"No Excuses" Charter Schools and College Enrollment New Evidence From a High School Network in Chicago*

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Original Draft: May 2015
This Version: July 2017

Abstract

While it is well-known that certain charter schools dramatically increase students' standardized test scores, there is considerably less evidence that these human capital gains persist into adulthood. To address this matter, we match three years of lottery data from a high-performing charter high school to administrative college enrollment records and estimate the effect of winning an admissions lottery on college matriculation, quality, and persistence. Seven to nine years after the lottery, we find that lottery winners are 10.0 percentage points more likely to attend college and 9.5 percentage points more likely to enroll for at least four semesters. Unlike previous studies, our estimates are powerful enough to uncover improvements on the extensive margin of college attendance (enrolling in any college), the intensive margin (persistence of attendance), and the quality margin (enrollment at selective, four-year institutions). We conclude by providing non-experimental evidence that more recent cohorts at other campuses in the network increased enrollment at a similar rate.

*Special thanks to Kyle Cole, Tom Mathews, Jessica Flores, and Noble staff for their support and leadership during this project. We are grateful to Roland Fryer, Josh Goodman, and Marty West for helpful comments and suggestions. A previous version of this paper circulated under the title "Do 'No Excuses' Charter Schools Raise More Than Test Scores? Evidence from a High School Network in Chicago." Financial support from the Education Innovation Laboratory at Harvard University and a National Science Foundation Graduate Research Fellowship is gratefully acknowledged. Correspondence can be addressed to the authors at: mattda@wharton.upenn.edu (Davis) and bheller@fas.harvard.edu (Heller). All errors are our own.

1 Introduction

As a college education becomes increasingly necessary to succeed in the modern American labor market and achieve upward socioeconomic mobility (Goldin and Katz 2007; Autor, Katz, and Kearney 2008; Dynarski 2008), a college degree remains one of the best investments available to young Americans (Barrow and Rouse 2005; Heckman, Loechner and Todd 2008; Acemoglu and Autor 2010; Greenstone and Looney 2011; Oreopoulos and Petronijevic 2013). It is therefore unsurprising that many high-performing urban schools have focused on making college the default option for low-income students. In the contentious debate over school accountability and high-stakes testing, both sides can agree that preparing students to reach their potential as productive citizens generally means preparing them to enroll and succeed in post-secondary studies.

The consensus on how to achieve this goal is far less clear. Many in the education reform movement advocate an interventionist approach, arguing that schools must take an intensive, hands-on role in the lives of poor children (Thernstrom and Thernstrom 2004, Whitman 2008). As evidence, these advocates point to the growing number of studies showing that high-performing charters dramatically raise the test scores of low-income students.[1] On the other hand, some critics argue that the relentless focus on test scores detracts from real learning and is harmful to students in the long run (Ravitch 2010). The root of the controversy is essentially an empirical question about the education production function. Even though there is a robust positive correlation between test performance and college-going,[2], it is not obvious that short-run achievement effects necessarily translate into college success.

To advance this debate, we exploit the outcomes of randomized admissions lotteries for three cohorts of students who applied to enter the Noble Street Charter School, a high-performing charter high school in Chicago. We match these records to administrative college enrollment data to estimate the effect of being selected to attend Noble on college matriculation and persistence. We find that lottery winners are 10.0 percentage points more likely to enroll in college and 9.5 percentage points more likely to stay enrolled for at least four semesters. Effects on college graduation are not statistically different from zero, but this is at least partially reflects the fact the largest cohort in

[1] See, for example, Angrist et al. (2010), Tuttle et al. (2010), Abdulkadiroglu et al. (2011), Dobbie and Fryer (2011), and Curto and Fryer (2014)

[2] In the National Longitudinal Survey of Youth 1979 sample (NSLY79), a one-standard deviation increase in a student's Armed Forces Qualifying Test (AFQT) score is associated with a 23.2 percentage point increase in the likelihood of attending college (authors' calculations based on OLS regression of college attendance on AFQT scores and a full set of race dummy variables for youths born before 1961.) Controlling for test scores in the NLSY79 sample is sufficient to close – and even reverse – the black-white gap in college attendance (Lang and Manove 2011).

our sample would not have been old enough to graduate from a four year college at the time of our data pull.

Importantly, the increase in quantity did not come at the expense of quality. We estimate nearly identical effects if we restrict attention to attendance at four-year colleges or schools with an average entering SAT score of at least 1,000 (out of 1,600). In addition, the effect on enrollment at two-year colleges is statistically zero. We interpret these findings as strong evidence that Noble is measurably increasing students' human capital as opposed to pushing marginal students into low-quality institutions.

Our work contributes to a broader literature that uses large administrative datasets to understand the causal relationship between school characteristics and college enrollment. Dobbie and Fryer (2015) find that sixth graders who are offered a spot in the Harlem Children's Zone Promise Academy are more likely to enroll in college immediately after graduating high school, though students in the control group eventually "catch up" and close the gap. In a similar vein, Angrist et al. (2016) show that attending a Boston-area charter school causes many students to enroll in four-year colleges instead of two-year colleges. Other work focuses on the long-term impacts of educational quality rather that school governance. Linking data from a large urban district to tax records, Chetty, Friedman, and Rockoff (2014) use a quasi-experimental design to show that a one-standard-deviation increase in teacher value-added for a single year increases a student's probability of college attendance at age 20 by 0.82 percentage points. Deming et al. (2014) examine school choice lottery results from Charlotte-Mecklenburg and conclude that attending a first-choice high-school leads to small but noteworthy increases in enrollment and graduation rates from competitive colleges.

We make three key contributions to the literature. First, we provide robust evidence that a school-based intervention can simultaneously improve overall enrollment, persistence, and college quality. In particular, we provide the first experimental evidence that a high-quality charter high school can increase overall college enrollment for more than one year.[3] In part, this is a consequence of improved statistical power. Noble's admissions lottery generates a very powerful first-stage effect on enrollment that allows us to estimate a variety of treatment effects more precisely than similar

[3] Booker et al. (2011, 2014) find that charter attendance is positively correlated with college enrollment after controlling for observable characteristics of students, and Sass et al. show that these effects extend to college persistence. Using similar methods, Dobbie and Fryer (2016) find that charter attendance has a small effect on two-year college enrollment only, though schools adhering to the "No Excuses" philosophy also improve enrollment at four-year colleges. Because these estimates do not benefit from random or quasi-experimental variation in charter enrollment, however, they require stronger identifying assumptions than studies exploiting admissions lotteries.

papers in the literature (Dobbie and Fryer 2015, Angrist et al. 2016).

Second, we demonstrate the medium-term effectiveness of a relatively late intervention. Noble conducts lotteries for incoming high-school freshman, many of whom have spent their formative years in low-performing traditional Chicago public schools. As other economists have emphasized the importance of early interventions for improving the economic outcomes of poor children (see Heckman 2012), this aspect of our results is noteworthy.

Finally, the Noble Network's rapid expansion since 2006 allows us to examine whether the intervention we study is scalable. Over the ensuing eight years, Noble added 15 high schools, and the network currently enrolls over 10,000 students each year. The expansion not only shows that the network merits study in its own right, due to the sheer number of students enrolled; it also provides an interesting test case for whether the intervention we study is scalable. An important question is whether the administrative and instructional practices that drove the flagship campus's impact on college outcomes will have similar impacts in new environments across the diverse communities of Chicago. More generally, Noble's rapid expansion offers a unique opportunity to assess how "No Excuses" practices scale and replicate in new settings and populations, albeit within the same city.

Therefore, the final portion of our analysis focuses on results at Noble's broader network. While we cannot exploit lottery-based randomization for this portion of our analysis, we make use of a college enrollment study conducted by Chicago Public Schools (CPS) to non-experimentally estimate treatment effects for two later cohorts attending seven separate Noble campuses. These results re-affirm our lottery-based findings for Noble's flagship campus. Conditional on observables, the college enrollment rate of students in Noble's expanded network exceeds that of both traditional public school and charter school students in Chicago.

The remainder of the paper is organized as follows. Section 2 describes the history and academic practices of the Noble Network of Charter Schools. Section 3 summarizes our data and empirical strategies. Section 4 presents our main results and our non-experimental analysis, and Section 5 concludes by interpreting our results in light of other recent research.

2 The Noble Network of Charter Schools

The Noble Charter Network began in 1999 as a single high school, Noble Street Charter School (now Noble Street College Prep). In the fall of 2006, the operator opened two new campuses to serve the growing demand for admissions, and the network has since grown to include 16 high schools serving roughly 10,000 students. College-readiness is an explicit part of Noble's stated mission "to prepare low-income students with the scholarship, dedication, and honor necessary to succeed in college and lead exemplary lives, and be a catalyst for education reform in Chicago."

Noble's campuses are spread across the west and south sides of Chicago. As Figure 1 shows, the expansion has been concentrated in high-poverty neighborhoods. Accordingly, Noble attracts a predominantly poor, minority student body. 95% of Noble students are Black or Hispanic, and nearly 90% are eligible for federal lunch subsidies based on their family's income.

Noble's educational philosophy is quite similar to that of other "No Excuses" charter schools, such as the Knowledge is Power Program (KIPP) and the Harlem Children's Zone Promise Academy.[4] While no two Charter Management Organizations (CMOs) follow precisely the same principles and practices, the philosophy largely aligns with those highlighted by Dobbie and Fryer (2013), who identify five within-school factors that explain roughly 50% of the variance in charter school performance in their sample. Noble Network schools academic programs can be understand within the context of these five practices.

While exact school calendars vary slightly by campus, Noble campuses offer longer class periods, a longer school day, and a longer school year than traditional Chicago Public Schools (CPS) high schools. On average, Noble students spend 7.5 hours per day and 185 days per year in school. For comparison, during the period covered by our analysis typical CPS high school students spent only 6.9 hours per day and 170 days per year in school on average.[5] This implies that Noble students spent 18.3% more time in school, amounting to 858 hours over 4 years or nearly three-quarters of a year of additional instruction.

Noble campuses structure their day to ensure that all students receive differentiated instruction in a smaller-group setting. Students attend daily 35-minute differentiated academic labs, where they get smaller-group instruction specific to their needs. Typical academic labs include reading groups organized by lexile level, math groups assigned by performance on interim assessments, AP course labs, and social emotional support seminars. Teachers are required to hold daily office hours that students can attend for academic support from 4:00-4:30pm. Office hour attendance becomes mandatory for students whose academic performance falls below certain thresholds. Most campuses also offer some form of after-school tutoring provided by outside organizations.

[4]Other well-known "No Excuses" CMOs include Yes Prep (Houston), MATCH (Boston), Mastery (Philadelphia), Excel (Boston), Green Dot (Los Angeles), Success Academies (New York City), and Uncommon Schools (New Jersey, New York, and Massachusetts.)

[5]Beginning with the 2012-13 school year, CPS and the Chicago Teacher's Union agreed to extend the school day by 34 minutes to 7.5 hours per day and extend the school week by two weeks to 180 days per year.

To help teachers use data to drive instruction, Noble schools administer quarterly interim assessments in all core content areas (in addition to traditional classroom summative and formative assessments). After students take interim assessments, teachers from all campuses meet in content area teams to analyze the data and collaboratively plan how to use the assessment data to drive their instruction. Morning and afternoon advisories track students' academic progress, mark behavioral infractions, and hold students accountable as a group for maintaining academic and behavioral standards.

To develop human capital, teachers receive regular feedback on their performance. Teachers attend campus wide professional development sessions every Friday, in addition to network-wide collaborative planning outlined above. Noble aggressively recruits teachers with a demonstrated track record of success and rewards teachers whose students demonstrate above-average academic growth with performance bonuses.

While school culture is notoriously hard to measure, Noble takes several explicit approaches to incubating a culture of high expectations for all. All students are expected to take college entrance exams and gain acceptance to college, regardless of their post-graduation plans. College acceptances are celebrated publicly, and counselors assist students in applying for grants and scholarships. Top students are invited to participate in the "Right Angle" program, which sends students to academic enrichment programs at highly-selective college campuses nationwide.[6]

3 Data and Research Design

3.1 Raw Data

We combine lottery data and student demographic information from Noble Network administrative records with college matriculation and degree attainment data from the National Student Clearinghouse to generate our empirical estimates. The lottery records span high school lotteries from 1999 to 2012 for all campuses in the Noble Network of Charter Schools, though we can only make use of lottery data for three early cohorts at the original Noble campus. We describe our data sources and the relevant institutional details in the following subsections, and the Online Appendix provides more detail about the raw lottery files and variable construction.

[6] Through the 2013-14 school year, one unique aspect of Noble's culture of high expectations was their use of financial penalties to disincentivize undesirable behavior. For example, students who earned three demerits for rules infractions not only earned a three-hour detention, but were also required to pay a five-dollar fine. Students with twelve or more detentions were required to cover fees totaling $140 for a behavior-improvement class. Starting with the 2014-15 school year, this practice is no longer used by any school in the Noble network.

A. Lottery Data and Sample Construction

Our analysis sample includes all students who entered a Noble admissions lottery between 2003 and 2005. Noble Street Charter School was not oversubscribed in 1999, 2000, or 2001; hence there was no lottery. While Noble did have a binding lottery in 2002, the records are less complete than in subsequent years. We made several attempts to construct the 2002 lottery sample from the files we were provided, but after consulting with Noble staff we were not fully confident in their accuracy. We therefore drop the entire cohort in the interest of caution, though adding them to our sample does does not change any of our findings (these results are available from the authors upon request.)

To meet increased demand, the Noble Network opened two new schools every year between 2006 to 2009 and a single school in 2010. As a result, almost every applicant was offered a spot at at least one of the network's schools. Students entering subsequent lotteries would not complete high school in time to enter our analysis. Therefore, our analysis focuses on the three cohorts of students who entered a lottery between 2003 and 2005.

The raw lottery files include a student's name, gender, date of birth, address, eighth grade school, sibling indicators, lottery result (accepted or waitlist), and waiting list position. Noble also provided us with enrollment records that include all students who enroll in a network school. We merge these files with the lottery data to identify which students eventually enrolled at Noble.

The precise lottery mechanisms used by Noble is essentially identical to those studied by other prominent papers in the literature (Dobbie and Fryer 2015, Angrist et al. 2016). Indeed, it is common to most charter schools that are do not participate in their school district's centralized school choice process, as in, e.g., the Denver charter schools studied by Abdulkadiroglu et al. (2015). Admission is entirely randomized subject to two accommodations for families with multiple children. First, students with an older sibling *already* enrolled at a Noble campus are automatically granted admission to that campus. Second, when two siblings enter the same lottery, both siblings are admitted if either student receives an admissions offer.[7] (We discuss how we incorporate these features of the lottery into our empirical specification in the following subsection.) Conditional on sibling status, admission is entirely random. An initial pool of predetermined size is selected and immediately offered admission. All other students are randomly sorted and placed on the waitlist. When an admitted students declines an admissions offer, the available slot is offered to the student

[7] If both students are put on the waitlist, they are both assigned to the more favorable waitlist position.

at the highest waitlist position.

We provide a detailed accounting of our sample and lottery results in Appendix Table 1. Pooling the 2003-2005 cohorts together results in an initial pool of 1,089 lottery entrants. We exclude students from our analysis sample for one of two reasons. First, 118 applicants (10.8% of the initial sample) are the younger siblings of a student enrolled at Noble. We drop these students because they are not subject to the random variation required for our research design. Second, we do not observe dates of birth for a small group of students (13 in total, representing 1.2% of the original pool). These appear to be due to recording errors. Because students' precise dates of birth are required to match them to entries in the NSC database, we are forced to drop these students from our analysis. Though lottery winners are slightly more likely to suffer from missing data than losers (the raw means are 1.4 and 0.9 percentage points), the difference is not statistically significant and the frequency of missing data is so small that even the most severe bounding methods do not threaten our findings.[8]

The remaining 958 students compose our experimental sample. We define lottery winners as students who were immediately offered a position at Noble or who received one of the first ten waitlist positions.[9] This categorization results in a final tally of 412 lottery winners and 546 lottery losers. The admissions offer is a very powerful predictor of enrollment. 81.5% of students who are immediately offered admission attend Noble for at least one year, as well as half of the students who receive one of the top ten waitlist positions. Only 14 applicants (2.6%) farther down the waitlist ever receive and accept an admissions offer. As we discuss in our Results section, this powerful first-stage effect grants us a degree of statistical precision unavailable to prior studies in this literature.

B. Aggregated CPS Administrative Data

Finally, for additional summary statistics and our non-experimental analysis, we combine a variety of publicly available datasets provided by Chicago Public Schools.[10] From these, we observe

[8]More precisely, we can assess the maximal extent to which attrition might bias our results if we impute missing outcomes according to the "worst case" scenario for our results – i.e. imputing zeros for treated students and ones for control students. In this scenario, our main treatment effect estimates decline by between one and two percentage points. Their statistical significance is unchanged. These results are available from the authors upon request.

[9]Since the waitlist order is assigned randomly, using any cutoff to assign treatment and control is a valid instrument for attending Noble. We chose to include the top ten students in treatment based on an informal review of the lottery files; as Appendix Table 1 shows, many students near the top of the waitlist end up attending Noble, while the likelihood of attending drops off considerably after ten. Using a lower threshold or assigning all waitlisted students to the control group does not change our results.

[10]These data can be acessed at the following url: `http://cps.edu/SchoolData/Pages/SchoolData.aspx`

a variety of demographic and performance indicators averaged to the school-grade level. These data sources vary in their coverage. Test scores, free lunch eligibility, bilingual status, and special education counts are only available after 2009. However, we observe racial composition, graduation rates, and dropout rates as early as 2000.

We supplement our microdata by linking these records to data on middle schools attended by students in our sample. We use three middle-school characteristics from this data: the percentage of eight graders who score proficient or better on the math section of the Illinois Standard Achievement Test, the percentage proficient on the reading section, and the percentage of the student body that is black or Hispanic. In total, we are able to match 71.7 percent of our sample to their 8th grade school's CPS summary records. The remaining students either had missing data, ambiguous data, or attended a private or parochial school for which CPS does not track demographics or test performance.

These aggregated data also provide our outcome measures and control variables for our non-experimental analysis. For clarity, we describe this portion of the data alongside our analysis strategy in Section 4.3.

C. College Enrollment Data

To track college-going, we submitted the name, date of birth, and expected high school graduation date of every student in the 2002 through 2005 lotteries to the National Student Clearinghouse (NSC), a non-profit organization that maintains a database of college enrollment and degree attainment. NSC returned this information with data on enrollment spells and graduation for every school attended by matched students. By necessity, we assume that students who were not matched to any records in the NSC database never enroll in college. In principle, both students and schools have the option of preventing researchers from accessing their records. This is not a significant concern for our analysis, however, as only 1.3 percent of the records we submitted were blocked by either the student or the school.

At the time of our submission, the NSC database included enrollment records at over 90 percent of colleges and universities in the United States. It is natural to ask whether coverage is similarly thorough for the colleges targeted by students in our sample, however. Fortunately, Noble provided us with a list of college enrollment histories of their graduating seniors, based on their own NSC submission combined with input from college counselors. While we do not use this information in any other part of our analysis, it allows us to observe the prevalence of false negatives in our treatment group. Out of 232 students graduating from Noble between 2007 and 2009, our NSC request flagged 211 are flagged in our NSC data – a match rate of 90.9 percent. This suggests that NSC coverage is not likely to be a major issue for our analysis.[11]

NSC also provides information on the timing of enrollment that we use to construct measures of persistence. Each row in the NSC match corresponds to an enrollment period, with a fixed beginning and end date. The majority of schools in our matched sample report semester-long enrollments, spanning August/September to December or January/February to May/June. However, some schools use a quarter system or report continuous enrollment durations of over a year. To combine all of these different formats into a consistent structure, we generate a series of indicators for enrollment in the fall and spring semesters between 2006 and 2012. We consider students to have been enrolled in a given fall semester if they are enrolled for at least thirty days between August 1 and December 31 of that year. For spring semesters, we use endpoints of January 1 and June 1. All of our results are robust to other plausible definitions of persistence, including the total number of days enrolled or the total time between the first and last enrollment.

3.2 Empirical Approach

To measure the effect of Noble attendance on college outcomes, we estimate both Intent to Treat (ITT) and Local Average Treatment Effect (LATE) specifications. Let Z_i be an indicator for receiving an offer to attend Noble. We estimate the following model via least-squares regression to recover ITT estimates:

$$Y_{ic} = \beta X_i + \tau Z_i + \gamma_c + \phi_c * 1(sibling_i) + \varepsilon_{ic} \tag{1}$$

The ITT effect τ measures the difference between lottery winners and losers, adjusted for chance imbalances in the vector of student-level controls X_i. This can be interpreted as the causal impact of receiving an offer to attend Noble. To ensure that lottery status is not correlated with unobservable student characteristics, we drop students who are automatically accepted because they have an older sibling enrolled in a Noble school. All regressions also include lottery-cohort fixed effect (γ_c) and interactions with an indicator for having a sibling entering the same lottery ($1(sibling_{ic})$), since

[11] In our analysis, we code these 21 students as never enrolling in college. If we had a similar external source on the college enrollments of control students, we could plausibly update our dataset. Given that we do not have such information, correcting only the treated false negatives (or dropping them from our sample) would positively bias our estimated treatment effects.

siblings in the same cohort are admitted if either draws a winning number.[12] When students enter multiple lotteries over a series of years, we only include the first entry.

Identification of τ requires only that lottery status Z_i be uncorrelated with the error term ε_i. If we also assume monotonicity in the first stage (i.e. the lottery offer does not discourage anyone from enrolling) and that the exclusion restriction holds (i.e. the offer only affects college outcomes by changing Noble enrollment status), the causal effect of attending Noble is also identified for a certain sub-population. In regression terms, we use lottery status Z_i to instrument for enrollment at Noble to identify the LATE parameter. Letting $Noble_i$ denote an indicator for whether a student has ever enrolled at a Noble charter school, the second-stage equation can be written as follows:

$$Y_{ic} = \alpha X_i + \rho Noble_i + \delta_c + \theta_c * 1(sibling_i) + \eta_{ic} \tag{2}$$

Estimating Equation 2 by two-stage least squares produces an estimate of the LATE parameter, defined as the average effect of attending Noble for "compliers" – students who are induced to attend Noble by winning the lottery (Angrist and Imbens 1994). We report ITT estimates throughout the text except where noted. The results of Equation 2 are reported in Table 2 alongside the corresponding ITT estimates.

3.3 Summary Statistics and Balance

Table 1 reports summary statistics for Noble students, other charter school students, and CPS public school students. The source for all of the statistics in this table is the publicly available, school-level data provided by CPS described in the previous section. We show statistics for two separate time periods to provide a picture of Noble's early years (when the students in our experimental sample were in high school) as well as the later period after Noble's expansion had begun.

Like most Chicago schools, Noble serves students who are roughly 90% minority, though Noble enrolls a much larger share of Hispanic students than the average charter or traditional public school in Chicago. Other demographic data are not available prior to 2009. In more recent years, the network's collective student body is quite similar to the rest of the district on non-racial dimensions. Noble students are marginally less likely than traditional public school students to qualify for special education considerations (11.6 percent vs. 13.5 percent) but slightly more likely to be eligible for

[12] None of our results are sensitive to dropping students with siblings in the same lottery.

free or subsidized lunch (88.5 percent vs. 84.9 percent).

While Noble students are demographically similar to students at other schools, the differences in test performance are striking. Throughout our sample period, CPS students took the Explore test during the *fall* of their freshman year.[13] Hence, ninth grade test scores are a good measure of the incoming ability of each schools' students. Based on these measures, Noble students in later cohorts enter high school with slightly lower test performance than the average traditional public school student (though significantly higher than the average charter student). However, by eleventh grade, Noble students overtake their peers and score markedly higher than the CPS average and the charter average on all sections of the ACT. In the 2013-14 school year, all ten Noble campuses with enrolled eleventh graders ranked in the top 33 CPS high schools in overall ACT performance (out of 156 schools with reported scores). Looking beyond test scores, students entering ninth grade at a Noble campus are 20.0 percentage points more more likely to graduate within five years of enrollment than their peers in traditional public schools, and 18.8 percentage points less likely to drop out within five years of enrollment.

These summary statistics naturally give rise to questions about what sort of high schools our control group attends. Put differently, we would like to better understand the counterfactual outcome for our treatment group. Unfortunately we do not observe what high school untreated students attend; our microdata only tells us whether students ever attended Noble. However, it is worth noting that the Chicago charter sector was extremely small at this time. The 2005 ninth grade cohort had only eight charter high school options, and those eight schools educated a population 5% of the size of the traditional public school population.[14] We also directly observe the middle schools attended by most applicants. Of those, less than one percent attended a charter middle school for eighth grade, largely reflecting the low supply of charter middle schools at the time (only 14 were active, relative to 484 traditional public middle schools). Of course, the lottery population has revealed their preference for a charter school education, indicating that they are likely not representative of the district at large. Nevertheless, given the dearth of other charter options, we believe it is likely that most lottery losers attended traditional public schools.

While detailed school-level data is useful for comparing Noble's student body to the broader

[13]Both the Explore exam and the tenth grade Plan exam are produced by ACT, Inc. and test the same four subjects as the ACT test: math, reading, English, and science. Both exams consist of a separate, multiple-choice section for each of the four subjects. We use the single "composite" score calculated by test providers throughout our analysis except where noted.

[14]Table 1 includes data from 23 charter schools because the "experimental sample period" extends forward until 2008.

CPS population, we require student micro-data to assess the balance of the admissions lottery. This is an important check; if lottery status is not determined randomly, neither of our empirical specifications is valid.

Our demographic data is somewhat limited because we do not have access to detailed, district-provided administrative data used in other studies exploiting charter school lotteries (e.g. Dobbie and Fryer 2011, Abdulkadiroglu et al 2011, and Curto and Fryer 2011, among many others). We address this issue by constructing several indirect proxies for key socioeconomic variables. First, though we lack direct information on race and ethnicity, we can glean some useful information from students' last names given the prevalence of Hispanic students in our sample. The Census Bureau provides a table containing surname-by-race frequencies for the 150,000 most common surnames, to which we matched 95% of the students in our sample.[15] We code a student as Hispanic if 80% of the individuals with his/her last name are Hispanic (though our results are not sensitive to using other reasonable thresholds).[16] Second, while we do not observe students' lagged test scores, we can use average test scores at their middle school as a proxy.

Table 2 displays summary statistics and balance test results for the short list of student-level covariates that we construct from the lottery files. Columns (1) and (2) present means and standard deviations for lottery winners and losers, respectively. Column (3) reports the p-value from a regression of each characteristic on lottery status, controlling only for cohort fixed effects and a cohort-by-sibling-status effect.

The lotteries are well balanced. Noble attracts roughly three female applicants for every two male applicants, and this ratio is balanced across treatment and control. 70.5 percent of lottery winners have distinctly Hispanic last names, compared to a statistically indistinguishable 67.2 percent of lottery losers. Lottery losers attend middle schools with slightly higher levels of math and reading proficiency, though once again differences are small and statistically insignificant. Both lottery winners and losers went to middle schools where 90 percent of students were black or Hispanic. None of differences in baseline variables are statistically significant, and a joint test of all six coefficients in Table 2 yields a p-value of 0.751. Taken together, these results provide a strong case for the internal validity of our evaluation, suggesting that lottery imbalance is unlikely to be

[15]The data are available at `https://www2.census.gov/topics/genealogy/2000surnames/`. We use the 2000 Census data because it is the decennial Census closest in time to the lotteries.

[16]Consumer Financial Protection Bureau (2014) and Elliot et al. (2009) show that surname-based proxies are very strong predictors of Hispanic ethnicity in samples of mortgage applicants and health plan enrollees, respectively. In particular, adding home zip codes to the predictive model only marginally improves one's ability to accurately classify Hispanic ethnicity in either population.

an issue for our analysis.

4 Results

4.1 Experimental Estimates

While aggregated statistics show that students at Noble significantly out-perform their CPS peers on standardized tests, it is not clear *ex ante* that these results would translate into improvements on medium-term outcomes. For instance, the differences may be driven by selection into Noble rather than any actual treatment effect. What's more, even if the difference is causal, the short-term treatment effect on test scores may not translate into long-term benefits if the effects are driven by test-specific preparation rather than true learning. While our lottery design does not allow us to directly measure underlying mechanisms, we can provide clear evidence on whether attending Noble leads to increased college enrollment, quality, and persistence.

Panel A of Table 3 shows the effect of Noble attendance on college enrollment outcomes. We report estimates of equations (1) and (2), controlling for student's gender, average test scores at the student's middle school, and student age to increase precision (our results are almost identical when we drop all demographic controls.) Lottery winners are 10.0 (3.1) percentage points more likely to enroll in college than lottery losers. For compliers, we estimate a LATE parameter of 13.5 (4.0) percentage points. Relative to the control mean of 58.8 percent, this is a 23 percent increase. If we restrict attention to "on-time" enrollments that begin the fall after a students' expected high school graduation dates, the ITT effect falls to 6.7 (3.3) percentage points but remains statistically significant.

To put these estimates in perspective, it is worth comparing our results those of several recent papers studying the impact of improved high school quality on college outcomes. Deming et al. (2014) use school choice lotteries in Charlotte-Mecklenburg to instrument for attending a first-choice high school. Their estimates are not statistically significant when aggregated across all students and colleges, but their LATE estimate for attending a competitive college is 4.2 percentage points. Students who win the the Harlem Children's Zone admissions lottery Promise Academy are 17 percentage points more likely to enroll in college the fall after graduation, though lottery losers close the enrollment gap fairly quickly (Dobbie and Fryer, 2015). Furthermore, this "treatment" includes middle school education, high-school education, and a wide range of community investments. Results for Boston-area charters do not show a significant effect on overall college enrollment rates despite large increases in test scores (Angrist et al. 2016).[17] In other words, the best evidence to date has shown improvements that are either transitory or much less robust than our estimates for Noble.

A natural concern is that the increase in college enrollment might come at the expense of quality. If Noble merely pushes marginal students into lower quality schools, then we would expect to see smaller effects on attending more rigorous and more competitive schools. The results in Panel B of Table 3 show that this is not the case. The treatment effect on attending a two-year college is statistically zero – an increase of 3.7 (3.3) percentage points – while the effect on four-year college attendance is 11.4 (3.2) percentage points, slightly larger than the ITT estimate for all colleges. We obtain similar results using the test scores of incoming college students to proxy for quality. Winning the lottery increases the probability that a student enrolls in a school where the median two-subject SAT score is above 1,000 by 10.5 (2.9) percentage points.[18] The LATE estimate (13.7 [3.8] percentage points) represents an increase of 65 percent over the control mean of 20.9 percent. Taken together, the results in Panel B suggest that attending Noble increases both the likelihood and quality of college enrollment.

Finally, we turn to measures of college persistence in Panel C. Lottery winners are 12.6 (3.2) percentage points more likely to enroll in at least two semesters and 9.5 (3.3) percentage points more likely to enroll in at least four semesters. Relative to the average control student, these effects represent increases of 26.0% and 25.9%, respectively. To calculate graduation effects, we drop students from the 2005 cohort because we would not observe graduation if they were making normal progress through high school and a four-year college.[19] Using students from the 2003 and 2004 cohort only, we calculate a small and statistically insignificant effect on degree completion: 0.7 (3.0) percentage points, with a control mean of 14.9%.

For completeness, we also calculate and present persistence and graduation effects separately for two-year and four-year colleges. The results in Appendix Table 2 confirm that the patterns we observe are driven by enrollment at four-year institutions. Treatment effects for two-year college

[17] It should be noted that our point estimates are fairly close to those of Angrist et al (2016), who estimate an 11.5 (8.4) percentage point increase in enrollment. Our estimates are substantially more precise, however, owing to an extremely powerful first stage. Across the three cohorts we consider, 79.5% of students offered admission enroll at Nobel, compared to only 2.5% of control students.

[18] We obtained average SAT scores for entering freshmen from the U.S. News and World Report. Certain schools provide average ACT scores instead of SAT scores. We use the ACT's concordance table (available at `http://www.act.org/solutions/college-career-readiness/compare-act-sat/`) to translate these scores into the equivalent SAT score.

[19] We obtained our NSC data in November 2012. If a member of the 2005 lottery cohort made regular progress through high school and immediately entered a four-year college after graduating, she would have been a college senior at the time.

outcomes are negligible; the only marginally significant estimate is a 5.5 (3.0) percentage point increase in the likelihood of enrolling for at least two semesters. The estimates for four-year college outcomes are quite similar to the main estimates in Table 3.

To provide a fuller picture of enrollment behavior, Figure 2 depicts how enrollment rates evolve over time. In each panel we normalize the time scale on the horizontal axis such that the date of the lottery corresponds to year 0. In other words, 4.5 on our axis corresponds to the first semester of college for a student making normal progress through school, 5 corresponds to the second semester, etc.

Panel A of Figure 2 displays the the percentage of students currently enrolled or graduated, separately for lottery winners and losers. Consistent with the on-time enrollment results, lottery winners are more likely to enroll in college immediately after graduating. They are also substantially more likely to be enrolled at every subsequent time period.[20] Aside from a small increase in treatment-group enrollment in the second fall after expected graduation, the lines also slope down at a similar rate, indicating that persistence conditional on starting college is similar in the two groups. Panel B of Figure 2 plots the percentage of students who have ever been enrolled in college. Both the treatment and control lines rise steeply in the first two years after expected high school graduation, indicating that substantial quantities of new students are enrolling after their predicted freshman fall. Importantly, however, the treatment line rises at roughly the same rate as the line for the control group. This indicates that Noble is truly increasing rates of college attendance rather than inducing students to enroll sooner than they otherwise would. Appendix Figures 1 and 2 show these graphical results separately for two- and four-year college enrollment outcomes.

4.2 Heterogeneity

To test for potential heterogeneity in our treatment effects, Table 4 reports ITT results estimated separately by gender, neighborhood poverty rate, and middle school quality. We also report p-values on the null hypothesis that treatment effects are equal for each pair of groups.

There are few consistent patterns in the results, though we are under-powered to detect moderate differences. None of the estimated parameters are statistically different across subgroups. At most, we find suggestive evidence that effects on total enrollment and enrollment in four-year colleges are larger for students from high-quality middle schools – the opposite effect found by

[20] As we only observe seven semesters of expected college enrollment for the 2005 cohort, we truncate the graph at this point to maintain a consistent sample.

Deming et al. (2014). The differences are within the range of normal statistical variation, however, so we caution against interpreting them too strongly. Similarly, students from lower-poverty neighborhoods have higher estimated effects, though there are no statistically significant differences.

It is perhaps noteworthy that we do not observe any differential effects by gender, since, as Angrist et al. (2016) note, relatively few late-stage interventions show promising results for boys. In this respect, the lack of gender differences is consistent with other work examining the effect of No Excuses charters on college enrollment (Dobbie and Fryer 2015, Angrist et al. 2016). Similarly, using our surname-based classifications, we cannot detect any differences in treatment effectiveness between Hispanic and non-Hispanic students. (Recall that we do not directly observe race or ethnicity in our dataset, and our sample does not contain enough students with distinctly black or white last names to examine other racial subgroups).

Finally, we investigated whether our treatment effect estimates differ across cohorts. If learning-by-doing helps teachers and school administrators prepare low-income students for college, one might expect Noble's treatment effect to increase over time. On the other hand, the network was preparing to open two new campuses in the Fall of 2006, and it is possible that this effort diverted resources from the main campus. In Figure 3, we plot ITT estimates of the effect on college enrollment separately for each lottery. The point estimates increase monotonically over time, reaching 16.6 (5.0) percentage point effect for the 2005 cohort. However, we cannot reject the null hypothesis of equal treatment effects across all three cohorts. At most, we can say that Noble's effectiveness did not appreciably decline over this period.

4.3 External Validity

As with any randomized experiment, it is worth stepping back to consider the external validity of our estimates that is, the extent to which these results might generalize to other settings. Any such discussion is necessarily somewhat speculative, as our experimental data cannot directly speak to generalizability. However, evidence from several recent papers can help us make some informed conjectures about the scope of our results.

First, there is strong evidence that replicating the practices of "No Excuses" charter schools is sufficient to produce significant gains in student achievement. Fryer (2014) evaluates school turnaround efforts in which low-performing public schools in Houston, Denver, and Chicago adopted educational practices common to successful charters. He finds large treatment effects in math. In a context closer to our own, Cohodes, Setren, and Walters (2016) show that successful Boston charter schools generate similar achievement gains when they open new campuses. Similarly, Tuttle et al. (2015) show that Knowledge is Power Program (KIPP) charter schools have sustained their achievement effects through a period of rapid growth (though the magnitude of the effects declined somewhat in the middle of their sample period).

While these papers analyze effects on test scores, not college enrollment, it is noteworthy that similar charter school models have maintained their effectiveness as they scale. We interpret the findings as evidence that school policies and practices are the most important determinants of student outcomes; effectiveness is generally not determined by an unreplicable, unobserved ingredient found only at one campus.

Some researchers also note that lottery estimates of charter school treatment effects necessarily draw from oversubscribed schools (Tuttle, Gleason, and Clark 2012). Since these schools are more popular than undersubscribed charters (almost by definition), their effectiveness is unlikely to be representative of the entire charter sector. We therefore are careful to interpret our results as representative of No Excuses charters specifically, whose general popularity and success in raising test scores has been documented by a host of lottery-based studies (Angrist et al. 2010, Abdulkadiroglu et al. 2011, Dobbie and Fryer 2013, Dobbie and Fryer 2015, Tuttle et al. 2015).

At the same time, Chabrier, Cohodes, and Oreopoulos (2016) note that a significant portion of "No Excuses" charters effectiveness in increasing test scores is explained by their tendency to locate in poor, urban areas. As Figure 1 shows, Noble campuses draw from these very neighborhoods. Therefore, we should not conclude that Noble schools would generate similar treatment effects for wealthy, suburban, or rural students. Given the immense remaining socioeconomic and racial disparities in college-going, however, we believe that increasing college enrollment among underserved communities is still a desirable and policy-relevant goal.

Given the balance of the evidence, we believe it is reasonable to conclude our lottery findings are likely to extend to similar charter schools in urban areas. In Section 4.4, we test this assumption directly using publicly available data on other Noble campuses. These non-experimental results are consistent with our lottery estimates, though their validity relies on much stronger identifying assumptions.

4.4 Non-Experimental Estimates

The experimental results clearly demonstrate that early cohorts attending Noble Street Charter School are more likely to enroll in college, enroll in selective four-year institutions, and remain enrolled for at least four semesters. We have interpreted these results to mean that the academic practices and policies practiced throughout the Noble network lead to improved college outcomes. One possible objection to this interpretation, of course, is that we have thus far only analyzed students attending one campus. Even though other Noble schools follow the same general practices and policies, it is possible that there is something unique about the original campus that accounts for the effects we estimate.

As we discussed in Section 2, however, our lottery design is not feasible for later cohorts of Noble students. The network's rapid expansion absorbed the excess demand that is essential for our identification strategy. We must therefore turn to non-experimental methods and aggregated data sources to provide evidence on the external validity of our estimates.[21] As discussed earlier, CPS publishes a variety of demographic and performance measures at the school-grade level. Most important for our purposes are the college enrollment data. Using their own NSC data pull, CPS has calculated and released the average college enrollment rate for every high school between 2010 and 2013. We use these as a performance measure for later cohorts.[22]

Estimating a plausible specification also requires, at the very least, a measure of the baseline ability of each graduating class. Ideally, we would observe middle school performance outcomes for each senior class, allowing us to construct ability measures that do not reflect any treatment impacts. Since we only observe school-level averages, however, this design is not feasible. Fortunately, as mentioned earlier, during our sample period all CPS students took standardized tests in the Fall of their freshman year. We can therefore use each cohort's average score on their ninth grade Explore exam. These measurements are taken so early in students' high school careers that we can plausibly assume that they reflect incoming ability, not the influence of the school they attend.

The basis of our empirical strategy is to use incoming ability to predict college-going rates for that cohort, as can be seen in Figure 4. Each dot represents the graduating class at a single high school. In Panel (a), we plot the school's average Comprehensive score on the Explore exam taken in the Fall of 2009 on the horizontal axis. The vertical axis records 2013 college enrollment rates.

[21] We made several attempts over the course of three years to enter into a data sharing agreement with CPS that would allow us to analyze confidential microdata, but none of our attempts were successful.

[22] CPS calculates college enrollment rates by dividing the number of enrollments by the number of graduating seniors. Of course, not every student who enrolls as a ninth grader eventually graduates, introducing the potential for selection bias. While we cannot directly rule out this explanation without student-level panel data, two facts suggest that this is not a major concern in our setting. First, we obtain similar results when we calculate the enrollment rates as a fraction of all students who start ninth grade at a given school. This suggests that Noble does not achieve its results by "weeding out" weaker students before graduation. Second, recall from Table 1, that Noble's dropout rates are significantly lower than other CPS schools, and their graduation rates are much higher. We interpret these facts as evidence that our non-experimental effects are not driven by de-selecting students after they start high school.

As is immediately apparent, this measure of baseline ability explains most of the between-school variation in college enrollment – the R^2 statistic of the quadratic fit plotted in the figure is over 70%.

The shaded markers denote the seven Noble campuses with graduating seniors in 2013. Despite their location in the middle of the incoming-score distribution, their college enrollment rates are among the highest in the district. In particular, all seven campuses are located well above the regression line. Many more Noble graduates enter college than one would predict based on their incoming ability.

Test results are not available prior to 2009. Thus, we can only observe both ninth grade test scores and college enrollment rates for a single cohort (the class of 2013). To provide further evidence, however, we can repeat the exercise using tenth grade test scores for the class of 2012. Because this measurement is taken after students have already spent a year in high school, this is not an ideal control. In particular, we might expect some of the early achievement gains induced by Noble to be reflected in tenth grade scores. If this is the case, we would under-estimate the true effect of attending Noble on college-going. With these caveats in mind, Panel (b) displays the same graphical results for the class of 2012. Reassuringly, the general pattern is similar to the Panel (a). Noble's enrollment rates are among the best in the district, and rates at all five schools surpass the best-fit line by a wide margin.

For more detail, and to investigate the robustness of this approach, Table 5 presents regression results analogous to the plots in Figure 4. Each column shows the results of regressing each school's college enrollment rate on the baseline ability measure, its square, and a varying set of controls. In all specifications we weight each observation by the number of students in the ninth grade class.

Columns (1) and (4) reiterate the graphical argument above. Controlling only for a quadratic in baseline ability, we estimate the effect of attending Noble on college attendance to be 19.2 (2.1) percentage points for the class of 2013 and 12.0 (2.0) percentage points for the class of 2012. Columns (2) and (5) add an indicator variable for charter schools to this specification. Because Noble is itself a charter school, one should add this coefficient to the Noble effect when comparing Noble to traditional CPS public schools. The sum of the Noble and charter coefficient is quite similar to the earlier estimates, as one would expect. For the class of 2012, however we cannot reject the hypothesis that Noble is as effective as the average charter school. When we add a series of demographic control variables, however, Noble's performance exceeds even other charter schools in both years; we estimate effects of 12.8 (9.9) and 9.6 (7.6) percentage points.

We should be careful when interpreting the non-experimental results, however. The research design does not benefit from the random variation used in our earlier analysis, so a causal interpretation requires an admittedly strong selection-on-observables assumption. Nevertheless, we find it reassuring that the best evidence we can muster indicates that Noble students continue to out-perform expectations even during the network's rapid expansion.

5 Discussion

Critics have long contended that the academic success demonstrated in many high-performing charter schools can be explained by test preparation and a paternalistic environment that does not improve true academic skills or promote non-cognitive development. This paper presents evidence from the Noble Network of Charter Schools in Chicago indicating that "No Excuses" schools also have a positive impact on college attendance, quality, and persistence. We believe that this is a cause for cautious optimism, as Noble's educational model is broadly consistent with the practices of other high-performing charter schools.

Our analysis of Noble's impact on college outcomes builds upon studies of other high-performing charter schools in New York and Massachusetts that employ similar practices, including extending the length of the school day and year, facilitating regular small group instruction, using data to plan lessons and target remediation, and providing regular feedback to teachers on their instruction (Dobbie and Fryer 2015; Angrist et al. 2016). We provide the most robust evidence to date that the impacts of these practices are both broad and long-lasting: our point estimates are large and precisely estimated, we see no evidence of fade-out, and the effects are driven by enrollment at selective, four-year institutions.

Furthermore, our results connect to a growing body of literature that indicates that the achievement gains of the "No Excuses" approach is broadly replicable (Cohodes, Setren, and Walters 2016, Fryer 2014, Tuttle et al. 2016). While our data only allows us to estimate lottery-based models for a single campus, we use non-experimental methods to test whether the gains were preserved as the network expanded. Encouragingly, these results are in line with our experimental findings.

It is important to emphasize that our new findings are not in conflict with prior work on the medium-run effects of high-performing charter schools. Rather, our contributions are primarily a function of improved statistical power, stemming from a particularly strong first-stage attendance effect. Viewed in concert with work by Dobbie and Fryer (2015) and Angrist et al. (2016), we see an increasingly clear picture of a rapidly growing style of school management[23] that boosts the college enrollment of poor urban high schoolers. Unfortunately, the similarity of the educational approaches considered in each study means that we can say very little about what mechanisms might be driving our results (at most, one might infer that the neighborhood-level interventions of the Harlem Children's Zone are not essential for increasing college enrollment, a conclusion shared by Dobbie and Fryer (2015)). Accordingly, understanding which "No Excuses" strategies are most important for generating long-term gains is an important topic for future research. Nevertheless, we are confident that this educational model has proved to be – and will continue to be – an effective part of the fight to increase low-income students' human capital.

[23]For instance, KIPP now manages 200 schools and enrolls 80,000 students. Noble operates 16 high schools in Chicago.

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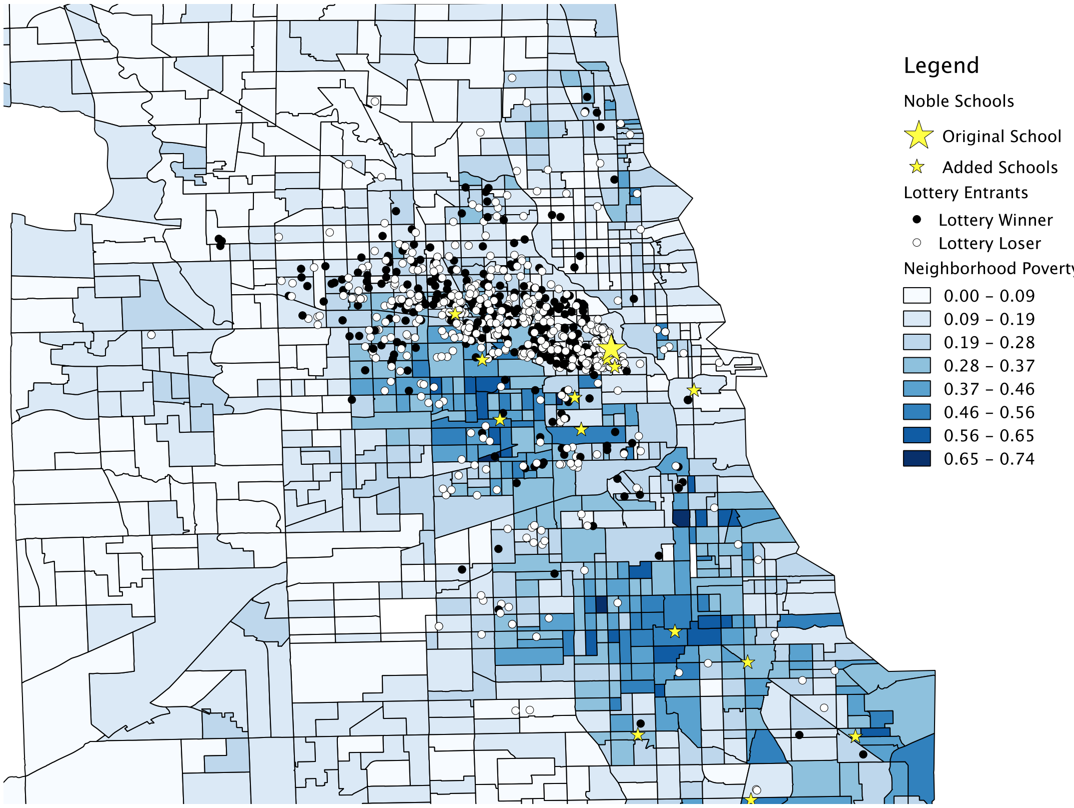
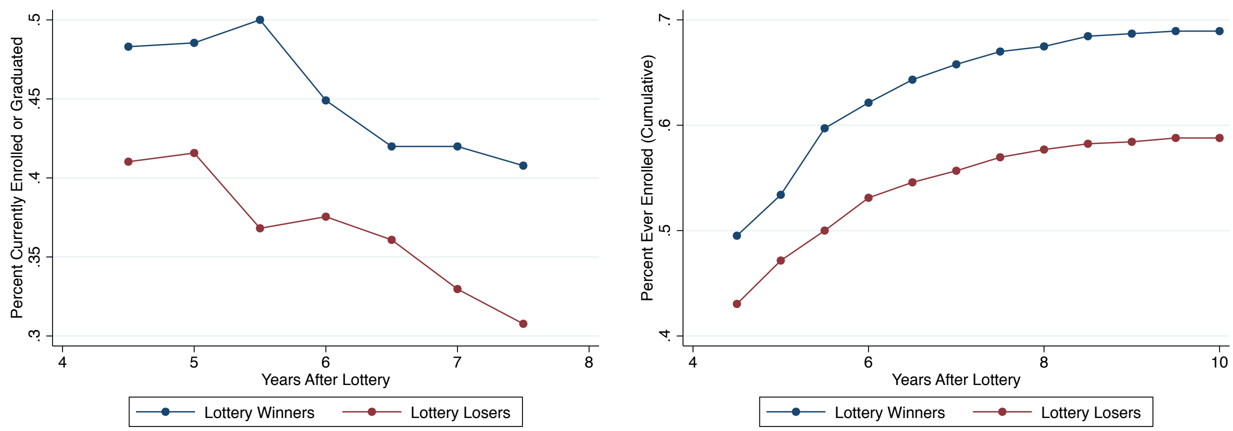


Figure 1: Noble Network Schools, Lottery Applicants, and Neighborhood Poverty Rates

NOTES: This figure plots the locations of the Noble Network of Charter Schools and the addresses of lottery applicants over a grid of Chicago-area census tracts. The largest star marks the location of Noble Street Charter school, the subject of our lottery results. The background color indicates the tract poverty rate averaged across the 2007-2012 American Community Surveys. Five-year periods are the shortest moving averages available at the tract level.



(a) Contemporaneous Enrollment

(b) Cumulative Enrollment

Figure 2: College Enrollment Over Time

NOTES: In both panels, the horizontal axis displays the number of school years after each cohort starts high school. Hence, 4.5 corresponds to the first semester of college for a student making normal and uninterrupted progress through school. Panel (a) records the percentage of lottery winners and losers who either (1) are enrolled in college in that period or (2) have graduated prior to that period. Panel (b) plots the percentage of students who have enrolled in college at any point up to the given time period. See Section 3 for more details on how we construct these variables.

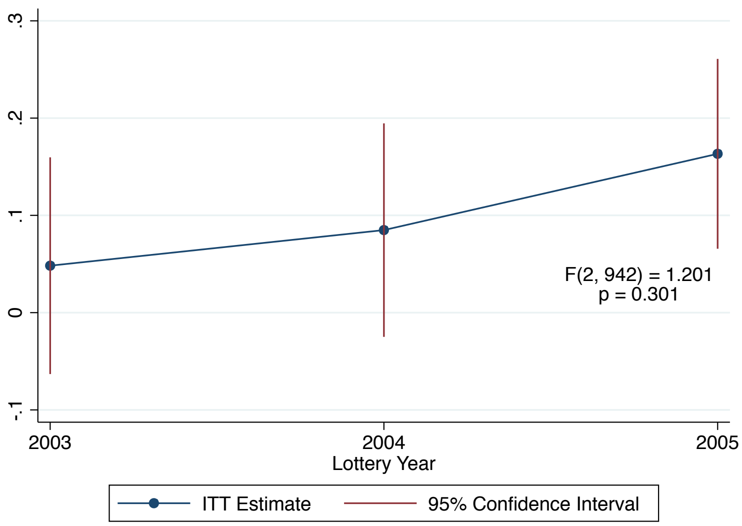
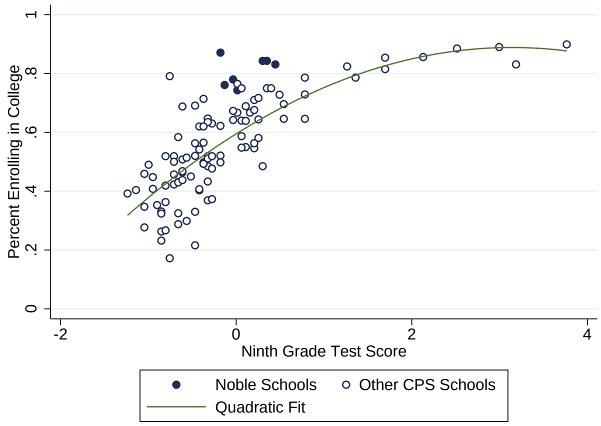
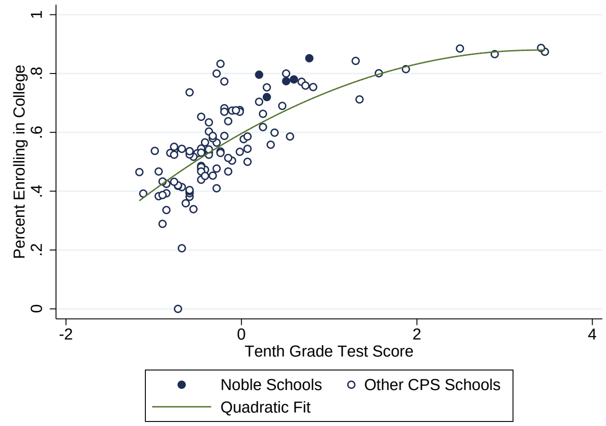


Figure 3: Treatment Effects on College Enrollment by Lottery Cohort

NOTES: See Table 3 notes for details of the regression specification. The caption shows the results of an F-test of the null hypothesis that the treatment effect is constant across cohorts.



(a) Class of 2013



(b) Class of 2012

Figure 4: Non-Experimental Estimates of College Enrollment for Later Cohorts

NOTES: See Table 5 notes for variable definitions. Ninth and tenth grade tests were administered in the fall semester for these cohorts. Ninth grade test scores are not available for the Class of 2012. All scores have been standardized to have mean zero and standard deviation one. Because these variables are measured at the school level, one standard deviation in this sample is smaller than the true student-level standard deviation.

Table 1
Characteristics of Chicago Public High Schools

	Experimental Sample Period 2003-2008			Non-Exp. Sample Period 2009-2013		
	Original Noble Campus	Other Charter	Traditional Public School	Noble Network	Other Charter	Traditional Public School
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Student Body Characteristics						
Black	0.115	0.712	0.493	0.349	0.619	0.449
Hispanic	0.825	0.214	0.361	0.602	0.324	0.413
Bilingual	.	.	.	0.053	0.045	0.068
Special Education	.	.	.	0.116	0.138	0.135
Free Lunch Eligible	.	.	.	0.885	0.888	0.849
Average Cohort Size	103.6	71.7	242.7	138.6	102.2	216.7
Panel B. Performance Measures						
9th Grade Math	.	.	.	0.008	-0.447	0.062
9th Grade Reading	.	.	.	0.037	-0.473	0.062
9th Grade English	.	.	.	0.005	-0.434	0.061
10th Grade Math	.	.	.	0.599	-0.469	0.057
10th Grade Reading	.	.	.	0.324	-0.448	0.065
10th Grade English	.	.	.	0.751	-0.420	0.038
11th Grade Math (ACT)	0.313	-0.277	0.004	1.028	-0.318	-0.003
11th Grade Reading (ACT)	0.276	-0.145	0.000	0.452	-0.288	0.023
11th Grade English (ACT)	0.446	0.059	-0.006	0.911	-0.215	-0.005
Five-Year Graduation Rate	.	.	.	0.816	0.713	0.616
Five-Year Dropout Rate	.	.	.	0.161	0.256	0.349
Number of Schools	1	23	116	12	33	128

NOTES: This table reports sample averages for a variety of variables and populations. The full sample includes all CPS high schools open at any point between 2003 and 2013. All results are calculated from school-level summary data published by CPS. We weight each observation by the number of enrolled students at each time period. The full sample includes all CPS high schools open at any point between 2003 and 2013. Bilingual status, special education stats, free lunch eligibility, and 9th and 10th grade test scores are not available before 2009. Five-year graduation (drop-out) rates are the percentage of original freshmen who graduate (drop out) within five years. We adopt the convention that the graduation (drop-out) rate at year t reflects the outcomes of students entering high school in year t-5. All test scores are normalized to have mean zero and standard deviation one in each year. Because these variables are measured at the school level, one standard deviation in our sample is smaller than the true student-level standard deviation.

Table 2
Lottery Sample Summary Statistics and Randomization Check

	Lottery Winners	Lottery Losers	p-value (1) = (2)
	(1)	(2)	(3)
Panel A. Student Characteristics			
Hispanic (Inferred)	0.705	0.672	0.190
	(0.457)	(0.470)	
Female	0.621	0.615	0.994
	(0.486)	(0.487)	
Age at HS Entry	14.520	14.556	0.810
	(1.124)	(1.177)	
Panel B. Eighth Grade School Characteristics			
Average Math Proficiency	0.341	0.358	0.114
	(0.152)	(0.163)	
Average Reading Proficiency	0.559	0.571	0.227
	(0.138)	(0.148)	
Percent Minority	0.904	0.903	0.749
	(0.118)	(0.118)	
Missing Baseline School Data	0.269	0.289	0.554
	(0.444)	(0.454)	
Observations	420	551	971
p-value from Joint F-Test			0.751

NOTES: This table presents summary statistics and balance tests for students who entered the ninth grade admissions lottery at the Noble Network of Charter Schools between 2003 and 2005. Students with an older sibling enrolled at Noble are dropped. All balance regressions include lottery fixed effects interacted with an indicator for having a sibling enter the same lottery. Hispanic ethnicity is inferred if 80% of Census respondents with the student's last name identify as Hispanic. Math and reading proficiency represent the percentage of eight graders at each student's middle school who scored proficient or higher on the Illinois Standard Achievement Test one year before the lottery. Heteroskedasticity-robust standard errors are reported in parentheses.

Table 3
The Effect of Attending a High-Quality Charter School
On College Enrollment, Quality, and Persistence

	Control Mean (1)	ITT (2)	LATE (3)
Panel A. Enrollment			
Enrolled in College	0.588	0.100***	0.131***
		(0.031)	(0.040)
Enrolled on Time	0.430	0.064*	0.083**
		(0.033)	(0.042)
Panel B. Quality			
Enrolled in 2-Year College	0.427	0.037	0.048
		(0.033)	(0.042)
Enrolled in 4-Year College	0.322	0.114***	0.149***
		(0.032)	(0.042)
Median SAT > 1000	0.209	0.105***	0.137***
		(0.029)	(0.038)
Panel C. Persistence			
Two Semesters or More	0.493	0.126***	0.165***
		(0.032)	(0.042)
Four Semesters or More	0.374	0.095***	0.124***
		(0.033)	(0.042)
Graduated from College	0.149	0.007	0.010
		(0.030)	(0.042)

NOTES: This table reports the effect of attending a school in the Noble Network of Charter Schools on college enrollment, quality, and persistence. Column (1) reports the control mean for each outcome. Column (2) reports Intent-to-Treat estimates following the specification described in the text. Column (3) estimates Local Average Treatment Effects, using lottery status to instrument for a binary variable indicating whether a student enrolled at Noble. All regressions control for gender; math and reading achievement at the student's middle school; the student's age; cohort fixed effects; and cohort effects interacted with an indicator for having a sibling entering the same lottery. Students who are automatically admitted because an older sibling is already enrolled at Noble are dropped. Our sample comprises 412 lottery winners and 546 lottery losers. On-time enrollment is defined as enrolling in the semester following each cohort's expected high school graduation date. Graduation effects exclude the 2005 lottery cohort, resulting in a sample size of 276 winners and 289 losers. Heteroskedasticity-robust standard errors are reported in parentheses.

Table 4

The Impact of Attending a High-Performing Charter High School for Various Subgroups

	Gender			Neighborhood Poverty			Middle School Quality			Inferred Ethnicity		
	Female	Male	p-val.	High	Low	p-val.	High	Low	p-val.	Hispanic	Non-Hisp.	p-val.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Enrolled in College	0.094**	0.112**		0.090**	0.113**		0.160***	0.074		0.080**	0.110**	
	(0.039)	(0.051)	0.785	(0.043)	(0.044)	0.715	(0.051)	(0.053)	0.245	(0.039)	(0.056)	0.656
	[0.620]	[0.536]		[0.614]	[0.559]		[0.580]	[0.537]		[0.595]	[0.605]	
Enrolled in 2-Year College	0.019	0.069		0.050	0.031		0.009	0.047		0.031	0.056	
	(0.042)	(0.052)	0.457	(0.046)	(0.046)	0.776	(0.056)	(0.053)	0.617	(0.041)	(0.061)	0.737
	[0.472]	[0.354]		[0.445]	[0.406]		[0.440]	[0.413]		[0.434]	[0.436]	
Enrolled in 4-Year College	0.127***	0.090*		0.108**	0.118***		0.193***	0.085*		0.083**	0.133**	
	(0.041)	(0.051)	0.569	(0.047)	(0.045)	0.870	(0.055)	(0.051)	0.151	(0.040)	(0.061)	0.489
	[0.323]	[0.321]		[0.345]	[0.297]		[0.320]	[0.274]		[0.330]	[0.326]	
Graduated from College	−0.010	0.031		−0.035	0.049		0.011	0.013		0.005	−0.019	
	(0.043)	(0.039)	0.477	(0.043)	(0.043)	0.164	(0.052)	(0.048)	0.981	(0.038)	(0.061)	0.736
	[0.203]	[0.062]		[0.174]	[0.119]		[0.154]	[0.147]		[0.137]	[0.172]	
Four Semesters or More	0.085**	0.104**		0.049	0.140***		0.056	0.123**		0.075*	0.083	
	(0.042)	(0.052)	0.784	(0.046)	(0.046)	0.159	(0.056)	(0.052)	0.384	(0.041)	(0.062)	0.913
	[0.398]	[0.335]		[0.407]	[0.336]		[0.405]	[0.303]		[0.385]	[0.366]	
Observations	593	365		482	476		340	372		620	285	

NOTES: This table reports the effect of attending a school in the Noble Network of Charter Schools for various subgroups of the data. We report Intent-to-Treat estimates throughout, controlling for the variables summarized in Table 1, cohort fixed effects, and cohort effects interacted with an indicator for having a sibling entering the same lottery. Hispanic ethnicity is inferred if 80% of Census respondents with the student's last name identify as Hispanic. Graduation effects exclude the 2005 lottery cohort. Heteroskedasticity-robust standard errors are reported in parentheses. The control mean of each outcome within each group is reported in square brackets below the standard error.

Table 5
Non-Experimental Treatment Effect Estimates for Two Later Cohorts

	Class of 2013			Class of 2012		
	(1)	(2)	(3)	(4)	(5)	(6)
Noble	0.193***	0.098**	0.128***	0.117***	0.022	0.096**
	(0.020)	(0.042)	(0.041)	(0.020)	(0.037)	(0.043)
Charter		0.104**	0.099***		0.100***	0.076***
		(0.040)	(0.038)		(0.034)	(0.029)
Ninth Grade Test	0.186***	0.187***	0.137***			
	(0.012)	(0.012)	(0.025)			
Ninth Grade Test^2	-0.030***	-0.029***	-0.026***			
	(0.005)	(0.005)	(0.005)			
Tenth Grade Test				0.167***	0.168***	0.147***
				(0.014)	(0.014)	(0.030)
Tenth Grade Test^2				-0.025***	-0.024***	-0.022***
				(0.005)	(0.005)	(0.006)
Free Lunch Eligible			-0.080			0.084
			(0.135)			(0.179)
Special Education			-0.708***			-0.432
			(0.239)			(0.274)
Bilingual			0.064			-0.233*
			(0.190)			(0.128)
Black			-0.060			-0.032
			(0.064)			(0.074)
Hispanic			-0.159**			-0.147*
			(0.073)			(0.082)
Observations	109	109	109	102	102	102
R-squared	0.774	0.801	0.843	0.731	0.755	0.845

NOTES: This table reports non-experimental estimates of the effect of attending a school in the Noble Network of Charter Schools for two recent cohorts of students. The sample includes all CPS high schools with graduating seniors in 2013 (columns (1)-(3)) or 2012 (columns (4)-(6)). All variables are school-level averages. The dependent variable in each regression is the fraction of graduating seniors who enroll in college. Each observations is weighted by the number of students in the original ninth grade class. Test score controls are the average composite score on the Explore (ninth grade) and Plan (tenth grade) standardized test, normalized to have mean zero and standard deviation one. Because these variables are measured at the school level, one standard deviation in this sample is smaller than the true student-level standard deviation. Ninth-grade test scores are not available for the Class of 2012. Heteroskedasticity-robust standard errors are reported in parentheses.

Online Appendix: Data Sources and Variable Construction

Lottery Records

Noble provided the results of all admissions lotteries taking place between 1999 and 2012. The files are quite detailed. For each cohort, we observe applicants' names, address, date of birth, gender, eighth grade school, his/her randomly-drawn lottery number, and the initial admissions offer outcome (either Accepted, Waitlist, or Sibling, where Sibling denotes students who received an admissions offer because of an older sibling already enrolled at Noble). The files also indicate which Noble Network campus, if any, the student attended in the fall the student attended the following fall. For the cohorts we consider, this field is always "Noble" or missing, the latter case indicating that the student did not attend Noble.

NSC requires both a name and a precise date of birth (or a social security number, which we never observe) to match students to their enrollment records. Unfortunately, birthday information is not available for 13 students who would be otherwise eligible for our sample (see Appendix Table 1 for a full accounting). Based on inspection of the raw files, the missing data fields appear to be simple transcription errors. Some are explicitly entered as "missing", "?", or "Mr. [redacted] forgot to write it down"; others are simply blank. These rows are also likely to be missing other fields such as parents' names or home addresses. Student's first and last names are never missing, however. We discuss the possible implications of attrition for our analysis in Section 3.1.A.

Lottery Variable Construction

As a first step, we drop all students whose admissions status is listed as "Sibling." These students are automatically admitted and are thus not included in our lottery analysis. Of the remaining students, we designate students who are immediately accepted as well as those offered one of the first ten waitlist slots as our treatment group. All other students are assigned to the control group, even if they are eventually offered a slot. We use the campus enrollment field to construct our endogenous treatment variable. The instrument is a strong (but not perfect) predictor of eventual enrollment. Averaging across all cohorts, 81.5% of treated students enroll at Noble, compared to only 2.6% of control students.

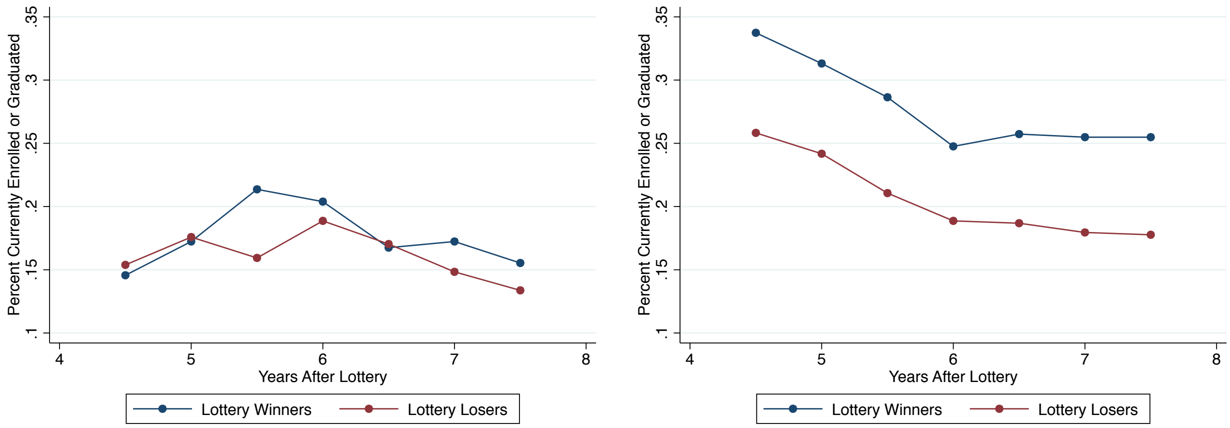
Our empirical specification adjusts for the fact that two siblings entering the same lottery have a higher probability of receiving an admissions offer than a student without a sibling. This information was not directly recorded for the cohorts we consider, so we infer it from the provided background information. Precisely, we assume that any students with the same last name and same address are siblings. Records for later cohorts include indicators for students with a sibling entered in the same lottery. Our method of identifying siblings coincides perfectly with these flags in these later years.

NSC Submission and Coverage

The National Student Clearinghouse is a national non-profit that provides enrollment and degree-verification services. We submitted the names, dates of birth, and estimated high school graduation dates for all students in our sample to the NSC's Student Tracker service in November 2012. In return, NSC provided detailed records of enrollment and graduation, which we use to construct our main outcome variables as described in Section 3.1. The Student Tracker database currently contains over 3,600 institutions enrolling over 98% of college students in the United States. Deming et al (2014) compare the list of covered schools to the complete roster of schools eligible for Title IV funding and find that for-profit schools and private religious colleges account for the majority of the missing schools.

While NSC does not provide the details of their matching algorithm, we were able to assess its quality using a separate data source. Noble provided us with their own internal records of college enrollment for seniors graduating between 2007 and 2009. Of the 239 students on this list, we matched 232 to our analysis sample using provided student ID numbers (wherever possible) and first and last names for students for whom ID numbers were not provided in one or both datasets. The remaining seven students were not included in our sample either because they entered with a different cohort, or because they were missing ID numbers and did not have a unique name that would allow for matching.

The schools attended by the 21 students who are not captured by our NSC pull do not exhibit any clear common characteristics. Noble's records indicate that 12 of these students attended either Northeastern Illinois University or a branch of the City Colleges of Chicago. These are among the most commonly attended campuses in our sample. There are six campuses that appear in the Noble list but not in our NSC dataset. Each is attended by a single student. Three are out-of-state private universities, one is an out-of-state public college, one is a community college, and one is a beauty school. Taken together, this evidence suggests that treatment students do not gravitate towards schools not covered by NSC.

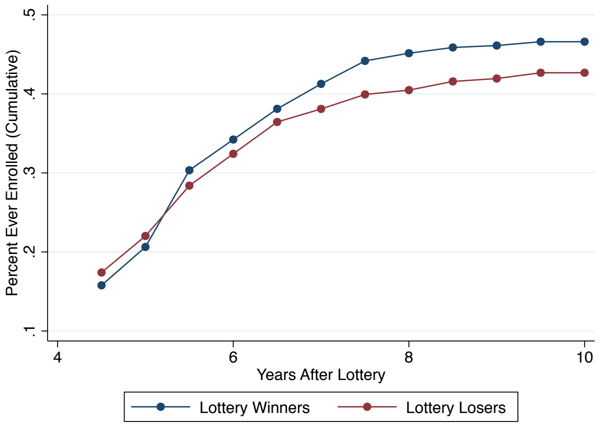


(a) Two-Year Colleges Only

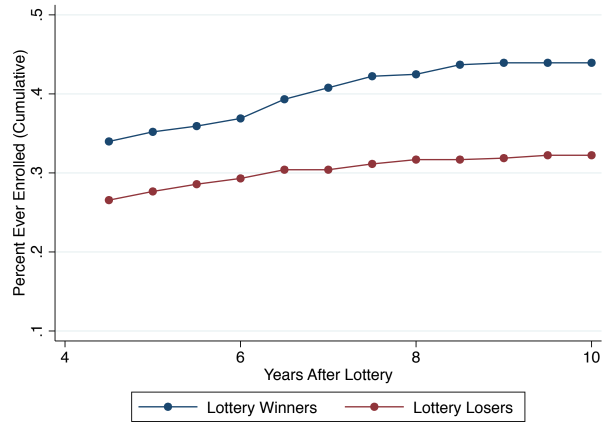
(b) Four-Year Colleges Only

Appendix Figure 1: Contemporaneous Enrollment Over Time, By College Type

NOTES: The horizontal axis displays the number of school years after each cohort starts high school. Hence, 4.5 corresponds to the first semester of college for a student making normal and uninterrupted progress through school. The vertical axis records the percentage of lottery winners and losers who either are enrolled in either a two-year college (Panel (a)) or a four-year college (Panel (b)) in that period. See Section 3 for more details on how we construct these variables.



(a) Two-Year Colleges Only



(b) Four-Year Colleges Only

Appendix Figure 2: Cumulative College Enrollment Over Time, By College Type

NOTES: The horizontal axis displays the number of school years after each cohort starts high school. Hence, 4.5 corresponds to the first semester of college for a student making normal and uninterrupted progress through school. The vertical axis records the percentage of students who have enrolled in a two-year college (Panel (a)) or four-year college (Panel (b)) at any point up to the given time period. See Section 3 for more details on how we construct these variables.

Appendix Table 1
Accounting for the Sample

	Immediately Accepted		Waitlist Number ≤ 10		Waitlist Number > 10		*Total*
	Attended	Didn't Attend	Attended	Didn't Attend	Attended	Didn't Attend	
All Lottery Entrants	418	90	15	15	14	537	1089
Older Sibling Enrolled at Noble	99	19	0	0	0	0	118
Missing Date of Birth	6	0	1	1	0	5	13
Experimental Sample	313	71	14	14	14	532	958
Sibling Entered in Same Lottery	12	0	0	0	0	14	26
No Relevant Siblings	301	71	14	14	14	518	932

NOTES: This table tabulates lottery and attendance results for students subjected to varying admissions policies. Students with an older sibling enrolled at Noble are automatically admitted; hence, they are not included in the Experimental Sample. We do not observe dates of birth for a small number of students; these observations cannot be matched to the NSC database and are hence dropped from our analysis. Students with a sibling entering the same lottery have a higher likelihood of receiving an admissions offer. They are included in the experimental sample, but all regressions include a currentSibling-x-cohort indicator variable to account for this increased probability of treatment. At the lottery, each student is either accepted immediately or given a waitlist number. In our empirical work, we define lottery winners as students who are either accepted immediately or given a waitlist number of at most ten.

Appendix Table 2
The Effect of Attending a High-Quality Charter School on
Two-Year and Four-Year College Outcomes

	Control Mean	ITT	LATE
	(1)	(2)	(3)
Panel A. Two-Year Colleges Only			
Enrolled in College	0.427	0.037	0.048
		(0.033)	(0.042)
Enrolled on Time	0.174	−0.015	−0.019
		(0.025)	(0.032)
Two Semesters or More	0.266	0.055*	0.072*
		(0.030)	(0.039)
Four Semesters or More	0.152	0.035	0.045
		(0.025)	(0.032)
Graduated from College	0.035	−0.011	−0.014
		(0.012)	(0.016)
Panel B. Four-Year Colleges Only			
Enrolled in College	0.322	0.114***	0.149***
		(0.032)	(0.042)
Enrolled on Time	0.266	0.071**	0.093**
		(0.030)	(0.040)
Two Semesters or More	0.271	0.112***	0.146***
		(0.031)	(0.040)
Four Semesters or More	0.203	0.059**	0.078**
		(0.028)	(0.037)
Graduated from College	0.090	0.039	0.055
		(0.026)	(0.036)

NOTES: This table reports the effect of attending a school in the Noble Network of Charter Schools on college outcomes separately for two- and four-year colleges. Four-year graduation results are not available for the 2005 cohort, though we do include this cohort in the two-year college graduation estimate. See Table 3 notes for other specification details.