.809
	(0.009)	(0.005)	(0.007)	(0.028)	(0.011)	(0.511)
Dep Var Mean, LR	.037	.014	.014	.079	.023	.519
Observations	8563	8563	8563	8563	8563	8563

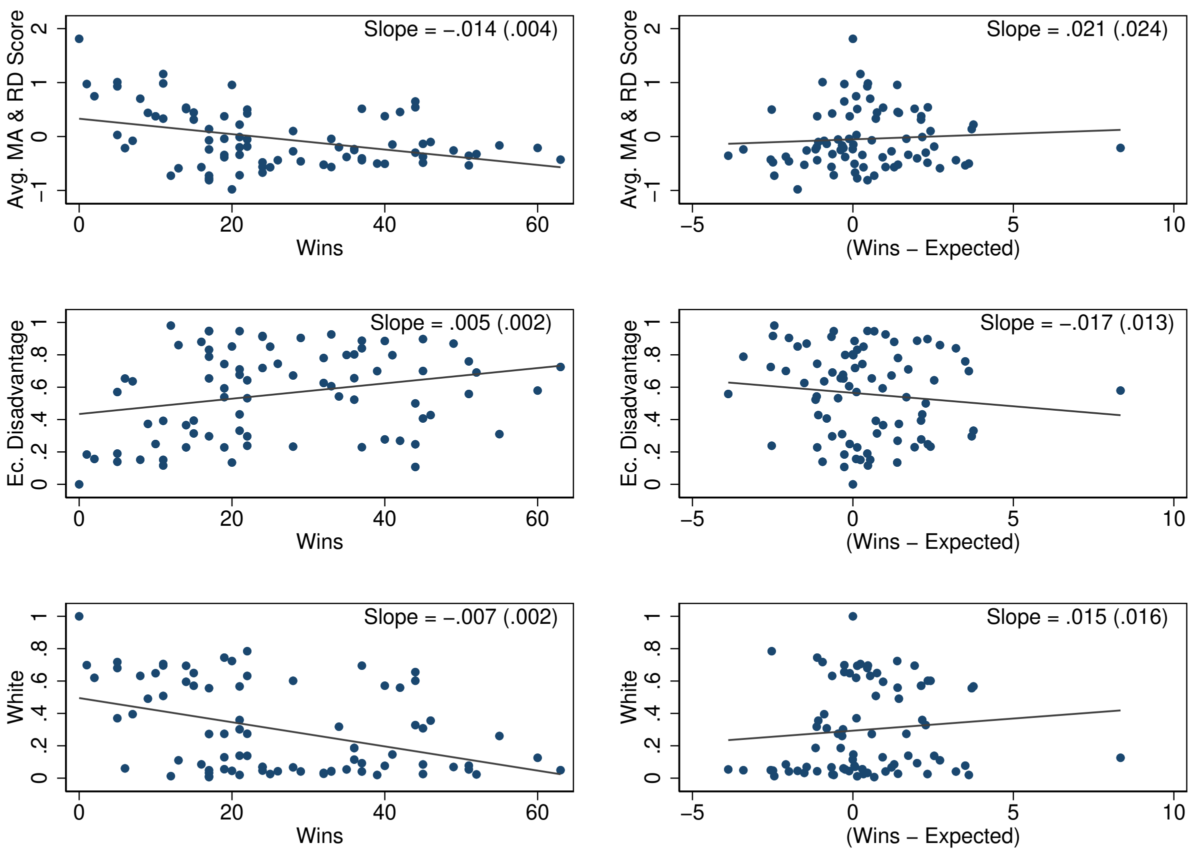
Notes: Estimated effects of the peer exits on outcomes. Panel A reports estimates on the arrest outcomes from ages 16 to 18 for the low-risk sample. Each estimate in Panel A is for the effect of the proportion of students from their neighborhood group who exit the neighborhood school on the arrest or incarceration outcome indicated by the column heading. Lottery wins interacted with risk indicators are used as instruments for the number of exits interacted with risk indicators. Each estimate is conditional on the number of expected wins interacted with risk indicators, a risk indicator, and CBG fixed effects. Panel B reports the same set of estimates for outcomes measured from ages 19 to 22 for low-risk students. Estimates are comparable to the low-risk estimates in Panel B of Table 5. The estimates in Table 5 use the outcomes measured over ages 16 to 22, whereas this table includes separate estimates for the outcomes measured at ages 16 to 18 and 19 to 22. These are estimated in a single regression using interaction terms, similar to Table 5. Standard errors are clustered at the CBG-Neighborhood School-Cohort level.

Figure 1: Estimated Effects on Low-Risk Peers by Age



Notes: Estimated effects of peer Exits on in- and out-of-school outcomes for the low-risk non-applicants. Each point is a standardized estimated of the effect on low-risk non-applicants from a regression based on Equation 12 using wins to instrument for exits. To obtain each point in the figure, we divide each estimate by the age- or grade-specific mean of the same outcome in the low-risk sample. Estimates are included for three distinct approximate age groups for each set of outcomes. For *Days Suspended* and *Days Absent*, one estimate includes the two years post-lottery (6th an 7th grade), one includes the third and fourth years post-lottery (8th and 9th grade), and one includes for the fifth year post-lottery. We do not include estimates for years that correspond to 11th and 12th grade, because students are eligible to drop out at age 17. For the other three outcomes *Arrests*, *Violent Arrests*, and *Days Incarcerated*, we include estimates for each of the three aggregations that we have in our data: Age 16 to 18, age 19 to 20, and age 21 to 22.

Figure 2: Lottery Wins and School Characteristics



Notes: Correlations between neighborhood school characteristics and lottery wins at the school level. Each panel shows observations and the slope estimate for the relation between some measure of wins represented on the x-axis and the school-cohort characteristic on the y-axis. Each dot represents one school-cohort observation. The left-hand column uses school-cohort level wins and the right-hand column uses the number of wins minus expected wins. The school-cohort characteristics are the average of the school-cohort math and reading scores (top two panels), the proportion economically disadvantaged students (middle two panels), and the proportion of white students (bottom two panels). Each school-cohort characteristic measure includes 8th grade students in the designated school 3 years post-lottery, i.e., the 8th grade year.

Online Appendix A Tables and Figures

Table A1: Outcomes Summary

	CMS	Lottery Apps	Non-Applicants
A) Crime Outcomes			
Pr(Any Arrest)	0.13	0.11	0.12
	(0.33)	(0.31)	(0.33)
Pr(Violent Arrests)	0.08	0.07	0.07
	(0.27)	(0.25)	(0.26)
Pr(Incarceration)	0.08	0.06	0.07
	(0.26)	(0.24)	(0.26)
Num. Arrests	0.49	0.40	0.45
	(2.00)	(1.78)	(1.90)
Num. Violent Arrests	0.21	0.18	0.18
	(0.96)	(0.93)	(0.88)
Days Incarcerated	8.95	6.41	7.50
	(60.88)	(49.08)	(54.03)
B) Other Student Outcomes			
Reading Test Scores (6th-8th)	-0.06	0.18	-0.03
	(1.06)	(0.94)	(1.06)
Math Test Scores (6th-8th)	0.05	0.23	0.10
	(1.09)	(1.00)	(1.11)
Days Suspended	2.86	2.17	2.71
	(7.66)	(6.02)	(7.50)
Days Absent	6.28	5.25	6.28
	(7.56)	(5.97)	(7.54)
Graduated HS	0.91	0.96	0.91
	(0.28)	(0.21)	(0.28)
Dropped Out of HS	0.09	0.05	0.09
	(0.28)	(0.21)	(0.29)
Num. of Students in Sample	13,491	1,802	8,563

Notes: Panel A includes the summary statistics of each arrest and incarceration outcome from age 16 to 22 for our main sample of male 5th grade students in Charlotte-Mecklenburg Schools (CMS) between the years of 2005-2006 to 2007-2008. The columns show means and standard deviations for CMS, the sample of lottery applicants and the non-applicant sample, respectively. Panel B includes means and standard deviations for other student outcomes for the same three samples. The last row refers to the number of students in each sample. The number of observations contributing to each mean in Panel B varies.

Table A2: Applicant Summary by Attendance

	Lottery Winners					Lottery Losers				
		Att. App		Att. Neighb.			Att. App		Att. Neighb.	
	All	Yes	No	Yes	No	All	Yes	No	Yes	No
Attend HS (Grade 6)	0.03	0.01	0.15	1.00	0.00	0.39	0.03	0.45	1.00	0.00
	(0.18)	(0.08)	(0.36)	(0.00)	(0.00)	(0.49)	(0.16)	(0.50)	(0.00)	(0.00)
Attend App 1 Schl (Grade 6)	0.83	1.00	0.00	0.18	0.85	0.14	1.00	0.00	0.01	0.23
	(0.38)	(0.00)	(0.00)	(0.39)	(0.36)	(0.35)	(0.00)	(0.00)	(0.10)	(0.42)
Attend App 2/3 Schl (Grade 6)	0.35	0.36	0.34	0.27	0.35	0.31	0.36	0.30	0.24	0.35
	(0.48)	(0.48)	(0.47)	(0.46)	(0.48)	(0.46)	(0.48)	(0.46)	(0.43)	(0.48)
Magnet First Choice	0.75	0.76	0.72	0.77	0.75	0.75	0.70	0.76	0.80	0.72
	(0.43)	(0.43)	(0.45)	(0.43)	(0.43)	(0.43)	(0.46)	(0.43)	(0.40)	(0.45)
Made Second Choice	0.80	0.78	0.86	0.73	0.80	0.80	0.77	0.81	0.78	0.81
	(0.40)	(0.41)	(0.35)	(0.46)	(0.40)	(0.40)	(0.42)	(0.39)	(0.41)	(0.39)
Made Third Choice	0.54	0.52	0.62	0.68	0.54	0.55	0.49	0.56	0.50	0.58
	(0.50)	(0.50)	(0.49)	(0.48)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.49)
Black	0.45	0.45	0.47	0.64	0.45	0.57	0.46	0.58	0.56	0.57
	(0.50)	(0.50)	(0.50)	(0.49)	(0.50)	(0.50)	(0.50)	(0.49)	(0.50)	(0.50)
White	0.36	0.37	0.33	0.23	0.37	0.24	0.33	0.23	0.25	0.24
	(0.48)	(0.48)	(0.47)	(0.43)	(0.48)	(0.43)	(0.47)	(0.42)	(0.43)	(0.43)
Hispanic	0.09	0.08	0.13	0.05	0.09	0.12	0.11	0.12	0.10	0.13
	(0.29)	(0.28)	(0.34)	(0.21)	(0.29)	(0.32)	(0.31)	(0.32)	(0.30)	(0.33)
Ec. Disadvantage	0.40	0.37	0.52	0.55	0.39	0.50	0.40	0.52	0.47	0.52
	(0.49)	(0.48)	(0.50)	(0.51)	(0.49)	(0.50)	(0.49)	(0.50)	(0.50)	(0.50)
Math (Grade 4)	0.45	0.47	0.32	0.39	0.45	0.23	0.43	0.19	0.23	0.22
	(0.98)	(0.96)	(1.05)	(1.02)	(0.98)	(0.95)	(0.91)	(0.96)	(0.97)	(0.94)
Read (Grade 4)	0.34	0.37	0.18	-0.01	0.35	0.15	0.39	0.12	0.15	0.16
	(0.91)	(0.89)	(0.96)	(0.90)	(0.90)	(0.91)	(0.76)	(0.93)	(0.92)	(0.91)
Missing test score	0.04	0.04	0.06	0.05	0.04	0.04	0.01	0.05	0.04	0.04
	(0.20)	(0.20)	(0.24)	(0.21)	(0.20)	(0.20)	(0.11)	(0.21)	(0.19)	(0.21)
ELL	0.04	0.04	0.06	0.05	0.04	0.06	0.07	0.06	0.06	0.06
	(0.20)	(0.19)	(0.24)	(0.21)	(0.20)	(0.23)	(0.25)	(0.23)	(0.23)	(0.23)
Observations	685	566	119	22	663	1,117	159	958	436	681
Proportion		0.83	0.17	0.03	0.97		0.14	0.86	0.39	0.61

Notes: Summary of applicants by application school and neighborhood school attendance. The first five columns show means and standard deviations for lottery winners, winners who do and do not attend the applications school in 6th grade, and winners who do and do not attend the initially assigned neighborhood school in 6th grade. The last five columns display the means and standard deviations for the analogous groups of lottery losers. *ELL* is an indicator for English Language Learner. *Ec. Disadvantage* is an indicator for economically disadvantaged. *Test Miss* is an indicator for missing either test score.

Table A3: Correlation Between Wins and Expected Wins

	Individual Level			Group Level		
	No Controls	CBG FE	All FE	No Controls	CBG FE	All FE
E[W]	0.948***	0.935***	0.931***	0.948***	0.935***	0.931***
	(0.060)	(0.073)	(0.075)	(0.060)	(0.085)	(0.088)
Observations	8563	8563	8563	1154	1134	1134
P-Value	.384	.374	.356	.384	.443	.433
SD(Residuals)	.676	.503	.496	.542	.456	.455

Notes: Estimated regression coefficients from regressing the number of wins on expected number of wins. Estimated at the individual and group levels with no controls, CBG fixed effects only, and all fixed effects (CBG, Cohort, and School). Group level regressions are weighted by the number of sample observations in group. The P-values are for tests against the null hypothesis that the coefficient is 1. Standard deviations of residuals are included in the last row.

Table A4: Power Analysis for Reduced Form, Rejection Rates (5%)

	Pooled		High-Risk		Low-Risk	
	One sided	Two sided	One sided	Two sided	One sided	Two sided
Panel A: Effect Size = 20% of Sample Mean						
Any Arrest	.873	.816	.590	.478	.864	.786
Any Violent Arrest	.682	.602	.391	.303	.764	.686
Any Incarceration	.704	.621	.408	.309	.779	.686
N Arrests	.614	.512	.343	.239	.793	.703
N Violent Arrests	.545	.446	.311	.209	.735	.651
Days Incarcerated	.388	.297	.208	.145	.711	.618
Panel B: Effect Size = 30% of Sample Mean						
Any Arrest	.984	.977	.836	.761	.976	.962
Any Violent Arrest	.917	.866	.626	.514	.943	.919
Any Incarceration	.902	.860	.635	.538	.944	.910
N Arrests	.826	.759	.541	.445	.937	.906
N Violent Arrests	.774	.691	.469	.363	.919	.881
Days Incarcerated	.598	.504	.333	.224	.887	.837

Notes: Rejection rates at 5% level from bootstrap analysis. Panels A and B are for effect sizes equal to 20% and 30% of the sample means, respectively. Each bootstrap sample draws random clusters in the exposed and unexposed sub samples, then random samples within cluster. Treatments = (wins, expected wins) pairs are randomly drawn from the distribution of sample treatments for the exposed bootstrap samples. 1000 repetitions.

Table A5: Effects of Peer Wins on Attendance and Movement

	Attend		Exit	
	Neighb. Sch.	Change NS	Grade 6	Grade 9
Panel A: Pooled				
N Wins	-0.014	0.021	-0.002	-0.005
	(0.014)	(0.016)	(0.006)	(0.009)
Sample Mean	.777	.232	.11	.256
N	8563	8563	8563	8563
Panel B: By Risk				
N Wins × HR	-0.007	0.022	0.001	-0.009
	(0.014)	(0.018)	(0.010)	(0.013)
N Wins ×*LR*	-0.021	0.022	-0.004	-0.002
	(0.021)	(0.022)	(0.008)	(0.011)
Observations	8563	8563	8563	8563

Notes: Estimated effect of wins on non-applicant outcomes. Panel A includes estimates from the full sample. Panel B reports the same set of estimates for the high- and low-risk students, based on the estimated probability of arrest. *Wins* × *HR* shows estimates for the group with higher than median estimated risk, and *Wins* × *LR* shows estimates for the group with lower than median estimated risk. Panel B is estimated using interaction terms between win proportion and high- and low-risk. All regressions include CBG fixed effects. Panel A regressions condition on the number of expected wins and Panel B regressions include interaction terms between risk indicators and expected wins, as well as a risk indicator. *Attend Neighb. Sch.* indicates whether the student attended their initially assigned neighborhood school in 6th grade. *Change NS* = Designated neighborhood school changes from year of lottery to the next year, indicating likely residential movement. *Exit* = Missing sixth grade school of attendance in CMS data, or missing 9th grade school of attendance in CMS data, which indicate likely district exit. Standard errors are clustered at the CBG-Neighborhood School-Cohort level.

Table A6: Effect of Peer Wins on Intensive Margin - Poisson

	All Arrests	Violent Arrests	Days Incarc.
Panel A: Pooled			
N Wins	0.102	0.083	0.155
	(0.082)	(0.081)	(0.111)
Observations	7764	6796	6650
Panel B: By Risk			
N Wins × HR	0.047	0.038	0.130
	(0.088)	(0.086)	(0.118)
N Wins × LR	0.456***	0.459***	0.672**
	(0.127)	(0.155)	(0.281)
Dep Var Mean, HR	.902	.403	17.671
Dep Var Mean, LR	.145	.053	1.263
Observations	7764	6796	6650
Panel C: LR, Age 18-22			
N Wins × LR	0.600***	0.678***	1.026***
	(0.155)	(0.183)	(0.367)
Dep Var Mean, LR	.097	.034	.805
Observations	7390	6314	6363

Notes: Estimated effects of peer wins on intensive margin outcomes using Poisson regression. Panel A reports estimates across the sample, pooling by estimated risk. Each estimate in Panel A is for the effect of the number of lottery applicants from their neighborhood group who won their first choice in the lottery on the arrest or incarceration outcome indicated by the column heading. Neighborhood groups include male students in the same cohort, initially assigned neighborhood school, and CBG. Each regression includes expected wins and CBG fixed effects. Panel B reports the same set of estimates for the high- and low-risk students, based on the estimated probability of arrest. *N Wins* × *HR* shows estimates for the group with higher than median estimated risk, and *N Wins* × *LR* shows estimates for the group with lower than median estimated risk. These are estimated in a single regression using interaction terms. Panel C reports estimates for the low risk group for outcomes measured over ages 18-22. Panel B and C regressions include CBG fixed effects, interaction terms between risk indicators and expected wins, and a risk indicator. Standard errors are clustered at the CBG-Neighborhood School-Cohort level.

Table A7: Days Incarcerated Robustness

	Main	Poisson	Topcode OLS	Topcode Poisson	ln(1+days)	Arcsin(days)
Panel A: Pooled						
N Wins	0.951	0.155	0.589	0.116	0.027	0.031
	(0.830)	(0.111)	(0.509)	(0.094)	(0.017)	(0.020)
APE		1.496		0.788		
Observations	8,563	6,650	8,563	6,650	8,563	8,563
Panel B: By Risk						
N Wins × HR	1.104	0.130	0.504	0.067	0.004	0.004
	(1.522)	(0.118)	(0.897)	(0.101)	(0.030)	(0.035)
N Wins × LR	0.889*	0.672**	0.720*	0.678**	0.049***	0.059***
	(0.530)	(0.281)	(0.406)	(0.287)	(0.0156)	(0.0182)
Observations	8,563	6,650	8,563	6,650	8,563	8,563

Notes: Effects of neighborhood wins on days incarcerated. Column labels refer to the method used for each result: (1) OLS; (2) Poisson regression; (3) and (4) are top coded, replacing the top 1% values of days incarcerated with the 99th percentile (250); (5) OLS using ln(1 + days incarcerated) as outcome; and (6) OLS using arcsin(days incarcerated) as outcome. Panel A reports estimates are for the full sample of non-applicants. Each estimate in Panel A is for the effect of the number of students from their neighborhood group who won their first choice in the lottery. Neighborhood groups include male students in the same cohort, initially assigned neighborhood school, and CBG. Each estimate is conditional on expected wins and CBG fixed effects. Panel B reports estimates for above and below median risk. Each column in panel B is estimated using interactions between risk indicators and the number of wins and expected wins.

Table A8: Selection of Applicants on Test Scores

	Math	Reading	Math	Reading
Applicant, Lost Lottery	0.371***	0.352***	0.080***	0.055**
	(0.045)	(0.040)	(0.028)	(0.026)
Math EOG 4th			0.647***	0.247***
			(0.010)	(0.010)
Read EOG 4th			0.161***	0.527***
			(0.009)	(0.011)
Observations	18397	18331	18397	18331

Notes: Comparison of applicants who lost the lottery and attended their initially assigned neighborhood school with non-applicants who attended their initially assigned neighborhood school. Each column reports estimates from a different regression of math or reading test score on an indicator for being in our lottery sample and losing the first choice in the lottery. Includes observations for up to 3 years post-lottery, i.e., 6th - 8th grade. Estimates in Columns 3 and 4 are conditional on 4th grade reading and math scores, with coefficients reported in the table. Each is also conditional on race/ethnicity, economic disadvantage, whether the student was ever ELL, indicators for missing lagged test scores, and CBG, neighborhood school, cohort fixed effects, and grade of test score. Sample limited to boys only. Standard errors are clustered at the CBG-Neighborhood School-Cohort level.

Table A9: Effect of Winning on Own Arrest Outcomes by Quintile

	Pr(Arrest)	Pr(Violent)	Pr(Incarc.)	All Arrests	Violent Arrests	Days Incarc.
Panel A: By Risk Quintiles						
Q5 (High)	-0.115	-0.113	-0.173***	-1.555***	-0.699**	-29.497**
	(0.078)	(0.074)	(0.050)	(0.563)	(0.321)	(14.925)
Q4	0.014	0.028	-0.000	-0.069	-0.007	-11.834
	(0.060)	(0.049)	(0.047)	(0.558)	(0.332)	(12.619)
Q3	0.027	0.052*	-0.006	0.132	0.075	4.809
	(0.039)	(0.031)	(0.029)	(0.222)	(0.100)	(4.250)
Q2	-0.031	-0.054***	-0.041*	-0.161*	-0.065	1.262
	(0.041)	(0.020)	(0.024)	(0.093)	(0.040)	(1.954)
Q1 (Low)	-0.011	-0.024	-0.023	-0.015	-0.027	0.112
	(0.022)	(0.022)	(0.016)	(0.031)	(0.026)	(0.328)
Observations	1802	1802	1802	1802	1802	1802
Panel B: By Risk						
Q5	-0.115	-0.112	-0.172***	-1.572***	-0.704**	-29.751**
	(0.078)	(0.074)	(0.050)	(0.563)	(0.322)	(14.915)
(Q1-Q4)	-0.001	-0.000	-0.017	-0.030	-0.007	-1.381
	(0.022)	(0.019)	(0.017)	(0.161)	(0.091)	(3.444)
Dep Var Mean, Q5	.262	.164	.171	1.025	.465	17.156
Dep Var Mean, Q1-Q4	.084	.048	.043	.286	.125	4.472
Observations	1802	1802	1802	1802	1802	1802

Notes: Estimated effect of attending the first choice school on own arrests using winning the first choice in the lottery as an instrument. Each is conditional on the set of student characteristics, group indicators, and the estimated win probability interacted with group indicators. Panel A includes separate estimates for each quintile of predicted arrest risk. Panel B includes a low-risk estimates, which pools quintiles 1-4, and high-risk estimate which includes quintile 5. Both panels are estimated using interaction terms between predicted risk indicators and the treatment. Standard errors are clustered at the application choice by year level.

Table A10: Effect of Peer Wins on Arrest Outcomes by Risk Quintile

	Pr(Arrest)	Pr(Violent)	Pr(Incarc.)	All Arrests	Violent Arrests	Days Incarc.
Panel A: By Risk Quintile						
Q5 (High)	-0.004	0.076	-0.001	0.305	0.264	9.353
	(0.065)	(0.062)	(0.057)	(0.583)	(0.244)	(12.201)
Q4	-0.011	-0.036	-0.031	-0.138	-0.109	-2.880
	(0.032)	(0.027)	(0.027)	(0.209)	(0.108)	(5.775)
Q3	0.018	0.008	0.047**	0.169	0.073	4.727
	(0.018)	(0.015)	(0.020)	(0.106)	(0.049)	(3.074)
Q2	0.023	0.006	0.014*	0.062	0.007	0.544
	(0.019)	(0.011)	(0.007)	(0.045)	(0.019)	(0.710)
Q1 (Low)	0.020*	0.005	0.013*	0.059	0.015	-0.136
	(0.011)	(0.006)	(0.008)	(0.042)	(0.018)	(0.459)
Observations	8563	8563	8563	8563	8563	8563
Panel B: By Risk						
Q5	-0.000	0.079	0.002	0.318	0.272	9.211
	(0.065)	(0.062)	(0.057)	(0.584)	(0.246)	(12.253)
(Q1-Q4)	0.013	-0.003	0.011*	0.043	0.000	0.559
	(0.008)	(0.006)	(0.006)	(0.045)	(0.020)	(1.007)
Dep Var Mean, HR	.314	.23	.224	1.463	.625	28.051
Dep Var Mean, LR	.082	.039	.038	.241	.088	3.19
Observations	8563	8563	8563	8563	8563	8563

Notes: Estimated effect of peer exits on outcomes. Number of peer wins in the lottery is used as an instrument for exits. Each is conditional on expected wins interacted with group indicators, group indicators, and CBG fixed effects. Panel A includes separate estimates for each quintile of predicted arrest risk. Panel B includes a low-risk estimate, which pools quintiles 1-4, and high-risk estimate which includes quintile 5. Both panels are estimated using interaction terms between predicted risk indicators and the treatment. Standard errors are clustered at the CBG-Neighborhood School-Cohort level.

Table A11: Peer Effects on Test Scores and Graduation

	Math	Reading	On Time Grad.	Dropout
Panel A: Pooled				
N Exit	-0.023	-0.028*	-0.002	0.009
	(0.015)	(0.017)	(0.010)	(0.009)
Observations	21875	21798	5936	5936
Panel B: Effect Win, by Risk				
N Exit X HR	-0.009	-0.023	-0.014	0.035
	(0.033)	(0.036)	(0.035)	(0.028)
N Exit X LR	-0.034*	-0.034*	0.002	-0.000
	(0.017)	(0.018)	(0.011)	(0.008)
Observations	21875	21798	5936	5936

Notes: Estimated effect of peer exits on non-applicant outcomes. Each is conditional expected wins and CBG fixed effects. Panel A includes estimates from the full sample. Panel B reports the same set of estimates for the high- and low-risk students, based on the estimated probability of arrest. *N Exit* $\times$ *HR* shows estimates for the group with higher than median estimated risk, and *N Exit* $\times$ *LR* shows estimates for the group with lower than median estimated risk. Panel B is estimated using interaction terms between win proportion and high- and low-risk. Panel B regressions also include interaction terms between risk indicators and expected wins, as well as a risk indicator. Test score results include up to three years post-lottery, grades 6-8 for most individuals. The last two columns show outcomes related to graduation and dropout. *On Time Grad.* is an indicator for graduating within 7 years post lottery (12th grade year for on-time progression). *Dropout* is an indicator equal to one if the student was ever observed as dropping out in the 7 years post lottery. The last two columns only include students who were observed graduating or dropping out in the NC data. These include up to one observation per student. Standard errors are clustered at the CBG-Neighborhood School-Cohort level.

Table A12: Heterogeneity in Peer Effects

	Pr(Arrest)	Pr(Violent)	Pr(Incarc.)	All Arrests	Violent Arrests	Days Incarc.
Panel A: Heterogeneity by Race / Ethnicity						
N Exit × White	0.025*	0.013	0.020**	0.138*	0.053*	1.305
	(0.014)	(0.008)	(0.010)	(0.071)	(0.028)	(1.027)
N Exit × Black	0.002	0.003	0.005	0.033	0.014	1.890
	(0.018)	(0.016)	(0.015)	(0.133)	(0.056)	(2.942)
N Exit × Hispanic	0.000	-0.018	0.008	-0.035	-0.030	1.792
	(0.029)	(0.024)	(0.020)	(0.132)	(0.064)	(4.315)
N Exit × Other	0.026	-0.004	0.011	0.046	0.010	-2.289
	(0.026)	(0.018)	(0.021)	(0.145)	(0.068)	(4.115)
N	8563	8563	8563	8563	8563	8563
Panel B: Heterogeneity by Test Scores						
Above Median	0.027*	0.002	0.009	0.005	-0.030	-1.922
	(0.015)	(0.009)	(0.010)	(0.081)	(0.046)	(2.208)
Below Median	-0.000	0.008	0.014	0.116	0.072	3.209
	(0.014)	(0.014)	(0.012)	(0.113)	(0.057)	(2.686)
Dep Var Mean, High	.065	.027	.027	.191	.061	2.162
Dep Var Mean, Low	.183	.118	.119	.746	.309	13.424
Observations	8563	8563	8563	8563	8563	8563

Notes: Heterogeneity in estimated effects of the peer exits on outcomes. The interactions between group indicators and the number of peer wins are used as instruments for the exit variables. Panel A reports estimates by race / ethnicity and Panel B reports estimates by whether the average of the student's 4th grade math and reading test scores was above or below the median. Each column in each panel reports estimates from a single regression using interactions. Each cell reports a group-specific estimate for the effect of peer applicant exits from their neighborhood group on the arrest or incarceration outcome indicated by the column heading. Each estimate is conditional on interactions between expected wins and group indicators, group indicators, and CBG fixed effects. Standard errors are clustered at the CBG-Neighborhood School-Cohort level.

Table A13: Effect of Peer Exits on Arrest Outcomes (Girls)

	Pr(Arrest)	Pr(Violent)	Pr(Incarc.)	All Arrests	Violent Arrests	Days Incarc.
Panel A: Pooled						
N Exit	0.003	0.012**	0.002	0.000	0.018	-0.117
	(0.008)	(0.006)	(0.005)	(0.022)	(0.013)	(0.210)
Observations	7985	7985	7985	7985	7985	7985
Panel B: With controls, By Risk						
N Exit × HR	-0.010	0.015	-0.000	0.001	0.020	0.021
	(0.015)	(0.012)	(0.010)	(0.048)	(0.026)	(0.387)
N Exit × LR	0.014	0.006	0.003	-0.005	0.012	-0.242
	(0.011)	(0.006)	(0.005)	(0.020)	(0.011)	(0.311)
Dep Var Mean, HR	.103	.054	.041	.221	.098	1.001
Dep Var Mean, LR	.019	.006	.005	.042	.013	.313
Observations	7985	7985	7985	7985	7985	7985

Notes: Estimated effects of peer exits on outcomes in the sample of girls. Panel A reports estimates for the full sample. Each estimate in Panel A is for the effect of the number of applicants from their neighborhood group who attend a different school than their initially assigned neighborhood school in the year after the lottery on the arrest or incarceration outcome indicated by the column heading. The number of peer applicants who win their first choice in the lottery is used as an instrument for peer exits. Neighborhood groups include female students in the same cohort, initially assigned neighborhood school, and CBG. Each estimate is conditional on expected wins, school-cohort fixed effects, and group level lottery controls. Panel B reports the same set of estimates for the high- and low-risk students, based on the estimated probability of arrest. *N Exit* × *HR* shows estimates for the group with higher than median estimated risk, and *N Exit* × *LR* shows estimates for the group with lower than median estimated risk. These are estimated in a single regression using interaction terms. Panel B regressions also include interaction terms between risk indicators and expected wins, interactions between risk indicators and group level lottery controls, and a risk indicator. Standard errors are clustered at the CBG-Neighborhood School-Cohort level.

Table A14: Effects of Same CBG-School-Cohort Girl Wins on Boy Outcomes

	Pr(Arrest)	Pr(Violent)	Pr(Incarc.)	All Arrests	Violent Arrests	Days Incarc.
Panel A: Pooled						
N Exits, Girls	0.003	0.005	0.009	0.046	0.027	-0.255
	(0.009)	(0.007)	(0.008)	(0.057)	(0.023)	(1.485)
Observations	8127	8127	8127	8127	8127	8127
Panel B: By Risk						
N Exits, Girls × HR	0.009	0.014	0.021	0.112	0.055	0.216
	(0.016)	(0.014)	(0.014)	(0.102)	(0.040)	(2.714)
N Exits, Girls × LR	0.000	-0.001	0.002	0.018	0.009	0.242
	(0.011)	(0.007)	(0.008)	(0.048)	(0.020)	(0.874)
Dep Var Mean, HR	.203	.131	.129	.834	.346	15.208
Dep Var Mean, LR	.053	.021	.02	.125	.038	.865
Observations	8127	8127	8127	8127	8127	8127

Notes: Estimated effects of wins among same School-CBG-Cohort girls on outcomes in the sample of boys. Panel A reports estimates for the full sample. Each estimate in Panel A is for the effect of same-neighborhood girls who won their first choice in the lottery on the arrest or incarceration outcome indicated by the column heading. Neighborhood groups include students in the same cohort, initially assigned neighborhood school, and CBG. Each estimate is conditional on wins among boys in the same peer group, expected wins for boys and girls, and school-cohort fixed effects, and group level lottery controls. Panel B reports the same set of estimates for the high- and low-risk students, based on the estimated probability of arrest. *(N Wins, Girls* × *HR* shows estimates for the group with higher than median estimated risk, and *N Wins, Girls* × *LR* shows estimates for the group with lower than median estimated risk. These are estimated in a single regression using interaction terms. Panel B regressions also include interaction terms between risk indicators and expected wins, interactions between risk indicators and group level lottery controls, and a risk indicator. Standard errors are clustered at the CBG-Neighborhood School-Cohort level.

Table A15: Estimates by Group Size (Split within Risk Type)

	Pr(Arrest)	Pr(Violent)	Pr(Incarc.)	All Arrests	Violent Arrests	Days Incarc.
Panel A: Pooled						
N Exit × Large N	0.004	-0.005	0.007	0.014	-0.008	0.504
	(0.007)	(0.006)	(0.006)	(0.055)	(0.022)	(1.118)
N Exit × Small N	0.079	0.068*	0.038	0.516*	0.260**	6.860
	(0.050)	(0.037)	(0.033)	(0.288)	(0.122)	(5.186)
Dep Var Mean, Large N	.115	.064	.062	.39	.155	5.912
Dep Var Mean, Small N	.13	.081	.081	.524	.211	9.27
Observations	8563	8563	8563	8563	8563	8563
Panel B: By Group Size and Risk						
Large N × HR	-0.021	-0.020	-0.015	-0.124	-0.071	0.680
	(0.021)	(0.018)	(0.020)	(0.166)	(0.072)	(3.649)
Large N × LR	0.016**	0.003	0.018**	0.074**	0.021	0.513
	(0.008)	(0.005)	(0.007)	(0.035)	(0.014)	(0.491)
Small N × HR	0.073	0.093	0.039	0.856	0.445*	9.736
	(0.079)	(0.066)	(0.058)	(0.545)	(0.230)	(10.151)
Small N × LR	0.098*	0.054	0.049	0.243	0.111	4.934
	(0.054)	(0.036)	(0.034)	(0.213)	(0.087)	(4.415)
Mean, Large N, HR	.186	.116	.113	.707	.296	12.181
Mean, Large N, LR	.057	.022	.019	.13	.039	.782
Mean, Small N, HR	.232	.157	.157	1.024	.425	19.6
Mean, Small N, LR	.048	.02	.02	.121	.038	.963
Observations	8563	8563	8563	8563	8563	8563

Notes: Estimated effects of the peer exits in large vs. small neighborhood groups. *Large N* refers to those in groups that are larger than the median size within risk type, and *Small N* refers to those groups that are smaller than median size within risk type. Panel A reports estimates by group size, pooling by estimated risk. Each estimate in Panel A is for the effect of exits of applicants from their neighborhood group on the arrest or incarceration outcome indicated by the column heading. Neighborhood groups include male students in the same cohort, initially assigned neighborhood school, and CBG. Each estimate is conditional on the group size indicator and CBG fixed effects. Panel B reports the same set of estimates for the high- and low-risk students, based on the estimated probability of arrest. Rows with × *HR* show estimates for the group with higher than median estimated risk, and rows with × *LR* show estimates for the group with lower than median estimated risk. These are estimated in a single regression using interaction terms. Panel B regressions also include interaction terms between group indicators and expected wins, as well as group indicators. Standard errors are clustered at the CBG-Neighborhood School-Cohort level.

Table A16: Effect of Peer Wins Interacted with Elementary School Proxy

	Pr(Arrest)	Pr(Violent)	Pr(Incarc.)	All Arrests	Violent Arrests	Days Incarc.
Panel A: Interaction with Mean of Winners from Same Elem School						
N Wins × HR	-0.003	-0.002	0.002	-0.020	-0.010	0.446
	(0.012)	(0.010)	(0.010)	(0.091)	(0.036)	(2.192)
× Mn(SameElem.)	0.007	0.005	-0.009	0.141	0.066	1.808
	(0.021)	(0.017)	(0.017)	(0.129)	(0.061)	(3.579)
N Wins × LR	0.015*	0.007	0.021***	0.100**	0.036**	1.571**
	(0.008)	(0.006)	(0.006)	(0.044)	(0.018)	(0.738)
× Mn(SameElem.)	0.010	-0.003	-0.009	-0.056	-0.028	-1.574*
	(0.011)	(0.007)	(0.008)	(0.049)	(0.020)	(0.803)
Observations	8563	8563	8563	8563	8563	8563
Panel B: School-CBG-Cohort Fixed Effects						
N Wins						
× HR × Mn(SameElem.)	0.000	-0.011	-0.025	-0.010	-0.003	-0.386
	(0.024)	(0.020)	(0.020)	(0.114)	(0.056)	(3.833)
× LR × Mn(SameElem.)	0.034**	0.001	-0.000	0.004	0.002	0.627
	(0.014)	(0.012)	(0.011)	(0.067)	(0.029)	(2.058)
Observations	8417	8417	8417	8417	8417	8417

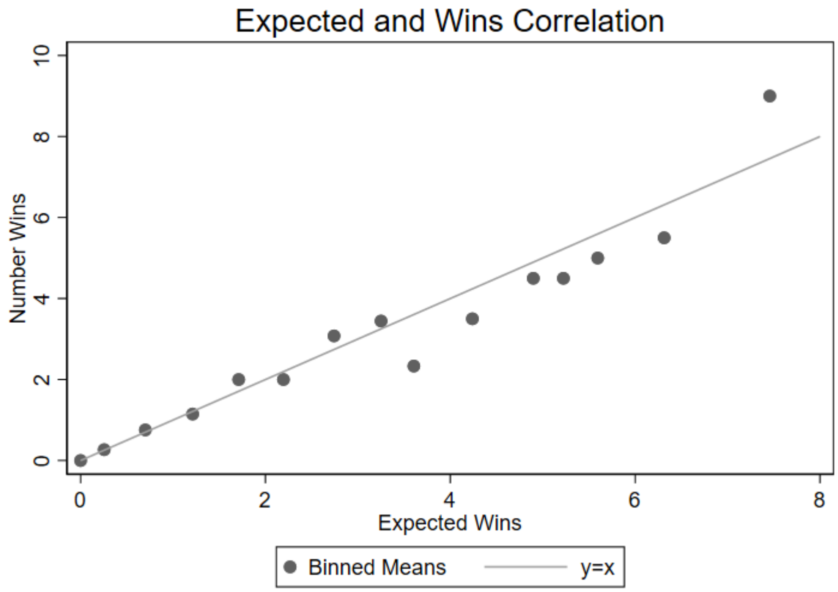
Notes: Estimated effect of wins on outcomes including interaction terms with the mean of winners from the same elementary school. Panel A includes the main effect, wins interacted with high- or low-risk, and two additional interaction terms. The interactions $\times Mn(Same\ Elem.)$ include wins from the group that the focal student attended school with in 5th grade. Panel A conditions on expected wins and CBG fixed effects. Panel B also includes school by cohort fixed effects. Panel C includes cohort by school by CBG fixed effects (rather than cohort, CBG, and School fixed effects separately), and separate estimates of interactions between wins and the fraction of winners who attended the same school in 5th grade for high- and low-risk non-applicants. Standard errors are clustered at the CBG-Neighborhood School-Cohort level.

Table A17: Estimates with Same Race/Ethnicity Interactions

	Pr(Arrest)	Pr(Violent)	Pr(Incarc.)	All Arrests	Violent Arrests	Days Incarc.
Panel A: Pooled						
N Exit	0.016	0.005	0.011	0.174*	0.031	4.366
	(0.014)	(0.012)	(0.011)	(0.104)	(0.038)	(2.735)
N Exit Same Race	-0.005	-0.001	0.000	-0.189	-0.013	-5.581
	(0.019)	(0.015)	(0.015)	(0.136)	(0.049)	(4.054)
Observations	8563	8563	8563	8563	8563	8563
Panel B: With Same Race/Ethnicity Interactions, by Non-Applicant Risk						
N Exit × HR	-0.024	-0.007	-0.035	0.263	0.003	10.794
	(0.035)	(0.026)	(0.035)	(0.243)	(0.089)	(8.222)
Same Race Exits × HR	0.040	0.016	0.062	-0.404	0.044	-16.125
	(0.055)	(0.043)	(0.052)	(0.401)	(0.149)	(14.018)
N Exit × LR	0.029*	0.007	0.028**	0.138**	0.035	1.650
	(0.016)	(0.011)	(0.012)	(0.064)	(0.030)	(1.140)
Same Race Exits × LR	-0.012	-0.000	-0.017	-0.081	-0.017	-0.788
	(0.016)	(0.011)	(0.011)	(0.065)	(0.033)	(1.081)
Observations	8563	8563	8563	8563	8563	8563

Notes: Estimated effects of the peer exits on outcomes and additional effect from alternate peer groups defined at the school-cohort-CBG-race/ethnicity level. IV estimates using number of peer wins as instrument. Panel A reports estimates across the sample, pooling by estimated risk. Each column in Panel A includes an estimate for the effect of applicant exits on the arrest or incarceration outcome indicated by the column heading, and another estimate for the proportion of students from their school-CBG-cohort-race/ethnicity group who won their first choice in the lottery. These are estimated in one regression. Each estimate is conditional on expected wins in each group and CBG fixed effects. Panel B reports the same set of estimates for the high- and low-risk students, based on the estimated probability of arrest. *N Exit* × *HR* shows main estimates for the group with higher than median estimated risk, and *N Exit* × *LR* shows estimates for the group with lower than median estimated risk. Each column in Panel B is estimated in a single regression using interaction terms. In Panel B, we also include interaction terms between risk indicators expected wins variables. Standard errors are clustered at the CBG-Neighborhood School-Cohort level.

Figure A1: Correlations Wins and Expected Wins



Notes: Binned averages of wins and expected wins with the line for y=x. 46% of the groups have expected wins = 0, i.e., not exposed. 98% have expected wins less than 3.

Appendix B

Estimating Lottery Win Probabilities and Expected Wins

We start with the sample of students who submitted at least one choice in the lottery over our sample period. The lottery proceeds in three rounds. In the first round, only the first choices of the applicants are considered. We focus on the first-round applications in our main analysis that meet the following criteria:

1. Submitted at least one choice in the lottery.
2. Their first choice was to a program that is not in their designated home school.
3. Their first-choice program did not include subjective criteria in the admission process.[25]
4. They did not receive a sibling placement, or other apparent magnet continuation placement.[26]

The resulting sample includes students for whom winning the lottery means that they will be assigned to a different school than their residentially zoned school based on their residence at the time of the lottery for 6th grade admissions, which takes place during their 5th grade school year. In some cases, students may apply to a program within their neighborhood school, but those applicants do not meet criteria (2), so they are not included in our main sample.

We group applicants based on application choice, year of application, and observable lottery criteria which include whether the student's neighborhood school or magnet continuation school is a Title I choice school (0 or 1), whether the student is economically disadvantaged (0 or 1), and whether the student was at grade level in reading on the fourth-grade standardized reading exam (0, 1, or missing). We construct the predicted win probabilities, $\hat{P}_{ibst}$, using the proportion of applicants within the application choice, year, Title I School Status, economic disadvantage status, and reading level groups who won the first choice in the lottery. The lottery sample includes the set of students with $\hat{P}_{ibst}$ between zero and one who also met the entrance requirements for their first-choice program. We use the predicted wins to construct expected wins by summing win probabilities within CBG-School-Cohort to obtain an expected number of wins.

Constructing Group Level Lottery-Related Controls

The focal students in this analysis are non-applicants who have a win probability of zero. The students who contribute to the expected wins are lottery applicants who meet the lottery sample criteria. The sample generating the expected wins and the estimation sample are distinct. In addition, students who specify a lottery choice, but do not meet the criteria

[25] Arts programs require an audition and leadership programs which require an interview. Both include admission criteria that are subjective and not observable.

[26] We filter out applicants that likely receive automatic placements which is inferred from the data and CMS documentation.

outlined above, do not contribute to either sample. Effectively, we can view their assignments as non-random, and they would not contribute to identification. However, we do use this subsample to construct some of the lottery-specific conditioning variables (used in Table 7). These additional conditioning variables account for sibling slots, non-first choices, and other applicants not meeting the criteria for random lottery assignment, which may impact student expectations regarding lottery win probabilities and peer movement. However, conditioning on these additional lottery-related outcomes is inconsequential to our results (see Panel C of Table 7).

The additional lottery-specific conditioning variables are:

1. *Number of students who won their second or third choice in the lottery*

The number of students who won their second or third choice in the lottery is constructed using the same sample of lottery applicants in oversubscribed lotteries who did not receive automatic placements and met the stated requirements for their program of application. We construct the total number of students in the group who won their second or third choice as the sum of an indicator for winning the second or third choice within the CBG-School-Cohort group.

2. *Number of other students submitting lottery applications*

As outlined in the previous section, we limit the lottery applicant sample to those who met several criteria, i.e., they are in an oversubscribed lottery and there is some random assignment that can be used for estimation. The remaining lottery applicants, e.g., who receive a sibling placement or apply to an undersubscribed lottery, are not used in either of the estimation samples. We construct two conditioning variables from this sample, the first of which is the number of applicants in the CBG-School-Cohort group who do not meet the criteria for the estimation sample.

3. *Number of other students assigned to their lottery choice*

Using the same set of students who applied but did not meet the criteria for the lottery estimation sample, we construct a variable for the number of applicants who were assigned to their lottery choice. This would include, for example, students who received sibling placements, lottery winners who applied to arts or leadership programs, or applicants to undersubscribed lotteries.

Estimating Arrest Risk

We start with the full sample of students in the district from the three cohorts that we use (Column 1 of Table 1). We predict the probability of arrest conditional on a set of individual characteristics, as well as school and CBG level means. The dependent variable is an indicator for any arrest from ages 16 to 22. The explanatory variables are indicators for race/ethnicity, economic disadvantage, 4th grade math and reading scores, dummy variables for missing test scores, age, a dummy for missing age, CBG level means of each race/ethnicity indicator, economic disadvantage, math and reading test scores, and school attendance zone level means of each race/ethnicity indicator, economic disadvantage, math and reading test scores. We also include individual indicators for lottery applicants (based on our lottery sample definition) and those who won their first choice in the lottery, as well as group level measures for the number of peer applicants and the number of peer lottery winners. We estimate the parameters using logistic regression and predict the individual probability of arrest, $\hat{R}_{ibst}$, omitting the contributions from the indicator for winning the lottery and the measure for the number of peer lottery winners. In our main heterogeneity analyses, e.g., Panel B of Table 3, we split the sample into groups with high and low arrest risk using the sample median $\hat{R}_{ibst}$ to create risk indicators.

Table B1: Arrest Prediction

	Boys	Girls
Applicant	-0.120	-0.279*
	(0.103)	(0.146)
Won First Choice	-0.181	-0.0902
	(0.174)	(0.246)
N of Peer Applicants	-0.0366**	0.00183
	(0.0178)	(0.0217)
N of Peer Wins	0.0434	-0.0131
	(0.0363)	(0.0466)
White	-0.0159	-0.235
	(0.137)	(0.194)
Black	0.590***	0.381**
	(0.120)	(0.160)
Hispanic	-0.225	-0.951***
	(0.140)	(0.208)
Ec. Disadvantage	0.669***	0.804***
	(0.0750)	(0.112)
Math EOG, 4	-0.213***	-0.142**
	(0.0435)	(0.0628)
Read EOG, 4	-0.0909**	-0.180***
	(0.0420)	(0.0614)
Math Missing, 4	-0.436	0.393
	(0.267)	(0.742)
Read Missing, 4	0.281	-0.638
	(0.257)	(0.736)
Age	0.0246***	0.0235***
	(0.00434)	(0.00648)
Age Missing = 1	-0.177	0.335
	(0.213)	(0.324)
Nbhd School Means		
White	0.00705	-0.542
	(1.445)	(2.160)
Black	-0.498	-0.658
	(1.487)	(2.161)
Hispanic	-0.813	-0.278
	(1.615)	(2.354)
Ec. Disad.	0.715	-0.0135
	(0.513)	(0.786)
Math, 4	1.205*	1.397
	(0.730)	(1.301)
Read, 4	-1.292*	-1.512
	(0.779)	(1.514)
CBG Means		
White	0.670	1.048
	(0.665)	(0.994)
Black	1.199**	1.021
	(0.596)	(0.899)
Hispanic	0.663	0.965
	(0.674)	(1.008)
Ec. Disad.	0.433	0.299
	(0.361)	(0.525)
Math, 4	0.670**	-0.844*
	(0.319)	(0.473)
Read, 4	-0.502	0.383
	(0.333)	(0.479)
Observations	13,491	13,039

Notes: Arrest prediction estimates from logistic regression with indicator for any arrest age 16-22.

Appendix C

Consistency using the Recentered Treatment Variable

To be consistent with the main specification in Borusyak and Hull (2023), we focus on recentering rather than condition on the expected value in this subsection. Specifically, we define the recentered treatment shock, number of applicant wins W_{bst}, experienced by non-applicants j for a given CBG (b)-School (s)-Cohort (t) as

$$\tilde{W}_{bst} = W_{bst} - E[W_{bst}|\Theta_{bst}] \tag{17}$$

where W_{bst} is the number of wins among male applicants in CBG b, neighborhood school attendance zone s and cohort t, and Θ_{bst} is a vector of the lottery schools and priority groups of all applicants.

The model of non-applicant outcomes (y_{jbst}) using the recentered treatment as an instrument for applicant departures T_{bst} is

$$T_{bst} = \gamma_1 \tilde{W}_{bst} + \mu_{bst} \tag{18}$$

$$y_{jbst} = \beta_1 T_{bst} + \epsilon_{jbst}. \tag{19}$$

While recentering assures that

$$E\left[\frac{1}{N}\sum_{j=1}^{N} \tilde{W}_{bst}\epsilon_{ibst}\right] = 0, \tag{20}$$

$$Var\left[\frac{1}{N}\sum_{j=1}^{N} \tilde{W}_{bst}\epsilon_{ibst}\right] = \frac{1}{N^2}\sum_{j,k} E[(\tilde{W}_{bst})_j(\tilde{W}_{bst})_k(\epsilon_{ibst})_j(\epsilon_{ibst})_k] \tag{21}$$

where the subscript pair j, k implies all permutations of non-applicant pairs and the subscripts on $\tilde{W}_{bst}$ identify the recentered treatment variable associated with the cluster to which individual j or k belongs, respectively.

Notably, this variance expression may not converge to zero as the sample becomes large, because of correlations between observations in either $\tilde{X}_{bst}$ or in μ_{ibst}. In Proposition S2, Borusyak and Hull (2023) show that the above expression converges to zero if

$$E\left[\frac{1}{N^2}\sum_{j,k} Cov\left[(\tilde{W}_{bst})_j(\tilde{W}_{bst})_k|(\Theta_{bst})_j,(\Theta_{bst})_k\right]\right] \to 0. \tag{22}$$

To obtain asymptotic consistency, we assume that the number of clusters, L (groups in CBG, school, and cohort, bst), grows more slowly than the sample size, N, and divide the expression into two parts: pairs of observations that belong to the same cluster, l, and observations that do not.

$$E\left[\frac{1}{N^2}\sum_{j,k}Cov\left[(\tilde{W}_{bst})_j(\tilde{W}_{bst})_k|(\Theta_{bst})_j,(\Theta_{bst})_k\right]\right]=E\left[\frac{1}{N}\sum_{j=1}^{L}\frac{N_l}{N}Var\left[(\tilde{W}_{bst})_j|(\Theta_{bst})_j\right]\right]+$$
$$E\left[\frac{1}{N^2}\sum_{j\in n_l,k\in n_m,l\neq m}Cov\left[(\tilde{W}_{bst})_j(\tilde{W}_{bst})_k|(\Theta_{bst})_j,(\Theta_{bst})_k\right]\right]. \tag{23}$$

where N_l is the number of observations within cluster, l, and n_l represents the set of observations in the cluster.

For the first expression, the cluster covariance contributions, the variance of $\tilde{X}_{bst}$ is finite by inspection. Further, the within-summation weights are equal to the ratio of cluster size to sample size, i.e., $\frac{N_l}{N}$, so the weights sum to one, when summing over the number of clusters. Therefore, the summation in the first expression is finite, and converges to zero with increasing N.

The second expression, across cluster covariances, is zero based on the equal treatment of equals aspect of lottery randomization. Because the random vector of draws for a given cluster are orthogonal to the random vector of draws for any other cluster, the recentered treatment variables are orthogonal across clusters. This conclusion follows from the more general proof of Lemma S2(ii) in Borusyak and Hull (2023) that the covariance condition is satisfied if the portion of the sample for which the random shocks are correlated grows more slowly than the sample size.

The Effect of Aggregation on First and Second Stage Estimates

The purpose of this section is to illustrate how the first stage estimates can be unstable to the inclusion of high dimensional fixed effects, even though the risk control assures conditional orthogonality to the error in the outcome equation, and empirically the treatment shock conditional on the risk control passes balance on observables.

We start with a relatively general IV model where the aggregate treatment shock W_{lo} and endogenous treatment T_{lo} is the result of many individuals i experiencing and responding to an exogenous treatment shock, w_{ilo} and t_{ilo} in location l that belongs to a larger cluster o. In our application, i represents lottery applicants, the treatment shock w_{ilo} is winning the lottery, individual treatment or take-up t_{ilo} is not attending the initially assigned neighborhood school, l represents block group by attendance zone by cohort clusters, and o could represent a broader neighborhood, like block group, or a broader group of cohort applicants such as across the entire attendance zone.

We specify aggregate treatment shock W_{lo} and treatment T_{lo} as simply the sum (or potentially the mean) of the individual treatment shocks w_{ilo} and treatment take-up decisions t_{ilo} where γ_1 defines the linear relationship between the individual shock and treatment. The standard Abdulkadiroğlu et al. (2017) (AEA) and Borusyak and Hull (2023) (BH) approach to handling endogeneity of the aggregate treatment shock is add a a linear control for the expectation of the shock. However, to allow for spillover effects of treatment shocks across individuals, we also assume that aggregate treatment in lo is affected by a general function f_{lo} of shocks that correlate with the clusters o, either non-linear functions of the individual treatment shocks within lo or even functions of a broader set of treatment shocks in o.

$$T_{lo} = \gamma_1 W_{lo} + \gamma_2 E[W_{lo}] + f_{lo}(\Omega_W) + \mu_{lo}$$

Where, in general, Ω_W can contain all treatment shocks in o for any lo. f_{lo} will also be correlated with T_{lo} even after conditioning on $E[W_{lo}]$ because f_{lo} contains the actual individual treatment shocks (both the random and systematic components) in lo. With finite observations in l and finite groups l in cluster o, the random component of the treatment shocks cannot be assumed away asymptotically.

We assume that the moment condition in BH for the IV estimates on the second stage error ϵ_{jk} is satisfied, and we specify the a model for aggregate outcomes Y_{lo} as

$$Y_{lo} = \beta_1 T_{lo} + \beta_2 E[W_{lo}] + \epsilon_{lo}$$

The first stage will deviate from the direct effect of the treatment shock experienced by an individual on their treatment take-up, γ_1, because the function f_{lo} is embedded in the error and the expectation of this function depends upon W_{lo}. For convenience, we will refer to this deviation as "bias."

$$E[f_{lo}(\Omega_W)|W_{lo}, E[W_{lo}]] = \phi_1 W_{lo} + \phi_2 E[W_{lo}]$$

Such that ϕ_1 enters as an additive bias in the estimate on W_{lo} in the equation for treatment take-up.

We write the expectation of the reduced form model by replacing T_{lo} with the first stage equation in the model for Y_{lo}. Taking the conditional expectation, we find that

$$E[Y_{lo}|W_{lo}, E[W_{lo}]] = \beta_1(\gamma_1 + \phi_1)T_{lo} + (\beta_2 + \beta_1(\gamma_2 + \phi_2))E[W_{lo}]$$

The first thing to realize is that given the assumption on the moment condition in the second stage the IV estimates are unbiased. The estimate on W_{lo} for T_{lo} is $(\gamma_1 + \phi_1)$ and the reduced form estimate for y_{lo} is $\beta_1(\gamma_1 + \phi_1)$, so the bias in the reduced form is precisely canceled out by the bias in the first stage.

Second, as shown by Peter Hull in our correspondence, this first stage and reduced form bias will be unaffected by the inclusion of controls Z_{lo} that are determined in nature if the sample used to estimate those controls is asymptotic, as in the case of standard demographic observables of the individuals in each group or subsample lo or observable attributes of the groups themselves, which might typically be used in a balancing test. The omission of Z_{lo} leads to a bias in the models above of the form

$$E[Z_{lo}|W_{lo}, E[W_{lo}]] = \phi_3 W_{lo} + \phi_4 E[W_{lo}]$$

So the inclusion of Z_{jk} should move estimates by the amount of the bias on the estimate for W_{lo}. However, given the identification assumptions in BH, $\phi_3 = 0$ because after conditioning on the expectation the only remaining variation in W_{jk} is the random shocks, which are uncorrelated with variables determined in nature. The conditional asymptotic independence of Z_{lo} and W_{lo} is the reason that models like these can pass balance even if the first stage is biased.

However, the implication from including high dimensional fixed effects is very different. Specifically, our sample is only asymptotic in the number of clusters lo in a non-fixed effects model and asymptotic in the number of clusters o in a model including fixed effects for o. Therefore, the fixed effect estimates are based on a finite sample and capture information on the actual treatment shocks W_{lo}. As a result, $\phi_3 \neq 0$ when Z_o is a vector of fixed effects over o, and the first stage estimates can change when high dimensional fixed effects are added as controls.

Next, we present two simple intuitive examples where, like in the more general model above, the moment condition from BH when applied to the first stage unobservable will not hold in a model controlling for a linear function of the aggregation of the shocks and its expectation. Notably, since these examples below arise from the aggregation of take-up for individuals who are exposed to the treatment shock and are making take-up decisions, the second stage unobservable for a sample of individuals who are not making these take-up decisions should still meet the orthogonality conditions in BH after conditioning on the expectation of the aggregate treatment shock.

Aggregate effects of Shocks on Take-up

Let's start with an individual i's take-up of binary treatment, $t_{ilo} = 0$ or 1, based on binary treatment shocks, $w_{ilo} = 0$ or 1, and allow take-up to depend upon both the shock that the individual experiences, as well as the aggregate pattern of shocks in the group W_{lo} where we measure aggregate patterns of take-up and shocks as the sum over all individuals potentially exposed to the shock. We also assume that take-up behavior varies by whether the individual experiences the shock $w_{lo} = 1$ or not as denoted below by the w index on the model parameters. In our application, the parameters γ_{11} and γ_{21} describe the decision to not attend the assigned neighborhood school t_{ilo} for lottery winners, and γ_{10} and γ_{20} describe the decision process for lottery losers.

$$t_{ilo} = \gamma_{1w_{ilo}} + \gamma_{2w_{ilo}} W_{lo} + \mu_{ilo}$$

Then, given the spillover effects, we can write the aggregate model for T_{lo} by summing over all individuals in lo where the sum for observations where $w_{ilo} = 1$ is just the model multiplied by W_{lo} and the sum for observations where $W_{ilo} = 0$ is just the model multiplied by $(N_{lo} - W_{lo})$, where N_{lo} is the number of individuals potentially exposed to the treatment shock (in our example lottery applicants) in group lo.

$$T_{lo} = \gamma_{10} N_{jk} + ((\gamma_{11} - \gamma_{10}) + \gamma_{20} N_{jk}) W_{jk} + (\gamma_{21} - \gamma_{20}) {W_{lo}}^2 + \mu_{lo}$$

where μ_{lo} is simply the sum of μ_{ijk} over all observations in lo. Taking the conditional expectation of the quadratic term conditional on W_{lo} and $E[W_{lo}]$ and using a linear projection yields

$$E[{W_{lo}}^2 | W_{lo}, E[W_{lo}]] = \phi_1 W_{lo} + \phi_2 E[W_{lo}]$$

where $\phi_1 \neq 0$ due to the non-linear effects of actual wins, which cannot be captured by the linear control for W_{jk}. If clusters o vary in the $E[W_{lo}]$, then clusters with higher expectations will tend to have more wins, higher W_{lo}, leading to this cluster operating on a different portion of the quadratic function. Therefore, conditioning on fixed effects over o in finite samples captures information on actual wins, absorbs some of the variation in the quadratic term, and so changes the value of the bias ϕ_1 in the first stage and reduced form estimates.

Heterogeneous Effects of the Shock over Unobserved Attributes

The next type of bias that can arise, creating instability in the first stage, comes from heterogeneity in the effect of winning on compliance. Specifically, an individual's unobservables are made up of many attributes X_{ilo} and the influence of these attributes on whether one takes up treatment (does not attend the assigned school) could vary dramatically based on the treatment shock, e.g., vary between lottery winners and losers. For example,

1. Very strong preference for the specific choice school selected. Presumably, this would have a large impact on leaving the home school if you win, but minimal impact on leaving the home school if you lose the lottery and do not have access to that school.

2. Substantial financial resources. This may not matter that much if you win in terms of not attending the home school because you can simply exercise the option to attend the lottery choice school. However, this could matter a lot for whether you leave the home school if you lose because one of the key ways of leaving if you lose is to move, which takes financial resources.

The resulting estimation model of individual take-up is

$$t_{ilo} = \gamma_1 w_{ilo} + \gamma_2 Pr[w_{ilo}] + (\alpha_1 X_{ilo}) w_{ilo} + (\alpha_2 X_{ilo})(1 - w_{ilo}) + \mu_{ilo}$$

where α_1 and α_2 capture the effect of a vector of unobservable attributes X_{ilo} on take-up for those who experience the shock $w_{ilo} = 1$ and those who do not, respectively, and μ_{ilo} represents a random stochastic shock remaining after the removal of these unobserved attributes.

Now, we aggregate t_{ilo} over all individuals at risk of experiencing the treatment shock in lo to obtain aggregate treatment.

$$T_{lo} = \gamma_1 W_{lo} + \gamma_2 E[W_{lo}] + (\alpha_2 \bar{X}_{lo}) N_{lo} + (\alpha_1 \bar{X}_{jk} - \alpha_2 \bar{X}_{lo}) W_{lo} + \mu_{ilo}$$

where $\bar{X}_{lo}$ is the mean of the unobserved attributes. The conditional expectation of these terms, which are part of the unobservable, can be written as

$$E[(\alpha_1 \bar{X}_{lo}) | W_{lo}, E[W_{lo}]] = \phi_1 E[W_{lo}]$$

$$E[(\alpha_2 \bar{X}_{lo}) | W_{lo}, E[W_{lo}]] = \phi_2 E[W_{lo}]$$

because $\bar{X}_{lo}$ is determined in nature and the correlation with W_{lo} will be zero conditional on $E[W_{lo}]$. Taking the conditional expectation of T_{lo} yields

$$E[T_{lo}|W_{lo}, E[W_{lo}]] = \gamma_1 W_{lo} + (\gamma_2 + \phi_2 N_{lo})E[W_{lo}] + (\phi_1 - \phi_2)W_{lo}E[W_{lo}] + \mu_{lo}$$

Again the conditional expectation depends upon a non-linear function of W_{lo}, in this case $W_{lo}E[W_{lo}]$, leading to a bias in the estimate on W_{lo}. As above, fixed effects in o when estimated with finite samples for each value of o will contain information on the treatment shocks in lo, and so conditioning on these fixed effects can change the bias in the first stage and in the reduced form estimates.

Empirical Evidence of these Biases in the First Stage

While we do not claim to know the correct underlying model of lottery winner and loser take-up, i.e., the model that individuals follow when deciding whether to depart from the currently assigned school or not, we have estimated empirical models that allow for the non-linear terms predicted by the models presented above. Specifically, in column 2 of Table C1, we include a control for the interaction of the number of wins minus sample mean number of wins with the number of expected wins, which captures the interaction between number of wins and expected number of wins while allowing the coefficient on number of wins alone to continue capturing an estimate of the effect of the number of wins at the sample mean. Similarly, in column 3, we include controls for the product of the number wins times the number of wins minus the sample mean of the number of wins, and in column 4 we include both controls.

The addition of the interaction of number of wins with expected number of wins (associated with heterogeneous effects of unobservables) is significant and negative, consistent with unobservables that are associated with a high expectation of winning the lottery being correlated with a high propensity to take-up home school departure if losing the lottery. On the other hand, in the fourth column after controlling for both the interaction with expected wins and the square of number of wins, the estimate on the square of expected wins is positive, consistent with a larger effect of the total number of wins on take-up for the lottery winners, relative to lottery losers. This behavior could occur if students in the same neighborhood, both same attendance zone and cohort, are aware that the others have applied and consider those wins as well as their own, as opposed to making decisions individually. Therefore, when many of these students win the lottery, they are all more likely to take-up their offers.

Regardless of the interpretation, the inclusion of these non-linear controls leads to substantial movement in the first stage estimates, even though we provide convincing evidence of passing balance tests conditional on the risk control.

Table C1: First Stage Estimates (With Added Controls)

	N Applicants Attend Different School			
Wins	0.469***	0.657***	0.588***	0.411**
	(0.185)	(0.170)	(0.123)	(0.171)
E[Wins]	1.197***	1.367***	1.212***	1.546***
	(0.223)	(0.248)	(0.236)	(0.281)
(Wins - mean)*E[Wins]		-0.109***		-0.266***
		(0.033)		(0.087)
(Wins - mean)*Wins			-0.037	0.161**
			(0.047)	(0.078)
Observations	8,568	8,568	8,568	8,568

Notes: Estimated first stage equations with additional controls. Outcome is the number of peer applicants who attend a different school in the year after the lottery. Each includes the control for the expected number of wins at the group level. Column 2 also includes the interaction between the demeaned number of wins and the expected number of wins. Column 3 includes an interaction between the demeaned number of wins and the number of wins. Column 4 includes both of the interactions added in Columns 2 and 3. Neighborhood groups include male students in the same cohort, initially assigned neighborhood school, and CBG. Standard errors are clustered at the CBG-Neighborhood School-Cohort level.