Better Schools, Less Crime?

Longer-Term Effects of School Choice in Charlotte-Mecklenburg

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September 2009

ABSTRACT

I estimate the effect of attending a first-choice middle or high school on young adult criminal activity, using data from school choice lotteries in Charlotte-Mecklenburg. The schools attended by lottery winners were higher quality according to measures of peer and teacher inputs, as well as revealed preference, and the gain was roughly equivalent to switching from one of the lowest quality schools to a school at the district average. Seven years after random assignment, lottery winners have been arrested for fewer and less serious crimes, and have spent fewer days incarcerated. The reduction in crime persists through the end of the sample period, several years after enrollment in the preferred school is complete. The effects are concentrated almost entirely among African-American males whose ex ante characteristics define them as "high risk". I estimate that giving priority to economically disadvantaged lottery applicants reduced crime by 12 percent, relative to a simple lottery like those implemented by many U.S. charter schools. The findings suggest that schools may be a particularly important setting for crime prevention policy.

Can improvement in the quality of public schools be an effective crime prevention strategy? Criminal activity often begins in early adolescence, and peaks when most youth should still be enrolled in secondary school (Wolfgang, Figlio and Sellin 1972, Farrington 1986, Sampson and Laub 1993, Levitt and Lochner 2001). Crime is concentrated among minority males from high poverty neighborhoods (Freeman 1999, Pettit and Western 2004, Raphael and Sills 2006). An influential literature on "neighborhood effects" links criminal activity to neighborhood disadvantage through peer interaction models (Sah 1991, Glaeser, Sacerdote and Scheinkman 1996), or processes of socialization and collective efficacy (Sampson, Raudenbush and Earls 1997).

Still, schools may be a particularly important setting for the onset of criminal behavior.[1] Urban schools in high-poverty neighborhoods have high rates of violence and school dropout, and struggle to retain effective teachers (Lankford, Loeb and Wyckoff 2002, Murnane 2008, Cook, Gottfredson and Na 2008). Only 35 percent of inmates in U.S. correctional facilities earned a high school diploma or higher, compared to 82 percent of the general population (Bureau of Justice Statistics, 2003). The best existing empirical evidence of the link between education and crime comes from Lochner and Moretti (2004), who use changes in compulsory schooling and child labor laws to estimate the effect of additional years of schooling on criminal activity. But the intensive margin of school quality is potentially much more relevant for policy. In a human capital framework, low-skilled youth will engage in crime early in life because of low anticipated returns to schooling (Lochner, 2004). If increased quality raises the return to investment in schooling, youth would stay in school longer, earn higher wages as adults, and commit fewer crimes.[2] Yet there is little evidence of the effect of school quality on crime.[3]

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[1] Since most public schools' assignment zones are defined by neighborhood, disentangling the separate influences of neighborhoods and schools is difficult. Jacob and Lefgren (2003) find that contemporaneous school enrollment leads to decreases in property crime but increases in violent crime, although their sample is not representative of large urban school districts.

[2]Additional compulsory schooling might accomplish the same goal, but the range of options for policymakers is limited. The minimum school leaving age is already 18 in 18 states, and enforcement of truancy laws is sporadic (Oreopoulos, 2008). Also, the population of "never takers" (i.e. youth who would always drop out of school at the same age regardless of the law) might be particularly important.

[3] Economic models of crime focus largely on changes in costs and benefits of crime for individuals on the margin of work and criminal activity (Becker 1968, Ehrlich 1975, Grogger 1995, Freeman 1999). A notable

In this paper I link a long and detailed panel of administrative data from Charlotte-Mecklenburg school district (CMS) to arrest and incarceration records from Mecklenburg County and the North Carolina Department of Corrections (NCDOC). In 2002, CMS implemented a district-wide open enrollment school choice plan. Slots at oversubscribed schools were allocated by random lottery. School choice in CMS was exceptionally broad-based. 95 percent of students submitted at least one choice, and about 40 percent chose a non-guaranteed school. Youth at higher *ex ante* risk for crime were actually more likely to choose a non-guaranteed school, allaying concerns about "cream-skimming" that might complicate the external validity of the findings (Epple and Romano 1998).

I estimate the causal effect of winning the lottery to attend a first-choice school on criminal activity through 2009, seven years after random assignment. Across various schools and for both middle and high school students, I find consistent evidence that winning the lottery greatly reduces crime. The effect is concentrated almost entirely among African-American males and youth who are at highest risk for criminal involvement. If I weight crimes by the best available estimate of their cost to society, attendance at a first choice school saves society at least $2,200 per participant, again with greater effects for youth who are at higher risk for crime. Lottery winners also showed immediate improvements on school-based behavioral outcomes such as absences and suspensions. There was no impact on test scores.

The pattern of effects is consistent with an increase in school quality that keeps youth enrolled in school longer and raises the opportunity cost of crime. Empirically, the treatment effects start small, peak at ages where both middle and high school applicants are in upper secondary school (around age 18), and decline in later years as crime-prone youth increasingly become incarcerated. Thus most of the reduction in crime occurs after enrollment in the preferred school is complete. By most measures, the changes in school and peer quality experienced by lottery winners were significant but modest, equivalent to moving from one of the worst schools in the district to a school of average quality. In sum, relatively modest improvements in school quality led to large and persistent reductions in criminal activity.

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exception is Lochner (2004), who examines the onset of criminal behavior in a life-cycle model of schooling, crime and work. A recent empirical paper by Weiner, Lutz and Ludwig (2009) finds a significant decline in homicide following school desegregation.

I also find strong evidence of heterogeneous treatment effects. I exploit the richness of pre-lottery administrative data and estimate the probability that a youth will be arrested in the future as a function of demographics, prior academic performance, behavior in school, and detailed neighborhood characteristics. The effect of winning admission to a preferred school on crime is strongly increasing in this *ex ante* prediction. Thus gains from targeting might be substantial. Although random assignment of slots to oversubscribed schools is an ideal research design, it may be suboptimal from a welfare perspective if treatment effects can be predicted on the basis of observable characteristics (Bhattacharya and Dupas, 2009). I simulate the effect of allocating slots on the basis of ex ante crime risk rather than at random, and I find that this would reduce the social cost of crime by an additional 27 percent. While this allocation method is controversial (and in the case of race, illegal), it was executed at least in part by CMS, which gave a "priority boost" in the lottery to applicants who met an income standard based on eligibility for free or reduced price school lunches. I estimate that this priority boost lowered crime by 12 percent, relative to a lottery without priority groups such as the ones typically administered by US charter schools.

This paper adds to the growing body of evidence of the benefits of school choice (e.g. Hoxby 2000). It also adds to the literature on the longer-term impacts of schooling interventions (see e.g. Krueger and Whitmore 2001, Garces, Thomas and Currie 2002, Belfield et al 2006, Deming 2009, Deming et al 2009). One consistent finding that emerges from this literature is the lack of correspondence between immediate test score gains and later outcomes of direct interest. Since most programs (and increasingly, teachers) are evaluated on the basis of test scores, this lack of connection is troubling. The results suggest that behavioral measures may be equal in importance to test scores as predictors of long-term success, particularly for the high-risk males who are most affected by school choice in this setting. Indeed, in a setting that is most similar to the one here, Cullen, Jacob and Levitt (2006) find no impact of school choice on test scores but some benefits on behavioral outcomes, including self-reported criminal activity. Finally, this paper adds to the body of empirical evidence that links early education to future criminality (Garces, Thomas and Currie 2002, Lochner and Moretti, 2004, Belfield et al 2006, Weiner, Lutz and Ludwig 2009).

Although more research is needed to disentangle the relative contributions of neighborhoods and schools, this paper provides some evidence that schooling exerts a particularly strong influence on criminal behavior. The Moving to Opporunity (MTO) Demonstration found mixed impacts on crime (Ludwig, Duncan and Hirschfield 2001, Ludwig and Katz 2005). MTO changed both neighborhoods and schools, although the change in measured school quality was not particularly large (Sanbonmatsu et al 2006). In contrast, the CMS open enrollment plan can be thought of as a pure school mobility experiment. Lottery winners continue to live in the same neighborhoods as lottery losers, and yet the reduction in crime persists even after schooling is largely complete.

Under relatively conservative assumptions, the implementation of open enrollment by CMS led to a reduction of over $6 million in criminal victimization costs and another $500,000 in incarceration, relative to a neighborhood schools model. Furthermore, these figures do not consider the private benefits to lottery winners. However, any welfare calculation must also account for the possible negative externality imposed by lottery winners on their new peers. Estimates from the literature suggest such spillovers are likely to be small in relation to the treatment effects, and ambiguous in sign depending on the nature of peer effects (Angrist and Lang 2004, Hoxby and Weingarth 2006, Carrell and Hoekstra 2009, Imberman, Kugler and Sacerdote 2009).[4] Still, because of the large one-year change in student assignments in CMS in the year of the choice plan, general equilibrium extrapolations of the effects estimated here should be viewed with caution.

2. Data Description and Institutional Detail

2.1 Data

With over 150,000 students enrolled in the 2008-2009 school year, Charlotte-Mecklenburg is the 20th largest school district in the nation. The CMS attendance area encompasses all of Mecklenburg County, including the entire city of Charlotte and several surrounding cities. Since the mid 1990s, the North Carolina Department of Public

[4] Carrell and Hoekstra (2009) estimate the negative externality caused by children from families that are exposed to domestic violence and find that adding one of these children to a class of 20 causes each other child to commit 0.093 more infractions. Importantly, they find that the spillover effects on misbehavior are larger for low-income peers, which implies that concentrations of troubled students will generate more disruption. Since the net effect of open enrollment in CMS was to distribute high-risk children across more schools than what would have happened in a pure neighborhood schools model, the total externality effect on misbehavior (and possibly crime) could be positive even lottery winners' peers were negatively affected.

Instruction (NCDPI) has required all districts to submit a set of end-of-year (EOY) files that include demographic information, attendance and behavioral outcomes, yearly test scores in math and reading for grades 3 through 8, and subject-specific tests for higher grades. This reporting system ensures that variables are consistently coded across years and that students are tracked longitudinally from kindergarten through 12th grade. Internal CMS files obtained under a data use agreement also include identifying information such as name and date of birth, and students' exact addresses in every year, which I use to create detailed geographic identifiers. For more details on the nature and quality of the CMS administrative data, see the Data Appendix.

I match CMS administrative data to arrest records from the Mecklenburg County Sheriff (MCS).[5] I obtain these arrest records directly from the MCS website, which maintains an online searchable database that covers arrests in the county for the previous three years, counting from the day the website is accessed.[6] The data include all arrests of adults (age 16 and over in North Carolina) that occurred in the county, even if they were handled by another agency. The unit of observation is the arrest, but criminals are tracked across incidents using a unique identifier that is established with fingerprinting. Each observation includes the exact nature of the charges, information about arrest processing (such as the arresting agency, whether there was an outstanding warrant, and the bond amount), and current address. Critically, each observation includes the name and date of birth of the criminal.

The match was done using full name and date of birth, and was exact in about 87 percent of cases. I obtained the remaining matches using an algorithm that assigns potential matches a score based on the number and nature of differences.[7] See the Data Appendix for details. I conducted a number of checks to assess the quality of the match. First, since each arrest is given a unique identification number that is assigned consecutively in the order it was processed, I can calculate the fraction of arrest numbers

[5] Since CMS is a "unified" school district, the geographic coverage of the school administrative data and the arrest records is identical.
[6] The web address is http://arrestinquiryweb.co.mecklenburg.nc.us/. I obtained the data by writing a script that loops over arrest number in consecutive order and copies the relevant information into a text file. See the Data Appendix for details.
[7] As a specification check I ran the partial match algorithm a number of different ways, and I also estimated all the results in the paper using exact matches only. This made little difference. See the Appendix for details.

that are missing from the data. Counting from the first day that the data were collected, this fraction is only 3.2 percent, and there are no large gaps. This suggests that nearly every arrest processed by MCS is present in the data.[8] Figure 1 plots the age profile of arrests in Mecklenburg County by type of offense. The Federal Bureau of Investigation (FBI) collects data on eight different "index" crimes for the Uniform Crime Reporting (UCR) Program, which covers law enforcement agencies across the country (FBI, 2009). Index property crimes are burglary, motor vehicle theft and felony larceny. Index violent crimes include murder/manslaughter, rape, robbery and aggravated assault.[9] The last category I include is felony drug offenses, which (based on weight) range from "possession with intent to distribute" all the way up to "trafficking". Index property and violent crimes peak at ages 17 and 18 respectively, which is consistent with other cohort studies of crime and delinquency (Wolfgang, Figlio and Sellin 1972, Farrington 1986, Sampson and Laub 1993). Interestingly, drug felony arrests do not peak until the early to mid twenties, and decline much more slowly with age than other categories of crime. The final panel of Figure 1 plots the social cost of arrested crimes by age, using estimates from Miller, Cohen and Wiersema (1996). They include victimization costs such as lost productivity and quality of life, adverse health effects and property damage, but not incarceration or other criminal justice-related costs. Since the vast majority of social costs come from fatal crime, the peak at age 20-22 of about $125 million coincides with the peak age of murder. Violent crimes have a much higher social cost than property crimes according to this methodology.

In the top panel of Table 1 I examine arrest rates of CMS attendees overall and by demographic group. I use six school cohorts of data, corresponding to students in grades 6 through 11 in 2002 and age 17 to 23 in 2009. The first and second rows show the fraction of CMS attendees who have a criminal record, and who have at least one felony arrest respectively, by race and gender. Not surprisingly, arrest rates vary dramatically, from about 34 percent for African-American males to about 3 percent for White or Asian females. In rows three through five I show arrest rates by type of crime. African-

[8] Most of the missing arrests have been expunged, and there is a slight increase in the number of expunged arrests in earlier years.
[9] The eighth crime is arson. The incidence of arson is very low in these data, so I do not include it here.

American males are about 6 times more likely than white males to have at least one felony arrest, and about 13 times more likely to be arrest for an index violent crime.

In the bottom panel of Table 1, I examine the percentage of arrests that are successfully matched to a CMS student by birth year and demographic group. Unmatched arrests could be students who were enrolled in private school, youth who travel to Mecklenburg County from elsewhere to commit crimes, or poor data quality. Match rates are highest for African-Americans (who are more likely than whites to attend public school) and for more recent birth years.[10] Since the CMS data only go back to the 1996-1997 school year, any student who left the district before that would not be matched. Since most criminals are high school dropouts, this is likely to result in fewer matches for the earliest birth cohorts. However, the weighted average match rate by birth year for the lottery sample exceeds 85 percent overall and 90 percent for African-American males. This high match rate is strong evidence of the quality of the data. It also highlights the important role that public school policies might play in city crime rates.

Since the CMS open enrollment plan began in 2002, some older members of the sample could have been arrested prior to 2006, when the arrest data begin. To address this issue directly, I also obtained historical arrest records directly from MCS for members of the lottery sample only.[11] Finally, I add incarceration records from the MCS jail system and the North Carolina Department of Corrections (NCDOC). County jail records are available only beginning in 2006, while the NCDOC state prison data go back to 2002 and earlier. The data include number of days incarcerated, but probation and parole records are not included. See the Data Appendix for details.

2.2 Policy Context and Open Enrollment in CMS

From 1971 until 2001, CMS schools were forcibly desegregated under a court order. Students were bused all around the district to preserve racial balance in schools. After several years of legal challenges, the court order was overturned, and CMS was

[10] Illegal aliens who are arrested by Immigration and Naturalization Services (INS) in North Carolina are often processed in Charlotte before they are sent to Atlanta and Deported (MCS, 2009). This, along with the transient nature of the Charlotte's rising Latino population, accounts for the very low match rate among Latino arrestees.

[11] These data were recorded in almost exactly the same format as the more recent arrest records, although I cannot check their quality as easily. See the Data Appendix for details.

instructed that it could no longer decide student assignments on the basis of race. In December of 2001 the CMS School Board voted on a policy of district-wide open enrollment for the 2002-2003 school year. School boundaries were redrawn as contiguous neighborhood zones, and children who lived in each zone received guaranteed access to their neighborhood school. The one-year change in student assignments was dramatic – about 40 percent of students at the middle and high school level were assigned to a different school than in the previous year. Because the inner city of Charlotte is both denser and more highly segregated, African-American and poor students were even more likely to be reassigned.

The open enrollment lottery took place in the spring of 2002. CMS conducted an extensive outreach campaign to ensure that choice was broad-based, and 95 percent of parents submitted at least one choice (Hastings, Kane and Staiger, 2008). Parents could submit up to three choices (not including their neighborhood school). Students were guaranteed access to their neighborhood school, and admission for all other students was subject to grade-specific capacity limits that were set by the district beforehand but were unknown to families at the time of the lottery (Hastings, Kane and Staiger 2008). When demand for slots among non-guaranteed applicants exceeded supply, admission was allocated by random lotteries that occurred within the following lexicographic priority groups:

1) Students that attended the school in the previous year and their siblings;
2) Free or reduced price lunch eligible (i.e. low income, "FRPL") students applying to schools where less than half of the previous year's school population was FRPL;
3) Students applying to a school within their own "choice zone".[12]

Applicants were sorted by priority group according to these rules and then assigned a random lottery number. Slots at each school were first filled by students with guaranteed access, and then remaining slots were offered to students within each priority group in order of their lottery numbers. CMS administered all of the lotteries centrally and applied

[12] CMS divided schools into 4 "choice zones" and guaranteed transportation for students who applied to a school within their zone. This included magnet schools. The zones were constructed so that there was an even mix of mostly white "suburban" and mostly black "inner city" schools in each zone. In practice, this priority group was rarely used since very few students applied outside their choice zone.

an algorithm known as a "first choice maximizer" (Abdulkadiroglu and Somnez, 2003). While this type of mechanism is not strategy-proof, Hastings, Kane and Staiger (2008) find little evidence of strategic choice by parents.

Under busing schools were racially balanced, but the surrounding neighborhoods remained highly segregated. Thus the redrawing of school boundaries led to concentrations of minority students in some schools. Students who were assigned to these schools attempted to get out of them. Figure 2 displays the strong correlation between the racial composition of a school's neighborhood zone and the percent of students assigned to it who choose not to attend. Table 2 presents the average characteristics of lottery applicants compared to all CMS students. Unlike many other instances of school choice, applicants to non-guaranteed schools are negatively selected compared to students who choose their neighborhood school. They have lower average test scores, are more likely to be nonwhite and FRPL, and have been suspended and absent more days in the previous school year. Among non-guaranteed applicants, the lottery sample is slightly positively selected, mostly because of the "priority boost" given to economically disadvantaged applicants.[13]

I begin with the full sample of middle and high school applicants. Since nearly all rising 12th graders received their first choice, I restrict the analysis sample to grades 6 through 11. Next I limit the sample to students who were enrolled in any CMS school in the previous year, which excludes about five percent of applicants. These students were much less likely to be enrolled in CMS in the following fall. Since previous enrollment was fixed at the time of the lottery, this restriction does not bias the results. The analysis sample consists of 21,132 high school students and 22,896 middle school students. About sixty percent of these students chose (and were automatically admitted to) their neighborhood school first. Of the remaining sample, about 75 percent were in lottery priority groups where the probability of admission was either zero or one. Even though these students chose a non-guaranteed school, there is no random variation in admission to exploit. Thus the final lottery sample consists of 1,891 high school students and 2,320 middle school students.

[13] Because of the separate priority group assigned to FRPL students who apply to non-FRPL schools, most schools either had lotteries for them and denied everyone else, or automatically admitted them and had lotteries for non-FRPL students.

3. Empirical Strategy

If lottery numbers are randomly assigned, the winners and losers of each lottery will on average have identical observed and unobserved characteristics. Thus with sufficient sample size, a simple comparison of mean outcomes between winners and losers would identify the causal effect of winning each individual lottery. However, the sample here is not large enough to estimate the effect of winning each individual lottery. Instead, following Cullen, Jacob and Levitt (2006), I estimate ordinary least squares regressions of the form:

$$Y_{ij} = \delta(\textit{Win Lottery})_{ij} + \beta X_{ij} + \Gamma_j + \varepsilon_{ij} \qquad (1)$$

Y_{ij} is the outcome variable of interest for student i in lottery j. $\textit{Win Lottery}_{ij}$ is an indicator variable equal to 1 if student i in lottery j had a winning lottery number. X_{ij} is a vector of covariates included only for balance, Γ_j is a set of lottery fixed effects and ε_{ij} is a stochastic error term. I consider only students' first choice here, so the number of observations is equal to the number of students in the lottery sample. In principle one could estimate a nested model that incorporates multiple choices. However, in practice nearly every student who did not receive their first choice was either automatically admitted to their second choice (if it was not oversubscribed) or automatically denied since all the slots were already filled.

Lottery fixed effects are necessary to ensure that the probability of admission to a first-choice school is uncorrelated with omitted variables in the error term. If, for example, savvy parents had some prior knowledge about the chance of admission, they might (all else equal) apply to schools where the probability of acceptance was higher. Thus comparing winners and losers across different lotteries might lead to a biased estimate. In the specification in equation (1), the δ coefficient gives the weighted average difference in outcomes between winners and losers across all lotteries, with weights equal to the number of students in the lottery times p*(1-p) where p is the probability of admission (Cullen, Jacob and Levitt, 2006). Thus δ represents the intention-to-treat (ITT) effect of winning admission to a first-choice school for students in priority groups with

non-degenerate lotteries. I cannot estimate the effect of attending a school for students with guaranteed access.

If the lotteries were conducted correctly, there should be no difference between winners and losers on any characteristics that is fixed at the time of application. I test this directly by estimating equation (1) with pre-treatment covariates such as race, gender and prior test scores as outcomes. The results are in the Appendix – for both the middle and high school samples, as well as important subgroups such as African-American applicants, the differences between winners and losers are small and no more likely to be statistically significant than what is expected by chance. This is strong evidence that initial assignment was random. Even with proper randomization, however, the estimates could still be biased by selective attrition if leaving CMS or Mecklenburg County is correlated with winning the lottery. More than 95 percent of lottery applicants in the spring of 2002 were enrolled in a CMS school the following fall, and there was no difference by lottery status. Still, since high school dropout rates are extremely high for crime-prone youth, selective attrition is a serious concern for outcomes that come from the CMS administrative data. Students who drop out of school and are subsequently arrested in Mecklenburg County, however, are included in the data. Thus the main issue is selective migration. If lottery losers are more likely to leave Charlotte and attend school elsewhere, they may commit crimes in other jurisdictions. This would bias estimates downward. On the other hand, lottery winners may perform better in school and be more likely to leave the county to go to college, for example. This would bias the estimates upward.

Still there are a few reasons to think that selective migration is not much of a concern here. First, the population of crime-prone youth is not very mobile. Attrition in grades K through 8 (where dropout is less of an issue) is negatively correlated with other predictors of crime and is much lower than average for future criminals.[14] Second, CMS assigns a withdrawal code to students who leave the district, and lottery status is uncorrelated with the code for out-of-county transfers. Additionally, the NCDOC state prison data includes information on county of arrest. Less than one percent of the sample

[14] 91 percent of future felons who were enrolled in CMS in 4th grade were still enrolled 4 years later (what would have been their 8th grade year). The overall average is 80 percent.

spent time in state prison for offenses committed outside of Mecklenburg County, and there is no difference between lottery winners and losers.

3.1 Early Predictors of Crime

In addition to reporting overall results, I examine heterogeneous treatment effects by exploiting the unusually long and rich panel of administrative data from CMS. Students with adult arrest records can be tracked all the way back to kindergarten in some cases, with yearly information on test scores and behavior and detailed neighborhood measures. I combine all of the individual correlates of criminal behavior into a single index and plot the treatment as a function of this *ex ante* crime risk. I estimate the probability that a student will have at least one arrest as a function of demographic variables, yearly math and reading test scores, absences, out of school suspensions, and special education classifications. For the high school sample I use data from grades 6 through 8, and grades 3 through 5 for the middle school sample.[15] I allow for third order polynomials in all of the continuous measures. Finally, I include neighborhood school zone by census tract fixed effects, using each student's exact address in the year prior to open enrollment. See the Appendix for more details on the estimation and for regression coefficients from this prediction. In general, these prior measures are strong predictors of future criminality.[16] Figure 3 plots the density of predicted criminality for all CMS students grades 6-11, then for African-American males overall and from the seven lowest-performing schools (defined by average test scores) in the district. The distribution shifts rightward noticeably for these "high risk" subgroups. In the main estimates in Section 4, I control for this prediction as a sufficient statistics for crime risk, in lieu of including all of the individual covariates (Angrist and Hahn, 2004). I also examine heterogeneous treatment effects by flexibly interacting the prediction with the lottery treatment indicator:

[15] In North Carolina, standardized End-of-Grade (EOG) tests in math and reading are administered for grades 3-8 only. While additional years of data would improve the precision of the estimates, it would also increase the percentage of respondents with missing data.

[16] The pseudo R-squared from the regression is about 0.23, compared to 0.24 when high school graduation is the dependent variable. Joint tests for the significance of each type of coefficient yield chi-squared values of 147 for test scores, 471 for behavior, and 249 for neighborhood fixed effects.

$$Y_{ij} = \delta(WinLottery)_{ij} * f(\Pr(crime)_{ij}) + \Gamma_j + \varepsilon_{ij} \quad (2)$$

The exact specification is the lottery indicator interacted with a sixth order polynomial in the *ex ante* crime prediction plus main effects, lottery fixed effects and a stochastic error term. I estimate equation (2) for a series of outcomes and then plot the treatment effects and associated confidence intervals against values of the crime prediction. An alternative specification would be some sort of nonparametric matching estimator, but the estimates are not precise enough without matching across lotteries, which would compromise the randomization. As a check on the pattern of results, I pursue a simpler alternative to matching by reestimating the main results and restricting the sample to some "high risk" cutoff.

3.2 The Effect of Winning the Lottery on Enrollment and Measures of School Quality

Table 3 presents the effect of winning the lottery on enrollment and school characteristics. Each estimate is the δ coefficient from a regression like equation (1). Below each estimate I report standard errors as the maximum of Huber-White clusters at the individual lottery (i.e. choice by grade by priority group) level, and block bootstrapped standard errors, again with individual lotteries as clusters (Angrist and Pischke, 2009). I also follow this rule in subsequent tables. Columns 1 and 2 show results for the full sample, and Columns 3 and 4 for African-American males only; Columns 5 through 8 repeat the same pattern but for middle schools. The first row shows the effect of winning the lottery on attendance at a student's first choice school on the 20th day of the 2002 school year. Overall, the first stage is strong – lottery winners are about 55 percentage points more likely to attend than losers, and the results are slightly larger for African-American males. The coefficient is less than one mainly because some lottery losers successfully enroll in their first choice anyway. This can be accomplished in several ways. First, the student could have moved into the school's neighborhood zone in the summer of 2002, after losing the lottery. Second, some lotteries were for special programs within schools, so the student might have been denied admission to the special program but accepted to the regular school. Finally, some students may have been admitted at the beginning of the school year when lottery winners did not enroll. These

"always-takers" (in the language of Angrist, Imbens and Rubin 1996) are positively selected on observables – they have higher baseline test scores, for example, than either treatment or control compliers. Similarly, "never-takers" are negatively selected compared to control compliers. Although I report ITT results here, I could also calculate local average treatment effects (LATEs) for compliers using lottery status as an instrument for enrollment. The second row shows the effect of winning the lottery on attendance at the student's neighborhood school, which is highly negative for all groups. Rows 3 through 5 show the effect of winning the lottery on the racial and family income composition of the school and on distance to assigned school. High school lottery winners attend schools that are demographically very similar to the schools attended by lottery losers, while middle school winners attend schools that are whiter and higher income on average. All lottery winners travel farther to attend their first choice school, but the effect is larger for high school students.

The remaining rows of Table 3 show the effect of winning the lottery on various measures of school quality. I normalize each of these measures to have mean zero and standard deviation one, to make them comparable to each other. Overall, lottery winners attend schools that are better on nearly every dimension. In general, the gain in quality for African-American males is very similar to the overall sample, even though the control group attends significantly worse schools, as indicated by the control means in each odd-numbered column. The first measure is average peer test scores in the year prior to the lottery. It is calculated as the average of the prior school year's standardized math and reading scores for students in the same grade, with sample members excluded from the calculation. The second measure is calculated the same way, but is the standardized average of absences and out-of-school suspensions for the student's peers in the year prior to the lottery. For high school lottery winners, the gains in average peer behavioral outcomes are much larger than gains in peer test scores (0.41 versus about 0.12 standard deviations). Overall, middle school lottery winners experience bigger gains in academics, but for African-American males the gains in test scores and behavior are both about 0.22 standard deviations. The third measure of school quality is a standardized composite of two teacher characteristics that have been shown to affect student performance – the percentage of novice (less than 3 years experience) teachers in a school, and the school's

yearly rate of teacher turnover.[17] Lottery winners attend schools that rate significantly higher on these measures of teacher quality. Again the gains are particularly large (about 0.4 standard deviations) at the high school level.

The fourth measure of school quality is an indicator of revealed preference for each school, based on the choices made by all the students in CMS in the year of the open enrollment plan. I estimate a conditional logistic regression that predicts the probability that students will choose each school (separately for the middle and high school samples), controlling for a polynomial in distance from each student's address to each school, plus home school fixed effects. The school-level residual from this regression is interpretable as an indicator of revealed preference.[18] Including distance and home school fixed effects controls for the fact that some schools might be more highly demanded because they are located in denser parts of the district. Since lottery schools are oversubscribed by definition, it is not surprising that winners are enrolled in more highly demanded schools. The effect is bigger for high school than for middle school (about 0.6 versus 0.3 standard deviations) and is slightly larger for African-American males in both groups. Finally, we can see that lottery winners are more likely to be enrolled in magnet schools, particularly at the high school level. Magnet school enrollment comprises a larger share of the treatment in the high school sample, mostly due to the opening of a new magnet high school (Philip Berry Academy of Technology, a "career academy" that focuses on vocational and technical education) in the 2002-2003 school year.

In sum, lottery winners attend schools that are significantly better on several observable dimensions of quality. If school quality were normally distributed through CMS, then winning the lottery results in modest average quality gains of between 0.2 and and 0.3 standard deviations for middle school students and 0.1 to 0.6 SDs for high school students, depending on the outcome. Based on the control means in Table 3, lottery losers attend schools that are slightly worse than the district average, and winners attend schools that are slightly better. For African-American males, the gain is of similar size but

[17] Other measures were available, such as the percentage of teachers with advanced degrees, but I did not include them since most evidence suggests they do not affect student performance.
[18] See Long (2004) for a more detailed explanation of the conditional logit setup for college choice, and Hastings et al (2008) for a richer specification of preferences in this setting that uses all 3 choices and allows for substitution patterns, in a mixed logit framework.

winning the lottery gets them into schools that are closer to the district average. This is important to keep in mind when we consider the results in Section 4.

4. Results

4.1 Crime

The main results of the paper are in Table 4. The layout is similar to Table 3, with ITT estimates overall and for African-American males in the first four columns for high school and columns 5 through 8 for middle school. The outcomes are measures of criminal activity from June of 2002 until June of 2009, seven years after randomization, when sample members are between 17 and 23 years of age. All estimates are calculated using ordinary least squares regressions like equation (1).[19] The first two rows show the effect of winning the lottery to attend a better school on the overall number of arrests and arrests with at least one felony charge respectively. High school lottery winners are less likely to be arrested, and the effect is stronger for felonies. African-American male lottery winners are arrested 0.22 fewer times, relative to the control mean of about 0.38 felony arrests. The effect on arrests for middle school lottery winners is much weaker. One thing to note is the very high rate of arrests – the control group averages about 0.85 and 0.95 arrests per person for high school and middle school respectively. Since high school students have had more years to accumulate arrests, this suggests that middle school lottery applicants are a higher risk group.

In the next four rows I report results for overall felony charges and by the three major charge types – index property crimes, index violent crimes, and drug felonies. Here the dependent variable is charges rather than arrests. If someone is arrested once for seven counts of burglary, for example, this counts as seven rather than one.[20] High school lottery winners commit fewer of all three types of crime, although the effect is much larger for drug felonies. Middle school lottery winners are charged with slightly more crimes overall, but they substitute toward property crimes and away from violent crimes.

[19] I experimented extensively with alternative specifications, such as poisson and negative binomial regression (to account for overdispersion). The results, which are qualitatively similar across different specifications, are in the Data Appendix. I also estimated probit models with dummy variables for ever having been arrested/charged with crimes of each type in Table 5. These results are in the same direction but weaker, suggesting that much of the treatment effect operates along the intensive margin of criminality.

[20] Often there will be an outstanding warrant for an arrestee and they are processed on multiple charges at the same time.

For African-American males, both effects are significant and large (about 0.15 charges) but in the opposite direction. Since violent crimes are more socially costly than property crimes, this substitution leads to large decreases in the accumulated social cost of crime among lottery winners. I calculate social cost in two ways. First, I directly follow the cost estimates produced by Miller, Cohen and Wiersema (1996) and used in analysis of the influential Moving to Opportunity Demonstration by Kling, Ludwig and Katz (2005). These cost estimates are dominated by fatal crimes (the estimated social cost of murder is about $4.3 million in 2009 dollars), and since more murders were committed by the control group than the treatment group (5 versus 1 in the combined high and middle school samples), these estimates are very large. The results in columns 2 and 6 indicate that enrollment in a first choice school led to average cost reductions of over ten thousand dollars per lottery applicant in both samples. The estimates are even larger (31 and over 40 thousand for high and middle schools respectively) when the sample is restricted to African-American males. In the second set of cost estimates I trim the cost of murder to be twice that of the next most costly crime (rape), again following Kling, Ludwig and Katz (2005). The estimates become much smaller but also more precise. For African-American males in the high school lottery sample, the per-applicant dollar value of crime reductions is $4,336 in the high school sample and $6,081 in the middle school sample. This reflects large decreases in violent crimes such as armed robbery, aggravated assault, and murder.[21]

The final set of estimates weighs crimes by the expected punishment that results from a successful conviction. In 1994 the state of North Carolina enacted the Structured Sentencing Act. Under structured sentencing, felony convictions are grouped into "classes" based on severity. This information is combined with the offender's prior record and other circumstances to determine a range of possible sentence lengths available to the judge. I group felony charges according to their class and assign the midpoint of the range of sentences for each of them. Lottery winners in both samples

[21] Miller, Cohen and Wiersema do not include social cost estimates for drug crimes. Following Kling, Ludwig and Katz (2005), I assign costs to drug crimes according to felonies of equivalent standing. If instead I set the cost of drug crimes to zero, the estimates fall by about 25% in the high school sample (but are still statistically significant) but are unaffected for middle schools. This comes mostly from a large difference in the incidence of drug trafficking charges across treatment and control high school students (there were 16 drug trafficking charges in this sample, of which 14 occurred in the control group).

commit less serious crimes by this metric, but the results are much more precisely estimated for the middle school sample. For African-American males in the middle school sample, attendance at a preferred school leads to an expected decrease of about 13.6 months in sentence severity, from a control baseline of about 21 months. The decrease is about 7.5 months for the high school sample, but with a standard error of 6.3. In the last row I show the effect of winning the lottery on actual prison time served. Unlike the arrest records, these data are only available from 2006 onward, and only for the subset of African-American males. Middle school lottery winners spend about 13.6 fewer days in prison, relative to a control complier mean of 22.4 days. There is no difference in prison time served for high school lottery winners.

4.2 School Quality and the Dynamics of Crime

Overall, I find strong evidence that winning the lottery to attend a first choice middle or high school reduces young adult criminal activity. The effects are concentrated almost entirely among African-American males. Depending on the student's grade, the treatment consists of between one and four years of guaranteed enrollment in an oversubscribed public school. Because the arrest data continue for seven years after randomization, they include several years after enrollment is complete, including postsecondary years for older lottery applicants. Thus an examination of the dynamics of the lottery treatment will lend insight into the mechanisms. If, for example, the effect of attending a better school operates through differential exposure to crime-prone peers, or concentration of drug market activity in a few schools, we might expect the treatment to be large initially but fade over time, as winners and losers are no longer enrolled in school and reside in similar neighborhoods.

On the other hand, an increase in school quality might generate decreases in crime that persist long after enrollment is complete. In a human capital framework, attendance at a better school would raise the marginal productivity of schooling investment. Youth who attend a better school would stay enrolled longer and acquire more skills, which would translate into a higher expected wage in the labor market. Higher wages raise the opportunity cost of crime and incarceration, lowering the optimal amount of crime

(Lochner, 2004). To the extent that skills acquired in school have a persistent effect on wages, reductions in crime would also be persistent.

I separate the effect of winning the lottery into three periods of time. Table 5 shows ITT estimates for the main outcomes in Table 4, separated into the years that applicants are ages 16-17 (roughly 9th-11th grade), 18-19 (11th grade to 1 year after high school), and 20 and older (post-high school). Since most of the effects estimated in Table 4 came from African-American males, I restrict the analysis to that sample in Table 5. Because lottery applicants began the 2002-2003 school year in different grades, not all entering grades appear in each category at all, or for the same amount of time. For this reason I scale the estimates and control means so that the treatment effects for each period are per-two-years, and comparable in size to each other.

For both high and middle school applicants, the effects of winning the lottery on crime are biggest in the upper secondary school years, at ages when many youth are mixing schooling, crime and work in some combination (Grogger, 1995). High school lottery winners are arrested for about 0.25 fewer felony charges and commit fewer of all types of crime, although the effect is individually significant only for drug felonies. The significant reduction in drug felonies continues into the post-high school years as well. None of the effects on crime severity measures such as social cost and sentence length are individually significant, although they are negative for the 2nd and 3rd periods. Data on incarceration are unfortunately unavailable for earlier periods in the high school sample.

For middle school lottery winners, all 3 periods are post-treatment. Nevertheless, the effects increase over time. Index violent crimes are lower in all 3 periods, but the effect is much larger in the 2nd period (-0.113 charges relative to a control complier mean of 0.137). Similarly, the reductions in social cost and expected sentence length are large in the 2nd period. Interestingly, lottery winners spend fewer days in prison in all 3 periods, but the effect is much larger (-15 days versus -4.5 and -1.5 in periods 2 and 1) for the post-high school period. This may help to explain the positive (though insignificant) increase in crime in period 3 – the most serious offenders, who come disproportionately from the control group, are incarcerated. Despite this, the effect on social cost is largest in period 3, mostly because this is when all of the murders happened.

In Table 6 I limit the sample further to the upper tercile of the ex ante crime prediction described in Section 3.1. Overall, the pattern of results is similar but much stronger for these "high risk" youth. The effect on felony charges for high school lottery winners is particularly strong in the age 18-19 years (0.79 fewer charges from a control mean baseline of 0.81). The point estimate is negative for all three crime types, but is largest for index property crimes and most precisely estimated for drug felonies. The crime severity estimates start out slightly positive but become more negative over time for the high school sample, although there is no effect on incarceration. High risk middle school lottery winners are arrested on fewer felony charges in all three periods. However, the effect is concentrated in index violent crimes. The effect on measures of crime severity grows strongly over time as well, and the effect on incarceration is particularly striking. High risk lottery winners spend over 50 fewer days incarcerated (off of a control mean baseline of 52) in the age 20 and over period.

One key limitation of this analysis is that I do not observe juvenile crime. Although Tables 5 and 6 show that the effect of attending a preferred school on crime grows over time, the lack of early data could mask big differences in juvenile offending in the early years of the treatment. As an alternative, I estimate the effect of winning the lottery on school disciplinary outcomes such as absences and suspensions. The results are in Table 7. The sample is restricted to African-American males. The even numbered columns present the same set of estimates but on a subsample of "high risk" youth, again defined as approximately the upper tercile of the *ex ante* crime prediction discussed in Section 3.1. The first 3 rows show the effect of winning the lottery on unexcused absences in the first 3 school years after the treatment, and the next 3 rows show the same thing but for out-of-school suspensions.[22] Overall, lottery winners in both samples spend more days in school. All six point estimates (2 samples, 3 years) for absences are negative, although they are not always statistically significant. The effects are generally larger for high risk youth. The effect of winning the lottery on school suspensions is also mostly negative, though it is much larger in the high school sample.

[22] Absence due to school suspension is considered an "excused" absence, so there is no overlap between the two categories.

One difficulty with interpreting effects on school suspensions is that schools may differ in their discipline policies. If, for example, one way a "better" school maintains order is to enforce rules strictly, then lottery winners might be more likely than losers to get suspended for equivalent behavior. This is particularly likely to be an issue in middle school, where suspensions are more common and more likely to be issued to "good kids" as a deterrent to future bad behavior. More directly, schools that succeed in keeping crime-prone youth in school longer may invest more resources in monitoring their behavior, whereas "bad" schools might simply allow them to drop out. I provide some limited evidence on this point by drawing from a detailed disciplinary incident file maintained by CMS beginning in the 2006 school year. The file contains information on the date and nature of each disciplinary infraction, as well as the resulting punishment. Because the data are only available beginning in 2006, I cannot look at incidents for the high school sample at all or for any of the treatment years in the middle school sample. I find that, conditional on still being enrolled in a CMS school, middle school lottery winners are in-school suspended about 0.3 *more* days in high school, but about 3 percentage points less likely to be involved in an incident where the punishment was long-term suspension, expulsion, or police involvement. The effects are very large and are highly concentrated among "high risk" youth, who are 23 percentage points less likely to face serious disciplinary action. Because all of these incidents occur in high school, it cannot be due to variation in treatment school policies, but it may be due to those schools' success in keeping crime-prone youth enrolled.

The final set of estimates in Table 7 examines the effect of winning admission to a preferred school on test scores and enrollment. The measure of test scores is a standardized math exam administered yearly to grades 3-8 (results for reading are very similar). Middle school lottery winners show no significant test score gain in either year, although the standard errors are too imprecise to rule out small differences in either direction. No such test is available for the high school sample – although high schools do offer standardized end-of-course exams in subjects such as Algebra and Geometry, selection into test-taking compromises the interpretation of these outcomes. In any case, there is no evidence of gains for any subject exams among high school lottery winners. The next six rows look at various measures of school enrollment. The grade enrollment

measures classify respondents as enrolled if they are present in CMS *in the year that they would have been in each grade if they progressed "on time"*. For example, rising 6th grade lottery applicants would be enrolled in 9th grade in the 2005-2006 school year, so if they are still enrolled in CMS at the end of 2006 they are counted, even if they are not in grade 9. An alternative would be to estimate final grade attainment – however, we might think that keeping youth in school longer lowers crime even if it does not increase grade attainment. Grade attainment results and other alternative specifications (with qualitatively similar findings) are in the Data Appendix. Overall, I find some evidence that "high risk" middle school lottery winners stay in school longer. The estimates for grades 9-12 are all positive, and they are relatively large and borderline significant for grades 9 and 10 (10.1 and 13.8 percentage points respectively). I find similar but weaker results for grades 11 and 12 in the high school sample, and no impact on high school graduation.[23] The final outcome is a measure of "on time" grade progression, measured to grade 10 for middle school applicants and grade 12 for high school applicants. Again, I find some evidence of positive impacts, but they are quite imprecise. In both samples combined, about 80 percent of students have already dropped out of school by the time they are arrested for their first felony. Furthermore, even among the remaining 20 percent, students with arrest records are often absent and/or suspended for long stretches of time before an arrest occurs. Thus it is plausible that keeping students enrolled longer, or maintaining a stronger attachment to school, reduces crime.

Overall, I find strong empirical support for the school quality explanation outlined above. The effect of winning the lottery on crime persists for several years after the treatment is complete. This is consistent with school quality improvements leading to a persistent gain in human capital, which increases market wages and raises the opportunity cost of crime. Still, I do not directly observe wages, and there are other explanations that are consistent with this pattern of results. Any explanation where crime or misbehavior committed early in life exerts a particularly strong influence on later criminality (either by raising the cost of legitimate activities, or forming long-lasting peer groups) would also lead to the patterns we observe in Tables 5 and 6. However, the results are

[23] I omit high school graduation from the middle school results because 6th grade lottery applicants have just finished 12th grade. Since many youth complete requirements for high school over the summer, CMS does not produce a high school graduate file until halfway through the following school year.

inconsistent with any model where crime is a function of contemporaneous exposure to other crime-prone peers. These models of criminal contagion, which are common in the neighborhood effects literature, would predict a strong initial effect that fades over time (Glaeser, Sacerdote and Scheinkman 1996, Kling and Ludwig 2006). Although I do not report the results here, I find no significant difference in high school characteristics among middle school lottery winners, nor do I find any difference in neighborhood relocation between control and treatment groups (and very few youth in either group move to another home in the years they are enrolled in CMS). Thus, once the treatment is complete, lottery winners and losers attend similar schools. After high school ends, they live in similar neighborhoods. Yet large differences in crime and incarceration persist into early adulthood.

4.3 Heterogeneous Treatment Effects

The average effect of attending a better school on crime is very large. However, many members of the sample are probably not at high risk for criminal offending. Likewise, it is has been shown that a small percentage of high-rate offenders are responsible for a large share of crimes (Wolfgang, Figlio and Sellin 1972; Freeman 1999). Although African-American males are already a relatively high risk group for offending, it may be valuable to know how attending a better quality school affects the highest risk youth. In Figures 4 and 5 I plot the linear predictions from equation (2) for selected outcomes as a function of the *ex ante* crime prediction estimated in Section 3.1. In each graph, the estimated ITT effect and associated 95 percent confidence intervals are plotted on the Y axis and the predicted arrest probability is on the X axis. Figures 4A-4B show results for high school and Figures 5A-5C show results for middle school applicants. The first thing to note is that, for both groups, the effects are all happening at the upper end of the prediction. Since crime is an extreme outcome this is not surprising, but it does show that the covariates in the CMS administrative data have considerable predictive power.

Interestingly, the pattern of effects differs noticeably between the high school and middle school samples. For high school lottery winners, crime reductions peak in the 40 to 60 percent probability range, but are insignificant or even positive for the highest risk

members of the sample. In contrast, the effect of winning the middle school lottery is much stronger for the highest risk members of the sample (60 percent probability and above). This is also true for the disciplinary outcomes and for enrollment in the 10th and 11th grade years, which according to Table 5 were around the years when the middle school treatment effects began to emerge. Although these results are speculative, they suggest the possibility that high school might be too late for the highest risk youth. A bit less than 10 percent of the middle school sample never appears in any high school grade but subsequently appears in the arrest data. Thus any intervention aimed at high school students would miss them altogether. Finally, it is worth noting that the patterns are similar for crime and non-crime outcomes. This correlation across outcomes provides support for the hypothesis that schooling experiences exert a strong influence on criminality.

5. Discussion and Policy Implications

The finding that most of the effect of attending a better school on crime comes from the highest risk youth has important policy implications. Since criminal involvement can be predicted using covariates that are readily available to the school district, resources could be targeted very efficiently toward these youth. While any reasonable social welfare function would also value other outcomes that I cannot measure here, it is important to note that crime, unlike many other potential benefits of increased school quality, inflicts large negative externalities on society. If we take the social cost estimates presented in Table 4 seriously, offering high risk youth admission to a better school generates large benefits for society as a whole.

CMS chose to implement an open enrollment school choice plan as an alternative to a traditional neighborhood schools model. They expanded capacity at schools where high demand was anticipated, including magnet schools that were located in the inner city. These schools increased yearly enrollment substantially and were in many cases still oversubscribed. Many low-performing schools, on the other hand, experienced large reductions in enrollment – by as much as 50 percent in some cases. Thus, relative to a pure neighborhood schools model, the net effect of open enrollment was to increase access to magnet and highly demanded schools for youth who would not otherwise be

able to enroll. This strong demand response means that the treatment is not just a transfer from losers to winners, and could represent a real welfare gain.

I find that, relative to a pure neighborhood schools model, offering admission to non-guaranteed schools for the over 1700 lottery winners in both samples combined led to a decrease of about $2.1 million in the social cost of arrested crimes. However, not all crimes lead to an arrest. If we assume that the crime to arrest ratio (the clearance rate) is the same for lottery winners and losers, as well as the nation overall, we could multiply this figure by approximately 3 (FBI, 2007). If we use the estimates that do not trim murder, they increase by a factor of 10. Notably, the social cost estimates from Miller, Cohen and Wiersema (1996) do not include the cost of incarceration. Taking the ITT estimates from Table 5 for days incarcerated, winning the lottery led to a cumulative decrease of about 15 years in prison, saving society about $500,000 based on the current estimated cost of incarceration (FBI, 2009).

While any welfare calculation would have to also include the possible negative externality imposed by these youth on their new peers, such an effect is likely to be a small fraction of the individual reduction in crime for two reasons. First, estimates from the peer effects literature are generally small (Angrist and Lang 2004, Hoxby and Weingarth 2006, Carrell and Hoekstra 2009, Imberman, Kugler and Sacerdote 2009). Second, lottery winners would need to have a much larger differential impact on their peers than they would have had in another school. Since they attended better schools on average (as did many other high risk youth who attended a non-guaranteed school but were not subject to randomization), disruptive students were less concentrated under open enrollment than they would have been in a neighborhood schools model. Depending on the nature of peer effects, the effect could go in either direction, but the available evidence suggests that concentrations of disruptive children increase overall misbehavior (Carrell and Hoekstra, Imberman, Kugler and Sacerdote 2009).

All of these calculations consider a pure neighborhood schools model as the counterfactual. In that case, the estimates are likely to understate the total effect, since many lottery losers ended up in other schools to which they were not guaranteed admission. While these schools were generally not oversubscribed and thus probably not as high quality as the schools attended by lottery winners, they may have been better than

the neighborhood schools assigned to many high risk youth. Because the crime reduction benefits of winning the lottery are increasing in youth's prior risk, an open enrollment plan that gives priority to these youth will be more effective at reducing crime.

To quantify the benefits of targeting, I simulate the lottery and resulting distribution of students to schools under two alternative assignment rules. First, I assign open slots to the highest risk students (based on the prediction generated in Section 3.1) in descending order, for each lottery. While such as allocation system would be controversial, it would be feasible since all the covariates are available to the school district. Second, I simulate a simple lottery with no priority groupings, similar to the decentralized lotteries conducted by many US charter schools. CMS assigned a "priority boost" to free lunch-eligible (FRPL) students who applied to schools with a low fraction of FRPL students in the previous year. As a consequence, many poor (and high crime risk) students were automatically admitted to schools when other students had to win the lottery (or, in some cases, only FRPL students could be admitted, and no other students were admitted).

For both assignment rules, I simulate the lottery 500 times and calculate the new distribution of students to schools according to their predicted crime risk. In the last step, I use the original parameter values from the estimation of equation (2) for the social cost outcome, and I integrate under the new distribution to obtain a new estimated crime reduction. This calculation makes some important assumptions. First, it assumes that students' choices were not strategic, and thus they would not have changed their preferences if the assignment rules changed. Second, it assumes that the parametric relationship I estimate between crime risk and the social cost outcome is generalizable out of sample.

I find that, under an assignment rule that allocates slots in order to the highest risk students, the social cost of crime would fall by an additional 27 percent. Most of this effect comes from changes in the middle school lottery, for two reasons. First, the effect is more strongly increasing in crime risk for the middle school lottery (see Figures 4 and 5). Second, there is much less sorting across choices at the middle school level, so there are many low and high risk students applying to the same schools. Still, this scenario is probably unrealistic. A more realistic form of targeting is the method actually pursued by CMS – a "priority boost" for economically disadvantaged students. I estimate that this

policy choice lowered the social cost of crime by about 12 percent, relative to a simple lottery with no preferential treatment. Again, the effect comes mostly from middle schools. See the Data Appendix for details on the lottery simulation and cost reduction calculations.

6. Conclusion

In this paper I estimate the effect of winning an admissions lottery to attend a better school on crime. I find that relatively modest increases in measured school quality lead to large reductions in crime. The effect is concentrated among the highest risk youth in the sample. Many of them spend months or years incarcerated for very serious crimes such as murder, robbery and drug trafficking. Relative to a pure neighborhood schools model, the open enrollment plan implemented by CMS allowed many high risk youth to attend a better school. As a consequence, fewer of these socially costly crimes were committed, generating millions of dollars of estimated savings to society.

Importantly the effects of increased school quality persist beyond the treatment years into the peak ages of criminal offending (and beyond high school years, in some cases). This is consistent with an increase in school quality that raises the return to investment in human capital and increases the opportunity cost of crime. After the treatment is complete, youth attend similar schools and live in similar neighborhoods. Yet the effect persists for seven years after random assignment.

The findings strongly suggest that schools are a particularly important setting for the prevention of future crime. Many of the high rate offenders in the sample live on the margins of society, and public schools may present the best opportunity to intervene. I find suggestive evidence that the highest risk offenders benefit greatly from attending a better middle school, but that high school might be too late. The No Child Left Behind Act of 2001 included a provision that allowed parents to transfer students from "persistently dangerous" public schools, but many states have set the legal threshold so high that very few schools qualify. The results here suggest that, to the extent that low quality schools are also persistently dangerous, allowing students to leave them for a better school might benefit individual students as well as society as a whole.

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Table 1: Arrest Rates and Match Between School District and Arrest Data

Panel A: Arrest Rates by Race/Gender and Crime Type

	Black		Hispanic		White/Asian		
	Male	Female	Male	Female	Male	Female	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Any Arrests	34.4%	13.3%	16.2%	4.2%	9.9%	3.0%	
Felony Arrest	20.3%	3.3%	7.9%	1.0%	3.3%	0.7%	
Index Property	9.4%	1.1%	4.2%	0.4%	1.5%	0.3%	
Index Violent	6.7%	0.5%	1.9%	0.2%	0.5%	0.0%	
Drug Felony	8.2%	0.9%	1.3%	0.2%	1.2%	0.3%	
Sample Size	8,834	8,493	519	504	9,095	8,748	

Panel B: Percent of Arrests Matched to a CMS Attendee

	Black		Hispanic		White/Asian		All
Year of Birth	Male	Female	Male	Female	Male	Female	Felonies
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1980	26.2%	19.7%	1.3%	0.0%	10.8%	4.5%	19.1%
1981	58.9%	38.6%	2.4%	2.8%	26.6%	21.7%	44.2%
1982	64.9%	56.2%	3.1%	8.1%	34.0%	24.7%	52.6%
1983	73.0%	72.6%	3.0%	8.6%	42.6%	33.3%	62.4%
1984	72.2%	65.5%	3.7%	8.7%	48.2%	42.4%	63.8%
1985	79.3%	76.3%	7.7%	3.9%	49.3%	42.2%	70.1%
1986	83.4%	74.3%	12.2%	23.7%	52.7%	43.1%	75.2%
1987	85.1%	78.2%	12.5%	24.1%	59.0%	53.4%	79.5%
1988	90.3%	85.6%	23.4%	30.6%	71.7%	66.7%	84.6%
1989	92.9%	88.2%	39.7%	75.5%	72.9%	70.8%	88.9%
1990	93.5%	91.3%	57.0%	74.5%	81.7%	67.6%	89.7%
1991	94.3%	92.1%	78.7%	88.2%	80.2%	80.6%	91.5%
1992	95.5%	94.1%	74.4%	83.3%	81.0%	80.4%	90.5%
1993	97.1%	82.4%	75.0%	100.0%	80.0%	57.1%	95.2%
Total	76.6%	71.8%	13.3%	22.4%	49.0%	42.0%	68.9%
Sample Size	32,598	7,459	10,392	715	12,161	4,085	19,184

Notes: The sample in panel A consists of CMS attendees in grades K-5 in 1997 (ages 17-23 in 2009) that are still in CMS in grade 8 or higher. Index property crimes are felony larceny, burglary and motor vehicle theft. Index violent crimes are murder/manslaughter, aggravated assault, robbery and kidnapping. In Panel B the denominator is all arrests in CMS.

Table 2: Comparison of Lottery Sample with All Applicants

	Male (1)	Black (2)	FRPL (3)	Math (4)	Reading (5)	Suspended (6)	Absent (7)	N (8)
Panel A: High School								
Chose Guaranteed School	51%	33%	36%	0.11	0.15	1.0	8.4	12,975
Chose Non-Guaranteed School	50%	59%	63%	-0.33	-0.31	2.2	11.7	8,157
Degenerate Lottery	49%	58%	63%	-0.34	-0.32	2.2	11.9	6,266
Lottery Sample	54%	62%	64%	-0.28	-0.26	2.2	11.0	1,891
African-American Males								
Chose Guaranteed School			73%	-0.63	-0.66	2.8	10.6	2,147
Chose Non-Guaranteed School			81%	-0.71	-0.79	3.9	13.2	2,377
Degenerate Lottery			82%	-0.77	-0.85	4.1	14.0	1,767
Lottery Sample			79%	-0.56	-0.60	3.2	10.8	610
Panel B: Middle School								
Chose Guaranteed School	51%	35%	44%	0.24	0.15	0.8	7.6	13,499
Chose Non-Guaranteed School	50%	61%	69%	-0.17	-0.23	1.7	9.5	9,397
Degenerate Lottery	51%	61%	70%	-0.22	-0.29	1.8	9.7	7,077
Lottery Sample	47%	62%	66%	-0.03	-0.07	1.4	8.9	2,320
African-American Males								
Chose Guaranteed School			79%	-0.50	-0.60	2.3	8.9	2,353
Chose Non-Guaranteed School			87%	-0.57	-0.69	3.4	11.0	2,846
Degenerate Lottery			88%	-0.59	-0.73	3.4	11.1	2,197
Lottery Sample			84%	-0.49	-0.58	3.2	10.7	649

Notes: The sample is all CMS students in rising grades 6-11 in the fall of 2002 who were enrolled in any CMS school in the previous year. The first two rows of each panel show summary statistics for students chose their home (guaranteed) school versus some other school. The 3rd and 4th rows divide students who chose a non-home school into those in lotteries where the probability of admission was zero or one, versus lotteries with some winners and losers (which forms the analysis sample for later results). FRPL is free or reduced price lunch, and is an indicator of socioeconomic status. Math and Reading are the standardized scores on students' end-of-grade exams administered in the years that students were in 5th grade (for middle school) and 8th grade (for high school). Suspended and Absent are reported in days.

Table 3: Effect of Winning the Lottery on Enrollment and School Characteristics

	High Schools				Middle Schools			
	All		Black Males		All		Black Males	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enrolled in 1st Choice	{0.361}	0.554***	{0.241}	0.663***	{0.215}	0.550***	{0.133}	0.571***
		[0.063]		[0.049]		[0.043]		[0.065]
In Home School	{0.413}	-0.372***	{0.440}	-0.400***	{0.513}	-0.324***	{0.427}	-0.252***
		[0.037]		[0.043]		[0.047]		[0.062]
School Characteristics								
Percent Black	{0.458}	0.023	{0.527}	0.048	{0.515}	-0.060**	{0.588}	-0.050*
		[0.040]		[0.040]		[0.023]		[0.030]
Percent FRPL	{0.504}	-0.005	{0.568}	0.016	{0.606}	-0.075***	{0.691}	-0.068**
		[0.037]		[0.040]		[0.022]		[0.027]
Distance	{6.29}	1.40**	{6.57}	1.67***	{5.62}	0.68***	{5.72}	0.64
(to assigned school)		[0.53]		[0.47]		[0.21]		[0.43]
School Quality Measures								
Academic	{0.162}	0.122	{-0.127}	0.110	{-0.064}	0.301***	{-0.344}	0.225*
(Test Scores)		[0.156]		[0.183]		[0.101]		[0.113]
Behavior	{-0.043}	0.409***	{-0.233}	0.500***	{-0.058}	0.216***	{-0.335}	0.224***
(Absent/Suspended)		[0.076]		[0.087]		[0.062]		[0.078]
Teacher Quality	{0.001}	0.359***	{-0.240}	0.399***	{-0.084}	0.279**	{-0.265}	0.187
		[0.126]		[0.142]		[0.127]		[0.179]
Revealed Preference	{-0.204}	0.594***	{-0.245}	0.765***	{-0.020}	0.313***	{-0.350}	0.361***
		[0.201]		[0.156]		[0.108]		[0.097]
Magnet School	{0.123}	0.296**	{0.160}	0.392***	{0.118}	0.197***	{0.075}	0.187***
		[0.115]		[0.122]		[0.044]		[0.043]
Sample Size	1891		610		2320		649	

Notes: Each point estimate is from a regression of the form $Y_{ij} = \delta(\text{Win Lottery})_{ij} + \Gamma_j + \varepsilon_{ij}$, with standard errors (below in brackets) calculated as the maximum of Huber-White cluster and block bootstrap procedures, with clusters at the individual lottery level. Odd numbered columns present control means for each outcome in curled brackets. Even numbered columns present intent-to-treat (ITT) estimates for the effect of winning the lottery. All school quality measures (except magnet school enrollment, which is 0/1) are in standard deviation units calculated separately for the middle and high school samples. * = sig. at 10% level; ** = sig. at 5% level; *** = sig. at 1% level.

Table 4: Effect of Winning the Lottery on Crime

	High Schools				Middle Schools			
	All		Black Males		All		Black Males	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of Arrests	{0.344}	-0.029	{0.762}	-0.059	{0.352}	0.008	{0.900}	-0.148
		[0.046]		[0.139]		[0.057]		[0.140]
Felony Arrests	{0.131}	-0.047**	{0.337}	-0.149**	{0.117}	0.033	{0.368}	0.009
		[0.018]		[0.062]		[0.026]		[0.079]
All Felony Charges	{0.256}	-0.075	{0.706}	-0.327*	{0.232}	0.099	{0.698}	0.040
		[0.068]		[0.183]		[0.067]		[0.183]
Index Property	{0.077}	-0.018	{0.191}	-0.099	{0.080}	0.077***	{0.216}	0.143*
(charges)		[0.046]		[0.135]		[0.026]		[0.079]
Index Violent	{0.034}	0.003	{0.110}	-0.010	{0.052}	-0.016	{0.197}	-0.157*
(charges)		{0.034}		{0.110}		{0.052}		{0.197}
Drug Felonies	{0.070}	-0.048**	{0.220}	-0.170*	{0.037}	0.010	{0.119}	0.022
(charges)		[0.021]		[0.085]		[0.016]		[0.054]
Total Social Cost	{6,590}	-10,656	{22,654}	-31,211	{14,281}	-11,586**	{43,009}	-40,390
		[9,205]		[25,511]		[5,718]		[24,625]
Total Social Cost	{2,579}	-1,236*	{8,045}	-4,336**	{2765}	-1,177*	{8,772}	-6,081***
(murder trimmed)		[653]		[1,896]		[596]		[2,240]
Expected Sentence	{7.44}	-0.96	{25.40}	-7.47	{6.06}	-0.54	{20.70}	-11.12*
(in months)		[2.61]		[6.30]		[1.92]		[6.37]
Total Days Incarcerated			{34.70}	-3.16			{22.44}	-13.59***
				[5.96]				[5.06]
Sample Size	1891		610		2320		649	

Notes: Each point estimate is from a regression of the form $Yij = \delta(\text{Win Lottery})ij + \beta Xij + \Gamma j + \varepsilon ij$, with standard errors (below in brackets) calculated as the maximum of Huber-White cluster and block bootstrap procedures, with clusters at the individual lottery level. Odd numbered columns present control means for each outcome in curled brackets. Even numbered columns present intent-to-treat (ITT) estimates for the effect of winning the lottery. Social cost estimates are obtained from Miller, Cohen and Wiersema (1996). Expected sentence length comes from the NC Structured Sentencing Act. * = sig. at 10% level; ** = sig. at 5% level; *** = sig. at 1% level.

Table 5 - Dynamics of Crime

	High Schools			Middle Schools		
	16-17	18-19	20+	16-17	18-19	20+
	(1)	(2)	(3)	(4)	(5)	(6)
Felony Charges	0.132	-0.253*	-0.073	-0.047	-0.069	0.212
	[0.080]	[0.135]	[0.063]	[0.076]	[0.114]	[0.186]
	{0.028}	{0.298}	{0.285}	{0.125}	{0.435}	{0.366}
Index Property	0.056	-0.183	0.019	0.006	0.079	0.072
	[0.036]	[0.122]	[0.026]	[0.037]	[0.056]	[0.064]
	{0.014}	{0.160}	{0.051}	{0.044}	{0.139}	{0.050}
Index Violent	0.046	-0.015	-0.006	-0.016	-0.113	-0.020
	[0.050]	[0.045]	[0.042]	[0.011]	(0.070)	[0.060]
	{0.000}	{0.064}	{0.046}	{0.025}	{0.130}	{0.039}
Drug Felonies	0.026**	-0.037***	-0.082*	-0.023*	0.001	0.070
	[0.012]	[0.010]	[0.043]	[0.012]	[0.036]	[0.048]
	{0.000}	{0.041}	{0.116}	{0.017}	{0.083}	{0.025}
Social Cost	654	-514	-1,180	-19	-1,808*	-4,396**
(murder trimmed)	[486]	[577]	[718]	[209]	[1,026]	[2,092]
	{112}	{1,384}	{1,618}	{425}	{2,584}	{2,815}
Expected Sent. Length	2.96	-3.78	-2.59	-1.80**	-8.39	-1.86
(in months)	[3.96]	[4.38]	[3.40]	[0.86]	[6.11]	[4.14]
	{1.89}	{8.41}	{7.47}	{2.49}	{12.93}	{11.72}
Days in Prison			-1.58	-1.50	-4.59	-15.08**
			[2.98]	[0.91]	[2.87]	[7.14]
			{15.72}	{1.50}	{10.57}	{22.42}

Notes: Each point estimate is from a regression of the form $Yij = \delta(\text{Win Lottery})\, ij + \beta Xij + \Gamma j + \varepsilon ij$, with standard errors (below in brackets) calculated as the maximum of Huber-White cluster and block bootstrap procedures, with clusters at the individual lottery level. The sample is African-American males only. All columns present intent-to-treat (ITT) estimates for the effect of winning the lottery. Each column contains estimates for the approximate age categories listed above, with actual categorization based on lottery applicants' rising grades. Control means are in curled brackets below each standard error. Social cost estimates are obtained from Miller, Cohen and Wiersema (1996). Expected sentence length comes from the NC Structured Sentencing Act. * = sig. at 10% level; ** = sig. at 5% level; *** = sig. at 1% level.

Table 6 - Dynamics of Crime (High Risk Sample)

	High Schools			Middle Schools		
	16-17	18-19	20+	16-17	18-19	20+
	(1)	(2)	(3)	(4)	(5)	(6)
Felony Charges	0.382	-0.789**	-0.267	-0.273*	-0.347	-0.354
	[0.254]	[0.342]	[0.204]	[0.146]	[0.361]	[0.288]
	{0.068}	{0.807}	{0.678}	{0.270}	{0.893}	{0.868}
Index Property	0.118	-0.522	0.077	-0.046	0.211	-0.172
	[0.084]	[0.307]	[0.083]	[0.073]	[0.144]	[0.148]
	{0.068}	{0.460}	{0.104}	{0.082}	{0.230}	{0.180}
Index Violent	0.182	-0.059	-0.088	-0.077***	-0.276	-0.188
	[0.176]	[0.137]	[0.135]	[0.024]	[0.192]	[0.158]
	{0.000}	{0.138}	{0.149}	{0.084}	{0.311}	{0.214}
Drug Felonies	0.072*	-0.138***	-0.181**	-0.068*	-0.081	0.192
	[0.036]	[0.029]	[0.085]	[0.039]	[0.130]	[0.138]
	{0.000}	{0.143}	{0.242}	{0.041}	{0.189}	{0.131}
Social Cost	2,044	-2,072	-3,937	-259	-3,001	-8,752*
(murder trimmed)	[1,672]	[1,712]	[2,423]	[615]	[2,333]	[4,896]
	{448}	{3,508}	{3,992}	{1,030}	{6,073}	{12,264}
Expected Sent. Length	7.96	-12.98	-8.02	-6.67***	-26.13	-24.68
(in months)	[15.38]	[13.07]	[12.38]	[1.98]	[20.61]	[13.34]
	{12.04}	{19.23}	{18.66}	{6.89}	{30.94}	{28.12}
Days in Prison			-6.00	-3.84**	-16.08**	-50.72***
			[17.01]	[1.78]	[7.86]	[21.12]
			{43.38}	{4.30}	{25.83}	{51.66}

Notes: Each point estimate is from a regression of the form $Yij = \delta(\text{Win Lottery})\, ij + \beta Xij + \Gamma j + \varepsilon ij$, with standard errors (below in brackets) calculated as the maximum of Huber-White cluster and block bootstrap procedures, with clusters at the individual lottery level. The sample is high risk (defined as the upper tercile of the crime prediction outlined in Section 3.1) African-American males only. All columns present intent-to-treat (ITT) estimates for the effect of winning the lottery. Each column contains estimates for the approximate age categories listed above, with actual categorization based on lottery applicants' rising grades. Control means are in curled brackets below each standard error. Social cost estimates are obtained from Miller, Cohen and Wiersema (1996). Expected sentence length comes from the NC Structured Sentencing Act. * = sig. at 10% level; ** = sig. at 5% level; *** = sig. at 1% level.

Table 7 - Effect of Winning the Lottery on Other Outcomes

	High Schools				Middle Schools			
	All		High Risk		All		High Risk	
Unexcused Absences	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2002-2003	{6.20}	-0.17	{10.56}	-1.17	{5.70}	-0.86***	{8.02}	-1.14*
(in days)		[0.27]		[0.83]		[0.32]		[0.67]
2003-2004	{7.42}	-0.52	{11.13}	-1.52	{6.85}	-0.36	{9.63}	0.34
(in days)		[0.88]		[2.87]		[0.54]		[1.38]
2004-2005	{8.37}	-1.70**	{14.62}	-4.74*	{7.87}	-0.59	{12.85}	-1.04
(in days)		[0.78]		[2.56]		[0.84]		[2.13]
Out-of-School Suspensions								
2002-2003	{3.80}	-1.43*	{9.06}	-3.86*	{5.64}	-0.14	{10.91}	-1.84
(in days)		[0.71]		[2.23]		[0.79]		[2.46]
2003-2004	{2.83}	-0.41	{6.35}	-3.57*	{5.58}	-0.55	{10.86}	-2.37
(in days)		[0.70]		[1.90]		[0.83]		[2.03]
2004-2005	{1.91}	0.17	{5.05}	1.07	{5.30}	1.02	{9.47}	1.96
(in days)		[0.75]		[3.30]		[1.12]		[3.68]
In School Suspension					{0.512}	0.295***	{0.614}	0.685**
(in days)						[0.094]		[0.293]
Serious Discipline					{0.056}	-0.033*	{0.133}	-0.232***
(Police, Long Term Suspension, Expelled)						[0.018]		[0.073]
Test Scores and Enrollment								
Math, 2003					{-0.569}	0.058	{-1.045}	0.130
(in SD units)						[0.089]		[0.146]
Math, 2004					{-0.520}	-0.084	{-0.965}	-0.170
(in SD units)						[0.108]		[0.206]
Grade 9					{0.850}	-0.038	{0.787}	0.101
						[0.028]		[0.062]
Grade 10					{0.751}	0.044	{0.615}	0.138
						[0.029]		[0.085]
Grade 11	{0.749}	0.017	{0.544}	0.094	{0.715}	0.009	{0.541}	0.057
		[0.028]		[0.095]		[0.034]		[0.107]
Grade 12	{0.631}	0.016	{0.356}	0.047	{0.579}	-0.055	{0.410}	0.021
		[0.039]		[0.093]		[0.040]		[0.091]
High School Grad	{0.569}	-0.010	{0.287}	0.013				
		[0.051]		[0.102]				
"On Time"	{0.549}	0.117	{0.217}	0.166	{0.547}	0.058	{0.419}	0.080
		[0.076]		[0.132]		[0.054]	[0.078]	[0.078]

Notes: Each point estimate is from a regression of the form $Yij = \delta(\text{Win Lottery})_{ij} + \beta Xij + \Gamma j + \varepsilon ij$, with standard errors (below in brackets) calculated as the maximum of Huber-White cluster and block bootstrap procedures, with clusters at the individual lottery level. The sample is African-American males only. All columns present intent-to-treat (ITT) estimates for the effect of winning the lottery. Even numbered columns restrict the sample to the upper tercile of predicted crime risk. Control complier means are in curled brackets below each standard error. * = sig. at 10% level; ** = sig. at 5% level; *** = sig. at 1% level.

Figure 1 – Age Profiles by Crime Type

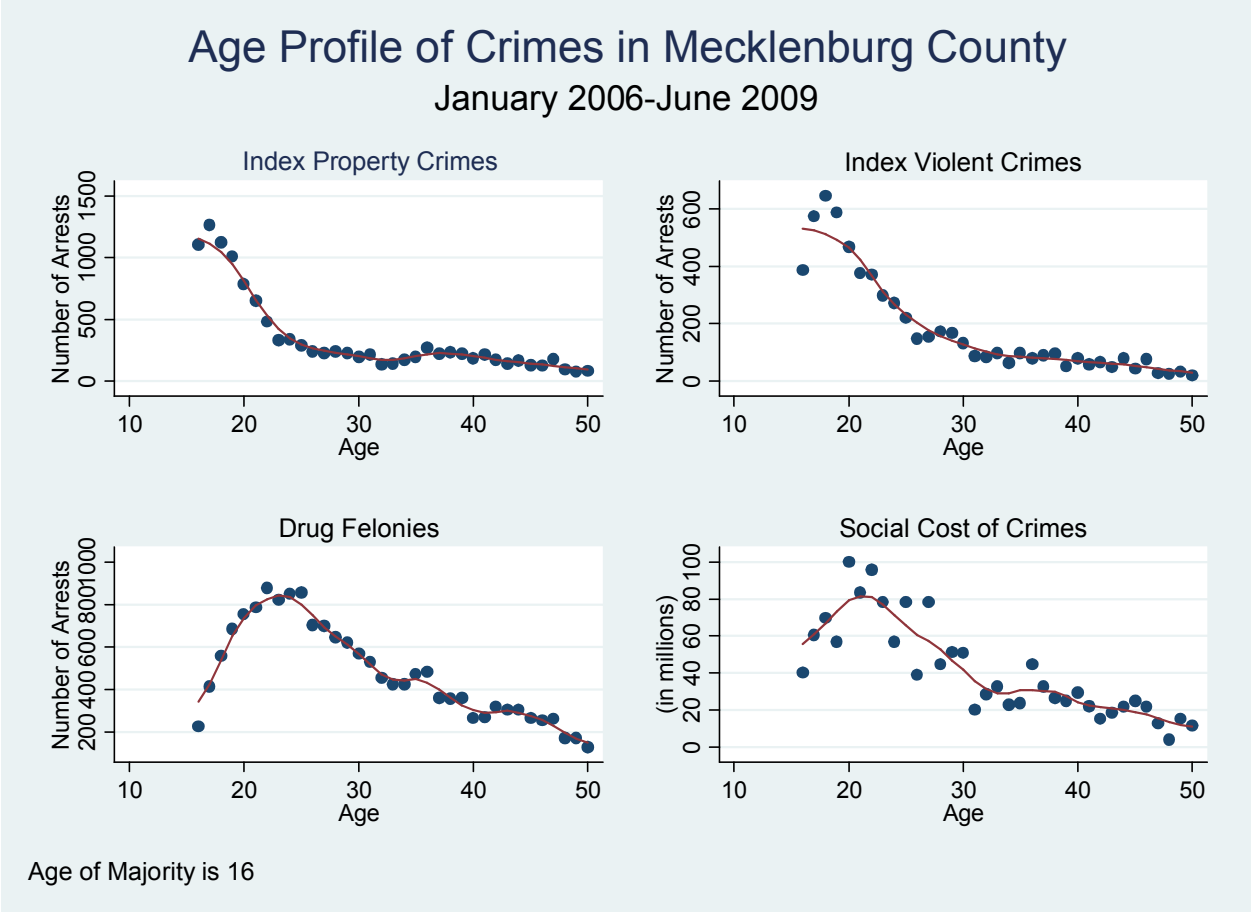


Figure 2

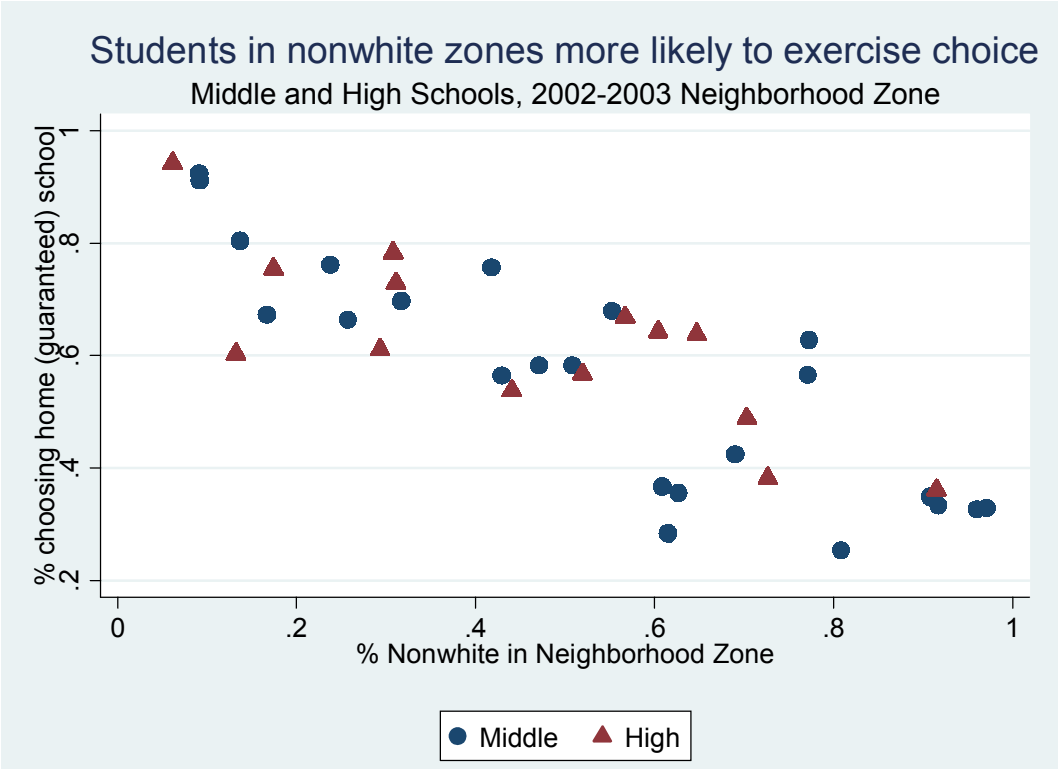

Students in nonwhite zones more likely to exercise choice
Middle and High Schools, 2002-2003 Neighborhood Zone
% choosing home (guaranteed) school
.2
.4
.6
.8
1
0
.2
.4
.6
.8
1
% Nonwhite in Neighborhood Zone
Middle
High

Figure 3

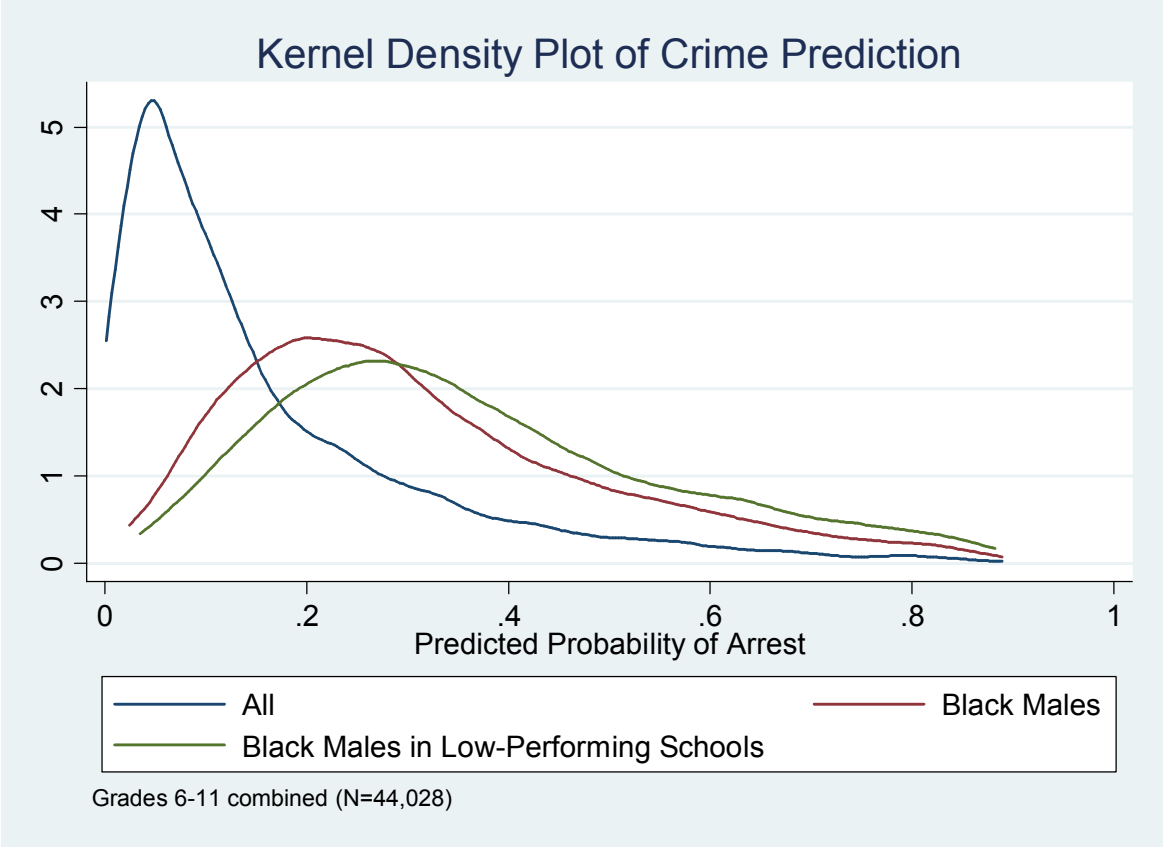

Kernel Density Plot of Crime Prediction
0
1
2
3
4
5
0
.2
.4
.6
.8
1
Predicted Probability of Arrest
All
Black Males
Black Males in Low-Performing Schools
Grades 6-11 combined (N=44,028)

Figure 4A

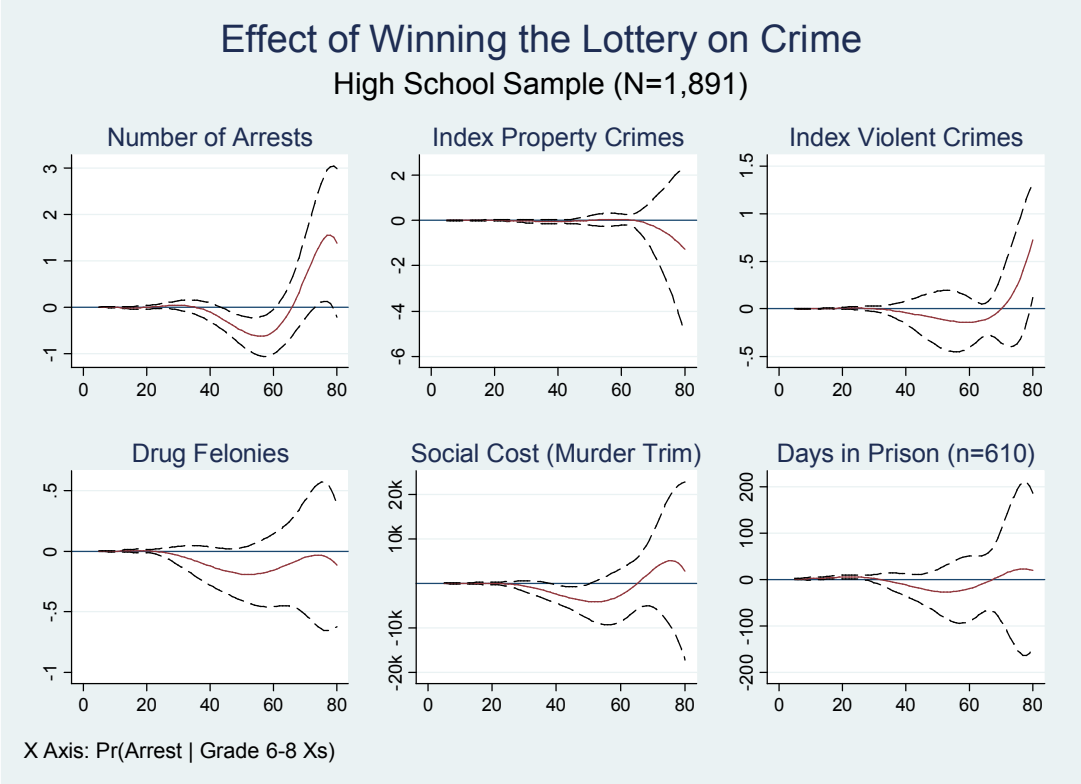


Figure 4B

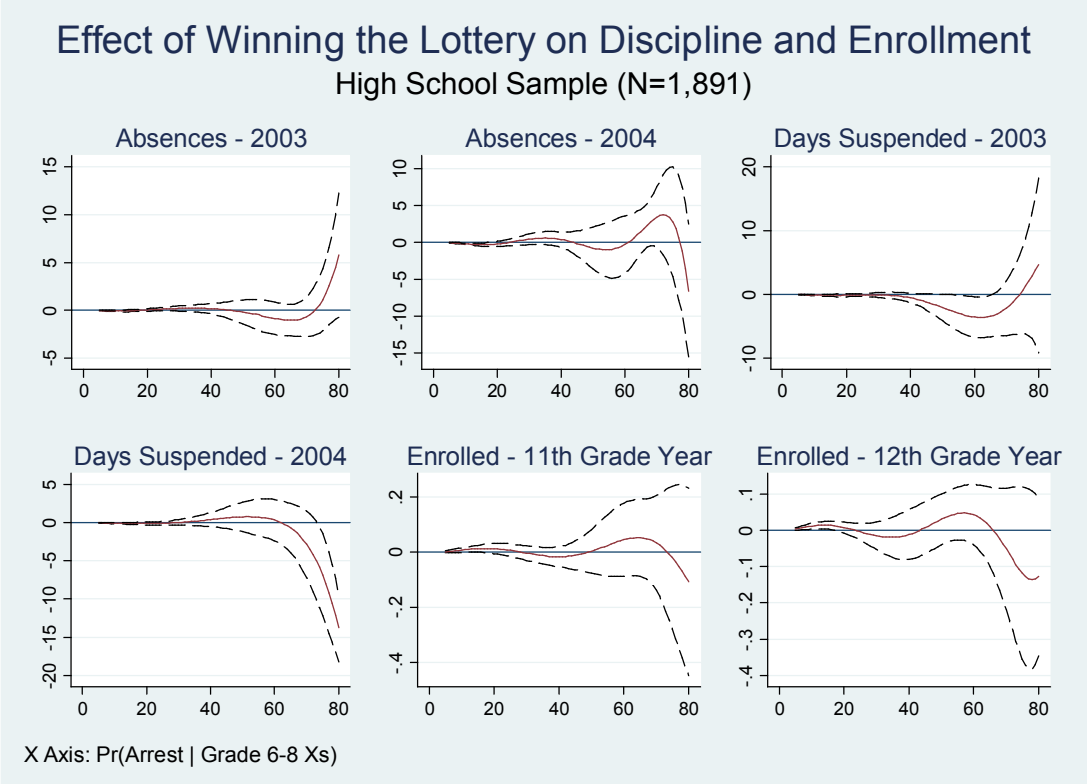


Figure 5A

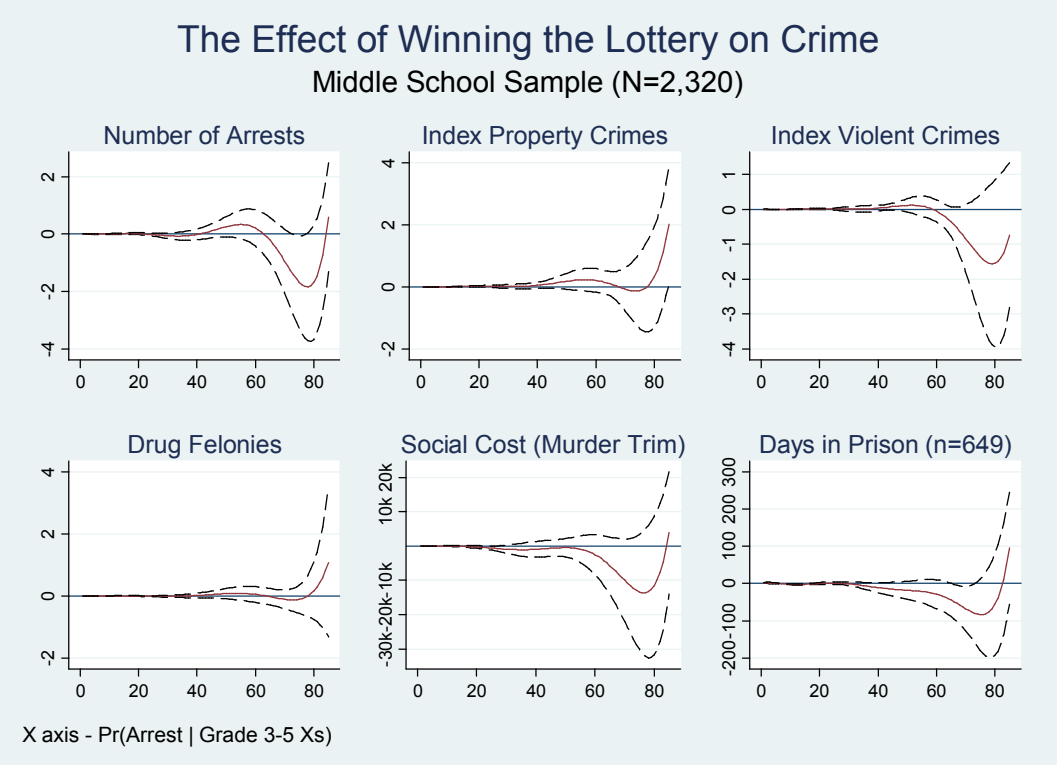


Figure 5B

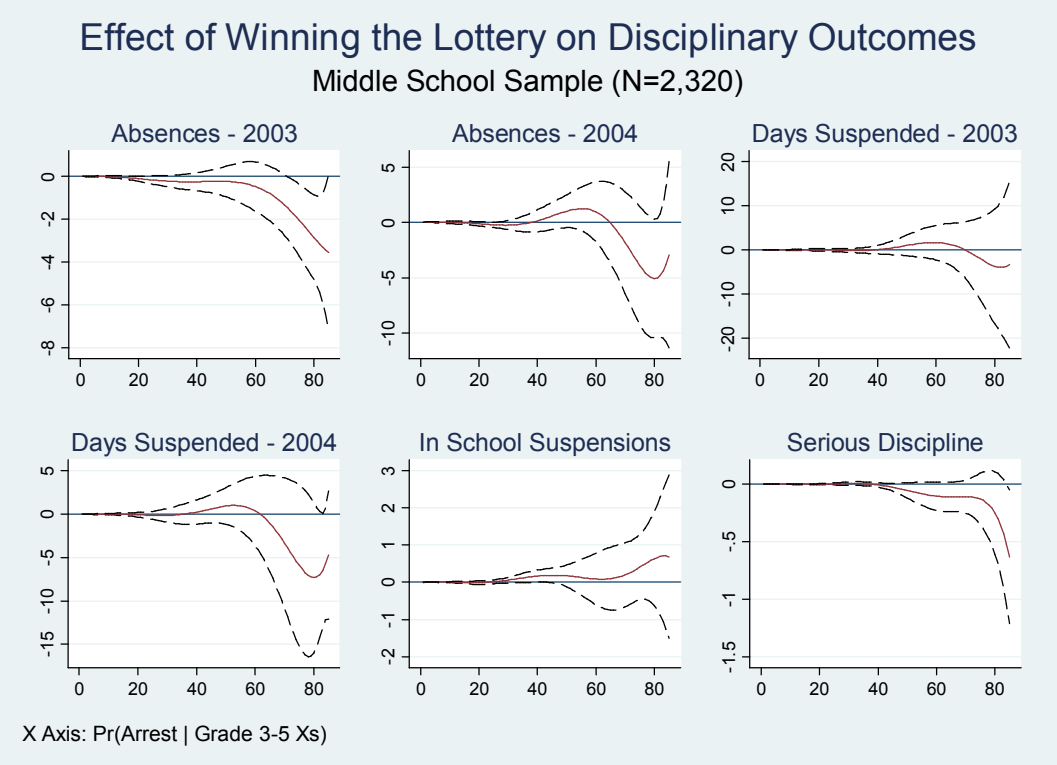
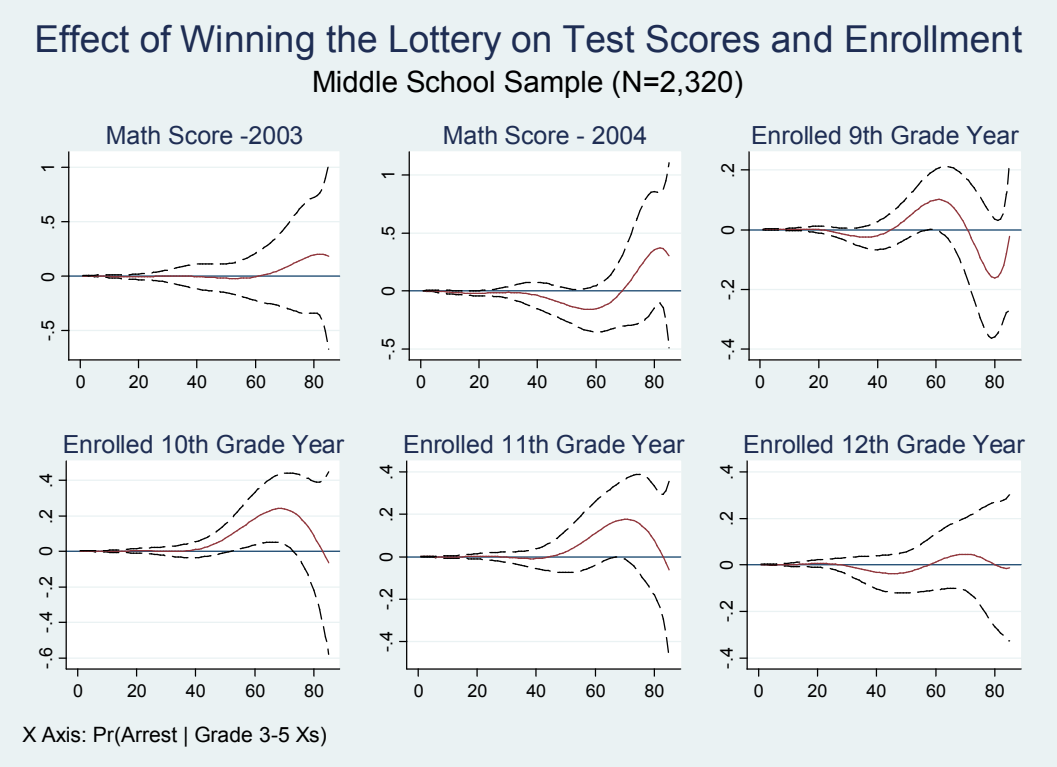


Figure 5C


Effect of Winning the Lottery on Test Scores and Enrollment
Middle School Sample (N=2,320)
Math Score -2003
Math Score - 2004
Enrolled 9th Grade Year
Enrolled 10th Grade Year
Enrolled 11th Grade Year
Enrolled 12th Grade Year
X Axis: Pr(Arrest | Grade 3-5 Xs)