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School Choice, School Quality, and Postsecondary Attainment†

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We study the impact of a public school choice lottery in Charlotte-Mecklenburg schools on college enrollment and degree completion. We find a significant overall increase in college attainment among lottery winners who attend their first-choice school. Using rich administrative data on peers, teachers, course offerings, and other inputs, we show that the impacts of choice are strongly predicted by gains on several measures of school quality. Gains in attainment are concentrated among girls. Girls respond to attending a better school with higher grades and increases in college-preparatory course taking, while boys do not. (*JEL* D44, H75, I21, I23, J16)

Today's urban schools face increasing pressure to matriculate students who are ready for college. Growing returns to postsecondary education and shrinking middle-wage employment make college degree completion necessary for upward mobility into the American middle class (Goldin and Katz 2007; Autor, Katz, and Kearney 2008). Improving the quality of high school education has become a first-order issue for economic growth, national competitiveness (US Department of Education 2006; Roderick, Nagaoka and Coca 2009), and equality of economic opportunity in light of the increasing wage returns to higher education (Acemoglu and Autor 2011). Yet there is little causal evidence on which policies can increase college attainment for students most in need (Murnane 2008).

In this article we study the impact of winning a lottery to attend a public high school in Charlotte-Mecklenburg Schools (CMS) on college enrollment and degree completion. CMS implemented an open enrollment public school choice

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program in the fall of 2002, ending three decades of busing for racial integration and offering high school choice to students from all socioeconomic backgrounds. Students were guaranteed admission to their neighborhood school but were allowed to choose and rank up to three schools in the district, and slots to oversubscribed schools were assigned by lottery number. Students coming from low-performing high schools actively participated in the choice plan, often choosing substantially higher-performing high schools over their neighborhood school option.

We use student-level administrative data from CMS linked to the National Student Clearinghouse (NSC), a national database of postsecondary enrollment which records college enrollment and degree completion for almost all colleges in the United States. We use assignment by random lottery numbers to chosen schools to identify the causal impact of attending a chosen school on secondary and postsecondary educational attainment. Our approach is similar to prior research that uses school lotteries to estimate impacts on elementary and secondary achievement (Rouse 1998; Howell and Peterson 2002; Hoxby and Rockoff 2005; Cullen, Jacob, and Levitt 2006; Hastings, Kane, and Staiger 2008; Wolf et al. 2008; Hoxby and Murarka 2009; Abdulkadiroğlu et al. 2011; Dobbie and Fryer 2011; Hastings, Neilson, and Zimmerman 2012).

Overall we find small but statistically significant increases in high school graduation, postsecondary attendance, and degree completion for students who win the lottery to attend their first-choice school. We also find that the gains from school choice are almost entirely concentrated among girls. Girls who attend their first-choice school are 14 percentage points more likely to complete a four-year college degree, yet we find no significant impacts for boys across a variety of measures of postsecondary attainment.

We then examine how the impact of choice varies with school characteristics. We construct a measure of college "value-added," which estimates a school's likelihood of sending students to college, conditional on prior characteristics. We show that lottery winners with the largest gains in school quality experience the largest gains in postsecondary attainment. This is possible because most students who did not get their first choice were assigned to their neighborhood school. Since the probability of winning the lottery is unrelated to neighborhood school assignment, and since it is a fixed characteristic at the time of application (like race or gender), we can compare applicants who choose the same school but who have neighborhood schools of different quality.

Using rich administrative data on school and peer inputs, we show that high-quality schools differ from low-quality schools along several dimensions. They have students with higher baseline math scores, a higher fraction of teachers with degrees from selective colleges, and a higher fraction of students completing college-preparatory course requirements. While we do not have enough statistical power to separate the contribution of each of these variables, we do show that only girls appear to gain from attending higher-quality schools.

This suggests that girls are more responsive to gains in school quality—or alternatively, that a change in environment is more costly for boys. While boys and girls choose similar-quality schools on average and start at their new schools in similar courses with similar class rank, only girls remain "on track" throughout high school. By the end of high school, female lottery winners had higher grade point averages, had completed significantly more college-level coursework, and were more likely to take the SAT. Male lottery winners, on the other hand, dropped significantly in class rank, showed no difference in college-level coursework, and were significantly *more* likely to fail an end-of-course exam in the upper grades. This pattern of results mirrors the results in the Moving to Opportunity (MTO) Experiment (Kling, Liebman, and Katz 2007), as well as many recent studies in school settings (e.g., Hastings, Kane, and Staiger 2006; Anderson 2008; Angrist, Lang, and Oreopoulos 2009; Angrist and Lavy 2009; Jackson 2010; Lavy and Schlosser 2011; Lavy, Silva, and Weinhardt 2011; Legewie and DiPrete 2012).

Taken together, the evidence suggests that girls responded to a more academically demanding environment with increased effort, while boys did not. This is consistent with prior work showing gender differences in study habits and time spent on homework (Jacob 2002; Hastings, Kane, and Staiger 2006; Frenette and Zeman 2007). Girls might also be more responsive to increased school quality because of differences in the expected return to a college education (Charles and Luoh 2003; Goldin, Katz, and Kuziemko 2006; DiPrete and Buchmann 2006). Boys may respond to changes in social environment with maladaptive behavior, perhaps due to differences in coping behavior, peer norms, or differential response to relative rank within social group (e.g., Roderick 2003; Clampet-Lundquist et al. 2006; Niederle and Vesterlund 2007; Barankay 2011). The bottom line is that the impacts we observe are the net effect of behavioral responses and adjustments by the students themselves, as well as their peers, teachers, and parents (Pop-Eleches and Urquiola 2011).

To our knowledge, this is the first study to examine the impact of school choice on postsecondary attainment using a lottery-based research design.[1] A series of recent papers use rule-based secondary school assignment to identify the impacts of school and/or peer quality for students at the margin of admission (Clark 2010; Jackson 2010; Pop-Eleches and Urquiola 2011; Abdulkadiroğlu et al. 2011; Dobbie and Fryer 2011). Like the research design here, these papers share the limitation that they cannot unpack the impact of changing school assignment into changes in peer quality, teacher quality, or other important inputs. However, unlike the studies cited above, our research design enables us to observe impacts across the full range of prior academic preparation and relative rank. Attending a higher-quality school may have heterogeneous impacts in a high school setting, where course tracking and peer group identity are important features of the schooling experience.

We also build on an important literature in economics that studies the determinants and impacts of school quality (e.g., Hanushek 1986; Card and Krueger 1992; Betts 1995; Rivkin, Hanushek, and Kain 2005). Ultimately, we cannot rule out the importance of peer effects versus other inputs that can be directly manipulated by schools. However, we can show that school "value-added" measures, which control directly for observed differences in peer quality, also predict the impacts of school choice. More generally, we show convincing evidence that school choice

[1] Cullen, Jacob, and Levitt (2006) and Deming (2011) examine misbehavior and crime using high school lotteries. Lavy (2010) and Booker et al. (2011) use difference-in-differences, instrumental variable, and regression discontinuity approaches to estimate the impact of public school choice on high school graduation. Fryer (2011) uses a differences-in-differences approach to estimate the impact of adopting charter school practices in low-performing public high schools on a variety of outcomes, including college attendance among students in twelfth grade at the beginning of the year.

benefits applicants only when they gain access to higher-quality schools, however defined. Thus at least in this setting, there is no benefit of choice per se—rather, school choice is a mechanism through which students can gain access to higher-quality schools. Finally, our finding of gender differences in responsiveness to school quality is an important potential explanation for the growing female advantage in completed schooling (Goldin, Katz, and Kuziemko 2006; DiPrete and Buchmann 2006; Bailey and Dynarski 2011).

I. Background

Charlotte-Mecklenburg is a large and diverse school district encompassing Mecklenburg County, which includes both the inner city areas of Charlotte, North Carolina and its suburbs. In 1971, the Supreme Court (in *Swann v. Charlotte-Mecklenburg Board of Education*) ruled that neighborhood segregation resulted in de facto segregated schools, and for over 30 years CMS schools bused students across the district to achieve racial desegregation. In 2001 this historic court order was overturned and the busing plan was terminated.

In December of 2001, the CMS School Board voted to move forward with districtwide open enrollment for the 2002–2003 school year. In the spring of 2002, CMS asked parents to submit up to three choices for the upcoming school year for each child, listed in order of preference. CMS conducted an extensive information campaign to encourage parents to submit choice forms, including a comprehensive booklet with information about each school (Hastings and Weinstein 2008). Importantly, they told parents that school choice forms were required to receive a school assignment in the subsequent year. This resulted in over 95 percent of parents submitting a choice application in the spring of 2002.

Each child received guaranteed access to his or her neighborhood school, which was usually (but not always) the closest to the child's home address. Students were assigned to their neighborhood school by default, and admission for all other students was subject to grade-specific capacity limits that were set by the district beforehand but were not known to families at the time of the lottery (Hastings, Kane, and Staiger 2008). When demand for slots among nonguaranteed applicants exceeded supply, admission was allocated by lottery. Random lottery numbers were assigned within the following priority groups—(i) students who attended the school in the previous year and their siblings; (ii) free or reduced-price lunch–eligible (FRPL) students applying to schools where less than half of the previous year's school population was FRPL; (iii) students applying to a school within their own choice zone. In addition, siblings of currently enrolled children received guaranteed access. CMS was also divided into four "choice zones," and free transportation was provided by the district, but only within each zone. Families could also provide their own transportation to any school.[2] The district expanded capacity at schools where it anticipated high demand in an attempt to give everyone his first choice. Still, many high schools were oversubscribed. Applicants were sorted by priority group according to

[2] The choice zones were constructed so that there was at least one predominantly white suburban and at least one predominantly black inner-city school in each zone. In addition, free transportation was provided to several "all-zone" magnets from any zone in the district.

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 allocated within each priority group according to lottery numbers. If all members of a priority group could be offered admission, slots were allocated to the next group in the order of lottery numbers. CMS administered the lottery centrally and applied an algorithm known as a "first-choice maximizer" (Abdulkadiroğlu and Sönmez 2003). This meant that CMS first allocated slots to all those who listed a school as their first choice and only then moved to second choices. As the name indicates, this maximized the share of students who received their first choice. However, it also meant that students who lost the lottery to attend their first-choice school almost always found that their second choice had been filled up in the previous round. While there is the potential for strategic choice with this type of lottery mechanism, Hastings, Kane, and Staiger (2008) show that this is not likely to have been a large problem in CMS, at least in the first year of the choice plan.

AQ 1

II. Data Description

We match the lottery applicant files to a panel of administrative data from CMS. The lottery applicant files contain individual choices, lottery numbers, priority groupings, and admissions outcomes. We supplement this with administrative data on all students from 1996 to 2009. These data contain detailed information on student demographics, enrollment histories, test scores, and course-taking. We use data from the school years prior to the lottery to construct a set of pretreatment variables that can be used to test the validity of the randomization and to examine treatment effect heterogeneity. These pretreatment covariates include demographic information such as gender, race, eligibility for free or reduced price lunch (an indicator of poverty), and students' scores on standardized End-of-Grade (EOG) exams in math and reading up to grade eight. We also use prior address information to calculate median household income in a student's neighborhood, and we assign students to "home" schools using high school neighborhood catchment areas.

The student records from CMS include linked individual-level information on college attendance from the National Student Clearinghouse (NSC). The NSC is a nonprofit organization that maintains data on enrollment and graduation for students at over 90 percent of colleges nationwide. In collaboration with CMS, we constructed a comprehensive list of students who had ever been enrolled in CMS and were old enough to have matriculated to college, regardless of the last grade they attended in CMS. CMS then provided this list to NSC for use in matching to postsecondary records. Due to limited coverage of college experiences in the NSC, our main outcome variables are enrollment and degree receipt by college type—both two-year and four-year, as well as measures of college selectivity that are based on the 2009 Barron's Profile of American Colleges.[3]

[3] While all colleges in the NSC data report graduation, some do not report degree type. However, this can be inferred by the level of the school (i.e., four-year or two-year college). Information on major and degree is available for graduates of about 65 percent of the colleges covered by the NSC in our data (the share is 75 percent when colleges are weighted by the number of total enrollment spells in our data). The NSC data have no information about grades and collect data on choice of major only for a subset of graduates.

Although not all colleges provide information to the NSC, the coverage is very good in North Carolina and the surrounding states. The online Appendix contains a list of colleges by coverage and a detailed analysis of the match process using data from the Department of Education's Integrated Postsecondary Data Source (IPEDS) as a reference. Critically, students who leave CMS are followed in the NSC data. While we cannot observe test scores or course taking for these students, we can measure their college attendance as long as they were ever enrolled in CMS. Thus, *for postsecondary outcomes only*, bias from nonrandom attrition is not a concern. Attrition is subject only to the NSC's coverage and the quality of the match. Unless coverage is differential for lottery winners and losers, the results may be attenuated but not otherwise biased.

We also use CMS administrative data to examine impacts on high school graduation and a variety of school outcomes such as grades, exam scores, and college-level course taking. We use these data to measure students' performance in and progress through courses that leave them "on track" to graduate with a college-preparatory diploma according to North Carolina standards, as well as participation in special programs such as AVID and ROTC.[4] We further add information on yearly measures of school resources such as class size, books, and computers from the North Carolina Department of Public Instruction (NCDPI). Finally, we use CMS personnel files to construct school- and class-level measures of teacher and guidance counselor characteristics such as years of experience, college quality, and licensing and certification.

III. Sample Characteristics and School Attributes

CMS received high school lottery applications from 29,584 high school students. We first limit the sample to students who were enrolled in any CMS school in the previous year. About 6 percent of applicants come from outside the district, and these students are much less likely to be enrolled in CMS the following fall. Since previous enrollment status is fixed at the time of the lottery, this sample restriction does not affect the validity of the randomization. We also exclude from the sample the small number of students who apply to special education programs. Finally, we exclude rising twelfth graders from the analysis sample because of concerns about correct randomization.[5] This leaves an analysis sample of 20,021 students.

About 51 percent (10,302) of students in the sample listed their neighborhood school as their first choice. Since admission to neighborhood schools was guaranteed, there is no random variation in school attendance for this group. Of the remaining 9,719 students, nearly half (4,736) applied to schools that were not oversubscribed and, thus, were automatically admitted. Another one-third of students (3,118) were

[4] The formal math and science requirements for graduation in North Carolina include only Algebra I and Biology, yet a "college prep" course of study requires the completion of Geometry and Algebra II as well. The UNC system required students to complete an additional math course that has Algebra II as a prerequisite, and two credits in the same foreign language. For more information see: http://www.dpi.state.nc.us/docs/curriculum/home/graduationrequirements.pdf.

[5] We have analyzed the individual choice lotteries to confirm that random numbers determine offers of admission and have found that they hold perfectly except for in the twelfth grade. In reviewing the historical documentation and in conversation with CMS, we have some concern that additional slots may have been made available at schools for rising twelfth-grade applicants. Thus we exclude from the analysis the 85 rising twelfth-grade applicants who were in marginal priority groups (about 4 percent of the lottery sample).

Table 1—Summary Statistics

	Not lottery	Lottery
Median household income	$56,625	$46,465
Male	0.494	0.533
Black	0.410	0.614
Hispanic	0.035	0.044
Free/reduced-price lunch	0.424	0.633
Eighth-grade reading score (standardized)	0.036	−0.254
Eighth-grade math score (standardized)	−0.009	−0.278
Missing reading and math scores	0.086	0.057
Eighth-grade days absent	9.020	10.890
Eighth-grade days out-of-school suspended	1.350	2.150
Distance to neighborhood school (miles)	4.320	4.540
Indicator if admitted to first choice	0.830	0.460
Sample size	18,156	1,865

Notes: Sample consists of rising ninth- to eleventh-grade students in the fall of 2002 who were also enrolled in CMS in the previous school year. Eighth-grade end-of-year (EOG) scores in math and reading are standardized at the state-year level. Median household income is calculated as the average value within each student's 2000 census block group.

in priority groups where no one was admitted, leaving 1,865 students who applied to schools where admission was determined by random lottery. We use this sample for our analysis. Note that about 6 percent of this remaining sample does not show up in any CMS school in the fall of 2002. Although these students can still be matched to the NSC data and are included in the results for college attendance and degree completion, we have no other outcome information for them.

Table 1 presents summary statistics for the overall analysis sample and the lottery subsample. Compared to the rest of the sample, lottery applicants are disproportionately low income, African American, and had lower test scores and higher absences and out-of-school suspensions in eighth grade. Overall about 46 percent of students in the lottery sample are admitted to their first choice, compared to 83 percent of other students. Approximately 63 percent of the lottery sample is composed of rising ninth-graders, while 25 percent are rising tenth-graders and the remaining 12 percent are rising eleventh-graders.[6]

Table 2 presents descriptive statistics for the 14 neighborhood and 3 magnet high schools in CMS. Schools vary widely in income, demographic composition, average student test scores, and postsecondary attainment. Median household income ranges from $89,089 (in 2000 dollars) in Providence to $32,744 in West Charlotte. Similarly, the share of minority (black or Hispanic) students ranges from 11 to 94 percent. Average eighth-grade math scores have a range of around 1.3 student-level standard deviations.

Magnet high schools serve predominantly nonwhite students in the lower end of the income distribution. This is due in part to their location in the central city, whereas

[6] About 50 percent of rising ninth-grade students in the lottery sample are admitted, compared to 37 percent for tenth- and eleventh-graders. Less than 2 percent of rising ninth-grade students have missing test score information, compared to about 10 percent for tenth- and eleventh-graders combined. This is due to the fact that eighth-graders would have taken End of Grade exams in CMS, whereas ninth- and tenth-graders do not have to take a uniform exam. Other than differences in missing scores and in admission rates, sample characteristics are very similar for students across grade cohorts.

Table 2—Descriptive Statistics by School

	Median household income (1)	Fraction black or Latino (2)	Eighth-grade math score (3)	Percent in lottery sample with this neighborhood school (4)	Percent of freshmen attend four-year college (5)	College "value added" (6)
Neighborhood schools						
Myers Park	63,382	0.286	0.441	0.050	0.590	0.047
West Charlotte	32,744	0.939	−0.745	0.150	0.320	0.038
Providence	89,089	0.110	0.603	0.010	0.670	0.037
South Mecklenburg	67,177	0.203	0.331	0.070	0.540	0.018
East Mecklenburg	50,890	0.467	−0.044	0.130	0.470	0.014
Garinger	37,273	0.807	−0.662	0.170	0.290	0.003
Hopewell	67,998	0.288	0.001	0.040	0.530	−0.001
North Mecklenburg	66,861	0.256	0.255	0.030	0.540	−0.007
Independence	49,287	0.536	−0.106	0.080	0.430	−0.008
Butler	59,113	0.249	0.168	0.030	0.480	−0.009
Vance	52,514	0.630	−0.239	0.110	0.420	−0.039
Olympic	53,027	0.499	−0.180	0.130	0.370	−0.042
Waddell	43,901	0.660	−0.491	0.150	0.280	−0.056
West Mecklenburg	40,534	0.649	−0.504	0.160	0.240	−0.093
Magnet schools						
Northwest Arts	52,654	0.388	−0.166	n/a	0.490	0.032
Harding University	43,643	0.678	0.089	n/a	0.530	0.007
Berry Academy	41,568	0.790	−0.223	n/a	n/a	n/a

Notes: The first 14 schools are neighborhood schools, listed in order of the college "value-added" measure in column 6. The last three schools are magnet schools with no assigned neighborhood zone. Column 1 shows median household income in the census tract where students in each school reside (based on the 2000 census). Eighth-grade math scores in column 3 are normalized at the state level. Column 4 shows the share of students from each neighborhood zone that are in the lottery sample. Columns 5 and 6 are calculated based on student average characteristics in the fall 1998 and 1999 rising ninth-grade cohorts, to minimize the influence of the lottery sample. College "value added" in column 6 is estimated as the school average residual from a student-level regression of an indicator for four-year college enrollment on the set of covariates in equations (2) and (3), including student demographics and prior math and reading scores. See text for details.

many of the higher-income schools are located in the surrounding suburbs. Magnet schools rank near the district average on measures such as average test scores, high school graduation, and college attendance. Overall, applicants to magnet schools constitute 35 percent of the lottery sample. Column 4 shows the percent of students in each neighborhood school zone that are in the lottery sample. While students in the lottery sample are drawn disproportionately from inner city schools with high shares of minority students, there are many different neighborhood school-by-choice school combinations. Online Appendix Table 1 shows a matrix of counts of neighborhood school-by-choice school combinations, separated by rising grade cohorts.

Column 5 of Table 2 shows the share of first-time rising ninth-grade students in each school who eventually enroll in a four-year college. Column 6 presents estimates of college "value added," constructed as the school average residual from a linear regression of four-year college attendance on a set of basic covariates from our main specifications, including a polynomial in prior math and reading scores.[7]

[7] We estimate $A_{ij} = \beta X_{ij} + \nu_{ij}$, where $\nu_{ij} = \mu_j + \varepsilon_{ij}$. A_{ij} is an indicator variable for whether a student ever attended a four-year college. The X_{ij} vector includes indicators for race, gender, free or reduced-price lunch, and third-order polynomials in state-standardized eighth-grade math and reading end-of-grade (EOG) exams. We pool

To minimize the mechanical influence of students in the lottery sample, we estimate college "value added" using first-time ninth-grade students from fall 1998 and 1999.[8] The base rates of four-year college attendance also come from these two older grade cohorts.

College attendance rates range from 67 percent in Providence to 24 percent in West Mecklenburg, while college "value added" ranges from 0.047 to −0.093. If the college "value-added" estimates were unbiased after controlling for prior characteristics, they could be interpreted as each school's contribution to the chances that a randomly chosen student in the ninth-grade cohort will attend a four-year college. In that case, switching from West Mecklenburg to Providence would make a student $0.047 - (-0.093) = 14$ percentage points more likely to attend a four-year college. We present the results in column 6 not as true unbiased measures of school quality, but to show that demographics are not a perfect predictor of college attendance rates. For example, while West Charlotte has lower average income and lower eighth-grade math scores than West Mecklenburg, freshmen in West Charlotte are nonetheless about 8 percentage points more likely to attend a four-year college (0.32 versus 0.24, column 5). This leads to the large disparity in college "value added" among the two schools. Later we will examine heterogeneity in the impact of choice by this and other measures of school quality.

IV. Empirical Strategy

AQ 2 We begin by following the standard approach in lottery-based studies of school choice, which estimate the average impact of winning the lottery across multiple schools and grades (Rouse 1998; Hoxby and Rockoff 2004; Cullen, Jacob, and Levitt 2006; Hastings, Kane, and Staiger 2008; Hoxby and Murarka 2009; Abdulkadiroğlu et al. 2011; Deming 2011; Hastings, Neilson, and Zimmerman 2012). We estimate

$$A_{ij} = \delta W_{ij} + \beta X_{ij} + \Gamma_j + \varepsilon_{ij}, \tag{1}$$

where W_{ij} is an indicator variable that is equal to one if student i has a winning lottery number for admission to school j, A_{ij} are academic outcomes of interest, X_{ij} is a vector of prelottery covariates that is included only for improved precision, Γ_j is a set of lottery fixed effects, and ε_{ij} is a stochastic error term.[9] We use only first choices in the model, so the number of observations in the regression is simply equal to the number of students in the sample. In principle we could estimate a nested model that incorporates multiple choices and accounts for students' "risk sets" (Abdulkadiroğlu

the ninth-grade cohorts of 1998 and 1999 and capture the school-level residual μ_j as our estimate of school "value added."

[8] Since the lottery sample comprises students from the 2000–2002 ninth-grade cohorts, there is no direct overlap in classrooms between lottery applicants and the students used to construct the "value-added" measure. The results are robust to using only one of the two years, and to using later grade cohorts with lottery applicants excluded from the calculation.

[9] The lotteries were actually conducted at the school-grade-priority group level, so the number of lotteries is greater than the number of schools. We suppress subscripts for grade and priority group for notational convenience. The X_{ij} vector includes controls for median household income in the 2000 census block group, race, gender, free or reduced-price lunch, a third-order polynomial in eighth-grade math and reading test scores plus indicator variables for missing scores, indicators for the level of math taken in eighth grade (since some students are already enrolled in advanced math), and neighborhood (i.e., sending) school fixed effects.

et al. 2011). However, since students who lost the lottery to attend their first-choice school were generally shut out of other oversubscribed schools, there is almost no randomization on second and third choices.

The β parameter from equation (1) gives the intent-to-treat (ITT) impact of winning the lottery on student outcomes. In most specifications, we use the lottery assignment as an instrumental variable (IV) for enrollment in a student's first-choice school in the fall of 2002. This results in the following two-stage least squares (2SLS) specification with enrollment E_{ij} as the endogenous variable in the first stage:

$$E_{ij} = \theta W_{ij} + \pi X_{ij} + \Gamma_j + \varepsilon_{ij} \tag{2}$$

$$A_{ij} = \delta \hat{E}_{ij} + \beta X_{ij} + \Gamma_j + \omega_{ij}. \tag{3}$$

Since some students who lost the lottery still managed to enroll in their first choice, these estimates are local average treatment effects (LATEs) for students who comply with their lottery status (Angrist, Imbens, and Rubin 1996). Lottery fixed effects Γ_j are necessary to ensure that the ex ante probability of admission to a first-choice school does not differ between lottery winners and losers (Rouse 1998). In equation (3), δ gives the weighted average of outcome differences summed over each individual lottery, with weights equal to $N \times [p(1-p)]$ where N is the number of applicants and p is the probability of admission (Cullen, Jacob, and Levitt 2006).

We can test the validity of the randomization by replacing the outcomes A_{ij} in equation (3) with predetermined covariates such as race, gender, and prior test scores. If the randomization was conducted correctly, winners and losers should be balanced on all characteristics that are fixed at the time of the lottery. We test this in online Appendix Table 2 and find no statistically significant differences between lottery winners and losers along predetermined covariates.

V. Results

A. *Main Results*

Table 3 examines the impact of attending a first-choice school on postsecondary outcomes including college enrollment and degree completion. Each row contains an estimate of the 2SLS model in equations (2) and (3), where the lottery is used as an instrument for enrollment. Standard errors appear below each estimate in brackets. They are clustered at the individual lottery level. Column 1 shows results for the full sample. Overall, we find small positive impacts of winning the lottery on four-year college enrollment and degree completion. However, we find statistically significant increases in enrollment and degree completion of about 4 percentage points in colleges that are classified by the 2009 Barron's Profile of American Colleges as "very competitive" or higher, which we refer to here as "selective" colleges.[10] These are large proportional impacts, about 40 and 60 percent increases from the control

[10] Schools in North Carolina with a rating of "very competitive" or higher include Appalachian State University, Duke University, Elon University, North Carolina State University, UNC-Asheville, UNC-Chapel Hill, UNC-Wilmington, and Wake Forest University.

Table 3—Impacts on Educational Attainment

		Gender		Neighborhood school quality		Low-quality neighborhood schools	
	All (1)	Male (2)	Female (3)	High (4)	Low (5)	Male (6)	Female (7)
Ever attended:							
Any four-year college	0.018	−0.091	0.169*	−0.031	0.081	−0.038	0.220*
	[0.058]	[0.056]	[0.076]	[0.065]	[0.077]	[0.073]	[0.110]
Selective four-year college	0.042*	0.007	0.084	0.040	0.042	0.040	0.052
	[0.019]	[0.024]	[0.045]	[0.026]	[0.032]	[0.049]	[0.040]
Earned a degree from:							
Any four-year college	0.047	−0.013	0.139*	−0.030	0.149**	0.106	0.226*
	[0.049]	[0.043]	[0.070]	[0.057]	[0.055]	[0.062]	[0.095]
Selective four-year college	0.040*	0.004	0.089*	0.026	0.057**	0.036	0.096*
	[0.017]	[0.014]	[0.046]	[0.023]	[0.026]	[0.025]	[0.047]
Attainment index	0.078*	−0.009	0.193**	−0.028	0.182**	0.100	0.288**
	[0.034]	[0.043]	[0.070]	[0.044]	[0.070]	[0.078]	[0.111]
Sample size	1,865	994	871	1,070	795	416	379

Notes: Each estimate reports the local average treatment effect (LATE) of attending a first-choice school, using enrollment in fall 2002 as the endogenous variable in the first stage of the 2SLS system in equations (2) and (3). Standard errors are below each estimate in brackets and clustered at the lottery (school-grade-priority group) level. In columns 2 through 7, indicators for winning the lottery are interacted with the subgroup categories as instruments, and each set of subgroups (i.e., gender, gender and school quality) is mutually exclusive and collectively exhaustive. "Low-quality" neighborhood schools are the four lowest ranked schools on the college "value-added" measure listed in Table 2—all others are defined as "high quality." The attainment index in the last row is a summary measure of all the outcomes above plus enrollment and degree completion in any college (including two-year and "most competitive" colleges), and is weighted to account for dependence across outcomes as described in the text. Measures of college quality are calculated using the 2009 Barron's Profile of American Colleges—see text for details.

** Significant at the 1 percent level.

* Significant at the 5 percent level.

mean baselines of 11 percentage points for attendance and 7 percentage points for degree completion.[11]

The last row of Table 3 presents results from a summary index that combines information across all attainment outcomes (O'Brien 1984; Kling, Liebman, and Katz 2007; Anderson 2008; Deming 2009).[12] In addition to the outcomes listed in Table 3, the summary index also includes enrollment and degree completion in any postsecondary institution (including two-year colleges) and in "most competitive" colleges, the most selective category according to the Barron's rankings.[13] To create the index, we first normalize each outcome to have a mean of zero and a standard deviation of one. We then create a single summary index variable that averages across outcomes and weights by the inverse of the sample covariance matrix to account for dependence across outcomes (O'Brien 1984). In the last row of column 1, we see

[11] Since some enrollment and degree outcomes are relatively rare, we explore the sensitivity of our results to a nonlinear logit specification in online Appendix Table 4. In general, the results and their statistical significance (overall and for the subgroups in Table 5 when applicable) hold up to nonlinear specifications such as logit and probit (not shown).

[12] We thank the editor for this suggestion.

[13] Schools in North Carolina with a rating of "most competitive" or higher include only Davidson, Duke, UNC-Chapel Hill, and Wake Forest. The additional outcomes are listed in online Appendix Table 3.

that lottery winners score 0.078 standard deviations higher on the attainment index, and the impact is statistically significant at the 5 percent level.

In columns 2 and 3 we examine gender heterogeneity in the impact of winning the lottery. The results are from a single estimate of equations (2) and (3) with a full set of interactions between winning the lottery and indicator variables for whether a student is male or female. There are large and statistically significant differences in impacts on four-year college attendance for girls versus boys. Girls who attend their first-choice school are almost 17 percentage points more likely to attend a four-year college and 8 percentage points more likely to attend a very competitive college. In contrast, boys are actually 9 percentage points less likely (but not significant) to attend a four-year college and no more likely to attend a very competitive college. Turning to degree completion, girls are 14 and 9 percentage points more likely to complete a degree at a four-year college and a very competitive college respectively, with no significant impact for boys.[14] Overall, we find an increase in postsecondary attainment of about 0.19 standard deviations for girls, with zero impact for boys, and the gender differences are statistically significant at the 1 percent level. This matches the growing body of evidence that girls benefit academically more than boys from educational interventions (e.g., Hastings, Kane, and Staiger 2006; Anderson 2008; Angrist, Lang, and Oreopoulos 2009; Angrist and Lavy 2009; Deming 2009; Jackson 2010; Lavy and Schlosser 2011; Lavy, Silva, and Weinhardt 2011; Legewie and DiPrete 2012).[15]

Finally, we examine heterogeneity by neighborhood (i.e., sending) school. Because every student who applies to the same school (within a given priority group) has the same ex ante chance of admission, an applicant's neighborhood school is a valid covariate on which to split the sample, similar to race or prior test scores. This setup allows us to compare treatment effects for students who applied to the same school, but who had outside options of different quality, generating variation in school quality gains within lottery. We divide schools into two groups based on their college "value added." We advisedly label the four lowest-ranked schools on the college value-added measure as "low quality" and all other neighborhood schools as "high quality." This dichotomization of schools is not sensitive to changes such as removing a particular school or including another.[16] On average, students who win lotteries and come from low-quality neighborhood schools experience a 5.3 percentage point increase in college value added, compared to a decline of

[14] Male lottery winners are somewhat more likely to attend and complete a degree at a two-year college, although the impact is not statistically significant. We do find marginally significant increases in "most competitive" college enrollment among boys, although the total number of attendees is very small. Part of the large increase in four-year (not competitive) college attendance and degree completion is driven by greater female attendance at for-profit colleges, which have shown mixed results in terms of return on investment (e.g., Deming, Goldin, and Katz 2012). The impacts are somewhat smaller (but still significant) when for-profit colleges are excluded. All these results are in online Appendix Table 4.

[15] We also examine heterogeneous impacts by race, poverty, whether students' eighth-grade math score is above or below the median in the sample, and rising grade cohort (ninth versus tenth or eleventh). While there are some differences in outcomes by student group, the summary index measures of attainment are never significantly different from each other, nor are they as large as the gender differences shown in Table 3. Those results are reported in online Appendix Table 5.

[16] In online Appendix Table 6, we present results based on some alternative rules for grouping schools based on quality. In online Appendix Table 7, we allow the impact of winning the lottery to vary continuously with college "value added" by using college "value added" rather than enrollment as the endogenous variable in the 2SLS system in equations (2) and (3). None of these alternative procedures changes the substantive nature of our conclusions.

1.5 percentage points among applicants with high-quality neighborhood schools. This difference in school quality "dosage" is statistically significant at less than the 1 percent level.

Columns 4 and 5 show results separated by neighborhood school quality. Overall, neighborhood school quality is a strong predictor of the impact of choice on postsecondary attainment. We find large and statistically significant increases in high school graduation and college degree completion among applicants with low-quality neighborhood schools, but no significant impacts in the "high-quality" sample. An F-test of the joint hypothesis that the results on the attainment index are significantly different by neighborhood school quality yields a p-value of 0.019. In columns 6 and 7, we show separate results by gender, within the low-quality neighborhood school sample. These estimates come from a single regression specification with all four gender by school quality combinations. In the low-quality neighborhood school sample, we find positive (but imprecise) gains for boys and large, statistically significant gains in attainment for girls. Notably, the gender difference in impacts in the low-quality neighborhood school sample (0.10 SDs for boys, 0.29 SDs for girls) is very similar in size to the full sample (−0.01 SDs for boys, 0.19 SDs for girls).

Before proceeding, we address some potential concerns with the interpretation of above results. First, we have college attendance data from the NSC through the spring of 2011. This means that rising ninth-grade students who progress normally through high school would be able to attend a maximum of ten semesters (fall 2006 to spring 2011) of college. Thus, for rising ninth-grade students in 2002 our outcome is completion of a degree within five years of high school graduation, with additional years available for tenth- and eleventh-graders. If lottery applicants are more likely to enroll and progress through college "on time" for their grade cohort, this limited window of data could upwardly bias our results for degree completion.

To address this concern, we examine the subset of students in our sample who appear to be persisting continuously in college through spring 2011. In online Appendix Table 8, we reconstruct our main outcomes in Table 3 under the assumption that all lottery losers, but no lottery winners, who persist continuously eventually obtain a degree. With this very conservative assumption, we find a decline in the impact on four-year degree completion of only about 3.5 percentage points, and zero impact on degree completion at very competitive colleges. Moreover, results for the summary index of attainment are still positive and marginally significant. This suggests that the increase in degree completion among lottery winners is unlikely to decline very much with additional years of data.

A second concern with our interpretation is that neighborhood school quality is simply an indicator for other differences between students in the two samples. For example, students from low-quality neighborhood schools differ systematically by income and prior test scores, and those characteristics may drive the gains from choice. However, in online Appendix Table 9 we show that the greater impacts in the "low-quality" neighborhood school sample hold within splits across a wide variety of covariates, including race, poverty, and prior test scores. Following Angrist, Pathak, and Walters (forthcoming), we implement a Oaxaca-Blinder-style decomposition, which shows that the difference in impacts by neighborhood school quality cannot be attributed to differences in observed student characteristics.[17] While we cannot fully rule out that lottery applicants differ across neighborhood schools along unobserved dimensions in ways that are correlated with the impacts of choice, we find no evidence that differences in observed student characteristics are driving the differences in impacts by neighborhood school quality.

B. *School Characteristics*

We next investigate the extent to which school characteristics, including peers, may contribute to our findings. Table 4 reports the impact of attending one's first-choice school on peer and school characteristics, including summary index measures of peer quality and school resources (including teachers).[18] It follows the same structure as Table 3. We report results for two measures of school quality that attempt to control for observed differences in peers—college "value added" (from Table 2), and a school-level measure of "on track" in ninth grade that controls for prior differences in academic preparation.[19] We note that these residualized measures of school quality will not account for unobserved differences or nonlinearities in peer quality, such as a "critical mass" of able peers, which could lead directly to changes in course offerings, teacher quality and improved resources. Due to space constraints we present only a limited selection of school characteristics, with the full set of results available in online Appendix Table 11.

The combined results of Table 4 lead to two important conclusions. First, comparing columns 2 and 3 and columns 6 and 7, we find no evidence for gender differences in measures of peer or school quality. Since boys and girls in the lottery sample apply to and attend similar schools and come from similar neighborhoods, the pattern of results must reflect gender differences in responsiveness to school or peer quality. Second, comparing columns 4 and 5, we find significant differences in both peer and school quality between applicants coming from low- versus high-quality neighborhood schools. Since our quality measure is college "value added" (seen here in the last row of Table 4), this amounts to observing that college "value added" is strongly correlated with a variety of measures of school quality.

In the first three rows of Table 4, we see that lottery winners from low-quality neighborhood schools who attend their first-choice school have peers that are significantly stronger academically (e.g., 0.36 standard deviations higher eighth-grade math scores), but relatively similar in terms of demographics. Moving to school

[17] The results of this decomposition are in online Appendix Table 10, along with a more detailed description of the procedure.

[18] Students in the lottery sample are excluded from the calculation of the peer quality measures to avoid a mechanical correlation with the outcome. Each index is normalized and weighted as described earlier in the text. The peer index contains measures of peers' eighth-grade math and reading scores, days suspended from school, days absent, and prior EOC math coursework (some students take Algebra I in eighth grade). The resource/teacher index includes measures of class size in EOC courses, the ratio of books to students and students to computers, whether students' EOC course teachers are first-year teachers, the selectivity of the colleges attended by EOC teachers (very competitive, most competitive), guidance counselors per capita, and the selectivity of colleges attended by guidance counselors.

[19] The college "value-added" measure is described in Section III. The "on track" measure is a summary index of the school-level residuals from regressions of the probability that a student is in Algebra I or higher and Biology or higher in ninth grade, controlling for a full set of covariates including prior test scores and EOC math placement. For both "on track" measures, we use the rising ninth grade cohorts of 1998 and 1999, several years prior to the lottery, in order to minimize reflection problems with the lottery sample (Manski 1993).

Table 4—Impacts on School Characteristics

		Gender		Neighborhood school quality		Low-quality neighborhood schools	
	All (1)	Male (2)	Female (3)	High (4)	Low (5)	Male (6)	Female (7)
Peer characteristics							
Percent Black/Hispanic	0.042	0.056	0.024	0.107	−0.044	−0.038	−0.050
	[0.069]	[0.071]	[0.067]	[0.077]	[0.082]	[0.082]	[0.083]
Peer math score—school	0.168	0.151	0.189*	0.018	0.363**	0.368**	0.357**
	[0.088]	[0.091]	[0.087]	[0.085]	[0.100]	[0.104]	[0.098]
Peer index	0.540**	0.494*	0.601**	0.199	0.982**	0.991**	0.869**
	[0.185]	[0.200]	[0.185]	[0.189]	[0.205]	[0.206]	[0.193]
School characteristics:							
Class size	−0.600	−0.720	−0.440	−0.470	−0.830**	−1.030*	−0.600
	[0.500]	[0.500]	[0.520]	[0.770]	[0.410]	[0.440]	[0.450]
Percent first year teachers	0.118	0.140	0.090	0.151*	0.078	0.092	0.060
	[0.073]	[0.070]	[0.076]	[0.076]	[0.079]	[0.074]	[0.084]
Teacher BA—selective college	−0.010	−0.025	0.009	−0.098**	0.095*	0.083	0.111*
	[0.036]	[0.036]	[0.038]	[0.030]	[0.049]	[0.047]	[0.051]
Resource/teacher index	−0.140	−0.148	−0.131	−0.257	0.006	−0.004	0.021
	[0.117]	[0.111]	[0.132]	[0.158]	[0.114]	[0.110]	[0.108]
College preparatory factors:							
Percent "on track" in math	0.115**	0.114**	0.116**	0.063*	0.177**	0.176**	0.178**
	[0.017]	[0.014]	[0.023]	[0.019]	[0.015]	[0.016]	[0.016]
Residual "on-track" index	0.545**	0.515*	0.585**	0.220	0.935**	0.985**	0.864**
	[0.202]	[0.207]	[0.220]	[0.199]	[0.257]	[0.265]	[0.244]
College "value added"	0.019	0.020	0.018	−0.015	0.053**	0.052**	0.059**
	[0.016]	[0.016]	[0.016]	[0.010]	[0.009]	[0.010]	[0.011]
Sample size	1,865	994	871	1,070	795	416	379

Notes: Each estimate reports the local average treatment effect (LATE) of attending a first-choice school, using enrollment in fall 2002 as the endogenous variable in the first stage of the 2SLS system in equations (2) and (3). Standard errors are below each estimate in brackets and clustered at the lottery (school-grade-priority group) level. In columns 2 through 7, indicators for winning the lottery are interacted with the subgroup categories as instruments, and each set of subgroups (i.e., gender, gender and school quality) is mutually exclusive and collectively exhaustive. "Low-quality" neighborhood schools are the four lowest ranked schools on the college "value-added" measure listed in Table 2—all others are defined as "high quality." The classroom and teacher measures are calculated for students' EOC math courses, which are required for graduation with a college-preparatory diploma. Measures of college quality are calculated using the 2009 Barron's Profile of American Colleges—see text for details. Each index variable is a summary measure of all the relevant outcomes listed above it plus additional outcomes listed in the text and in online Appendix Table A11, and they are weighted to account for dependence across outcomes as described in the text.

** Significant at the 1 percent level.
* Significant at the 5 percent level.

characteristics, we see that they have significantly smaller classes in End-of-Course (EOC) subjects and are substantially more likely to have a teacher with a bachelor's degree from a selective college. In contrast, lottery winners from high-quality neighborhood schools experience no significant increases in observed measures of peer or school quality, with significant declines in resources in a few cases. Finally, we also find that lottery winners attend schools with a greater share of students who are "on track" in their math and science courses toward a college-preparatory diploma. This measure reflects a combination of (i) differences in the prior academic preparation of peers, and (ii) differences in the academic rigor of the school, holding peer quality constant. While these mechanisms cannot be fully separated, we can show that lottery winners from low-quality neighborhood schools attend schools where students are more likely to stay on track *conditional on baseline test scores*. For lottery winners with low-quality neighborhood schools, the magnitude (0.93 standard deviations on the residual "on-track" index, column 5) is very similar to the change in peer quality (0.98 SDs).

While the impacts on some measures appear larger than on others, this is difficult to interpret without information about the causal impact of each input on postsecondary attainment. For example, even though the mean impact of choice on resource and teacher quality is not statistically significant, perhaps there are certain inputs (for example, teacher or guidance counselor college selectivity) that have a large influence on student outcomes, and thus should be weighted more heavily. Without a variety of experiments that carefully manipulate teachers, peers, and other school attributes, we cannot separately identify the impact of each input. Moreover, as Table 4 shows, they are highly collinear.

Still, the distinction between peers and inputs that can be affected directly by policy is particularly important. Jackson (2010) finds that direct peer quality accounts for only 10 percent of school "value added" overall, but one-third among highly selective schools. Similarly, recent work that identifies the impacts of attending a better school for students at the margin of admission using a regression discontinuity (RD) design finds mixed results (Clark 2010; Jackson 2010; Pop-Eleches and Urquiola 2011; Abdulkadiroğlu et al. 2011; Dobbie and Fryer 2011). In particular, Dobbie and Fryer (2011) find no impact of admission to selective exam schools in New York City on postsecondary outcomes, despite large gains in peer quality. Because these RD studies necessarily compare the highest-scoring students in a lower-ranked school to the lowest-scoring students in a higher-ranked school, they identify peer effects (and responses to resources and other school-level differences) at a particular margin. These impacts will not necessarily generalize to the range of applicants in a school choice lottery, particularly in high school when identity and peer sorting can generate unpredictable results (Akerlof and Kranton 2002; Carrell, Sacerdote, and West 2011; Cicala, Fryer, and Spenkuch 2011).

Our main results for attainment are broadly consistent with the gains we find on measures of peer quality. However, we also find that the impacts for lottery winners are larger when they gain more resources and teacher quality (although the estimates are noisy), and when they gain more on school-level measures of college "value added" and keeping students "on track." Importantly, these last two measures explicitly control for observed differences in peers across schools and may be proxies for important school policies and practices that we do not observe. Nonetheless, the evidence on mechanisms is ultimately suggestive, particularly in an environment where students, peers, teachers, and parents may respond by adjusting their behavior or effort in a variety of ways (Pop-Eleches and Urquiola 2011).

One alternative approach, which we pursue in online Appendix Table 7, is to use various measures of school quality as first-stage endogenous variables. In principle, we could determine which measures of quality are most correlated with increased attainment, controlling for the others. However, while many of the measures are strong predictors independently, there is insufficient variation to identify mechanisms when multiple measures are included.

TABLE 5—IMPACTS ON MEDIATING OUTCOMES

		Gender		Neighborhood school quality		Low-quality neighborhood schools	
	All (1)	Male (2)	Female (3)	High (4)	Low (5)	Male (6)	Female (7)
High school GPA	0.127*	0.043	0.239**	0.077	0.189	0.119	0.271*
	[0.058]	[0.079]	[0.088]	[0.058]	[0.101]	[0.117]	[0.107]
GPA rank within school cohort	−4.230	−6.440*	−1.930	−6.490*	−3.060	−4.510	−1.340
	[2.520]	[3.160]	[3.120]	[2.740]	[3.990]	[4.350]	[4.360]
Number EOC math courses	0.230**	0.084	0.424**	0.101	0.353**	0.200	0.534**
	[0.050]	[0.069]	[0.087]	[0.069]	[0.091]	[0.127]	[0.106]
AP calculus	−0.004	−0.008	0.001	−0.031	0.031	0.026	0.037
	[0.025]	[0.035]	[0.054]	[0.029]	[0.047]	[0.039]	[0.080]
"On track" in math, 2002–2003	0.116**	0.110*	0.124**	0.071	0.161*	0.147*	0.175*
	[0.040]	[0.053]	[0.035]	[0.038]	[0.063]	[0.072]	[0.068]
"On track" in math, 2003–2004	0.090*	0.038	0.158**	0.009	0.186**	0.091	0.256**
	[0.043]	[0.043]	[0.053]	[0.055]	[0.063]	[0.073]	[0.071]
Ever failed an EOC math course	0.051	0.093*	−0.005	0.073	0.040	0.034	0.022
	[0.031]	[0.039]	[0.034]	[0.046]	[0.040]	[0.066]	[0.053]
SAT score	−3.570	4.780	−13.190	−26.750	5.480	5.180	3.890
	[14.850]	[21.830]	[18.170]	[26.840]	[17.810]	[27.760]	[18.740]
Took the SAT	0.036	−0.070	0.177**	−0.048	0.124*	0.013	0.254**
	[0.028]	[0.052]	[0.060]	[0.050]	[0.063]	[0.080]	[0.096]
Graduated from CMS	0.055	0.031	0.082	−0.022	0.138**	0.121	0.142
	[0.032]	[0.039]	[0.065]	[0.053]	[0.055]	[0.077]	[0.075]
Mediator index	0.056*	−0.004	0.135**	−0.006	0.121**	0.069	0.229**
	[0.021]	[0.037]	[0.047]	[0.033]	[0.037]	[0.048]	[0.056]
Sample size	1,865	994	871	1,070	795	416	379

Notes: The sample size listed is for the mediator summary index—sample sizes for individual outcomes vary. Each estimate reports the local average treatment effect (LATE) of attending a first-choice school, using enrollment in fall 2002 as the endogenous variable in the first stage of the 2SLS system in equations (2) and (3). Standard errors are below each estimate in brackets and clustered at the lottery (school-grade-priority group) level. In columns 2 through 7, indicators for winning the lottery are interacted with the subgroup categories as instruments, and each set of subgroups (i.e., gender, gender and school quality) is mutually exclusive and collectively exhaustive. "Low-quality" neighborhood schools are the four lowest ranked schools on the college "value-added" measure listed in Table 2—all others are defined as "high quality." EOC math are state standardized courses in Algebra I, Geometry, and Algebra II and are required for graduation with a college-preparatory diploma. The mediator index in the last row is a summary measure of all the outcomes above it plus the outcomes listed in Appendix Table A12 and is weighted to account for dependence across outcomes as described in the text.

** Significant at the 1 percent level.

* Significant at the 5 percent level.

More important, however, any explanation of our results must account for gender differences in responsiveness to peer or school quality. We examine these in more detail in Table 5.

C. *Mediating Outcomes in High School*

Table 5 presents results for a broad range of academic outcomes in high school. Due to space constraints we present only a small selection of academic outcomes, with the full set of results available in online Appendix Table 12. The last row of Table 5 is a summary index of all mediating outcomes (constructed identically to the summary indices in Tables 3 and 4), including those not presented in the table. In reading this table it is important to remember that we do not observe high school outcomes (as opposed to college outcomes) for students who leave CMS.[20] However, high school outcomes may uncover important intermediate changes in student's achievement and experiences, pointing to potential mechanisms that underlie our main results.

Lottery winners who attend their first-choice schools have higher cumulative grade point averages, complete more total EOC math courses, and are more likely to stay "on track" towards completing college preparatory math requirements.[21] We find no increases in advanced placement (AP) math course-taking, nor do we find evidence of increased enrollment in advanced math or science classes such as precalculus, statistics, or physics (results for these subjects and many others, including extracurricular programs, are included in online Appendix Table 13). In the last row of column 1, we find an overall increase of about 0.06 standard deviations on a summary index of all mediating outcomes. In columns 2 and 3, we see that, like postsecondary attainment, these overall gains are driven entirely by girls. The pattern of impacts by gender and school quality for the main results in Table 3 also holds here, with small and imprecise gains for boys but large gains for girls from "low-quality" neighborhood schools.

We find no impact on the SAT scores overall or in any of the subsamples in Table 4. Yet we do find large and statistically significant increases in SAT exam-taking among girls, especially girls in the low-quality neighborhood school sample. This pattern of imprecise impacts on scores but increases in taking the exam and "on-track" course-taking also holds across all of the EOC math and science subjects. Lottery winners who attend their first-choice school are more likely to graduate from a CMS high school, with a larger point estimate among girls, though coming from a low-quality school appears to be the driving factor for this outcome.

Overall, the pattern of impacts for mediating outcomes closely matches the main results in Table 3. The Mediator Index in the final row tells a clear story of girls responding positively to their new academic environment, suggesting that the gender differences we find in our main results are reflected in gender differences in high school experiences.

VI. Explaining Gender Differences in Responsiveness to School Quality

We consider three broad explanations for gender differences in responsiveness to school quality. First, the girls may differ from the boys in the lottery sample in terms of prior academic preparation or other characteristics. However, we find no

[20] In online Appendix Table 13 we show that lottery winners are 2 percentage points more likely to enroll in CMS in the fall after choice, and approximately 5 percentage points more likely to be enrolled in CMS by spring 2004. We investigate the sensitivity of these results to a wide variety of assumptions about differential attrition in online Appendix Table 14. In general, imputation of missing scores and course-taking variables leaves the results substantively unchanged. Bounding exercises for exam and course-taking results generally fail to diminish the statistical significance of those findings. However, bounding exercises for EOC test score or SAT impacts lead to confidence intervals that are too wide to draw any firm conclusions.

[21] EOC math courses are Algebra I, Geometry, and Algebra II, with state-standardized exams at the end of course. "On Track" is defined as taking Algebra I by ninth grade, Geometry by tenth grade and Algebra II by eleventh grade, given that the North Carolina requirements for a college-preparatory diploma are the three EOC courses plus one additional more advanced course (four total).

evidence of gender differences in pretreatment covariates such as income and prior test scores. Girls and boys in the lottery sample have nearly identical eighth-grade test scores, and we find no difference in covariates by gender within choice lotteries. Moreover, girls and boys are balanced across neighborhoods and choice schools, and an analysis of individual lotteries reveals that the treatment effect for girls is greater in nearly every case. Thus we conclude that the pattern of impacts by gender is not a function of other observed characteristics.

Second, girls may respond to new environments and peer groups in ways that are more conducive to academic achievement. Several recent studies in school settings have found greater impacts for girls (e.g., Hastings, Kane, and Staiger 2006; Kling, Liebman, and Katz 2007; Anderson 2008; Angrist, Lang, and Oreopoulos 2009; Angrist and Lavy 2009; Jackson 2010). Qualitative work on gender differences in the Chicago public schools and in the Moving to Opportunity housing mobility experiment found important gender differences in coping mechanisms and responses to the stress of a new environment, perhaps because of an absence of same-sex role models in the home or in school, and greater conflict with the norms of an academic or culturally dominant environment (Roderick 2003, Clampet-Lundquist et al. 2006).

A related possibility is that boys respond less productively to increased competition from peers. Niederle and Vesterlund (2007) find that while males are more likely to seek out tournament competition conditional on ability, they also overestimate their performance rank within a group. Interestingly, Barankay (2011) finds that when employees are privately informed about their performance rank within a group, males respond more negatively to declines in relative ranking. The results in Table 5 show that female lottery winners have higher GPAs, while boys do not. However, GPAs are higher on average in higher quality schools. We find that the GPA improvements for female lottery winners place them at the same class rank as girls who lose the lottery (second row of Table 5, columns 2 and 3). However, the insignificant results for boys' GPA lead to a statistically significant decline of about 6.5 percentile ranks within grade cohort. This evidence is consistent with boys responding negatively (or less positively) to increased competition in a new school environment. This pattern holds as well for the summary index of mediating outcomes—female lottery winners and losers perform at about the median for their school, but lottery winners attend schools where students have better grades and more difficult coursework. On the other hand, male lottery winners typically rank lower in the distribution of grades and course rigor when they attend their first-choice school.

Third, girls may increase effort more than boys in response to a more academically demanding environment. While boys and girls are equally likely to be "on track" in math at the end of the first school year after choice, by the end of the second year only girls are still more likely to be "on track" for a college-preparatory diploma (rows four and five of Table 5). In row six of Table 5, we see that this is likely due to a statistically significant *increase* in the probability that male lottery winners (especially in the "high-quality" neighborhood school sample) will fail an EOC math course at some point during high school. The lack of any initial gender difference in the probability of being "on track" suggests that while boys and girls are initially assigned to similar classes, boys are more likely to struggle. Several studies have found that conditional on academic ability, girls spend more time on homework and have better study habits (Jacob 2002; Hastings, Kane, and Staiger 2006; Frenette and Zeman 2007). Girls might work harder in higher-quality schools because they have higher expected returns to a college education (Charles and Luoh 2003; Goldin, Katz, and Kuziemko 2006; DiPrete and Buchmann 2006). Increased effort could also be a response to gender differences in peer group pressure (Akerlof and Kranton 2002, Clampet-Lunquist et al. 2006).

VII. Discussion and Conclusion

In this article we study the impact of winning an admissions lottery to attend a public high school in Charlotte-Mecklenburg on college enrollment and degree completion. We find increases in postsecondary attainment that are concentrated among girls and students from low-quality neighborhood schools. We show that the benefits of choice are greater for lottery applicants who experience larger gains in school quality, although we are unable to separately disentangle mechanisms such as peer effects, resources, and teachers that may be at play.

This finding is important in light of the growing returns to postsecondary education and increasing inequality of opportunity by race and income in the United States (Duncan and Murnane 2011). Our findings imply that school choice can lead to long-run gains in educational attainment, but only when applicants gain access to higher-quality schools. Our results also show that high school quality exerts an important influence on some students' life chances, suggesting that later life interventions may have a high social return on investment, provided that we can uncover the correct mechanisms (e.g., Heckman 2006).

Finally, we find that girls are more responsive than boys to gains in school quality. While ultimately we can only speculate about the reasons, we note that the results are consistent with growing evidence on the reverse gender gap in achievement when low-income children are moved into a more academically competitive environment (Goldin, Katz, and Kuziemko 2006; Hastings, Kane, and Staiger 2006; Kling, Liebman, and Katz 2007; Bailey and Dynarski 2011). Uncovering the underlying reasons for gender differences in responsiveness to an improved environment remains an important issue for explaining the growing female advantage in completed schooling.

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