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Does School Choice Leave Behind Future Criminals?

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Abstract

School choice lotteries are an important tool for allocating access to high-quality and oversubscribed public schools. While prior evidence suggests that winning a school lottery decreases adult criminality, there is little evidence for how school choice lotteries impact non-lottery students who are left behind at their neighborhood school. 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 boys are more likely to be arrested as adults when applicants from their neighborhood win the school choice lottery. These effects are concentrated among boys who are at low risk of being arrested based on observables. Finally, we confirm evidence in the literature that students who win the lottery decrease adult criminality but show that after accounting for the negative impact on the students who forego the lottery, lotteries increase overall arrests and days incarcerated for young men.

Key Words: School Choice Lotteries, Students Left Behind, Arrest, Crime, Middle School, Neighborhood Effects, Peers

JEL Codes: I24, I28, K42, R23

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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, Gleason & Clark, 2012). School choice also has the potential to delink residence from schools which may address recent social trends of increasing residential segregation (Schwartz, Voicu & Mertens Horn, 2014; Hess, 2021). One of the byproducts of school choice is the oversubscription to high quality and specialized schools for which school choice lotteries are used to assign limited classroom space to students.

Using the random assignment mechanism from school choice lotteries, several scholars (Abdulkadiroglu, 2011; Abdulkadiroglu, Pathak, & Walters, 2018; Cullen, Jacob, & Levitt, 2006; Hastings, Kane, & Staiger, 2006; Hastings & Weinstein, 2008; Deming et al., 2014; Muralidharan & Sundararaman, 2015; Mills & Wolf 2017) estimate effects winning a school choice lottery on later end-of-grade exams and other academic outcomes, finding mixed evidence. In contrast, scholars consistently find benefits in terms of non-academic outcomes with lower self-reported disciplinary issues, arrest, and incarceration for lottery winners (Cullen, Jacob, & Levitt, 2006; Deming 2011). However, we have a limited sense of how the kids left behind are affected by winners opting out of their neighborhood school. Muralidharan & Sundararaman (2015) offer some insight into the potential spillovers to non-school choice students through their examination of a lottery that provides private school vouchers in India, finding limited benefits in test scores to winners with no adverse effects on non-lottery participants in the same village.[1]

[1] Muralidharan & Sundararaman (2015) provide well-identified field experimental results, but the US public school context and our focus on behavioral outcomes within narrowly defined peer groups likely generates quite different dynamics (e.g., type of students who apply to the lottery and the relationships between lottery applicants and those left behind).

In the study presented here, we examine the impact of school choice lotteries on the applicant's neighborhood peers, as well as the direct effects on lottery winners. Thus, we provide novel evidence on the adult criminality of students who experience the loss of a lottery winner in their neighborhood middle school. We then combine the indirect effects (on peers left behind) with the direct effects (on lottery winners) to provide a more comprehensive assessment of the overall impact of school choice lotteries on young adult criminal justice outcomes.

While prior literature suggests winning a lottery decreases adult criminality, the expected effects on non-applicants who are left behind is less clear. If lottery winners are positive school peers and/or their families offer positive parental inputs to neighborhood schools, then kids left behind may be worse off in terms of criminality. Furthermore, the relationship between lottery winners and non-participants may be even more complicated by the behavioral response of parents and the sorting of teachers to high-achieving students (Pop-Eleches & Urquiola, 2013). Existing literature on the role of positive peer effects on youth criminality is very limited and primarily based on lottery winners moving to schools with positive peer attributes (e.g. Deming, 2011; Cullen, Jacob, & Levitt, 2006). Alternatively, a number of peer effects papers (e.g. Glaeser, Sacerdote & Scheinkman 1996; Bayer, Hjalmarsson & Pozen, 2009; Billings, Deming, & Ross, 2019; Damm & Gorinas, 2020; Kim & Fletcher, 2018; Billings & Hoekstra, 2022) highlight the negative influence of peers on adult crime. Therefore, if lottery winners are negative or neutral peers, then students who are left behind may be less likely to engage in future criminal activity.

To isolate the effects on kids left behind, we exploit prior work indicating that neighborhood effects on youth crime 1. operate over a narrow spatial range, 2. are tied to similar-aged students and 3. appear strongest among students who attended the same middle school (Billings, Deming, and Ross, 2019). We focus our analysis on a sample of three cohorts of male

5th grade students in Charlotte, NC in the 2005-2006 to 2007-2008 school years. The geography and cohorts included in our sample are dictated by the use of student level lottery and non-applicant administrative data (North Carolina Education Data Research Center) matched to arrest and incarceration data (Mecklenburg County Sheriff's Office and NC Dept of Public Safety). Our primary estimates are for the effects of losing peers through the lottery on a student's adult (age 16-22) arrest and incarceration. Our primary estimation sample is restricted to students who did not apply to the school lottery and were initially assigned to their neighborhood school in 6th grade.

We estimate the effect of peers winning a school choice lottery using the number of 5th grade applicants in the same cohort and same neighborhood (i.e., same Census Block Group (CBG) based on 5th grade residential location and same neighborhood middle school zone) who won their first choice in the lottery, divided by the total number of other 5th grade students in this cohort and neighborhood. To isolate the random portion of the lottery assignment, we condition on the expected share of winners in a neighborhood.[2] We confirm that lottery winners are substantially more likely to attend a non-neighborhood school than lottery losers, even though lottery losers are more likely to move to a different neighborhood.[3]

Our estimation strategy allows us to isolate the effects of changes in the school attendance patterns of lottery winners that occur simply due to chance, and not due to variation in lottery priorities or application rates in each neighborhood. Looking within assigned middle school, we find that male students whose immediate, same grade neighbors win the lottery are more likely to

[2] Our estimation strategy is similar in concept to a control function approach (Wooldridge, 2015). We argue that the share of lottery winners from a CBG-School-Cohort is exogenous to adult crime once we condition on the expected win share. The expected win share captures nonrandom factors such as application choices, neighborhood, year of application as well as characteristics related to lottery priorities (e.g., Title I choice status, economic disadvantage) that determine win probabilities. We also include fixed effects for CBG, Middle School attendance zone and cohort to address spatial and temporal impacts on expectations about win shares.

[3] Consistent with Bibler & Billings (2020), we show that lottery losers are more likely to change residences to access higher-quality schools in different neighborhoods, but this effect is quite small relative to the compliance of lottery winners with their first choice.

engage in criminal behavior at ages 16-22. A change from no winning peers to the average proportion of winners in a neighborhood (4.4% of same grade male students) implies a 0.04 increase in the number of arrests from ages 16-22, which is an 8.3% increase over the sample average. The effects are more precisely estimated and considerably larger for students with below median arrest risk. Among those with low predicted risk of future arrest, a change from no winners to the average win share leads to a 24.7% increase in the probability of arrest, 59.5% more violent arrests and 84.6% more days incarcerated.[4]

Lottery applicants have substantially higher test scores than non-applicants, and thus the loss of these high-achieving, neighborhood peers from the school environment appears to negatively affect student's later life outcomes, especially students at low a priori risk of arrest who may have been more likely to associate with high-achieving peers. Further, we show that the negative effects of lost peers start to emerge in the short run by documenting effects on disciplinary outcomes starting in middle school. These results are consistent with Bacher-Hicks, Billings & Deming (2019) who find that school discipline problems contribute to future arrest and incarceration as an adult.[5] We also show that the effects on arrest persist and increase in absolute magnitude after age 19 when in most cases the youth are no longer in high school. The persistence of these effects contrasts with findings on high school peer effects on academic outcomes by Bifulco, Fletcher & Ross (2011), which are shown to fade over time (Bifulco et al. 2014). Together these results support the importance of peer influences that are often formed and solidified in middle school and show that the loss of positive role models and friendships may lead to an increase in anti-social behavior that lasts beyond participation in K-12 education.

[4] While the percentages changes are large for the low-risk subsample, absolute changes are more modest since the group affected is at lower risk of arrest based on observables.

[5] Also, Sorensen, Bushway & Gifford (2022); and Fabelo et al. (2011) show that school discipline has a negative impact on several academic and behavioral outcomes.

Given that our main specification controls explicitly for the likelihood of peers winning the lottery, our identifying variation is based on the random selection of winners from the population of applicants from a given neighborhood to a first-choice school. To confirm the quasi-random nature of this variation, we highlight that conditional on the expected lottery win share, students exposed in their neighborhood to lottery winners and losers are similar on observable student attributes. Furthermore, we show that using future win shares for the neighborhood produces insignificant estimates that are substantially smaller in magnitude than our main estimates. This falsification test limits concerns that specific neighborhood trends may correlate with unexpected win probabilities.

Since lottery applicants tend to be positively selected on academics, the finding that most of our effects are concentrated among low crime risk (and higher achievement) students is consistent with peer effects that operate between similar students (Billings, Deming & Ross (2019), Fletcher, Ross & Zhang (2020)). Therefore, these findings speak to a broader literature on school segregation and the criminal activity among youth; most notably, Billings, Deming & Rockoff (2014), Weiner, Lutz & Ludwig (2009) and Johnson (2011) who show that racial segregation contributes to African-American youth involvement in the criminal justice system. However, unlike Billings, Deming & Ross (2019) and Fletcher, Ross & Zhang (2020), we do not find any evidence that the impact of being left behind by lottery winners has larger effects when the winner and the students left behind are the same race.

To examine the role of benefits to lottery winners, we estimate the direct effects of winning the lottery on adult criminality in the sample of lottery applicants to oversubscribed schools. This analysis is similar to Deming (2011) who also estimates the effects of winning the lottery on criminality in Mecklenburg County, but our sample time period and even the mix of schools are different given our focus on more recent cohorts. We confirm Deming's finding that winning the lottery leads to substantial declines in both number of arrests and days incarcerated for students in the top quintile of predicted arrest risk, but also show that winning the lottery reduced criminality among students at median and below median risk of arrest in our sample.

Taken together, our results provide insight into the net effects of lotteries on young adult criminality. Specifically, we use the population of students in neighborhoods that are exposed to lottery applicants to calculate the number of additional arrests and incarceration spells accumulated by non-applicants left behind as well as the predicted reductions in arrests and incarceration for lottery winners. We find that the current constrained school choice system generated benefits to winners of at least 72 fewer arrests and 2,497 fewer days incarcerated and generated costs to non-applicants left behind of at least 100 more arrests and 5,434 more days incarcerated. In aggregate, we estimate at least a 0.6% net increase in total arrests and 3.9% more days incarcerated due to the lottery application process for our three cohorts of 5th grade students in our estimation sample (n=10,032). These net negative effects could be an understatement if lottery winners have broader effects on students assigned to the same middle school beyond peers residing in their local neighborhood. In the end, our net effects are partial equilibrium in nature and thus cannot account for student sorting that would occur in the absence of school choice.

While school choice in the U.S. has grown dramatically over the last few years and many papers have examined the impacts on lottery winners, the literature on the broader impacts of school choice is quite modest in size. Our results of worse net outcomes are 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 quality is fixed. However, these results run contrary to several existing empirical papers that study the broader impacts of school choice on academic outcomes outside of the lottery setting. Gilraine, Petronijevic, & Singleton (2021) find positive effects in North Carolina and Mumma (2020) finds no effects in Massachusetts and North Carolina for students who attend schools near charter school openings. In Israel, Lavy (2010, 2021) documents cognitive and behavioral benefits for children in school districts that adopt or expand school choice, and Hsieh and Urquiola (2006) in Chile find little to no aggregate impact from expanded school choice.[6]

These findings are especially important because school choice has proven popular among local voters and has helped to reduce the rates at which high-quality peers leave poorer, more disadvantaged school districts. More targeted school choice programs that promote school choice among lower-achieving students might change the type of selection into choice documented here as well as by Hastings, Kane, & Staiger (2006), Burgess et al. (2015) and Barseghyan et al. (2019). A more representative and less positively selected composition of lottery winners would likely generate lower costs on kids left behind. For example, Rucinski & Goodman (2022) recommend 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 school, which might limit the ability of schools to address the negative effects experienced by students left behind. Ultimately, our findings suggests that the gains of school choice from retaining high-achieving students in public school districts likely comes at a significant cost to non-school choice students in the form of increased adult criminality.

[6] In related work, several papers examine the effects of school choice on racial segregation (Mumma 2022; Monarrez, Kisida and Chingos 2022; Bifulco, Ladd and Ross 2009a, 2009b) finding at most modest effects of choice on racial segregation.

Background

By default, all students enrolled in Charlotte-Mecklenburg Schools (CMS) are assigned to a neighborhood (home) school based on residential address. Since 2002, CMS has operated a school choice system that allows students to apply for one of several magnet school options or even neighborhood schools that are not their residential assignment. To ration oversubscribed schools, CMS uses a centralized lottery system. Students can apply for the lottery by submitting up to three program choices in order of preference.[7] Younger siblings are guaranteed a spot if an older sibling already attends a given school. Nonguaranteed seats are assigned in three rounds. Only first choices are considered in the first round. If there are fewer applicants than seats available to a given program, then all applicants to that program will be assigned to their first choice.

When the number of applicants is greater than the number of available seats (the choice is oversubscribed), seats are assigned quasi-randomly. Seat assignment is not purely random because the probability of winning for a particular student depends on that person's priority group. "Priority groups" refer to sets of students who meet some prespecified criteria based on geographic location and whether the student's neighborhood school is a Title I choice school.[8] If a student is not assigned to their first choice, they may win a seat to their second or third choice in the following rounds. If a student does not win any of the three choices, he or she is assigned to the neighborhood school determined by location of residence and pre-specific neighborhood school boundaries. The lottery considers student choices in sequence, so students are most likely to win a choice by picking it first, and most seats are awarded in the first round.

[7] We use the term program rather than school because students apply for specific grades as well as special magnet programs that encompass only a portion of classrooms in a school.

8 Title I schools are those with a high percentage of economically disadvantaged students. When a Title I school fails to meet adequate yearly progress in the same subject for two consecutive years it become a Title I choice school. No Child Left Behind (NCLB) required districts to allow students assigned to Title I choice schools the opportunity to attend a non–Title I choice school, but it did not require the district to allow students to choose the school they were offered.

In addition to the lottery rules, some magnet programs restrict access to students who meet certain requirements. These requirements are generally based on whether the student scored at grade level or higher on end-of-grade exams in the prior year. For example, students who wish to enter one of the STEM programs in sixth grade must score at grade level in reading, math, and science on his or her fifth-grade end-of-grade exams. In this case, we can check whether each student met the stated requirements for the program applied to with his or her first choice in the lottery. The share of applications who won their first choice is about 35% over our sample period, which includes those entering sixth grade in the 2006-2007 to 2008-2009 school years.

To examine the effects of the lottery on both individual winners as well as students who forego the lottery, our analyses are based on two distinct samples: one sample of lottery applicants and one with non-applicants. To construct the sample of lottery applicants, we use the applicants who apply to a school (other than their residentially assigned neighborhood school) and who do not have a guaranteed seat. We restrict 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 who are initially assigned to their neighborhood school – the school assigned based on their 5th grade residence – for their 6th grade year. Some of the students in the non-applicant sample have exposure to lottery applicants, meaning that would-be peers from their CBG and neighborhood school zone did apply to the lottery. Others 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.

[9] In some cases, we cannot view whether the student met the stated requirements. Specifically, arts schools require an audition or portfolio assessment, and leadership schools require an interview. We drop these programs from the analysis because assignment is not random, conditional on observables.

Data

Given our focus on lottery applicants as well as non-applicants, our main data sample is comprised of administrative records from Charlotte-Mecklenburg Schools (CMS) for 24,883 5th grade students who attended public school in the county between the years of 2005-2006 to 2007-2008. We focus on the 6th grade lotteries for these cohorts, which allows us to observe the sample as adults. The data include student gender, race, yearly end-of-grade (EOG) test scores, days absent and days suspended from school. The EOG tests are standardized and administered across the state of North Carolina from 1993 to the present. Because we define our small, within school neighborhood areas to include students in the same CBG and same attendance zone, we limit the sample to students for whom we observe both the CBG and middle school attendance zone.

To measure adult criminal justice outcomes, the North Carolina Education Research Data Center (NCEDRC) linked CMS data to arrest registry data for Mecklenburg County using first and last name as well as date of birth. The arrest data includes individual names and identifiers, and information on the number and nature of charges.[10] The NCEDRC merge algorithm sequentially matches individuals first on exact full name (including middle name) and date of birth. Matching then proceeds to excluding middle name and then fuzzy matching on full name. Based on NCEDRC rules, non-unique matches are not provided thus not allowing probabilistic matching or multiple matches, limiting our analysis to unique matches only.

In general, we have tested the validity of the match rates and compared them to other papers (Deming, 2011; Billings, Deming & Rockoff, 2014; Billings, Deming & Ross, 2019) that have used administrative data in Mecklenburg County. Our merged data generates arrest rates of 13% for ages 16-22 which is in line with the 10-16% found in this literature with some variation due to

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

the age ranges and years upon examining adult arrest. We define "offenders" as students who were arrested by Charlotte-Mecklenburg Police Department (CMPD) during our sample period between the ages of 16 and 22.[11] While this data allow us to observe the future criminal behavior of CMS students, regardless of whether they transfer or drop out of school, they are limited to crimes committed within Mecklenburg County.[12]

Our empirical analyses are focused on two distinct samples: a sample of lottery applicants, and a sample of non-applicants. Out of the 13,493 boys that we observe in CMS in these cohorts, 4,166 specified some choice in the lottery. We exclude 2,025 applicants from the applicant sample because they were either guaranteed admission, specified a choice in their neighborhood school, they were in lotteries for which either all or no applicants won their first choice, they applied to a magnet program with subjective admission criteria that we do not observe, or they did not meet the specified admission requirements for the magnet program to which they applied.[13]

For our non-applicant sample, we restrict to male students who are observed in public school in the county in 5th grade in the 2005-2006 through 2007-2008 cohorts, did not apply to the school choice lottery for their sixth-grade year and were assigned to attend their neighborhood school for 6th grade (during the school assignment process in the prior year). Of the 9,327 students who did not specify any choice in the lottery, after restricting to non-applicants who were assigned to their neighborhood school in the lottery and who had at least one other student in their CBG-

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[11] Individuals who committed crimes at age 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. Further, the surrounding counties are lower density, have lower crime rates, and do not have any urban centers near the boundary with Mecklenburg County. As a result, it is unlikely that there are significant numbers of arrests of CMS youth in the surrounding counties, as evidenced by the fact that there are very few young adult arrestees in Mecklenburg County who were not observed in CMS schools.

[13] The admission process to Arts programs and Leadership programs includes subjective unobservable criteria, e.g., interviews or auditions. Aside from applying to an undersubscribed program, students may also have been guaranteed admission through a sibling placement, or by participating in a magnet program in 5th grade for which they received an automatic placement into the associated middle school magnet program.

School-Cohort group, there are 7,903 students. Finally, 12 students are dropped, because they would be the only student left from their CBG in the sample. After making our sample restrictions, the analysis samples include 2,141 6th grade lottery applicants and 7,891 non-applicants of which 5,433 had at least one applicant in their geography and cohort.

Table 1 provides descriptive statistics for our different measures of criminality for all students in these cohorts, our lottery applicant sample, and our non-applicant sample. The last column of Table 1 includes the subset of students who reside in the same local neighborhood as a lottery applicant. Across these outcomes, lottery applicants have slightly lower arrest and incarceration rates, while students with exposure to lottery applicants have somewhat higher arrest and incarceration rates than the full non-applicant estimation sample. Table 2 presents descriptive statistics for the average number of applications and total number of male students in local neighborhoods, and student level attributes including race, ethnicity, and test scores. It is important to note that the typical size of neighborhood groups used in this analysis is quite small (around 20 kids) and thus reflects the scale more commonly associated with peer groups.

Students in the estimation sample have more peers in the local neighborhood and cohort group and are more likely to be white and have lower test scores, relative to the lottery applicants. The Table 2 sample averages highlight that lottery applicants are higher performing (in terms of test scores) and more likely to be black rather than white or Hispanic. Higher achievement is consistent with households that have strong preferences for school quality and thus we would expect greater selection into applying for lottery admission to an oversubscribed school. The pattern of more black students selecting into the lottery likely relates to an overrepresentation of lower-performing neighborhood schools in neighborhoods with a higher proportion of minority students. Given this pattern of applicants, later analysis will highlight how results vary by race. The last column of Table 2 presents balancing tests that we discuss in more detail below.

Methodology

In our main analysis, we estimate the impact of lottery winners on non-applicants. For this analysis, we focus on isolating random variation in lottery winners for a given neighborhood-home school cohort. In short, we do this by estimating the effect of the CBG-School-Cohort specific win share for the 6th grade lottery on arrest and incarceration outcomes at age 16 – 22 for non-applicants. We adopt a control function type approach by conditioning on the expected group level win share.[14] Because the lottery win probabilities depend on observables, we construct the expected win shares based on student-specific win probabilities. We predict student-specific win probabilities using lottery-specific win shares for the student's first choice in their school choice application, as well as dummy variables for characteristics that determine lottery priority groups.[15] Specifically, we define $\hat{P}_{ibst}$ as the predicted probability that student *i,* from census block group *b* and neighborhood school zone *s* and cohort *t* wins their first choice in the lottery.[16] We construct group level expected win shares (w_{bst}) using the student-specific win probabilities: $w_{bst} =$

[14] In our context, we can explicitly use factors that impact expected lottery wins for a specific program to predict win probabilities and thus we can add control variables (function thereof) to limit variation in our main independent variable to be conditionally random. Wooldridge (2015) and Petrin & Train (2010) provide a good discussion on control function approaches in econometrics.

[15] The prediction is based on estimates from probit regressions of a dummy variable for winning their first choice in the lottery on a vector of lottery observables. In all cases, the model includes the win ratio of applicants who applied to the same program as a predictor. For non-magnet applications, we also include year-specific dummies for the applicant scoring below grade level in reading and applying to an above average school in reading, applying to a non-Title I Choice school from a Title I Choice school, economically disadvantaged student applying to a non-Title I Choice school, and for living in the transportation zone of the application school. For magnet applications, we include year-specific dummy variables for applying to a non-Title I Choice school from a Title I Choice School, economically disadvantaged student from a Title I Choice school applying to a non-Title I Choice school, and for living in the transportation zone of the application school. In all cases, we also include the interaction term for each dummy with the lottery-specific ratio of applicants who won their first choice in the given year.

[16] Results are robust to using the lottery-specific win ratios in place of the predicted win probabilities, where the win ratios are a key predictor in the regressions that we use to create win probabilities.

$\frac{1}{n_{bst}}\sum_{i=1}^{n_{bst}} \hat{P}_{ibst}$, where n_{bst} is the number of other students in CBG b, neighborhood school zone *s* and cohort *t.* The win probability, $\hat{P}_{ibst}$, is equal to zero for non-applicants.[17] Now, W_{bst} represents the aggregated expected outcome of the lottery, while accounting for the lottery rules and neighborhood-school-cohort groupings. That is, the average lottery realization that we would expect if the randomization process were repeated many times. The variation that we use is based on the actual realization of the lottery, while conditioning on the expected outcome. The win share, i.e., the actual realization of the lottery, is our main independent variable and can be written in the following way: $Z_{bst} = \frac{1}{n_{bst}}\sum_{i=1}^{n_{bst}} 1[won\ lottery]_{ibst}$, which is the number of students in the same CBG, school zone, and cohort who won their first choice in the lottery, divided by the number of students in the group.

Using the variation in Z_{bst}, while conditioning on the expected win share, W_{bst}, means that identification is based simply on the lumpiness of aggregated lottery wins. Among groups with the same expected win share, some groups will happen to have a larger share of students in the given neighborhood and home school win the lottery, while others will have a lower than expected share win the lottery. Because most lottery winners comply with their assignment to a non-neighborhood school, we consider the random shock of applicants winning the school choice lottery as imposing a treatment on the non-applicant students residing in same neighborhood and assigned to the same school.

Specifically, we estimate Equation 1 with our outcomes of arrests and incarceration (y_{ibst}) of student *i* residing in the same small neighborhood (same block group b and attendance zone *s*) and belonging to the same cohort (same grade and application year) as a function of the fraction

[17] When calculating win ratios, we use the total number of male students in a neighborhood and cohort minus one, so this represents the number of would-be, or potential, male peers for each male.

of same cohort students in this small neighborhood who win the lottery (Z_{bst}). Since both the number of lottery applicants and the likelihood of winning may correlate with school and neighborhood unobservables, we condition on the expected fraction of winners (w_{bst}) from the same small neighborhood (*b,s*) and cohort, pre-determined student attributes (X_{ibst}), as well as broader neighborhood (δ_b), school attendance zone (η_s) and cohort (γ_t) fixed effects.

$$y_{ibst} = \beta_1 Z_{bst} + \beta_2 W_{bst} + \beta_3 X_{ibst} + \delta_b + \eta_s + \gamma_t + \varepsilon_{ibst} \qquad (1)$$

Note that W_{bst} is a vector of controls including, most importantly, (1) the expected fraction of winners (w_{bst}), but also including additional lottery related controls for (2) 2nd and 3rd choice wins in the lottery, (3) other applications, and (4) other wins. For (3) and (4), "other" refers to applications that are not in the applicant sample, like sibling placements, applications to undersubscribed lotteries, and applications to programs with subjective placement criteria. The vector of additional control variables, X_{ibst}, includes a set of dummy variables for race / ethnicity, a dummy variable for economic disadvantage, lagged math and reading scores, and an indicator for English language learner.[18]

This model is identified by randomness in the school choice lottery process where students in local neighborhoods with the same expected number of lottery winners experience different treatments because one neighborhood has a higher than expected win rate and the other has fewer wins than expected. To test this identification strategy, we first conduct balance tests by regressing the individual student attributes on the actual and expected win shares for each student attribute *k*.

[18] For those missing a lagged test score, we use the mean value, and include a dummy variable for missing lagged score. 6% of the non-applicant sample is missing at least one lagged test score.

$$X_{ibst}^{k} = \beta_{1k} Z_{bst} + \beta_{2k} W_{bst} + \delta_{bk} + \eta_{sk} + \gamma_{tk} + \varepsilon_{ibstk} \tag{2}$$

Returning to our descriptive statistics in Table 2, the final column presents estimates of β_{1k} from Equation 2 for the student attributes identified in each row. All student attributes appear to be uncorrelated with the share of lottery winners, as we fail to reject the null hypothesis of β_{1k} = 0 in every case. In addition, we report the p-value from the joint significance test including all of the attributes from a single regression of the win share, Z_{bst}, on the student attributes (X_{ibst}), while conditioning on the other lottery related variables. The p-value from that test for joint significance is 0.93, which highlights the insignificant explanatory power of the student level attributes in explaining the win shares. Therefore, we find no evidence that the portion of students winning the lottery is related to student attributes once we properly control for neighborhood-cohort expected win share.

Finally, we estimate heterogeneous effects across students by interacting Z_{bst} with a student attribute indicator variable (X_{ibst}^{k}). The most important source of heterogeneity in our analyses, presented with the main results, is on the a priori predicted arrest risk for our non-applicant sample. For this analysis, we start by predicting the probability of any arrest between ages 16-22.[19] Using the predicted arrest risk, we construct dummy variables for high- and low-risk. HR_{ibst} is equal to one for individuals with predicted risk in the top half of the distribution and zero otherwise. LR_{ibst} is equal to one for those with predicted risk below the median, and zero

[19] We estimate a logistic regression model for a dummy variable for any arrests between ages 16-22 on individual, CBG, and neighborhood school level covariates. The individual level predictors are a set of race / ethnicity dummy variables, 5th grade math and reading scores, and dummy variables for economically disadvantaged and exceptionality, 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 CBG and neighborhood school levels.

otherwise. We then use the following specification to estimate the differential effects by predicted level of arrest risk.

$$y_{ibst} = \beta_1^{HR} Z_{bst} \times HR_{isbt} + \beta_1^{LR} Z_{bst} \times LR_{isbt} + \beta_2^{HR} W_{bst} \times HR_{isbt} + \beta_2^{LR} W_{bst} \times LR_{isbt} + \beta_3 X_{ibst} + \delta_b + \eta_s + \gamma_t + \varepsilon_{ibst} \quad (3)$$

As shown in equation (3), when estimating heterogeneous effects, we also interact the entire vector W_{bst} with the same attribute. Now, β_1^{HR} and β_1^{LR} represent risk-specific coefficients, which will allow us to test whether any effects are concentrated on individuals with low- or high-risk of future arrest. We include heterogeneity estimates on several other dimensions, as well as by quintile of arrest risk using the analogous specifications. In each case, we interact the win share, Z_{bst}, and the vector of lottery-related variables, W_{bst}, with the set of dummy variables.

Results

Since one of the main assumptions is that lottery winners are less likely to attend their neighborhood school, relative to lottery losers, we formally test this assumption in the sample of lottery applicants in Table 3 using regressions of several outcomes related to school attendance and movement on a dummy variable for winning their first choice in the lottery.[20] 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.[21] The column one outcome is an indicator

[20] Each regression is conditional on lottery fixed effects (application choice by cohort), other lottery-related controls, and a set of individual controls including dummies for race/ethnicity, economic disadvantage, and English language learner status, as well as 4th grade math and reading scores. When lagged test scores are missing, we use the mean value and include a dummy for missing any test score.

[21] We estimate an individual crime risk based on a set of student attributes prior to 6th grade and thus provide a composite measure of a student's likelihood of being arrested age 16-22 based on demographics and elementary

for having won the lottery for any of their choice schools yielding an estimate of about 0.75, which illustrates a level of imperfect compliance that arises because some losers of the first lottery may win their second or third choice. Winning any choice in the lottery provides some opportunity for students to leave their assigned middle school without residential movement. Columns 2 and 3 indicate that lottery winners are almost 70 percentage points more likely to attend their first-choice school in 6th grade and 35 percentage points less likely to attend their neighborhood school in 6th grade, relative to lottery losers. The outcomes in columns 4 and 5 are dummy variables for whether the student changed neighborhood school (*Change NS*), a sign of residential movement, and a dummy for exiting the district (*Exit*), respectively. These results indicate limited attrition by lottery winners in terms of residential relocation or exit from the school district, which suggests that much of the gap between estimates for attending first choice school and not attending the neighborhood school arises from losers winning the lottery for non-first choice schools.[22] Panel B indicates limited heterogeneity in these outcomes by crime risk.

Table 4 provides our main results for impacts 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 high- and low-risk non-applicants. Results in panel A are positive and often sizable in magnitude, but imprecisely estimated with only number of arrests and number of violent arrests significant at the 10% level. To interpret coefficients, we use the change from no winners to the average proportion of winners in a neighborhood (4.4% of same grade male students), which indicates a 0.04 (0.872*0.044)

school performance/attributes as well as school attendance zone and CBG fixed effects. This crime risk estimation results are provided in Appendix Table A9.

[22] Students who lost their first choice are placed on a waitlist. Students may also be admitted through the first quarter of the school year as seats become available in their first-choice option.

increase in the number of arrests as a young adult, which is an 8.3% (0.04/0.46) increase over our estimation sample average. Turning to Panel B, we find that effects are more precise and considerably larger in percentages for students at below median arrest risk. Among the low-risk non-applicants, a change from no winners to an average win share in their neighborhood generates a 1.4 percentage point increase in the probability of arrest, 0.02 more violent arrests and 1 more days incarcerated. Given lower rates of arrest and incarceration for this subsample, the estimates imply increases of 24.7, 59.5 and 84.6% of the respective means. All estimates are significant at a 1% level or higher.

The presence of effects for low crime risk individuals is consistent with the fact that lottery applicants are positively selected in terms of achievement and negatively selected in terms of crime risk and thus are more similar in attributes to low-risk non-applicants. The presence of stronger peer effects between individuals with similar attributes is well established in the literature (Billings, Deming & Ross, 2019; Black, Devereux & Salvanes, 2013; Lavy & Schlosser, 2011). The positive selection into applying to the lottery can be seen in Table 2 with applicants having higher elementary school test scores on average. Further, in Appendix Table A1, we show that lottery losers attending their home school score substantially higher on middle school test scores than non-applicants, even after conditioning on lagged test scores and school fixed effects.

To test the validity of our identification strategy, we implement a falsification test for our main specification in which, for each student, we assign the win share and the expected win share from the cohort in the following year in the same small neighborhood. The results are displayed in Table 5, which is effectively a replication of the main results in Table 4, using the treatment from the adjacent cohort in the same neighborhood. The maintained hypothesis is that unexpected wins for a future cohort of students in the school should not affect the outcomes of the current cohort.

All estimates are statistically insignificant and, in most cases, substantially smaller in magnitude, relative to the main results.

Heterogeneity and Mechanisms

Given the strong evidence that unexpected lottery winners increases arrests and incarcerations, primarily for low crime risk individuals, we test whether these results extend to other non-criminal justice outcomes and whether the behavioral effects emerge in the short-term, in addition to the long-term adult outcomes presented in Table 4.

Table 6 provides evidence that children left behind by lottery winners experience substantially worse outcomes on absences and suspensions. Panel A presents estimates for the full sample. Again, while some estimates are sizable, only the estimate for attendance is statistically significant at the 10% level. Panel B presents estimates for the above and below median risk of ever being arrested. As with arrests and incarceration, we observe large effects across the board for the low-risk subsample with substantial increases in the number of days absent and the number of days suspended, with suspension effects concentrated on out of school suspensions. Among the low-risk non-applicants, we estimate that an increase from no winners in their neighborhood (CBG-school-cohort group) to the average number across our sample increases the number of absences by 0.33 (0.044*7.40) per year from 6th through 10th grade, which is an increase of 5.5% (0.32/5.87) over the mean absences among low-risk individuals. In addition, an increase in the win share from zero to the sample average increases the number of days in out of school suspension by 0.12 (0.044*2.74) per year from 6th through 10th grade, which is a 19% (0.12/0.63) increase over the sample average among low-risk non-applicants.

Finally, Panel C presents results restricting only to middle school attendance and suspension. We observe the same pattern for below median risk middle school students. The negative behavioral effects of losing peers begin in middle school when the peers are lost. We do not observe any effects on test scores or high school graduation, as shown in Appendix Table A2.

Table 7 provides results when we examine our arrest and incarceration outcomes separately for age 16 to 18 and age 19 to 22. Coefficients indicate that going from no winners to the sample average win share generates a 28% ((0.044*0.17)/0.027)) increase over the sample average among low-risk individuals in the probability of arrest at age 16-18. Despite the higher incidence of arrest at ages 19-22, we estimate an even larger effect on the probability of arrest, 36% ((0.044*0.307)/0.038)), for the age 19-22 subsample. This table provides evidence that effects persist into early adulthood after affected students have left school. In fact, the absolute effects of losing lottery applicants on arrest and incarceration appear to be substantially larger for post-high school ages across all outcomes. Figure 1 displays percentage effects for a set of behavioral outcomes at different ages for the sample of low crime risk individuals. This figure highlights a larger trend across a number of our results in that the magnitude of our effects, in percentage terms, is constant up until age 17 and then increases in adulthood. This age trend provides evidence consistent with several papers in the education literature (Deming (2009); Jacob, Lefgren, and Sims (2010); Carrell and West (2010); Chetty et al. (2014)) that positive educational treatments (e.g., Head Start, high-quality teachers and in our case peers) can have benefits that fade-out initially but grow as young adults.

Finally, we recognize that two key correlates of likelihood of future arrest are race and academic performance. Therefore, we estimate heterogenous effects for young men using race/ethnicity dummy variable interactions with win share and expected win share, as well as dummies for above and below median test scores interacted with win share and the lottery controls including expected win share. The results are shown in Appendix Table A3. Panel 1 includes results for race and ethnicity and Panel 2 for student test scores. The results for white students closely parallel the results for students with low risk of arrest in significance and magnitude. In fact, for white students, the estimate for days incarcerated is now significant with an estimate that implies an extra 1.73 (0.044*39.355) days incarcerated for white students exposed to the sample average win share, as opposed to no winners.[23] For test scores, all estimates are positive, but smaller in magnitude than for white students. The only significant effects are for students with above median test scores on likelihood of arrest and likelihood of violent arrest.

Lottery Winners and Adult Crime

To calculate the net benefits of school choice, we must first estimate the direct effect of winning the first choice in the lottery on the arrest and incarceration outcomes for the lottery winners themselves. In this section, we largely attempt to replicate existing work by estimating the direct effect of winning a school choice lottery on the adult crime of lottery winners. We limit our analysis to the sample of lottery applicants and focus on comparing winners and losers. We initially mirror our sample split on above versus below median crime risk given our earlier analysis, but also provide splits by quintile of arrest risk consistent with Deming (2011).

Specifically, we model arrests and incarceration (y_{ibst}) of lottery applicant student i who applied in lottery l, which represents an application choice by year combination. As discussed earlier, we include controls for lottery fixed effects (ρ_l) and control for other characteristics that

[23] We also examine whether the effects on those left behind are concentrated among same race relationships controlling for share of wins among lottery applicants of the same race. However, we do not find any evidence of same race effects associated with the effects on those left behind by lottery winners. See Appendix Table A4.

affect priority groups, which are captured in student attributes (X_{il}).[24] We include additional controls in X_{il} for race and ethnicity, economic disadvantage, 4th grade math and reading scores, and ELL status.[25] The specification is displayed in Equation 4.

$$y_{il} = \gamma_1 W_{il} + \gamma_2 X_{il} + \rho_l + \varepsilon_{il} \tag{4}$$

where W_{il} is a dummy variable equal to one if the applicant won their first choice in the lottery, and zero otherwise. Now, γ_1, describes the difference in outcomes, y_{il}, between the lottery applicants who won and lost their first choice in the lottery. After conditioning on lottery fixed effects and other controls that affect lottery priority, we argue that this comparison describes the causal effect of the lottery outcomes (for 6th grade lottery) on arrest and incarceration outcomes measured from ages 16 to 22.

Table 8 provides descriptive statistics for the applicant sample and standard balancing test to show that lottery variation is random in this sample of students. The first two columns contain means and standard deviations among lottery winners and losers in our sample. Balance tests are included in columns 3 through 6. Each balance test is from a linear regression of the dummy variable for winning the lottery on the student level covariates. We report the estimated coefficients on the covariates in the table. Column 3 reports the coefficients when no lottery fixed effects or

[24] Lottery fixed effects are application choice by year indicators. The additional controls in X_{il} include year-specific dummy variables for economic disadvantage, Title I choice school, interactions between economically disadvantaged and Title I choice, below grade level in reading and applying to a nonmagnet with above average reading scores, scoring at grade level in both math and reading in 4th grade, academically gifted status, interactions between grade level achievement and academically gifted status with applying to an IB program, We also include single dummies for grade level in 4th grade math and reading, a dummy for missing grade level info, and two dummies indicating geographic proximity to a full magnet school that the student applied to.

[25] For students who are missing a 4th grade test score, we use the mean value and include a dummy for missing any test scores. 7% of the applicant sample is missing at least one 4th grade test score. We use 4th grade test scores for this part of the analysis, because the 5th grade testing occurs after the lottery.

other controls are included. In columns 4-6, we include lottery fixed effects and other controls that affect priority groupings. Consistent with prior work, we find that winning the lottery is uncorrelated with student characteristics, after controlling for lottery priorities in the CMS school choice lottery (Deming, 2011; Deming et al., 2014; Bibler & Billings, 2020). As with prior work, our variation is limited to oversubscribed programs.

The main results from estimating Equation 4 are included in Table 9. While the estimated effects of winning the lottery on the arrest and incarceration outcomes are all negative, suggesting that lottery winners benefit, most of the estimates in the pooled sample are statistically insignificant. However, we show that in the pooled sample, from Panel A, that lottery winners are 2.5 percentage points less likely to be incarcerated anytime between ages 16 to 22. This rather large effect represents a 23% decrease off the mean incarceration rate for lottery applicants. When we turn to our sample of low risk lottery applicants, we find significant declines across both arrests and incarcerations with the most precise effects coming from violent arrests and incarceration on the extensive margin. However, the differences between the above and below median sample estimates primarily arise in the standard errors. Results for the low-risk subsample are much more precisely estimated and statistically significant, while the estimates for the high-risk subsample are often sizable and negative, but much more noisy.

Since prior work (Deming (2011)) focused on those with the highest ex-ante crime risk (top quintile), we replicate his analysis using our sample of lottery applicants and focusing on middle school students. Appendix Table A5 provides results for 5 groups of lottery applicants based on quintiles of ex-ante crime risk for several criminal justice outcomes. As with Deming (2011), we observe statistically significant declines in the highest quintile of arrest risk on the intensive margin for both arrests and incarceration. However, we also estimate sizable and statistically significant effects in the second and third quintiles.[26]

Appendix Table A7 replicates Table 6 for lottery applicants. In contrast to the estimated peer effects, we do not find any statistically significant effects of winning the lottery on the winner's number of absences or suspensions. However, we do observe positive effects of between 0.06 and 0.10 standard deviations on test scores for lottery winners, which we do not find for winner's peers who were left behind. Therefore, while not the focus of our paper, the school choice system appears to lead to slightly higher test scores for winners with no offsetting test score losses on the lottery winner's local neighborhood peers, similar to Muralidharan & Sundararaman (2015) in India.

Net Benefit Analysis

One of the main takeaways from results so far is that some lottery winners appear to benefit in terms of adult arrests and incarceration while the departure of lottery winners appears to hurt some non-applicants in terms of adult arrests and incarcerations. Given the heterogeneity in both benefits and costs, a formal accounting of effects by different levels of crime risk is required to assess the net benefit of constrained school choice on criminal justice outcomes. To implement this analysis, Table 10 provides two sets of results based on a simple split by above and below median crime risk and by risk quintiles. This Table breaks down benefits to lottery winners in terms of the number of arrests and days incarcerated avoided and costs in terms of increases in arrests and incarcerations among non-applicants.

[26] Appendix Table A6 presents similar results for non-applicants exposed to a lottery winner by risk quintile. Statistically significant effects are mostly concentrated in the 1st through the 3rd quintile.

To construct the aggregate effects of winning the lottery for each group, we use the risk-specific expected number of wins and risk-specific estimates from the previous section. For example, summing the predicted win probabilities across all high- and low-risk applicants suggests that expected number of high- and low-risk winners is 343 and 397, respectively, which are displayed under *E[Wins]* in the first column of Panel A. From Table 9, the estimated effects of winning the lottery among high- and low-risk applicants on the number of all arrests are -0.16 and -0.13, respectively. Using the risk-specific expected number of wins and estimates, we estimate that the lottery decreased all arrests by about 53 and 51 among, high- and low-risk applicants, respectively, which we show under the *All Arrests* column near the top of Panel A. Below the risk-specific effects, we sum up to calculate the *Total Effect on Applicants* of -104. We compute the results for each of the intensive margin outcomes in a similar manner and aggregated in two ways: all coefficients, regardless of statistical significance, and including only the statistically significant results. Significant estimates and the benefits/costs arising only from significant estimates are always indicated by bold numbers, which results in an estimated effect of 51 fewer arrests among lottery applicants.

In the second half of Panel A, we estimate the corresponding aggregated change in the number of arrests among the non-applicants with peers who applied to exit their school through the lottery. To do this, we use the group-specific expected win share along with the risk-specific estimated effects to generate the aggregated effects. For example, we sum the product of the expected win share (W_{bst}) and the estimated effect of peers winning the lottery on arrests for high-risk non-applicants, 0.61 from Table 4, across all high-risk students in the non-applicant sample, which suggests that the lottery increased arrests by 126 among this group. Similarly, we find an estimated increase in arrests of 194 among the low-risk non-applicants. We replicate this for

Violent Arrests and *Incarceration Days* in the following columns. We include the analogous calculations for risk-quintile specific estimates and aggregates for each outcome in Panel B. Aggregating across both groups, applicants and non-applicants, produces a total aggregated effect.

In all cases, whether using above and below median arrest risk, or by quintiles, and using all estimates or only significant estimates, the increased criminal activity of students residing in the local neighborhood of lottery winners substantially exceeds the declines in criminal activity of the lottery winners themselves. Based on the more conservative results using quintiles of crime risk, the main results highlight that aggregate arrests increased by 170 when considering all estimates, or 28 additional arrests when aggregating across significant coefficients only. Summing across the affected populations, we estimate that there are 3,539 additional incarceration days when aggregating across all coefficients, or 2,937 additional days incarcerated when aggregating for significant coefficients only.

Conclusions

We estimate the effects of having peer lottery winners on arrest and incarceration using three cohorts of 5th grade students in Charlotte, NC. Looking within assigned middle school, we find that male students whose immediate, same grade neighbors win the lottery are more likely to engage in criminal behavior later as an adult. Specifically, we find large negative effects of neighborhood peers winning the lottery on 5th grade boys who do not apply to the school choice lottery, including an increased likelihood of arrest and number of arrests, and increased days of incarceration between ages 16 and 22, with the largest effects between age 19 and 22. The negative effects of increased arrests and incarcerations are concentrated among students who are at below median risk of being arrested, and to some extent among students who are white and have higher elementary school test scores. The larger effects for students at low risk of future arrest appears consistent with effects for students who would have been more likely to interact socially with those positively selected lottery applicants. We also observe evidence of negative behavioral effects in middle school in terms of attendance and suspension, also among students at low risk of future arrest.

We also show benefits for lottery winners in terms of reductions in young adult criminality. Unfortunately, these effects are not large enough to offset the negative effects on students left behind. We show through an accounting of costs and benefits to adult crime that school choice increased the total amount of arrests and incarceration between ages 16 and 22 in our sample. This finding empirically validates the theoretical prediction of Barseghyan et al. (2019), that school choice can be welfare decreasing in the presence of strong peer preferences. 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 represents a significant cost that might be considered when deciding whether, or how, to expand school choice opportunities. Our results have important implications for public school systems as the popularity of school choice grows (Brunner et al., 2012; Tuttle, Gleason & Clark, 2012) and scholars continue to study and refine the lottery mechanisms used to implement school choice (Abdulkadiroğlu & Sönmez, 2003; Abdulkadiroglu & Andersson, 2022; Pathak, 2017).

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Tables and Figures

Table 1: Outcomes Summary

	CMS	Lottery Apps	Est. Sample	With Exposure
Pr(Any Arrest)	0.13	0.11	0.12	0.13
	(0.33)	(0.32)	(0.33)	(0.34)
Pr(Violent Arrest)	0.08	0.07	0.07	0.08
	(0.27)	(0.25)	(0.26)	(0.27)
Pr(Incarceration)	0.08	0.06	0.07	0.08
	(0.26)	(0.24)	(0.26)	(0.27)
Num. Arrests	0.49	0.42	0.46	0.50
	(2.00)	(1.85)	(1.92)	(2.00)
Num. Violent Arrests	0.21	0.19	0.18	0.20
	(0.96)	(1.01)	(0.89)	(0.91)
Days Incarcerated	8.95	7.38	7.53	8.12
	(60.88)	(53.61)	(54.00)	(54.79)
Observations	13,493	2,141	7,891	5,433

Notes: Summary of arrest and incarceration outcomes from age 16 to 22 for our main data sample of 13,493 male 5th grade students that attended public school between the years of 2005-2006 to 2007-2008 in Charlotte-Mecklenburg Schools (CMS). The columns show the mean and standard deviation for CMS, the sample of lottery applicants, the non-applicant estimation sample, and the non-applicant sample with exposure to lottery sample applicants in their neighborhood, respectively.

Table 2: Characteristics Summary and Tests

	Summary of Characteristics			
	CMS	Lottery Apps	Est. Sample	Tests
Num. Apps	2.81	4.55	2.60	
	(3.31)	(3.55)	(3.23)	
Group N	18.66	16.68	21.65	
	(16.85)	(15.18)	(17.52)	
Num. Wins	0.96	1.57	0.90	
	(1.42)	(1.76)	(1.36)	
Black	0.42	0.52	0.36	-0.02
	(0.49)	(0.50)	(0.48)	(0.12)
White	0.36	0.29	0.42	-0.08
	(0.48)	(0.45)	(0.49)	(0.09)
Hispanic	0.14	0.12	0.13	0.10
	(0.35)	(0.32)	(0.34)	(0.10)
Ec. Disadvantage	0.48	0.47	0.45	0.03
	(0.50)	(0.50)	(0.50)	(0.10)
Math EOG, 5th Grade	0.13	0.33	0.13	-0.25
	(1.00)	(0.95)	(1.01)	(0.22)
Reading EOG, 5th Grade	-0.01	0.21	0.00	-0.21
	(1.00)	(0.95)	(1.01)	(0.23)
Missing Test Score	0.07	0.03	0.06	-0.00
	(0.26)	(0.17)	(0.24)	(0.07)
ELL	0.10	0.06	0.10	-0.01
	(0.31)	(0.24)	(0.30)	(0.09)
Observations	13,493	2,141	7,891	7,891
Clusters				1,134
P-value				0.929

Notes: Summary of student and group level attributes. The first three columns show the mean and standard deviation for CMS, the sample of lottery applicants, and the non-applicant estimation sample, respectively. *Ec. Disadvantage* is an indicator for 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 in student's cohort-school-CBG group. *Group N* = the number of other male students in student's cohort-school-CBG group. *ELL* is an indicator for English Language Learner. Last column displays estimated coefficients from regressing the row variable on the win proportion, conditional on expected win proportion, covariates related to lottery applications and wins (number of 2nd and 3rd choice wins, number of other wins, and number of other applications), and school, CBG and year fixed effects. Standard errors are clustered at the CBG-Neighbohood School-Cohort level. The P-value refers to the joint test on the individual characteristics and test scores from the regressing the win proportion on the characteristics, conditional on the other lottery related covariates and school, CBG, and year fixed effects.

Table 3: Effects of Winning Lottery on Own Attendance and Movement

		Attendance			
	Won Any	App School	Neighb. Sch.	Change NS	Exit
Panel A: Pooled					
Win	0.748***	0.680***	-0.355***	-0.054*	-0.014
	(0.018)	(0.029)	(0.030)	(0.030)	(0.024)
Sample Mean	.512	.378	.267	.272	.121
Observations	2,140	2,140	2,140	2,140	2,140
Panel B: By Risk					
$Win \times HR$	0.763***	0.704***	-0.329***	-0.005	0.005
	(0.019)	(0.024)	(0.028)	(0.032)	(0.022)
$Win \times LR$	0.732***	0.660***	-0.376***	-0.097**	-0.029
	(0.029)	(0.043)	(0.040)	(0.038)	(0.035)
Observations	2,140	2,140	2,140	2,140	2,140

Notes: Estimated effect of winning first choice on attendance and movement of applicants. Each estimate is conditional on the set of student characteristics, a set of covariates related to the lottery priority groups, and lottery fixed effects. 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* × *HR* refers to estimates for the group with higher than the median estimated risk, and *Wins* × *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 a dummy for winning any choice in the lottery (first, second, or third). The outcomes for columns 2 and 3 are measures of attendance in 6th grade. *App School* is an indicator variable for attending the school they applied to with their first choice in the lottery. *Nieghb. 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, likely indicating district exit. Standard errors are clustered at the application choice by year level.

Table 4: Effect of Peer Wins on Arrest Outcomes

	Pr(Arrest)	Pr(Violent)	Pr(Incarc.)	All Arrests	Violent Arrests	Days Incarc.
Panel A: Pooled						
$(Wins/N_g)$	0.091	0.084	0.002	0.872*	0.432*	14.860
	(0.097)	(0.079)	(0.083)	(0.529)	(0.234)	(14.679)
Observations	7,891	7,891	7,891	7,891	7,891	7,891
Panel B: By Risk						
$(Wins/N_g) \times HR$	-0.037	0.028	-0.130	0.609	0.354	8.789
	(0.137)	(0.117)	(0.121)	(0.800)	(0.352)	(22.314)
$(Wins/N_g) \times LR$	0.309***	0.176***	0.223***	1.311***	0.568***	23.976***
	(0.089)	(0.066)	(0.070)	(0.415)	(0.205)	(8.965)
Dep Var Mean, HR	.207	.136	.135	.859	.361	15.284
Dep Var Mean, LR	.055	.022	.021	.129	.042	1.247
Observations	7,891	7,891	7,891	7,891	7,891	7,891

Notes: Estimated effects of the peer win proportion on outcomes. Panel A reports estimates across the sample, pooling by estimated risk. Each estimate in Panel A is for the effect of the proportion of students 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 estimate is conditional on the set of student characteristics, expected wins as a proportion of the group size, other lottery related group-level covariates (number of 2nd and 3rd choice wins, number of other wins, and number of other applications), and cohort, school, 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. *(Wins / N)* $\times$ *HR* shows estimates for the group with higher than the median estimated risk, and *(Wins / N)* $\times$ *LR* shows estimates for the group with lower than median estimated risk. These are estimated in a single regression using interaction terms. In Panel B, we also include interaction terms between the high-risk dummy variable and the movement related covariates, including the expected win share. Standard errors are clustered at the CBG-Neighborhood School-Cohort level.

Table 5: Falsification Test, Lag Wins on Arrest Outcomes

	Pr(Arrest)	Pr(Violent)	Pr(Incarc.)	All Arrests	Violent Arrests	Days Incarc.
Panel A: Pooled						
Lag $(Wins/N_g)$	0.007	-0.033	-0.037	-0.073	0.043	25.815
	(0.115)	(0.096)	(0.093)	(0.727)	(0.358)	(19.310)
Observations	7,636	7,636	7,636	7,636	7,636	7,636
Panel B: By Risk						
Lag $(Wins/N_g) \times HR$	0.052	-0.083	-0.049	0.035	0.081	35.122
	(0.156)	(0.124)	(0.122)	(0.959)	(0.469)	(25.741)
Lag $(Wins/N_g) \times LR$	-0.133	0.038	-0.045	-0.350	-0.099	7.375
	(0.149)	(0.105)	(0.100)	(0.592)	(0.287)	(18.147)
Observations	7,636	7,636	7,636	7,636	7,636	7,636

Notes: Estimated effect of the win proportion from the same school-cbg in the following year on outcomes. Panel A reports estimates across the sample, pooling by estimated risk. Each estimate in Panel A is for the effect of the proportion of students from their same neighborhood school and CBG in the *following* cohort (one year later) who won their first choice in the lottery on the arrest or incarceration outcome indicated by the column heading. Each estimate is conditional on the set of student characteristics, expected wins as a proportion of the group size from the following year, other lottery related group-level covariates from the following year (number of 2nd and 3rd choice wins, number of other wins, and number of other applications, and cohort, school, 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. *Lag (Wins / N)* $\times$ *HR* shows estimates for the group with higher than the median estimated risk, and *Lag (Wins / N)* $\times$ *LR* shows estimates for the group with lower than median estimated risk. These are estimated in a single regression using interaction terms. In Panel B, we also include interaction terms between the high-risk dummy variable and the movement related covariates, including the expected win share. Standard errors are clustered at the CBG-Neighbohood School-Cohort level.

Table 6: Peer Effects on Other Outcomes

		Suspensions			
	Absences	Total Days	Days ISS	Days OSS	Any Susp.
Panel A: Pooled					
$(Wins/N_g)$	3.487*	0.475	-0.017	0.492	-0.185
	(2.060)	(1.786)	(0.432)	(1.476)	(0.513)
Dep Var Mean	7.674	2.814	.713	2.101	.96
Observations	32,993	31,717	31,717	31,717	31,717
Panel B: By Risk					
$(Wins/N_g) \times HR$	1.538	-1.161	-0.450	-0.710	-0.714
	(2.809)	(2.657)	(0.641)	(2.187)	(0.746)
$(Wins/N_g) \times LR$	7.401***	3.502**	0.766*	2.736**	0.780*
	(2.246)	(1.372)	(0.390)	(1.082)	(0.450)
Dep Var Mean, HR	9.961	5.181	1.249	3.932	1.72
Dep Var Mean, LR	5.874	.906	.28	.626	.348
Observations	32,993	31,717	31,717	31,717	31,717
Panel C: Grades 6-8, Low Risk					
$(Wins/N_g) \times LR$	6.106**	4.285***	1.074**	3.211***	0.973**
	(2.443)	(1.585)	(0.522)	(1.204)	(0.481)
Dep Var Mean, LR	5.496	.94	.301	.64	.339
LR Observations	11,606	10,654	10,654	10,654	10,654
Observations	20,763	19,249	19,249	19,249	19,249

Notes: Estimated effect of win proportion on non-applicant outcomes. Each is conditional on the set of student characteristics, other lottery related group-level covariates (number of 2nd and 3rd choice wins, number of other wins, and number of other applications), and cohort, school, and CBG fixed effects. ISS = In School Suspension; OSS = Out of School Suspensions. 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, pooling across estimated risk. Panel B reports the same set of estimates for the high- and low-risk students, based on the estimated probability of arrest. *(Wins / N)* × *HR* shows estimates for the group with higher than the median estimated risk, and *(Wins / N)* × *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 each estimated using interaction terms with the win proportion and high- and low-risk. In Panels B and C, we also include interaction terms between the high-risk dummy variable and the movement related covariates, including the expected win share. Standard errors are clustered at the CBG-Neighbohood School-Cohort level.

Table 7: 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						
$(Wins/N_g) \times LR$	0.170***	0.082*	0.119***	0.297**	0.111	2.949
	(0.062)	(0.048)	(0.042)	(0.135)	(0.073)	(2.596)
Dep Var Mean, LR	.027	.011	.01	.048	.016	.447
Observations	7,891	7,891	7,891	7,891	7,891	7,891
Panel B: Low Risk, Age 19 to 22						
$(Wins/N_g) \times LR$	0.307***	0.175***	0.175***	1.013***	0.457***	21.027***
	(0.082)	(0.057)	(0.063)	(0.333)	(0.166)	(7.708)
Dep Var Mean, LR	.038	.014	.014	.081	.026	.8
Observations	7,891	7,891	7,891	7,891	7,891	7,891

Notes: Estimated effects of the peer win share 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 won their first choice in the lottery on the arrest or incarceration outcome indicated by the column heading. Each estimate is conditional on the set of student characteristics, expected wins as a proportion of the group size, other lottery related group-level covariates (number of 2nd and 3rd choice wins, number of other wins, and number of other applications), and cohort, school, 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 **??**. The estimates in Table **??** use the outcomes measured over ages 16 to 22, whereas in this table we provide 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 **??**. We also include interaction terms between the high-risk dummy variable and the movement related covariates, including the expected win share. Standard errors are clustered at the CBG-Neighbohood School-Cohort level.

Table 8: Characteristics Summary and Tests (Applicant Sample)

			Tests		
	Won	Lost	Uncond.	Lottery FE	Lot FE w/cons
Made 2nd Application Choice	0.79	0.79	0.02	-0.02	-0.03
	(0.41)	(0.41)	(0.04)	(0.04)	(0.04)
Made 3rd Application Choice	0.55	0.53	0.04	0.04	0.03
	(0.50)	(0.50)	(0.03)	(0.02)	(0.02)
Black	0.46	0.55	-0.07	-0.03	-0.03
	(0.50)	(0.50)	(0.05)	(0.04)	(0.04)
White	0.35	0.25	0.02	0.05	0.05
	(0.48)	(0.43)	(0.06)	(0.04)	(0.05)
Hispanic	0.09	0.13	-0.08	-0.03	-0.03
	(0.29)	(0.33)	(0.07)	(0.06)	(0.05)
Lunch	0.41	0.50	-0.03	-0.05*	
	(0.49)	(0.50)	(0.03)	(0.03)	
Math EOG, 4th Grade	0.40	0.20	0.03	0.00	0.00
	(0.99)	(0.94)	(0.02)	(0.01)	(0.02)
Reading EOG, 4th Grade	0.28	0.12	0.00	-0.01	-0.00
	(0.95)	(0.92)	(0.02)	(0.01)	(0.02)
Test Miss	0.06	0.09	-0.06	-0.05	-0.04
	(0.24)	(0.28)	(0.04)	(0.04)	(0.03)
ELL	0.05	0.07	-0.02	-0.03	-0.02
	(0.21)	(0.25)	(0.05)	(0.05)	(0.05)
Observations	753	1,388	2,141	2,140	2,140
Clusters			62	61	61
P-value			0.02	0.09	0.28

Notes: Summary of characteristics in the lottery sample. *ELL* is an indicator for English Language Learner. The first two columns show the mean and standard deviation for lottery winners and losers. The last three columns display estimated coefficients from regressing the dummy for winning the first choice in the lottery on the listed characteristics, conditional on other lottery-related control variables and lottery fixed effects (application by year). 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, conditional on other lottery features and fixed effects. The column labeled *Uncond.* reports coefficients without conditioning on any fixed effects or covariates. The *Lottery FE* column conditions on the application choice by year fixed effects only. The *Lottery FE w/cons* column reports estimates when conditioning on the lottery fixed effects plus other lottery-related control variables.

Table 9: Effects of Winning Lottery on Own Arrest Outcomes

	Pr(Arrest)	Pr(Violent)	Pr(Incarc.)	All Arrests	Violent Arrests	Days Incarc.
Panel A: Pooled						
Win	-0.012	-0.014	-0.025**	-0.148	-0.067	-3.282
	(0.015)	(0.014)	(0.012)	(0.129)	(0.072)	(3.179)
Observations	2,140	2,140	2,140	2,140	2,140	2,140
Panel B: By Risk						
$Win \times HR$	-0.002	0.003	-0.032	-0.155	-0.054	-5.759
	(0.030)	(0.027)	(0.022)	(0.238)	(0.136)	(6.397)
$Win \times LR$	-0.019	-0.029***	-0.019**	-0.129*	-0.073*	-1.082
	(0.013)	(0.009)	(0.008)	(0.069)	(0.037)	(3.047)
Dep Var Mean, HR	.181	.109	.104	.735	.344	12.82
Dep Var Mean, LR	.04	.021	.019	.084	.032	1.704
Observations	2,140	2,140	2,140	2,140	2,140	2,140

Notes: Estimated effect of the winning first choice in the lottery on own arrests and incarceration. Each is conditional on the set of student characteristics, other covariates related to lottery outcomes, and lottery fixed effects. 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* × *HR* shows estimates for the group with higher than the median estimated risk, and *Wins* × *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.

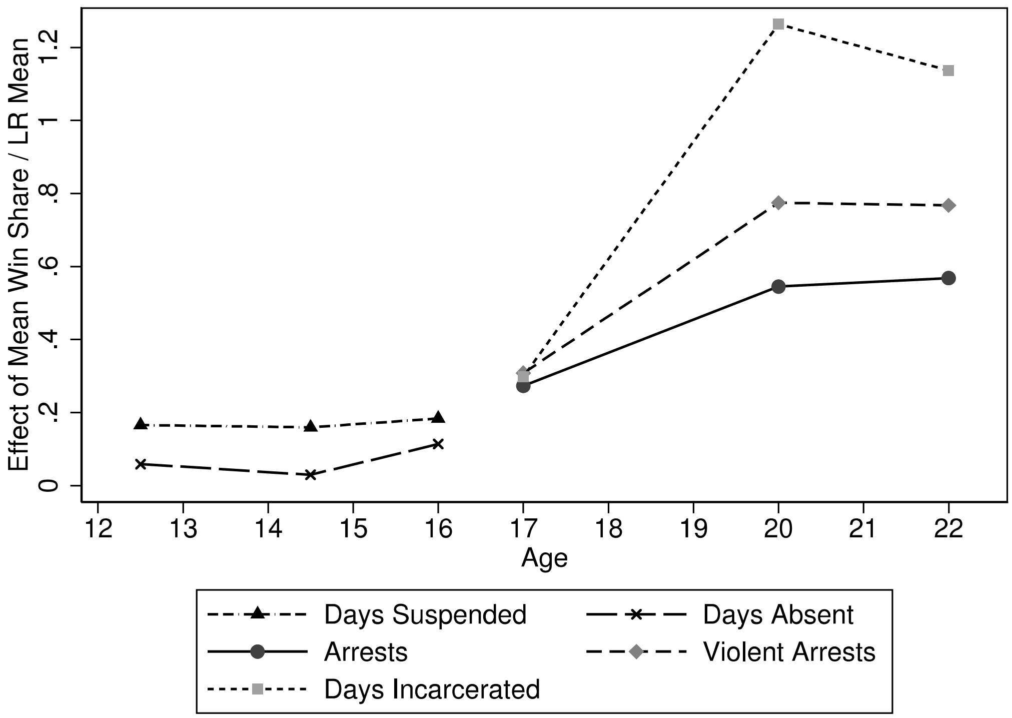
Table 10: Aggregated Effects

Panel A: By Above and Below Median Risk		Aggregated Effects: All Arrests	Violent Arrests	Days Incarcerated
	$E[Wins]$			
High-Risk Applicants	342.92	-53.15	-18.51	-1974.89
Low-Risk Applicants	396.71	**-51.17**	**-28.96**	-429.24
Total Effect on Applicants		-104.32	-47.47	-2404.13
Significant Effects		**-51.17**	**-28.96**	
	$E[Displacements]$			
High-Risk Peers	3393.25	126.24	73.38	1821.95
Low-Risk Peers	3608.23	**193.64**	**83.89**	**3541.5**
Total Effect on Peers		319.88	157.27	5363.45
Significant Effects		**193.64**	**83.89**	**3541.5**

Panel B: By Risk Quintile		Aggregated Effects: All Arrests	Violent Arrests	Days Incarcerated
	$E[Wins]$			
Q5 (High)	105.65	**-47.65**	-19.44	**-1846.18**
Q4	153.55	18.58	19.50	793.70
Q3	177.50	-42.42	**-30.00**	-196.50
Q2	141.81	**-24.53**	**-14.61**	**-651.05**
Q1 (Low)	161.13	-7.41	-3.38	-348.53
Total Effect on Applicants		-103.44	-47.93	-2248.56
Significant Effects		**-72.18**	**-44.60**	**-2497.23**
	$E[Displacements]$			
Q5 (High)	1115.74	18.46	55.54	2592.53
Q4	1572.08	85.30	-0.18	-2239.52
Q3	1444.51	69.11	**63.17**	**3663.49**
Q2	1522.98	**67.14**	**31.05**	**1138.37**
Q1 (Low)	1346.27	**33.27**	7.07	**632.41**
Total Effect on Peers		273.27	156.65	5787.28
Significant Effects		**100.41**	**94.22**	**5434.27**

Notes: Aggregated effects on lottery applicants and their peers. The first column reports the expected number of wins among lottery applicants in each group *E[Wins]* or the expected number of displacements *E[displacements]*. *E[Wins]* are calulated as the sum of the estimated win probabilities for all applicants in the corresponding group. *E[Displacements]* are calculated as the sum of the expected number of wins for each non-applicant in each group. That is, each lottery applicant contributes $P_i \cdot N_g$ to the total number of displacements, where P_i refers to their win probability and N_g refers to the number of non-applicants in their group, i.e., the number of students exposed to the peer who may experience a displacement through the lottery. The aggregated effects on applicants are calculated using the expected number of wins and the relevant corresponding estimates for effects of winning on *All Arrests*, *Violent Arrests*, and *Days Incarcerated*, which come from Table **??** for Panel A and Table **??** for Panel B. The estimated effects on peers are calculated using the sum of the expected lottery win share of each peer's group multiplied by the relevant correspoding estimated effect on peer outcomes (Table **??** for Panel A and Table **??** for Panel B. Aggregates using only estimates with p-value<0.1 are bolded.

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



Notes: Estimated effects of peer wins 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 (β_1^{LR}) from a regression based on Equation (3). To obtain each point in the figure, we multiply the estimated effect on the outcome by the mean displacement (.044) and divide by the age- or grade-specific mean of the same outcome in the low-risk sample. We include estimates at for three distinct approximate age groups for each set of outcomes. For *Days Suspended* and *Days Absent*, we estimate one for the two years post-lottery (6th an 7th grade for most), another for the third and fourth years post-lottery (8th and 9th grade for most), and one for the fith year post-lottery. We do not include estimates for years, which correspond to 11th and 12th grades, 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.

A Supplemental Tables and Figures

Table A1: Selection of Applicants on Test Scores

	Math	Reading	Math	Reading
Panel A: Lottery Losers				
Applicant, Lost Lottery	0.35***	0.34***	0.06**	0.05**
	(0.04)	(0.04)	(0.02)	(0.02)
Math EOG 5th			0.66***	0.23***
			(0.01)	(0.01)
Reading EOG 5th			0.18***	0.57***
			(0.01)	(0.01)
Observations	18,390	18,323	18,390	18,323
Panel B: Lottery Winners				
Applicant, Won Lottery	0.27***	0.25***	0.03	-0.02
	(0.06)	(0.06)	(0.04)	(0.03)
Math EOG 5th			0.67***	0.22***
			(0.03)	(0.02)
Reading EOG 5th			0.16***	0.59***
			(0.02)	(0.03)
Observations	3,207	3,191	3,207	3,191

Notes: Panels A compares applicants who lost the lottery and attend their initially assigned neighborhood school with non-applicants who attend their initially assigned neighborhood school. Panel B compares applicants in our sample who won their first choice and attended their first choice school in 6th grade with other students who attended the same schools, but are not in our lottery sample due to gauranteed placements. Each column reports estimates from a different regression of a math or reading test score on a dummy for being in our lottery sample and losing (Panel A) or winning (Panel B) their first choice in the lottery. Estimates in Columns 3 and 4 are conditional on 5th 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, dummy variables for missing lagged test scores, and CBG, neighborhood school (Panel A) or application school (Panel C), 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 A2: Peer Effects on Test Scores and Graduation

	Math	Reading	On Time Grad.	Dropout
Panel A: Pooled				
$(Wins/N_g)$	0.080	-0.107	0.156	-0.041
	(0.132)	(0.138)	(0.126)	(0.114)
N	19,677	19,609	5,417	5,417
Panel B: By Risk				
$(Wins/N_g) \times HR$	0.244	-0.082	0.343*	-0.187
	(0.157)	(0.185)	(0.181)	(0.169)
$(Wins/N_g) \times LR$	-0.227	-0.167	-0.073	0.144
	(0.222)	(0.201)	(0.151)	(0.099)
N	19,677	19,609	5,417	5,417

Notes: Estimated effect of win proportion on non-applicant outcomes. Each is conditional on the set of student characteristics, other lottery related group-level covariates (number of 2nd and 3rd choice wins, number of other wins, and number of other applications), and cohort, school, and CBG fixed effects. Panel A includes estimates from the full sample, pooling across estimated risk. Panel B reports the same set of estimates for the high- and low-risk students, based on the estimated probability of arrest. *(Wins / N)* $\times$ *HR* shows estimates for the group with higher than the median estimated risk, and *(Wins / N)* $\times$ *LR* shows estimates for the group with lower than median estimated risk. Panel B is estimated using interaction terms with the win proportion and high- and low-risk. In Panel B, we also include interaction terms between the high-risk dummy variable and the movement related covariates, including the expected win share. Test score results include up to three years post-lottery, grades 6-8 for most individuals. We also include dummies for years post-lottery as controls in the test score regressions. The last two column show outcomes related to graduation and dropout. *On Time Grad.* is a dummy variable for graduating within 7 years post lottery (12th grade year for on-time progression). *Dropout* is a dummy variable equal to one if the students 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 A3: Heterogeneity in Peer Effects

	Pr(Arrest)	Pr(Violent)	Pr(Incarc.)	All Arrests	Violent Arrests	Days Incarc.
Panel A: Heterogeneity by Race / Ethnicity						
White	0.361***	0.255***	0.192**	2.055***	0.736***	39.355**
	(0.116)	(0.087)	(0.079)	(0.672)	(0.225)	(19.994)
Black	-0.052	0.051	-0.156	0.457	0.518	10.455
	(0.157)	(0.141)	(0.138)	(0.936)	(0.427)	(20.773)
Hispanic	0.100	-0.007	0.167	0.364	0.040	25.803
	(0.179)	(0.147)	(0.167)	(0.949)	(0.441)	(27.924)
Other	0.150	-0.213	-0.185	-0.829	-0.458	-146.791
	(0.307)	(0.259)	(0.252)	(2.397)	(1.039)	(129.877)
Observations	7,891	7,891	7,891	7,891	7,891	7,891
Panel B: Heterogeneity by Test Scores						
Above Median	0.350***	0.170**	0.122	0.774	0.321	7.345
	(0.105)	(0.087)	(0.085)	(0.494)	(0.228)	(12.710)
Below Median	-0.073	0.034	-0.071	0.988	0.545	21.285
	(0.141)	(0.117)	(0.123)	(0.824)	(0.365)	(23.116)
Dep Var Mean, High	.071	.033	.034	.206	.074	2.777
Dep Var Mean, Low	.18	.117	.113	.729	.306	12.724
Observations	7,891	7,891	7,891	7,891	7,891	7,891

Notes: Estimated effects of the peer win proportion on outcomes. Panel A reports estimates by race / ethnicity and Panel B reports estimates by whether the average of the student's 5th grade math and reading test scores was above or below the median. Each column in each panel reports estimates from a single regression using interaction terms between group indicators and the proportion of students from their neighborhood group who won their first choice in the lottery. Each cell reports a group-speicific estimates for the effect of the proportion of students from their neighborhood group who won their first choice in the lottery on the arrest or incarceration outcome indicated by the column heading. Each estimate is conditional on the set of student characteristics, interactions between the set of group indicators and expected wins as a proportion of the group size, interactions between the set of group indicators and other lottery related group-level covariates (number of 2nd and 3rd choice wins, number of other wins, and number of other applications), and cohort, school, and CBG fixed effects. Standard errors are clustered at the CBG-Neighborhood School-Cohort level.

Table A4: Effect of Peer Wins on Arrest with Race/Ethnicity Grouping

	Pr(Arrest)	Pr(Violent)	Pr(Incarc.)	All Arrests	Violent Arrests	Days Incarc.
Panel A: Pooled						
$(Wins/N_g)$	-0.023	-0.047	-0.023	-0.318	0.020	2.763
	(0.059)	(0.054)	(0.047)	(0.455)	(0.200)	(7.668)
Observations	7,226	7,226	7,226	7,226	7,226	7,226
Panel B: By Risk						
$(Wins/N_g) \times HR$	-0.063	-0.060	-0.032	-0.020	0.195	8.708
	(0.083)	(0.075)	(0.062)	(0.428)	(0.218)	(11.683)
$(Wins/N_g) \times LR$	0.045	-0.015	-0.003	-0.735	-0.200	-4.058
	(0.080)	(0.074)	(0.071)	(0.916)	(0.374)	(10.845)
Observations	7,226	7,226	7,226	7,226	7,226	7,226

Notes: Estimated effects of the peer win proportion on outcomes with alternate groupings that include race/ethnicity. Panel A reports estimates across the sample, pooling by estimated risk. Each estimate in Panel A is for the effect of the proportion of students from their group who won their first choice in the lottery on the arrest or incarceration outcome indicated by the column heading. Groups include male students in the same cohort, initially assigned neighborhood school, CBG, and same race/ethnicity. Each estimate is conditional on the set of student characteristics, expected wins as a proportion of the group size, other lottery related group-level covariates (number of 2nd and 3rd choice wins, number of other wins, and number of other applications), and cohort, school, 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. *(Wins / N)* $\times$ *HR* shows estimates for the group with higher than the median estimated risk, and *(Wins / N)* $\times$ *LR* shows estimates for the group with lower than median estimated risk. These are estimated in a single regression using interaction terms. In Panel B, we also include interaction terms between the high-risk dummy variable and the movement related covariates, including the expected wins varable. Standard errors are clustered at the Race-CBG-Neighborhood School-Cohort level.

Table A5: 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)	-0.005	-0.032	-0.040	-0.451**	-0.184	-17.475**
	(0.053)	(0.044)	(0.041)	(0.205)	(0.129)	(7.608)
Q4	0.033	0.053	-0.001	0.121	0.127	5.169
	(0.042)	(0.034)	(0.030)	(0.407)	(0.220)	(13.311)
Q3	-0.056**	-0.040*	-0.042**	-0.239	-0.169*	-1.107
	(0.026)	(0.021)	(0.021)	(0.191)	(0.085)	(6.733)
Q2	-0.031	-0.049**	-0.031**	-0.173**	-0.103**	-4.591*
	(0.026)	(0.019)	(0.014)	(0.085)	(0.050)	(2.469)
Q1 (Low)	0.009	-0.005	-0.010	-0.046	-0.021	-2.163
	(0.014)	(0.010)	(0.009)	(0.056)	(0.032)	(1.938)
Observations	2,140	2,140	2,140	2,140	2,140	2,140
Panel B: Effect Win, by Risk						
$Win \times Q5$	-0.007	-0.036	-0.041	-0.474**	-0.196	-18.202**
	(0.053)	(0.044)	(0.041)	(0.205)	(0.126)	(7.896)
$Win \times (Q1 - Q4)$	-0.012	-0.010	-0.021*	-0.087	-0.043	-0.618
	(0.016)	(0.014)	(0.012)	(0.145)	(0.075)	(3.893)
Dep Var Mean, Q5	.27	.175	.175	1.09	.505	17.714
Dep Var Mean, Q1-Q4	.078	.043	.039	.272	.124	5.165
Observations	2,140	2,140	2,140	2,140	2,140	2,140

Notes: Estimated effect of the winning first choice in the lottery on own arrests. Each is conditional on the set of student characteristics, covariates related to lottery outcomes, and lottery fixed effects. Panel A includes separate estimate for each quintile of predicted arrest risk. Panel B includes a low-risk esitmate, 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 A6: 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 Quntile						
Q5 (High)	-0.277	-0.069	-0.337*	0.223	0.671	31.321
	(0.200)	(0.191)	(0.192)	(1.447)	(0.670)	(34.330)
Q4	0.153	0.047	0.043	0.968	-0.002	-25.416
	(0.213)	(0.170)	(0.165)	(1.173)	(0.549)	(47.414)
Q3	0.212	0.346**	0.318**	0.989	0.904*	52.427*
	(0.176)	(0.149)	(0.152)	(1.016)	(0.467)	(26.941)
Q2	0.306**	0.162**	0.157*	1.053**	0.487**	17.854*
	(0.119)	(0.078)	(0.086)	(0.521)	(0.230)	(9.535)
Q1 (Low)	0.244*	0.055	0.088	0.659*	0.140	12.527*
	(0.131)	(0.059)	(0.071)	(0.361)	(0.153)	(7.097)
Observations	7,891	7,891	7,891	7,891	7,891	7,891
Panel B: Boys, By Risk						
$(Wins/N_g) \times Q5$	-0.265	-0.066	-0.333*	0.252	0.697	33.053
	(0.200)	(0.191)	(0.192)	(1.446)	(0.673)	(34.574)
$(Wins/N_g) \times (Q1 - Q4)$	0.240**	0.140*	0.146*	0.985**	0.345	8.333
	(0.102)	(0.075)	(0.076)	(0.489)	(0.216)	(17.559)
Dep Var Mean, Q5	.313	.231	.222	1.492	.65	28.553
Dep Var Mean, Q1-Q4	.082	.039	.039	.232	.084	2.988
Observations	7,891	7,891	7,891	7,891	7,891	7,891

Notes: Estimated effect of the win proportion on outcomes. Each is conditional on the set of student characteristics, other lottery related group-level covariates (number of 2nd and 3rd choice wins, number of other wins, and number of other applications), and cohort, school, and CBG fixed effects. Panel A includes separate estimates for each quntile of predicted arrest risk. Panel B includes a low-risk esitmate, which pools quintiles 1-4 and high-risk estimate which includes quintile 5 only. 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 A7: Effects of Winning on Own Other Outcomes

				Suspensions					
	Math	Read	Absences	Total Days	Days ISS	Days OSS	Any Susp.	On Time Grad.	Dropout
Panel A: Pooled									
Win	0.017	0.061**	-0.510	-0.335	-0.042	-0.292	-0.063	0.002	0.004
	(0.040)	(0.024)	(0.383)	(0.356)	(0.087)	(0.288)	(0.084)	(0.017)	(0.010)
Observations	5,555	5,540	9,256	8,691	8,691	8,691	8,691	1,558	1,558
Panel B: Effect Win, by Risk									
$Win \times HR$	0.077**	0.096***	-0.213	-0.381	-0.047	-0.334	-0.122	0.020	0.000
	(0.035)	(0.030)	(0.580)	(0.661)	(0.162)	(0.530)	(0.161)	(0.031)	(0.017)
$Win \times LR$	-0.044	0.024	-0.697*	-0.249	-0.032	-0.217	-0.002	-0.015	0.009
	(0.048)	(0.030)	(0.367)	(0.184)	(0.062)	(0.142)	(0.054)	(0.014)	(0.010)
Observations	5,555	5,540	9,256	8,691	8,691	8,691	8,691	1,558	1,558

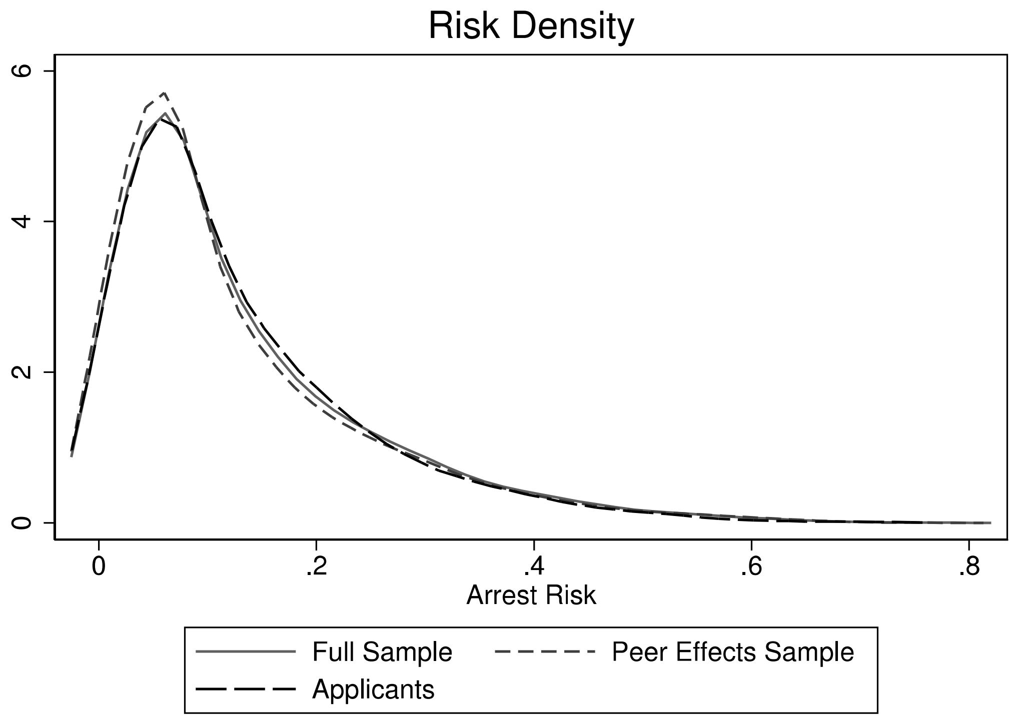
Notes: Estimated effect of winning choice on lottery applicant outcomes. Each is conditional on the set of student characteristics, characterstics related to lottery outcomes, and lottery fixed effects. Standardized math and reading scores in columns 1 and 2 are from up to three years after the lottery, or 6th, 7th, and 8th grade for most students. Absences and Suspension are from the 5 years post lottery, or 6th through 10th grade years. The last two column show outcomes related to graduation and dropout. *On Time Grad.* is a dummy variable for graduating within 7 years post lottery (12th grade year for on-time progression). *Dropout* is a dummy variable equal to one if the students 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 application choice by year level.

Table A8: Sample Counts

	Applicant Sample			Peer Effects Sample	
	All Applicants	Won	Lost	PE Sample	Mean Win Prop.
High & Low Risk					
Above Median	1,093	323	770	3,530	.055
Below Median	1,047	429	618	4,361	.035
By Risk Quintile					
Q5	378	107	271	1,401	.052
Q4	456	145	311	1,428	.06
Q3	524	167	357	1,453	.048
Q2	384	156	228	1,769	.037
Q1	398	177	221	1,840	.028
All Risk Categories					
Total N	2,140	738	1,385	7,891	.044

Notes: Number of observationsby sample and predicted risk categories. Last column includes the average of the win proportion for each subgroup, i.e., lottery wins over the group size.

Figure A1: Density of Arrest Risk



Notes: Kernel density of predicted arrest risk for the full sample, the lottery applicant estimation sample, and the non-applicant esimation sample. Predicted risk based on regression of a dummy variable for ever being arrested between ages 16-22 in our data on a set of individual, CBG, and Neighborhood School covariates.

Table A9: Arrest Prediction

	Fixed Effects	School and CBG Means
White	0.0352	0.0255
	(0.140)	(0.138)
Black	0.632***	0.595***
	(0.123)	(0.120)
Hispanic	-0.236*	-0.232*
	(0.143)	(0.141)
Lunch	0.702***	0.676***
	(0.0769)	(0.0749)
Math 5	-0.238***	-0.227***
	(0.0441)	(0.0430)
Read 5	-0.120***	-0.117***
	(0.0438)	(0.0426)
Math 5 Missing	-0.00821	-0.0363
	(0.272)	(0.257)
Read 5 Missing	0.321	0.290
	(0.259)	(0.245)
Age (Jan 1. Grade 5)	0.0246***	0.0242***
	(0.00460)	(0.00438)
School Means		
White		-0.0731
		(1.288)
Black		-1.120
		(1.304)
Hispanic		-1.162
		(1.389)
Lunch		0.851*
		(0.471)
Math 5		-0.249
		(0.672)
Read 5		-0.00698
		(0.607)
CBG Means		
White		0.465
		(0.553)
Black		0.904*
		(0.496)
Hispanic		0.186
		(0.569)
Lunch		0.614**
		(0.313)
Math 5		0.544**
		(0.233)
Read 5		-0.287
		(0.244)
Pseudo R-squared	0.137	0.111
Observations	12,716	13,493

Notes: Arrest prediction estimates from logistic regression with indicator for any arrrest age 16-22. First column includes neighborhood school and CBG fixed effects. The Second uses neighborhood school and CBG mean characteristics. In addition to the listed covariates, we include a dummies for missig birthdate and 5th grade exceptionality.