The Long-Term Effects of Public School Choice: Lottery Evidence from San Diego

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Abstract

School choice offers an important mechanism for equalizing educational opportunity, given that American public schools differ strongly in terms of achievement and student demographics. This paper estimates the causal impact of school choice in San Diego Unified School District on high school outcomes and, notably, postsecondary educational attainment, by studying lottery applications to a magnet program, an open enrollment program and a program designed to integrate schools. Families on average applied to schools at which students had higher achievement, grades, behavioral outcomes and socioeconomic status than their local alternative schools. Yet in most cases, intent-to-treat and treatment-on-the-treated estimates suggest that school choice does not significantly influence students' high school or postsecondary outcomes.

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1 Introduction

Although the public debate over whether to expand school choice in primary and secondary schools tends to focus on private schools, school choice programs limited to public school options have proliferated. Public school choice is an increasingly widespread policy tool aimed at improving public schools. Grady and Bielick (2010) calculate that between 1993 and 2007 the percentage of students attending public schools through choice programs rose from 11 to 16%, and that by 2007-2008, 48% of school districts offered some kind of school choice. These trends are likely to continue in the wake of ambitious expansions of charter school options in many states, and concomitant efforts by public school districts to offer alternative forms of school choice such as magnet schools in response.

Proponents of school choice argue that by giving students and their families a range of school options they will be able to identify schools that best meet the needs of those children. They also argue that choice exerts competitive pressures on other public schools to improve their performance. Critics of choice programs worry that the programs will lead schools to segregate along racial, ethnic, and socioeconomic lines. They also question whether choice programs will truly improve student outcomes.

School districts and the federal government invest substantial sums of money on school choice. In 2010 the federal government devoted $509 million to various school choice initiatives, including $100 million to the Magnet Schools Assistance Program and $256 million to charter schools, among other initiatives (U.S. Department of Education, 2011). Individual school districts collectively spend far more than this on supporting schools of choice and providing transportation to students. For example, in 2009-2010, a single school district, San Diego Unified School District, budgeted $9.4 million for its magnet school program and $39.4 million for transportation services, much but not all of which was devoted to busing for its various school choice programs (San Diego Unified School District, 2010).

Given the wide adoption of school choice and the public expenditures it creates, it is important to know whether school choice improves student outcomes. This study contributes to a surprisingly small literature looking beyond the immediate impact on test scores and instead examines high school graduation and postsecondary outcomes. We argue that these longer term measures of educational attainment are more vital to adult success than student scores on tests that students will view as low stakes. We study a variety of high school and postsecondary outcomes using data from seven years of applications to choice programs in the San Diego Unified School District (SDUSD), which is the second largest school district in California.

Importantly, schools in these choice programs use a lottery when oversubscribed. The lottery

is crucial because its randomization procedure creates the equivalent of experimental treatment and control groups, removing statistical bias in non-experimental studies that may arise due to unobserved differences between those who do and do not enter school choice programs.

2 Related Literature

There is a large and growing literature on the relation between various forms of school choice and student achievement in the United States. However, much of the research uses non-experimental methods which necessitate finding a non-experimental control group. We briefly review findings of the impact of three forms of school choice examined in this paper — magnet schools, open enrollment, and voluntary busing aimed at integrating districts — with an emphasis on lottery-based studies that can exploit randomization among students.[1]

Among the papers studying these forms of school choice, we know of only one paper, by Deming, Hastings, Kane and Staiger (2014), that uses lottery data to rigorously analyze the effect of school choice on postsecondary outcomes. The general literature instead focuses mostly on test scores.[2] The shortage of studies looking beyond test scores is unfortunate, as a good school can likely help students in ways that test scores cannot measure. Indeed, Heckman, Stixrud and Urzua (2006) make a persuasive case that what they label non-cognitive skills may be as important as cognitive skills in determining adult outcomes.

2.1 Achievement of Students Participating in Choice

Magnet Schools

Ballou (2009) reviews the literature on magnet schools and achievement, and finds four studies that use lotteries to estimate causal impacts, and overall he reports mixed results. (One of these papers, Betts, Rice, Zau, Tang and Koedel (2006), henceforth Betts et al. (2006), finds mostly no effects of winning a magnet school lottery in San Diego Unified School District, but a moderate positive impact on mathematics achievement in magnet high schools. They study only a single year of school choice applications, compared to the seven years studied here.) More recently, Betts, Kitmitto, Levin, Bos and Eaton (2015) study a national sample of elementary schools that converted

[1] Separate literatures exist on charter schools and voucher programs. For recent reviews of the literature on charter schools, see Betts and Tang (2014) and Epple, Romano and Zimmer (2015b). For a recent meta-analysis of the U.S. and international literatures on vouchers, see Shakeel, Anderson and Wolf (2016). Other useful reviews of vouchers include Figlio (2009), Epple, Romano and Urquiola (2015a) and Egalite and Wolf (2016).

[2] Unlike the case for the types of school choice we study, a number of papers on charter schools have looked at postsecondary outcomes. See Betts and Tang (2014) for a review.

to magnet schools and found no overall impact on the achievement of resident students, although results varied school by school.

Open Enrollment

Four recent studies use lotteries to evaluate the impact of open enrollment policies.

In the paper most closely related to the current piece, Deming, Hastings, Kane and Staiger (2014) study postsecondary outcomes in Charlotte-Mecklenburg. They find evidence that students whose neighborhood public school was of low quality had better postsecondary outcomes if they won a lottery to attend another high school through an open enrollment program, with girls benefiting more than boys.

Several papers have examined K-12 school outcomes. Cullen, Jacob and Levitt (2006) provide analysis of the Chicago Public Schools' (CPS') open enrollment program. Focusing on applications to the district's high schools, they find that lottery winners and losers fare almost identically on subsequent tests of math and reading achievement, and other measures of academic outcomes such as absences and courses completed. However, after the lottery, lottery winners are slightly less likely to be arrested than lottery losers. Betts et al. (2006) provide an experimental analysis of the impact of open enrollment programs on math and reading achievement in SDUSD, using data for roughly 2,000 students who applied to various lotteries in 2001. They find, in almost all cases, that lottery winners in the district's open enrollment program fare no better or worse in math and reading than did lottery losers one, two and three years after the lottery. Hastings, Kane and Staiger (2009) study school choice in Charlotte-Mecklenburg, North Carolina and conclude that there are heterogeneous effects on test scores, with bigger academic gains to students whose families seek high value-added schools.

Traditional Busing

Bradley and Bradley (1977), Crain and Mahard (1978, 1981) and Cook et al. (1984) summarize the voluminous early literature on the effect of busing programs designed to integrate schools racially. Early studies tended to find no (or in a few cases positive) effects on achievement. Betts et al. (2006) find no impact on test scores from SDUSD's traditional busing program, VEEP, based on a one-year sample of lottery applications.

In conclusion, we note that although there exists a non-lottery based literature that focuses mostly on the impact on test scores, the lottery-based literature is smaller. Only one of the lottery-based papers on the impact of magnet schools, open enrollment and voluntary busing school choice examines longer-term student educational outcomes, which is the main goal of the current paper.

2.2 School Choice in San Diego

School choice is popular in SDUSD. At the start of our study period (2001-02), roughly 25% of students attended a school outside their local attendance zone through one of the district's choice programs, with this number growing to 40% in 2009-10. Furthermore, because SDUSD uses a random drawing process to admit applicants to schools, the district provides an excellent opportunity to causally analyze the impact of school choice on student outcomes.

During the period we study, SDUSD students who wished to attend a public school other than their neighborhood school had four main options available: magnet schools, the Voluntary Enrollment Exchange Program (VEEP), the state-mandated open enrollment program (known locally as School Choice), and charter schools. In addition, Program Improvement School Choice (PISC), initiated in fall 2002, responded to the No Child Left Behind (NCLB) requirements to provide choice options to students at schools that fail to make Adequate Yearly Progress in a given subject for two consecutive years. SDUSD met this challenge by using or adapting its bus routes under the VEEP and Magnet programs to provide school choice to such students. Students who were eligible for PISC were also given higher priority within the VEEP and Magnet programs, and were more likely to gain admission than other students.

SDUSD's magnet school and Voluntary Enrollment Exchange Program (VEEP) have their roots in a court desegregation order in 1977, and have operated for almost forty years. Since 1993, SDUSD has also had an open enrollment program mandated under California state law, known as Choice. (See Zau and Betts (2005) for more information on the history of school choice in SDUSD.) Thus, the programs are well-established, with histories tracing back two to four decades. Charter schools also have become an important and well established component of SDUSD. By 2009-2010, 41 charters schools accounted for 11% of district enrollment.

In fall 2009 the percentages of students in each program were 9% in magnet schools, 6% in VEEP, 14% in Choice, and 11% in charters. PISC enrollment totaled 7% of students, spread across the Magnet, VEEP and Choice programs, plus a small percentage of students in transportation routes specifically designed for PISC.

During the application years we study, fall 2001 to fall 2007, the district distributed a detailed brochure to each family early in the school year, advertising the various choices available. Parents then had until mid-winter to apply to any of these choice programs. Where necessitated by over-subscription, the district held a centralized random drawing to determine which students were accepted to a given school and program. (Except for three charter schools converted from regular public schools, the district did not process applications for charter schools.)

Students could apply to any VEEP, Magnet, Choice, and charter schools in a given year. All four options were readily available to elementary, middle and high school students. To be eligible for

PISC, however, a student had to be attending a school designated as needing program improvement because it had failed to meet requirements of California's definition of Adequate Yearly Progress (under NCLB) for two years in a row.

Magnet Schools

At its inception, the goal of the magnet school program was to attract students from primarily white areas into non-white areas by offering specialized curricula and additional resources such as reduced teacher/student ratios, teaching labs, and field trips. In some cases magnet schools attempted to create flows in the opposite direction, into schools in predominantly white areas, in order to provide students with opportunities they would not otherwise have at their neighborhood school and to improve the diversity of the student body. As of the 2010-11 school year, 32 magnet schools operated in SDUSD, up slightly from 29 magnet schools in the 2001-02 school year.

Any student who resided within the district's boundaries could apply to a magnet school. Admissions otherwise used lotteries with two dimensions of priority sorting, before random numbers entered. The first dimension is unique to magnet schools (not present in the other choice types), and is intended to encourage greater integration. Students were grouped into one of four "clusters" based on their local default school. These clusters were sorted by racial composition, with cluster 1 having the largest percentage of white students, and cluster 4 having the largest percentage of non-white students. Magnet schools with a large percentage of white students first accepted applications from students living in cluster 4, then 3, 2, and 1. Conversely, schools with a large percentage of non-white students accepted applications in cluster order 1, 2, 3, and 4. (Within each cluster, however, no distinction was made between applicants of different races and ethnicities.) After this consideration of students' school cluster, priority was then assigned based on factors like whether a student had a sibling already attending the school. This second dimension of sorting is common to all types of school choice that we study, and not just magnet schools.

The district provided transportation for district students who attend a magnet school outside of their neighborhood and had a walking distance greater than two miles. These students were termed "non-resident" students. Non-resident attendees of magnets accounted for 8.5% of district enrollment in 2009-2010, up from 6.2% in 2001-2002. In the last few years, non-residents have accounted for one-third to one-half of total enrollment at these magnets. Demand for these programs was high during the years we study, and not all students could be accommodated. In 2001-2002, there were 17,621 applications to magnet schools from 11,368 applicants, for 2,217 open slots.

The Voluntary Enrollment Exchange Program (VEEP)

The VEEP program typically attracted students from primarily non-white neighborhoods to schools in primarily white neighborhoods. VEEP, formerly known as the Voluntary Ethnic Enrollment Program, for many years supported a court desegregation order. Throughout the period that we study, however, the district was no longer under court supervision and did not focus solely on racial and ethnic integration, precipitating the name change. The program sought to increase diversity among students in the receiving schools, and to improve achievement among VEEP students by enabling them to attend higher-performing schools.

As with magnet schools, the district provided transportation for VEEP. Unlike the magnet program, however, VEEP limited the choices available to each student, so as to contain the costs of transportation. It did so by offering students in "VEEP-sending" schools a short list of VEEP-receiving schools to which they could apply.

The district allowed students of any race to apply to the VEEP program. The nature of the VEEP sending/receiving schools still tended to lead to racial and ethnic desegregation, but not as strongly as in the past because, for instance, white students at VEEP-sending schools could now apply to leave for schools that typically have a greater share of white students than their current schools. The program was open to all students who resided in the local attendance zone of a school with a VEEP pattern. VEEP students at the elementary level followed the feeder pattern of their new school, rather than having to return to their neighborhood for middle and high schools.

Receiving schools, in conjunction with the district's Enrollment Options Office, determined the number of slots available at the school each year. As with the magnet program, demand for this program was strong. In 2001-2002, there were 8,099 applications from 5,636 applicants, for a total of 2064 available slots. VEEP's share of district enrollment fell slightly between fall 2001 and fall 2009, from 6.4% to 5.9% of district enrollment.

The application process for VEEP was similar to magnet schools: students who applied before a mid-winter cutoff date were given first priority, and siblings of students currently in the program were at the top of that list. Wait lists did not carry over, so if a student was not accepted (did not "win" the random lottery), he or she would have to re-apply the following year. Admission was also based on availability, with the highest-achieving schools typically having the fewest openings due to demand.

School Choice (Open Enrollment) Program

The third choice program, the district-wide School Choice program, provided any student with the opportunity to attend any school within the district's boundaries. It is mandated by state law

passed in 1993 as a way of increasing the options for every student. The main drawback of this program is the lack of district-provided transportation. In 2001-2002, there were 12,705 applications from 8,311 applicants for 3045 new slots. This program has grown markedly, doubling its share of district enrollment between fall 2001 and fall 2009, to 13.6%.

Charter Schools

Charter schools — or publicly funded schools that operate more independently of the school district than traditional public schools — are an increasingly popular alternative to the traditional school. These schools obtain their charters from the district, which in turn provides oversight. As of 2009-2010, 41 charter schools accounted for 10.8% of district enrollment, up from 5.5% in 2001-2002.

Apart from a few settings such as New York City, it is a commonplace finding that charter schools are not oversubscribed, and thus do not conduct lotteries. Gleason et al. (2010) conducted the first nationwide lottery-based study of charter schools. They focus on middle schools, and report having exceptional difficulty finding charter schools that were oversubscribed. Indeed, only 130 out of 492 such schools nationwide used admission lotteries, and only a fraction of these schools agreed to participate, compounding potential selection effects. Thus the problem we face in San Diego with a lack of lottery data mirrors the (lack of) use of lotteries nationwide. Due to the lack of lottery data for charter schools, we do not examine this type of school choice.

3 Data and Methods

3.1 Sample and Lottery Basics

SDUSD began to use a lottery to admit students to school choice programs in 2001. We have access to lotteries from fall 2001 through fall 2007, a total of 7 years. We restrict our sample to applications to middle and high schools, in grades 6 to 12, because most students applying to the elementary grades (grade 5 and below) have either not graduated from high school, or graduated too recently for our desired postsecondary outcomes to be well-defined.[3] We also omit all applicants who applied from outside the district, regardless of whether they won or lost their lotteries. Our concern was that we would typically only observe follow-up data for these out-of-district lottery winners, and not the lottery losers, making for an unfair comparison.[4]

In any lottery-based study, it is crucial to know about the rigor with which the lottery is conducted, and whether it is conducted consistently over time. The same district staff member in

[3]Results were similar when we added elementary school applications to our analysis.

[4]That is, the admission offer itself makes it much more likely for out-of-district lottery winners to transfer to SDUSD than out-of-district lottery losers, who do not receive such an offer.

the central district offices conducted the lotteries each of the years we studied.

After the student applied to a specific school and grade, applications were assigned to priority groups (such as sibling reunification) within school and grade. Lotteries were then conducted, with separate lotteries for each school, grade and priority group. Applications received random numbers within each school × year × grade × priority group cell. Based on their random number, students were contacted in strict rank order and offered admission. An identical approach was taken for all three of the choice programs. Figure 1 illustrates using a simple hypothetical example. In this example, a true lottery occurs for group B. Figure 2 shows the full list of priority groupings for all three choice types, which sometimes vary by year.

In many cases, all students in the top priority group(s) for a given school and grade were admitted, since the enrollment limit was large enough to accommodate all of these students. However, the enrollment limit would often "hit" within some lower priority group. Students in this priority group experienced a "true" lottery, in that some students were admitted and others not, depending on their random number draw. There also exist cases where students in all priority groups were admitted to a given school and grade, signaling that the school truly was not oversubscribed in that grade. However, since the randomization from the lottery is essential for obtaining exogenous variation, we do not include such cases because there is no randomly created control group.

Analytically, we treat each lottery as a separate experiment. Thus, in addition to applying to the same school in the same year, students must also be in the same grade level and priority group to be compared against one another. We do so because students who apply to (e.g.) grade 6 at a magnet school may not be identical on average to students who apply to the same magnet in grade 7. Further, among the applications to a given school and grade, students in the (e.g.) sibling reunification group are likely to differ from students in the other priority groups.

Overall, we identified, for students applying to choice programs for grades 6-12, the number of true lotteries and applications listed in Table 1. The bottom part of the table displays mean characteristics of the students making the applications to each program. For both the Magnet and VEEP programs, students' baseline reading and math scores are below the district average (scores are scaled to have a mean of zero and standard deviation of one within the district), and Hispanic and black students represent the two largest racial/ethnic groups applying. Very few students applying to these programs have a parent who holds a Bachelor's degree or higher. The open enrollment program, known as Choice, attracts an applicant pool that is quite different. Their baseline math and reading scores are above the district average, the two largest racial/ethnic groups among applicants are Hispanics and whites. Notably, 40 percent of students applying to Choice have parents with a Bachelor's degree or higher, compared to just 21 and 11 percent for Magnet and VEEP programs, respectively. The intuition for this difference is that unlike the Magnet and

VEEP programs, Choice does not provide busing, thus requiring families to drive their students to choice schools, or to make other transportation arrangements. Providing this transportation may prove more difficult for families with fewer resources.[5]

3.2 Outcomes of Interest

We examine the impact of winning a lottery to attend a middle or high school through a school choice program on two types of outcomes: (1) outcomes which are measured during the high school years but which are likely to be related to longer term outcomes such as college graduation and earnings, and (2) postsecondary outcomes. The specific outcomes are detailed below.

High School Outcomes

Because we focus on those applying to school choice programs in grades 6 to 12, we include a number of secondary school outcomes:

- An indicator for student being on track in grade 9, meaning that they passed at least ten units, had no more than one semester F in a college preparatory course, and maintained a Grade Point Average (GPA) of at least 2.0[6]
- An indicator for student passing the California High School Exit Examination in their first attempt in grade 10
- The percentage of days attended in grade 12
- The GPA in grade 12
- The citizenship grade in grade 12 (based on teachers' report card reports on behavior in each class, this measure is an average across all courses, scaled to 0 to 4)
- An indicator for whether the student graduated on time

Postsecondary Outcomes

We examine two types of postsecondary outcomes — enrollment and degree attainment — at various points after high school graduation. For students indicated as dropouts in the data, prior

[5] Koedel, Betts, Rice and Zau (2009) show that the Magnet and VEEP programs tend to integrate SDUSD racially and in terms of socioeconomic status, but that the Choice program slightly increases segregation. They argue that the lack of busing for Choice explains why its impact on segregation is different.

[6] We include the GPA provision because throughout the period studied SDUSD required students to have a GPA of at least 2.0 in order to graduate.

to high school graduation, we maintain these students in our analysis but set all postsecondary outcomes to zero.

i) Enrollment

- Three separate indicators for postsecondary enrollment in the first year after high school graduation: enrollment in any postsecondary institution; enrollment in two-year institutions; and enrollment in four-year institutions.
- Three separate continuous measures for the number of years of postsecondary enrollment in the first four years after high school graduation: the number of years in any postsecondary institution; the number of years in two-year institutions; and the number of years in four-year institutions.

ii) Degree Attainment

- Three indicators for Bachelor's degree within four, five or six years of high school graduation
- Three indicators for an Associate's degree or other community college graduation event within four, five and six years of high school graduation

The postsecondary data are derived from the National Student Clearinghouse (NSC). The NSC is a nationwide student-level database of postsecondary enrollment and degrees granted for postsecondary institutions, which jointly account for about 93% of total postsecondary enrollment nationwide. In the NSC data, many of the San Diego community colleges did not name the degrees awarded, and so for two-year colleges, our outcome consists of a measure of an Associate's degree or any graduation event recorded by a two-year college. This measure thus combines Associate degrees and sub-Associate certificates.

3.3 Estimating Treatment Effects

The first central part of our analysis consists of generating intent-to-treat (ITT) estimates of the impact of winning a lottery to either the Magnet, VEEP, or Open Enrollment ("Choice") programs. Our ITT equations typically take the form:

$$Y_{ijgst} = \sum_{j=1}^{J} \alpha_j + WIN_{ijsg}\beta + \delta_M S^M_{i,0} + \delta_R S^R_{i,0} + MALE_i\gamma + \epsilon_{ijgst} \quad (1)$$

where i indexes students, j indexes lotteries, and g and s denote the intended grade level and school of the lottery application, with $g \in \{6, 7, ..., 12\}$.[7] The base year of the application is defined as

[7]The lottery indicator j makes s and g subscripts redundant, since lotteries are specific to a given school and grade (and lottery group). However, we include s and g for clarity.

time 0, such that Y_{ijstg} denotes an outcome measured t years after the application year, and all other subscripts continue to refer to the original lottery application.

We model these outcomes as a function of, most importantly, a dummy variable WIN_{ijsg} indicating whether the student won the given lottery, and lottery fixed effects, α_j. Other terms in the equation include math and reading achievement from state-administered standardized tests, expressed as Z-scores, from the baseline school year in which the student applies (the $S_{i,0}$ terms); a dummy variable for males; and an error term ϵ_{ijstg}.[8] The coefficient of interest is $\hat{\beta}$, which states the estimated impact of winning a lottery on the given outcome. We also cluster standard errors by lottery in all regressions.

It is important to obtain ITT estimates, as described above, because they tell policymakers the gains from making one more school choice offer. However, policymakers are also interested in knowing the causal impact on a student of entering a school choice program. This impact of treatment on the treated will generally be bigger than the intent to treat estimate because not all lottery winners take the offer (the take-up effect). We can estimate the impact of winning a lottery and attending the lottery school using two stage least squares (2SLS), in which we instrument for the new regressor of interest — enrolling at the school to which the student applied the next year — with a dummy variable indicating whether the student won the lottery. This approach simply scales up the intent-to-treat estimate by the reciprocal of the of the take-up rate.[9] This approach assumes the entire intent-to-treat effect derives from students who win the lottery and actually enroll at that school the following year. It otherwise assumes no causal effect on lottery winners who do not take up the offer.

There is a second and broader adjustment that one can make to the ITT estimator to estimate the impact of treatment on the treated. This second approach not only accounts for lottery winners who do not switch to the school, but for students who lose a lottery but manage to gain entry to another school of choice by winning a different lottery (the substitution or cross-over effect). In this version of estimating the average impact of treatment on the treated, we can use an instrumental variables (IV) approach in which we first alter equation (1) so that the regressor of interest is not the indicator for winning a lottery, but whether the student the following year switched into any school participating in the given choice program. We then instrument for this endogenous

[8] If the lottery is conducted fairly, then we should obtain unbiased causal estimates of the impact of winning a lottery from equation (1) even if we exclude the baseline achievement and male indicator. However, their inclusion can increase precision. We replicated all models without the baseline test scores and results were generally similar but less precise.

[9] Bloom (1984) is one of the early papers suggesting this scaling factor, although not in an instrumental variables framework. Thus, the estimator is essentially what has become known as the single Bloom adjustment, although the 2SLS approach also makes appropriate adjustments to the standard errors.

variable using the indicator for whether the student won the current school choice lottery (lottery j). Mechanically, the IV estimator is identical to dividing the ITT estimates by $(b-a)$, where b is the proportion of lottery winners who accept the admission offer and a is the proportion of lottery losers who nonetheless enter another school of choice (e.g. by winning in another lottery).[10] Some researchers refer to this as the double Bloom (1984) adjustment.

4 Results

4.1 Testing for Lottery Fairness and Differential Attrition

We define the lottery process as "fair" if there is no statistically significant difference in observable characteristics between lottery winners and lottery losers. Because all of our outcome variables are related in some way to academic achievement, it is particularly important to test whether the initial test scores of applicants who win a given lottery and those who lose a given lottery are identical. If we retain the null of no difference in baseline test scores between lottery winners and losers, it greatly buttresses the argument that the lottery is conducted fairly.

Since lotteries differ greatly in their percentages of winners and losers, we do not show a summary statistics table comparing the average characteristics of winners and losers, which in this context compares more across lotteries than within a given lottery. Instead we use a regression approach: regressing a given characteristic on lottery fixed effects plus an indicator for whether a student won a given lottery. These results are displayed in Table 2.

The first two rows of Table 2 show the coefficient for lottery winners in models of baseline Z-scores in math and reading — that is, in the year that students applied — for true lotteries. For none of the three types of school choice is there a significant difference between lottery winners and losers in baseline test scores. The third row examines whether the presence of these lagged test scores, which we include as controls in our impact analysis below, are equally likely to be missing for lottery winners and losers. No significant differences emerge. The next six rows of Table 2 test for any differences in basic demographics between lottery winners and losers. We find no meaningful differences here, except that lottery winners are slightly more likely to be white (about 2.0 percentage points more likely, when pooling across all three choice types). Despite generally few differences in observed characteristics between lottery winners and losers, we control for baseline achievement in math and reading, as well as gender, in all outcome models as a precaution.

The final row of Table 2 does not test for lottery fairness so much as differential attrition. The outcome here is whether the student leaves the district the year after the school choice application,

[10] This statement is in fact contingent upon identical treatment effects across lotteries. See section 5 of Heckman, LaLonde and Smith (1999), for a description of the underlying assumptions.

based on enrollment records. We find no significant differences for Magnet or VEEP programs, but a small but significant difference for the Choice program, and in the pooled case. This result hints at differential attrition between lottery winners and losers. Table A1 of the appendix presents additional results about differential attrition, on an outcome-by-outcome basis. For most outcomes that we study, we see a slightly higher rate of attrition among lottery losers than lottery winners, though this difference often is not statistically significant. Differential attrition appears most pronounced for postsecondary graduation outcomes, specifically, which contain all of the significant cases in Table A1.

4.2 Verification That the Intensity of the Treatment Contrast Is Large

One concern is whether the intensity of the treatment contrast is large between students' local schools and the schools to which they apply. Because many of the factors that influence a student's educational trajectory are not readily observed, we cannot measure the differences in all of the relevant measures of school and classroom environment. However, we can test for differences in observable characteristics.[11]

Therefore, we compare lottery winners' local school with the school to which they applied. The comparisons include 14 variables — nine related to academic and behavioral outcomes for students attending each school, and five measures of the socioeconomic and racial/ethnic mix of the student body. All these variables are averaged for each school over the school years 2001-2002 through 2007-2008.

We derive many of the school academic measures from the statewide achievement test, the California Standards Test (CST), which during the period we study tested students annually in reading and mathematics between grades 2 and 11. We transformed test scores for students into Z-scores by grade and year. For English we averaged the Z-scores across the middle or high school for all students enrolled (up to grade 11). A limitation of the math scores is that, after grade 7, students were given math tests that varied depending on the math course taken, making it very difficult to compare achievement among all students in any grade above grade 7. Therefore, for math CST results we were able to characterize middle schools by math performance in grades 6 and 7 only, omitting grade 8. For the same reason we lacked math scores for high schools, and so in the tables described below school math performance results include only middle schools. We characterized schools by four test-score measures: average Z-scores in reading and math, and average school value-added. The latter we calculated by regressing test scores on students' lagged

[11] Research by Betts, Zau and Rice (2003) suggests that San Diego schools, especially at the middle and high school levels, vary considerably in terms of peer achievement and the qualifications of teachers, and that these variables are associated with the student's own subsequent gains in achievement.

test scores, as well as controls for gender, race/ethnicity, parental education level and grade level, and then averaging the residuals for each school.

Other academic measures of the schools included the percentage of students who attended that school who ultimately graduated on time, the percentage of days absent, the percentage of students on track in grade 9, and a citizenship grade. For middle schools, the measures on on-time graduation and being on track in grade 9 are observed during high school but calculated among all students who attended the given middle school. But the absence and citizenship variables refer to outcomes while the students were attending the given middle or high school. Being on track in grade 9 means that a student in his or her first time in grade 9 completed at least the expected ten units, with no more than one F in a college-preparatory course, and with a cumulative GPA of at least 2.0. (A cumulative GPA of 2.0 has been a graduation requirement in the district for the entire period we study.) The citizenship variable is averaged over the citizenship grade that every teacher gives to students in their classes in the respective middle and high schools. It provides important insights into teachers' views of how well behaved students are in the classroom, and thus provides a proxy for the non-cognitive behavior of students at a school.

School comparisons also include socioeconomic measures including the proportion of students from each of four racial/ethnic groups, and the proportion of students whose parents hold a Bachelor's or higher degree.

Tables 3A through 3C compare the local schools and the schools to which students applied in the Magnet, VEEP and Choice programs, respectively, while Table 3D pools all three programs. The sample consists of applications to true lotteries, in which students were both lotteried in and lotteried out. Tables show the mean of the given characteristic in the school to which the student applied, the mean difference between the choice school and the student's local default school, along with the p-value for a test that the local school and the choice school were identical on the given characteristic.

Several striking patterns emerge. First, across all three types of choice, and in the pooled analysis in Table 3D, the mean difference between the school in the choice program and the student's local default school is highly significant, with p-values well below 0.01.

Second, the differences mostly indicate that students were applying to schools that outperformed the local schools across the panoply of academic measures, as well as the two behavioral outcomes: the citizenship grades and percentage of days absent. The schools to which students applied also tended to have student bodies with a higher socioeconomic status and a greater proportion of white students, and a lower proportion of black and especially Hispanic students. The lone exception is magnet schools, for which the schools to which students applied had about 2 percentage points more black students. This could reflect both the result of past moves to these schools by non-resident

black students as well as the fact that most of the magnet schools operated in lower-income areas of the city and offered specific curricular emphases in the hope of attracting students from higher income areas of the district. Results on the proportion Asian are mixed but the differences were closer to zero than for the other groups.

A third striking result from the tables is that the value-added in test scores at the schools to which students applied was near zero, meaning that student gains in achievement were on average similar to the district as a whole. (Value-added here is computed based on all students at each school, rather than just lottery applicants.) Nonetheless, value-added was significantly higher at the choice schools relative to the local schools in all but one of eight cases. (The exception was middle school math at magnet schools, where math value-added was lower by 0.05 standard deviation than at local public schools.) That is, in all but one case, students were applying to schools where students made more progress on average in reading and math than at the student's local school. Of course, parents cannot readily observe value-added but do observe average achievement through annual publication of CST results. These test score levels were consistently higher at the choice schools, ranging from 0.16 to 0.65 of a standard deviation.

The overall conclusion is that in our sample of applications to true lotteries, students were on average applying to schools that differed significantly from their local schools. Schools of choice had higher test scores and value-added in test scores, better behavioral outcomes and better outcomes in high school such as graduating on time. They also tended to have greater proportions of students who were white and whose parents held a Bachelor's degree or higher.

4.3 Intent to Treat Estimates

We begin with an intent-to-treat analysis of the impact of winning a lottery to any of the programs. Tables 4 to 6 summarize the key coefficient ($\hat{\beta}$) and its standard error for high school outcomes, postsecondary enrollment outcomes, and postsecondary graduation outcomes, respectively.[12] To help provide context for the size of estimated impacts, the bottom row of these tables shows the mean value of the outcomes for the students in the pooled set of applications.

In Table 4, we see that there were no statistically significant differences between lottery winners and losers for any of the six high school outcomes, for any of Magnet, VEEP, or Choice. The final row pools all of the lotteries across the three types of choice and again finds no significant differences. Further, the coefficients were typically small, with roughly as many negative as positive.

Table 5 shows results when we model the number of years enrolled in postsecondary education by given numbers of years after high school graduation. As mentioned earlier, postsecondary outcomes

[12] Appendix tables A2 to A4 show slightly more detailed versions of these results, providing additional information about R-squared, the number of observations, and number of clusters.

are set to 0 for high school dropouts. Again, with one exception, none of the ITT estimates is statistically significant. The exception is that when we pool all types of school choice, the number of years enrolled in a two-year college four years after graduation is 0.08 higher for lottery winners. This pooled result probably derives from the VEEP and Choice lotteries, where positive estimates were close to becoming significant. Note however that there is no overall significant effect on overall time enrolled in postsecondary education, and that the estimated effect on enrollment on four-year college enrollment is negative, although not significant. This pattern of findings hints weakly at the possibility that VEEP and Choice programs could induce a slight substitution towards two-year and away from four-year colleges.

Table 6 shows impacts of winning a lottery on degrees obtained four, five and six years after high school graduation. As discussed earlier, our community college graduation measure combines Associate degrees and sub-Associate certificates. For Magnet, VEEP, and the pooled case, there is a significant 1-3 percentage point increase in the likelihood that lottery winners complete an Associate or other two-year college certification four or five years after high school graduation. However, no significant effect is found six years after high school graduation. One interpretation is that winning a school choice lottery may accelerate time to degree for community college degrees and certificates, while having no impact on the overall probability of obtaining a degree or certificate by six years after graduation.

For the most part, Table 6 shows no significant relationship between winning a lottery and whether students subsequently obtain a Bachelor's degree. One exception is that winners of magnet school lotteries appear to be about 5 percentage points less likely to obtain a Bachelor's degree within six years of high school graduation. Another exception is that Choice lottery winners are about four percentage points less likely to obtain a Bachelor's within four years of graduation, but for these students, no significant difference remains between lottery winners and losers five or six years after graduation.

Table A5 of the appendix provides an alternative view, combining 2-year and 4-year college certifications into a single outcome measured 4, 5, and 6 years after high school graduation. Using this version of the outcomes, ITT estimates of the effects on postsecondary degree attainment remain mostly insignificant. At four years after high school graduation, lottery winners in the VEEP program have about a 4 percentage point higher chance of having either a 2-year or 4-year college certification compared to lottery losers. But this effect subsequently dissipates and becomes insignificant. For none of the choice programs, nor the pooled case, is there a significant impact on the combined outcome of having either a 2-year or 4-year college certification either five or six years after high school graduation.

Overall, our ITT analysis of the impact of choice on postsecondary enrollment and degrees

suggests some additional interest in 2-year colleges, but perhaps partly as a result of students shifting away from 4-year colleges, rather than all new interest in postsecondary education. We find some evidence of positive and significant effects on both enrollment and degree completion at 2-year colleges, while estimated impacts for 4-year colleges are typically negative (though usually not significant). Results are usually insignificant when looking at degree or completion or the total number of years enrolled in either type of program, combined.

For students who applied to only one school, the intent-to-treat estimate is straightforward to interpret: it is the impact of being offered a chance to enroll in the choice school, instead of enrolling in the default local school. Interpretation is somewhat more difficult in cases where students applied to multiple schools. Now, we are estimating the impact of making one more offer to attend a choice school than the student would otherwise have received. It could be that the first lottery won has the biggest impact. (If all the schools to which a student applied offered similar advantages over the local school, then the impact of winning a lottery could be smaller if the student had already won a lottery to another the schools to which she applied.)

To examine this possibility, we re-ran all the intent-to-treat estimates for each type of choice, as well as pooled, after restricting the sample to students who had applied to only one school. If we found much bigger estimated impacts, and impacts that were significant, in this exercise, it would support the notion that most of the gains from school choice accrue with the first lottery won. However, we found almost identical patterns of statistical significance to those reported above. In some cases coefficients were slightly bigger, but in almost all cases the estimated impacts remained insignificant.

4.4 Estimated Impacts of Treatment on the Treated

Tables 7, 8 and 9 show estimates of the impact of treatment on the treated for the sets of high school outcomes, measures of postsecondary enrollment and college graduation, respectively. Table 7 shows no significant effect of enrolling in the school to which a student applied on various high school outcomes. The precision varies by type of school choice and outcome. While there are some quite precise "zero" estimates, the magnet school estimate of the impact on the probability of graduating on time is both large and imprecise. The estimated impact is about 0.10, with a 95% confidence interval of roughly -0.07 to 0.27.

Table 8 turns to the impact of winning a lottery and enrolling on six different measures of the number of years of postsecondary enrollment. In general there are no significant estimated impacts. The only exception is that the impact of winning a Choice (open enrollment) lottery on the number of years enrolled in a two-year college within one and four years of high school graduation are significant, with effects of 0.14 and 0.44 respectively. When we pooled across types

of school choice, we found a significant 0.27-year increase in the number of years enrolled in a two-year college four years after high school graduation. This pooled effect probably derives mainly from the large and significant effect for Choice. Countering these positive effects on two-year college enrollment, the table provides some evidence that Choice may cause substitution away from four-year colleges to two-year colleges: the impact of winning a Choice lottery and enrolling on number of years of postsecondary enrollment is never significant, but the negative point estimates suggest that Choice is associated with lower enrollment in four-year colleges.

Table 9 examines the impact of winning a lottery and enrolling on postsecondary degrees and certificates. Again, in most cases the impacts are not statistically significant, but exceptions emerge. For all cases except Choice, the impact of winning a lottery and enrolling is linked to a modest but significant boost in the probability of earning a community college Associate degree or certificate within four and/or five years, but the effects become insignificant after six years. As for the Bachelor's outcomes, most of the estimates are insignificant. Both exceptions point to negative impacts. The more notable finding is that, for those who won a Magnet lottery and enrolled at the magnet, there is an estimated drop in the probability of earning a Bachelor's degree six years after graduating from high school of 0.22. This is a large effect, given that the proportion of all students in the pooled set of true lotteries who obtained a Bachelor's degree after six years was 0.183.[13] Second, winning a Choice lottery and enrolling is associated with a drop of 0.12 in the probability of obtaining a Bachelor's degree in four years, but the impacts after five and six years are very close to zero and are not significant.

Table 10 shows the impact of winning a lottery and enrolling the following year in any school participating in the given type of school choice. In general the coefficients are higher than in Table 7 but not markedly — meaning that it is not common for a student to win one lottery, turn that school down, and then enroll in another — but it certainly does happen. None of the estimated impacts is significant, but the precision is somewhat lower for this version of the impact of treatment on the treated.

Turning to postsecondary enrollment, Table 11 shows highly similar patterns of statistical significance compared to the single Bloom (1984) adjustment. Again there are significant effects for Choice on years enrolled in a two-year college one and four years after graduation, and when pooling all three types of choice for impacts on years enrolled four years after high school graduation. One newly significant effect is for VEEP, where enrolling in any of the VEEP schools to which a student applied is significantly negatively related to enrollment in a four-year college in the first year after completing high school. However, the effect becomes insignificant when looking at years enrolled

[13]We find some corroboration for this effect in Table 8, where the estimated number of years enrolled in a four-year college after six years was negative for magnet schools, although very imprecisely estimated.

in the first four years after high school.

Table 12 shows the impacts of enrolling in a choice program on our measures of postsecondary degree and certificate attainment. With the exception of the impact of enrolling in a magnet school on obtaining a two-year college degree or certificate within four years of graduation, the patterns of statistical significance are identical to the results in Table 9 from the impact of enrolling in the specific school to which one applied. The most striking finding is that enrolling in a magnet school has a large negative impact on the probability of obtaining a Bachelor's degree within six years of graduating from high school. There are positive impacts of enrolling in a VEEP school on obtaining a degree or credential from a two-year college within four or five years, but again the effects dissipate by year six.

4.5 Test for Variations in the Effect by School Characteristics

Schools vary dramatically in demographics and academics, and it may not be sensible to assume that the impact of winning a lottery to one school should be the same as for another school. The overall tenor of the intent-to-treat estimates is that winning a lottery has no significant effect on high school and postsecondary outcomes, with a few exceptions, typically indicating negative effects. It could be that there are significant effects, positive or negative, for certain types of choice schools. For example, if a student wins a lottery to attend a school having much higher value-added in test scores than the local school, one could imagine enrolling in such a school could have a positive impact. Indeed, Hastings, Kane and Staiger (2009) found, in one district, that school choice led to test score gains for those applying to schools with higher average test scores.

Theoretically, what should matter for impacts are the *differences* in characteristics between the school to which the student applied and the default local school which the student might otherwise attend. Accordingly, we re-estimated the intent-to-treat models after adding to equation (1) the difference in a given characteristic between the applied versus local school, and an interaction of this difference with the dummy for winning the lottery. We measure differences for the same 14 school characteristics discussed in the earlier tables, covering measures of students' average academic achievement, behavior, and demographics.

We estimated these interactions after pooling all true lotteries across the three types of school choice, and also separately by each program. We emphasize the pooled results because we primarily seek patterns that are consistent across types of choice, rather than idiosyncratic to just one choice program.

Table 13 summarizes the level of significance of the interactions, as well as the sign of the interaction in cases where the interaction is significant at the five percent level. Panel A shows results for high school outcomes, with Panels B and C showing results for postsecondary enrollment

and postsecondary degree attainment. What is most apparent across the three panels is how rarely interactions between winning a lottery and differences in school characteristics are statistically significant. Further, when the impact of winning a lottery changes with the gap in a given type of school trait, it is usually for isolated outcomes.

We estimated these interaction models separately for the three school choice programs as well. Results are not shown but are available on request. It was more frequently the case that interactions were significant in these models but the patterns varied across outcomes and types of school choice in ways that suggested at most idiosyncratic effects. The percentage of interactions that were significant at the five percent level were 6.3% for Magnet, 11.5% for Choice and 21.8% for VEEP. These first two percentages are close to the type I error rate expected. VEEP was the outlier, and had two outcomes that accounted for about half of the significant effects. These two grade 12 outcomes — the percentage of days attended and citizenship grades — showed significant positive interactions between winning the lottery and the differences between the choice school and the local school in reading scores, math scores, reading value-added, mean GPA, the proportion of students who were white and the proportion of parents with a Bachelor's or higher. While all of these relationships are easy to motivate, for example in terms of positive peer effects, these patterns did not show up for other high school or postsecondary outcomes, with a few exceptions.

4.6 Test for Variations in the Effect by Student Subgroup

It seems plausible that students from different subgroups could benefit differentially from winning a lottery into a given choice program. For instance Deming et al. (2014), in the only lottery-based study of the impact of open enrollment on postsecondary outcomes, found that girls benefited from school choice, but boys did not. It is crucial to test for these interactions from a policy perspective. It could be the case that choice programs have no impact on many or even most students, but could lead to better outcomes for some students, and potentially a Pareto improvement.

We tested for variations in the effects of school choice by student gender, race/ethnicity, English Learner status, special education status, and parental education. Additionally we tested whether the grade to which a student applied was related to the impact. Presumably, the earlier a student applies to the school choice programs the greater the impact. (This logic follows because the choice programs allow students to articulate from middle school to the high school in the same attendance area, or in the case of magnets, to magnet high schools with a similar theme, if available. So applying to a middle school choice program could offer a choice path all the way through high school graduation.)

Table 14 summarizes the interactions between these student characteristics and winning a lottery for the pooled sample, using similar notation to that in Table 13. The only difference in the way that

we characterize significant interactions was that when interactions were significant but the impact of winning a lottery did not increase or decrease monotonically we indicate significance at the 1% and 5% levels using ** and * respectively. (Non-monotonicity could arise for personal characteristics that were measured not by binary or continuous variables but rather by a series of dummy variables. Such was the case for the grade applied to, parental education and race/ethnicity.) The top part of each panel shows interactions with the student's own lagged achievement, with the bottom panel showing various demographic characteristics along with English Learner and special education status.

The three panels of Table 14 suggest that overall few personal characteristics significantly mediated the impact of winning a lottery. The grade level for which students applied was often significant, but rarely in a monotonic way. For example, the impact of winning a lottery was in some cases larger in grade 8 but smaller in grade 10, both relative to the impact in grade 6.

5 Implications and Conclusion

The results suggest that on average granting a slot in a school of choice to a student does not significantly alter his or her high school or postsecondary trajectories. Some exceptions do emerge. Winning a lottery may shorten the time taken to obtain a community college degree or certificate, but with no effect on the long run likelihood of obtaining that degree or certificate. The only effect that may be lasting is that six years after high school graduation, Magnet lottery winners may be less likely to have obtained a Bachelor's degree than lottery losers.

Policymakers should care about the intent-to-treat results because they speak to the value of expanding a school choice program by offering one more spot. But policymakers may care even more about the impact on a student of actually enrolling in a school of choice. We estimated two types of impacts of treatment on the treated. Using winning a lottery as an instrument, we estimated the impact of enrolling in the school applied to, as well as that of enrolling in any school in the given program, such as the Magnet program. Overall, the results were similar to the intent-to-treat estimates.

These results may mask larger impacts in specific cases. Theoretically, the bigger the difference between a student's default local school and the school to which she applies, the bigger the impact of winning a lottery to attend the school of choice. We tested whether the intent-to-treat effects varied by the difference between the two schools in terms of achievement, student behavior and student demographics. Although on average these inter-school differences were quite large, we typically did not find that any of these differences mediated the impact of winning a lottery. The biggest exception was that winning a VEEP lottery had bigger positive impacts on the two grade

12 behavioral outcomes — attendance and citizenship grades — if the VEEP school to which one applied enrolled students with higher achievement and/or better behavior, or had a greater share of white students.

Overall, these results do not suggest that school choice has generated meaningful gains for students in San Diego. But this conclusion must be tempered for several reasons. First, there could be other unmeasured outcomes on which students gain if admitted to a choice program. Second, there could be larger effects, either positive or negative, for specific types of students applying to specific types of schools. We hope to study this in the future by looking for interactions between student background and the difference in school characteristics between the local school and the school of choice. Third, further robustness checks are planned. Among these planned extensions are checks that differential attrition has not biased the results. Future extensions will likely include estimation of another type of impact of treatment on the treated, which instruments for the number of years attending a school of choice, and robustness checks to make sure that substitution towards charter schools has not biased the results.

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6 Tables and Figures

Priority	Description	Who Is Admitted?	
A	Sibling	All	
B	Specialized Course	Some	⟵ True Lottery
C	Before Priority Deadline	None	
D	After Priority Deadline	None	
E	Gifted Cluster	None	

Notes: For a given school and grade, applications are grouped in descending order by the above priority groups. In this example, there are adequate seats to admit all students applying from the Sibling priority group, A. Enough seats are then available to admit a portion, but not all, of the students in group B. A true lottery occurs for group B, with only those students with the lowest random numbers in the Specialized Course category being admitted. No applications from groups C, D or E are then admitted, due to a lack of seats, in this hypothetical example.

Figure 1: Simple Example of Lottery Admissions

Magnet

Priority Type	
0	Sib/Continuity
1	Continuity
2	Sib/Before 9/3*
3	Sibling
4	Before 9/3*
5	Before Priority Deadline
6	After Priority Deadline
7	Before 9/3 OD*
8	Before Priority Deadline OD
9	After Priority Deadline OD

*Eliminated

Priority Description 04-05 to 05-06	
0	Sib/Continuity
10	Continuity
30	Sibling
31	Priority for PISC-see attached
32	Priority for PISC
33	Priority for PISC
34	Priority for PISC
35	Priority for PISC
36	Priority for PISC
50	Before Priority Deadline
60	After Priority Deadline
80	Before Priority Deadline OD
90	After Priority Deadline OD

Priority Description 06-07 to 07-08	
0	Sib/Continuity
100	Continuity
300	Sibling
301	Priority for PISC-see attached
302	Priority for PISC-see attached
303	Priority for PISC-see attached
304	Priority for PISC-see attached
305	Priority for PISC-see attached
306	Priority for PISC-see attached
307	Priority for PISC-see attached
308	Priority for PISC-see attached
309	Priority for PISC-see attached
310	Priority for PISC-see attached
500	Before Priority Deadline
600	After Priority Deadline
800	Before Priority Deadline OD
900	After Priority Deadline OD

VEEP

Priority Type	
A	Sibling
D	Before 9/3*
E	Before Priority Deadline
F	After Priority Deadline
H	Before Priority Deadline OD
I	After Priority Deadline OD

*Eliminated

Priority Description 04-05 to 05-06	
A0	Sibling
B1	Priority for PISC-see attached
B2	Priority for PISC-see attached
B3	Priority for PISC-see attached
B4	Priority for PISC-see attached
B5	Priority for PISC-see attached
B6	Priority for PISC-see attached
E0	Before Priority Deadline
F0	After Priority Deadline

Priority Description 06-07 to 07-08	
A00	Sibling
B01	Priority for PISC-see attached
B02	Priority for PISC-see attached
B03	Priority for PISC-see attached
B05	Priority for PISC-see attached
B05	Priority for PISC-see attached
B06	Priority for PISC-see attached
B07	Priority for PISC-see attached
B08	Priority for PISC-see attached
B09	Priority for PISC-see attached
B10	Priority for PISC-see attached
E00	Before Priority Deadline
F00	After Priority Deadline

Choice

Program Criterion	
1	Sibling-concurrenly enrolled
2	Gifted Cluster
3*	General indistrict
4	General out of district

*2001-02 was specialized course
General indistrict was 4 and out of district was 5.

Priority Type	
A	Sibling
B*	Calendar Change
C*	Specialized Course
E	Before Priority Deadline
F	After Priority Deadline
H	Before Priority Deadline OD
I	After Priority Deadline OD
J	Gifted Cluster

*Eliminated after 2001-02

Figure 2: Priority Codes for Magnet, VEEP, and Choice Programs

Table 1: Applications to True Lotteries, and Characteristics of Applicants

	Magnet	VEEP	Choice	Pooled
Number of Applications	7,140	3,810	3,415	14,365
Win That Lottery	0.46	0.69	0.43	0.51
Prior Math Score (Z)	-0.13	-0.24	0.25	-0.07
Prior Reading Score (Z)	-0.12	-0.32	0.31	-0.07
Male	0.47	0.5	0.47	0.48
Parent Ed $\geq$ Bachelor	0.21	0.11	0.40	0.23
White	0.12	0.04	0.35	0.15
Black	0.27	0.16	0.14	0.21
Asian	0.13	0.07	0.14	0.12
Hispanic	0.47	0.73	0.36	0.52

Notes: Apart from the number of applications and test scores, all other rows refer to proportions of applications in which students had the given characteristic. The sample consists of all applications to true lotteries for admission in fall 2001 through fall 2007.

Table 2: The Relationship Between Winning a Lottery and Student Characteristics

	Magnet	VEEP	Choice	Pooled
Prior Math Score	0.000	0.039	-0.073	-0.007
	(0.026)	(0.032)	(0.051)	(0.020)
Prior Reading Score	0.014	0.040	-0.032	0.010
	(0.034)	(0.037)	(0.055)	(0.024)
Presence of Lagged Scores	0.010	0.003	-0.015	0.001
	(0.008)	(0.010)	(0.011)	(0.006)
Male	0.010	-0.018	-0.014	-0.004
	(0.019)	(0.023)	(0.017)	(0.012)
Parent Ed $\geq$ Bachelor's	0.004	0.013	0.026	0.012
	(0.014)	(0.015)	(0.029)	(0.011)
White	0.024	0.009	0.027	0.020*
	(0.015)	(0.010)	(0.018)	(0.009)
African-American	-0.021	-0.008	-0.002	-0.013
	(0.012)	(0.018)	(0.016)	(0.008)
Hispanic	0.020	0.009	-0.016	0.008
	(0.014)	(0.023)	(0.024)	(0.011)
Asian/Pacific Islander	-0.021	-0.007	-0.009	-0.014
	(0.013)	(0.011)	(0.023)	(0.009)
Left District After Lottery	-0.004	-0.015	-0.029*	-0.013*
	(0.009)	(0.010)	(0.013)	(0.006)

Notes: For each choice type, various student characteristics are used as outcomes and regressed on a dummy for winning the lottery plus lottery fixed effects. This table reports the coefficients on 'win lottery' from these regressions, along with their standard errors. Results are informative of lottery fairness. Insignificant results indicate that lottery winners and losers show no statistically significant difference on the given characteristic. Robust standard errors are in parentheses, clustered by lottery. ** $p<0.01$, * $p<0.05$.

Table 3: Comparison of School Characteristics: Choice Program Schools Versus Local Schools

Panel A: Magnet Schools

School Characteristic	Mean at Applied School	Mean Difference (Applied−Local)	p-value Difference = 0
Reading, Z-score	0.00	0.36	<0.00005
Math, Z-score[a]	-0.12	0.15	<0.00005
School Value-Added, Reading	0.00	0.04	<0.00005
School Value-Added, Math[a]	-0.08	-0.05	<0.00005
Mean Grade Point Average	2.50	0.39	<0.00005
Proportion on Track in Grade 9	0.68	0.18	<0.00005
Proportion Graduating HS on Time	0.77	0.15	<0.00005
% Days Absent	5.24	-1.83	<0.00005
Mean Citizenship Grade	2.75	0.17	<0.00005
Proportion Students White	0.25	0.17	<0.00005
Proportion Students Black	0.21	0.02	<0.00005
Proportion Students Asian	0.14	-0.04	<0.00005
Proportion Students Hispanic	0.39	-0.15	<0.00005
Proportion Parent Ed $\geq$ Bachelor's	0.21	0.09	<0.00005

Panel B: VEEP Schools

School Characteristic	Mean at Applied School	Mean Difference (Applied−Local)	p-value Difference = 0
Reading, Z-score	0.16	0.65	<0.00005
Math, Z-score[a]	0.20	0.61	<0.00005
School Value-Added, Reading	0.00	0.04	<0.00005
School Value-Added, Math[a]	0.02	0.04	<0.00005
Mean Grade Point Average	2.63	0.54	<0.00005
Proportion on Track in Grade 9	0.66	0.20	<0.00005
Proportion Graduating HS on Time	0.79	0.21	<0.00005
% Days Absent	5.42	-1.76	<0.00005
Mean Citizenship Grade	2.96	0.46	<0.00005
Proportion Students White	0.37	0.31	<0.00005
Proportion Students Black	0.10	-0.08	<0.00005
Proportion Students Asian	0.13	0.01	0.0006
Proportion Students Hispanic	0.39	-0.25	<0.00005
Proportion Parent Ed $\geq$ Bachelor's	0.29	0.19	<0.00005

a — Restricted to middle schools and grades 6–7

Table 3: Comparison of School Characteristics: Choice Program Schools Versus Local Schools, Continued

Panel C: Choice Schools

School Characteristic	Mean at Applied School	Mean Difference (Applied−Local)	p-value Difference = 0
Reading, Z-score	0.32	0.53	<0.00005
Math, Z-score[a]	0.41	0.54	<0.00005
School Value-Added, Reading	0.01	0.03	<0.00005
School Value-Added, Math[a]	0.03	0.04	<0.00005
Mean Grade Point Average	2.72	0.42	<0.00005
Proportion on Track in Grade 9	0.72	0.16	<0.00005
Proportion Graduating HS on Time	0.82	0.14	<0.00005
% Days Absent	5.31	-1.13	<0.00005
Mean Citizenship Grade	3.03	0.32	<0.00005
Proportion Students White	0.42	0.22	<0.00005
Proportion Students Black	0.08	-0.07	<0.00005
Proportion Students Asian	0.13	-0.02	<0.00005
Proportion Students Hispanic	0.37	-0.14	<0.00005
Proportion Parent Ed ≥ Bachelor's	0.37	0.23	<0.00005

Panel D: All Schools (Magnet, VEEP, and Choice)

School Characteristic	Mean at Applied School	Mean Difference (Applied−Local)	p-value Difference = 0
Reading, Z-score	0.12	0.48	<0.00005
Math, Z-score[a]	0.09	0.40	<0.00005
School Value-Added, Reading	0.01	0.04	<0.00005
School Value-Added, Math[a]	-0.03	0.04	0.0039
Mean Grade Point Average	2.59	0.44	<0.00005
Proportion on Track in Grade 9	0.69	0.18	<0.00005
Proportion Graduating HS on Time	0.79	0.16	<0.00005
% Days Absent	5.30	-1.64	<0.00005
Mean Citizenship Grade	2.87	0.28	<0.00005
Proportion Students White	0.32	0.22	<0.00005
Proportion Students Black	0.15	-0.03	<0.00005
Proportion Students Asian	0.14	-0.02	<0.00005
Proportion Students Hispanic	0.38	-0.17	<0.00005
Proportion Parent Ed ≥ Bachelor's	0.27	0.15	<0.00005

a — Restricted to middle schools and grades 6–7

Table 4: Intent to Treat Estimates of the Impact of Winning a Lottery on High School Outcomes

	(1) On Track in Grade 9	(2) Passed Exit Exam in Grade 10	(3) % Days Attended in Grade 12	(4) Grade 12 Citizenship Grade	(5) GPA in Grade 12	(6) Graduated on Time
Magnet	-0.00758	0.000665	-0.174	-0.0198	-0.0160	0.0254
	(0.0123)	(0.0174)	(0.314)	(0.0372)	(0.0327)	(0.0228)
VEEP	-0.0114	-0.0136	-0.0250	0.0839	0.0276	0.0136
	(0.0264)	(0.0235)	(0.526)	(0.0553)	(0.0400)	(0.0237)
Choice	0.0241	0.0148	-0.460	-0.0472	0.00494	-0.00602
	(0.0344)	(0.0224)	(0.318)	(0.0428)	(0.0379)	(0.0176)
Pooled	-0.000268	0.00207	-0.201	-0.000635	0.00104	0.0151
	(0.0127)	(0.0120)	(0.220)	(0.0264)	(0.0215)	(0.0134)
Outcome Mean (Pooled)	0.581	0.614	93.57	3.206	2.893	0.725

Notes: Robust standard errors are in parentheses, clustered by lottery. ** p<0.01, * p<0.05.

Table 5: Intent to Treat Estimates of the Impact of Winning a Lottery on Postsecondary Enrollment Outcomes

	(1) # Years Enrolled in Postsecondary 1 Year After HS Graduation	(2) # Years Enrolled in 2-Year College 1 Year After HS Graduation	(3) # Years Enrolled in 4-Year College 1 Year After HS Graduation	(4) # Years Enrolled in Postsecondary 4 Years After HS Graduation	(5) # Years Enrolled in 2-Year College 4 Years After HS Graduation	(6) # Years Enrolled in 4-Year College 4 Years After HS Graduation
Magnet	-0.000545	-0.0108	-0.00427	-0.0164	0.0244	-0.0804
	(0.0212)	(0.0246)	(0.0228)	(0.0590)	(0.0506)	(0.0661)
VEEP	0.0185	0.0437	-0.0449	0.00397	0.131	-0.0786
	(0.0253)	(0.0322)	(0.0250)	(0.0772)	(0.0952)	(0.0882)
Choice	0.0294	0.0471	-0.0216	0.120	0.145	-0.0276
	(0.0167)	(0.0253)	(0.0237)	(0.0829)	(0.0793)	(0.0708)
Pooled	0.0122	0.0181	-0.0185	0.0256	0.0838*	-0.0656
	(0.0125)	(0.0155)	(0.0144)	(0.0416)	(0.0397)	(0.0438)
Outcome Mean (Pooled)	0.700	0.494	0.300	2.296	1.581	0.992

Notes: Robust standard errors are in parentheses, clustered by lottery. ** p<0.01, * p<0.05.

Table 6: Intent to Treat Estimates of the Impact of Winning a Lottery on Postsecondary Graduation Outcomes

	(1) Associate or Other 2-Year College Cert. 4 Years After HS Graduation	(2) Associate or Other 2-Year College Cert. 5 Years After HS Graduation	(3) Associate or Other 2-Year College Cert. 6 Years After HS Graduation	(4) Bachelor's 4 Years After HS Graduation	(5) Bachelor's 5 Years After HS Graduation	(6) Bachelor's 6 Years After HS Graduation
Magnet	0.0163*	0.0166*	0.0131	-0.00234	-0.0196	-0.0505*
	(0.00701)	(0.00716)	(0.0106)	(0.0107)	(0.0148)	(0.0213)
VEEP	0.0226**	0.0338**	0.0170	0.0171	-0.00737	0.0106
	(0.00786)	(0.0120)	(0.0144)	(0.0105)	(0.0172)	(0.0234)
Choice	0.0138	0.0217	0.0183	-0.0437*	-0.00793	0.00905
	(0.0177)	(0.0199)	(0.0278)	(0.0196)	(0.0282)	(0.0273)
Pooled	0.0170**	0.0219**	0.0157	-0.00657	-0.0135	-0.0193
	(0.00587	(0.00643)	(0.00879)	(0.00799)	(0.0105)	(0.0142)
Outcome Mean (Pooled)	0.032	0.045	0.062	0.061	0.136	0.183

Notes: Robust standard errors are in parentheses, clustered by lottery. ** p<0.01, * p<0.05.

Table 7: Estimates of the Impact of Winning a Lottery and Enrolling on High School Outcomes

	(1) On Track in Grade 9	(2) Passed Exit Exam in Grade 10	(3) % Days Attended in Grade 12	(4) Grade 12 Citizenship Grade	(5) GPA in Grade 12	(6) Graduated on Time
Magnet	-0.0282	0.00261	-0.649	-0.0735	-0.0602	0.101
	(0.0433)	(0.0677)	(1.142)	(0.135)	(0.123)	(0.0885)
VEEP	-0.0279	-0.0344	-0.0619	0.209	0.0688	0.0343
	(0.0627)	(0.0586)	(1.289)	(0.127)	(0.0974)	(0.0602)
Choice	0.0694	0.0458	-1.325	-0.136	0.0142	-0.0189
	(0.100)	(0.0691)	(0.938)	(0.124)	(0.107)	(0.0553)
Pooled	-0.000820	0.00668	-0.622	-0.00197	0.00322	0.0492
	(0.0386)	(0.0385)	(0.680)	(0.0815)	(0.0665)	(0.0437)

Notes: Robust standard errors are in parentheses, clustered by lottery. ** p<0.01, * p<0.05.

Table 8: Estimates of the Impact of Winning a Lottery and Enrolling on Postsecondary Enrollment Outcomes

	(1) # Years Enrolled in Postsecondary 1 Year After HS Graduation	(2) # Years Enrolled in 2-Year College 1 Year After HS Graduation	(3) # Years Enrolled in 4-Year College 1 Year After HS Graduation	(4) # Years Enrolled in Postsecondary 4 Years After HS Graduation	(5) # Years Enrolled in 2-Year College 4 Years After HS Graduation	(6) # Years Enrolled in 4-Year College 4 Years After HS Graduation
Magnet	-0.00215	-0.0425	-0.0168	-0.0647	0.0964	-0.317
	(0.0829)	(0.0962)	(0.0893)	(0.228)	(0.199)	(0.250)
VEEP	0.0439	0.104	-0.107	0.00944	0.313	-0.187
	(0.0609)	(0.0808)	(0.0582)	(0.181)	(0.231)	(0.207)
Choice	0.0895	0.143*	-0.0658	0.365	0.444*	-0.0842
	(0.0461)	(0.0724)	(0.0735)	(0.234)	(0.208)	(0.213)
Pooled	0.0390	0.0576	-0.0590	0.0817	0.267*	-0.209
	(0.0406)	(0.0495)	(0.0455)	(0.133)	(0.125)	(0.138)

Notes: Robust standard errors are in parentheses, clustered by lottery. ** p<0.01, * p<0.05.

Table 9: Estimates of the Impact of Winning a Lottery and Enrolling on Postsecondary Graduation Outcomes

	(1) Associate or Other 2-Year College Cert. 4 Years After HS Graduation	(2) Associate or Other 2-Year College Cert. 5 Years After HS Graduation	(3) Associate or Other 2-Year College Cert. 6 Years After HS Graduation	(4) Bachelor's 4 Years After HS Graduation	(5) Bachelor's 5 Years After HS Graduation	(6) Bachelor's 6 Years After HS Graduation
Magnet	0.0658*	0.0685	0.0574	-0.00949	-0.0811	-0.220**
	(0.0334)	(0.0353)	(0.0503)	(0.0422)	(0.0624)	(0.0714)
VEEP	0.0500**	0.0749**	0.0377	0.0381	-0.0164	0.0236
	(0.0163)	(0.0259)	(0.0314)	(0.0245)	(0.0372)	(0.0526)
Choice	0.0376	0.0565	0.0508	-0.122**	-0.0212	0.0261
	(0.0464)	(0.0511)	(0.0789)	(0.0441)	(0.0734)	(0.0772)
Pooled	0.0519**	0.0671**	0.0483	-0.0202	-0.0417	-0.0602
	(0.0185)	(0.0207)	(0.0285)	(0.0236)	(0.0323)	(0.0433)

Notes: Robust standard errors are in parentheses, clustered by lottery. ** p<0.01, * p<0.05.

Table 10: Estimates of the Impact of Enrolling in Any School to Which Applied on High School Outcomes

	(1) On Track in Grade 9	(2) Passed Exit Exam in Grade 10	(3) % Days Attended in Grade 12	(4) Grade 12 Citizenship Grade	(5) GPA in Grade 12	(6) Graduated on Time
Magnet	-0.0346	0.00312	-0.859	-0.0925	-0.0757	0.124
	(0.0532)	(0.0809)	(1.508)	(0.171)	(0.155)	(0.110)
VEEP	-0.0401	-0.0476	-0.0864	0.298	0.0946	0.0492
	(0.0907)	(0.0814)	(1.796)	(0.188)	(0.134)	(0.0849)
Choice	0.0929	0.0629	-1.874	-0.194	0.0200	-0.0265
	(0.133)	(0.0930)	(1.338)	(0.172)	(0.152)	(0.0782)
Pooled	-0.00149	0.0114	-1.068	-0.00335	0.00547	0.0863
	(0.0703)	(0.0657)	(1.156)	(0.139)	(0.113)	(0.0744)

Notes: Robust standard errors are in parentheses, clustered by lottery. ** p<0.01, * p<0.05.

Table 11: Estimates of the Impact of Enrolling in Any School to Which Applied on Postsecondary Enrollment Outcomes

	(1) # Years Enrolled in Postsecondary 1 Year After HS Graduation	(2) # Years Enrolled in 2-Year College 1 Year After HS Graduation	(3) # Years Enrolled in 4-Year College 1 Year After HS Graduation	(4) # Years Enrolled in Postsecondary 4 Years After HS Graduation	(5) # Years Enrolled in 2-Year College 4 Years After HS Graduation	(6) # Years Enrolled in 4-Year College 4 Years After HS Graduation
Magnet	-0.00271	-0.0535	-0.0212	-0.0815	0.121	-0.400
	(0.104)	(0.121)	(0.113)	(0.289)	(0.250)	(0.324)
VEEP	0.0634	0.150	-0.154*	0.0136	0.451	-0.270
	(0.0876)	(0.113)	(0.0778)	(0.262)	(0.319)	(0.291)
Choice	0.128	0.204*	-0.0937	0.519	0.632*	-0.120
	(0.0664)	(0.0992)	(0.104)	(0.339)	(0.293)	(0.302)
Pooled	0.0704	0.104	-0.107	0.148	0.483*	-0.378
	(0.0737)	(0.0881)	(0.0807)	(0.242)	(0.220)	(0.241)

Notes: Robust standard errors are in parentheses, clustered by lottery. ** p<0.01, * p<0.05.

Table 12: Estimates of the Impact of Enrolling in Any School to Which Applied on Postsecondary Graduation Outcomes

	(1) Associate or Other 2-Year College Cert. 4 Years After HS Graduation	(2) Associate or Other 2-Year College Cert. 5 Years After HS Graduation	(3) Associate or Other 2-Year College Cert. 6 Years After HS Graduation	(4) Bachelor's 4 Years After HS Graduation	(5) Bachelor's 5 Years After HS Graduation	(6) Bachelor's 6 Years After HS Graduation
Magnet	0.0939	0.0987	0.0952	-0.0133	-0.114	-0.350**
	(0.0506)	(0.0534)	(0.0776)	(0.0590)	(0.0903)	(0.132)
VEEP	0.0727**	0.112**	0.0563	0.0547	-0.0242	0.0348
	(0.0269)	(0.0375)	(0.0438)	(0.0372)	(0.0555)	(0.0762)
Choice	0.0570	0.0826	0.0752	-0.183**	-0.0307	0.0386
	(0.0706)	(0.0750)	(0.119)	(0.0629)	(0.106)	(0.116)
Pooled	0.101**	0.130**	0.0928	-0.0381	-0.0787	-0.112
	(0.0381)	(0.0421)	(0.0541)	(0.0439)	(0.0610)	(0.0814)

Notes: Robust standard errors are in parentheses, clustered by lottery. ** p<0.01, * p<0.05.

Table 13: Significance of Interactions Between Winning a Lottery and Differences in School Characteristics

Panel A: High School Outcomes

Outcome:	On Track in Grade 9	Passed Exit Exam in Grade 10	% Days Attended in Grade 12	Grade 12 Citizenship Grade	GPA in Grade 12	Graduated on Time
DIFFERENCES (APPLIED−LOCAL) IN:						
Reading, Z-score						
Math, Z-score[a]						
School value-added, Reading			++			
School value-added, Math[a]						
Mean Grade Point Average						
Proportion on Track in Grade 9						
Proportion Graduating HS on Time						
% Days Absent				−		
Mean Citizenship Grade						
Proportion Students White						
Proportion Students Black						
Proportion Students Asian						
Proportion Students Hispanic				−		
Proportion Parent Ed ≥ Bachelor's						

Notes: +, ++, − and −− indicate that the interaction between winning a lottery and the difference in the school characteristic listed was positive and significant at the 5% level, positive and significant at the 1% level, negative and significant at the 5% level, and negative and significant at the 1% level, respectively. Blank cells indicate the interaction was not significant.

a — Restricted to middle schools and grades 6–7

Table 13, Continued

Panel B: Postsecondary Enrollment Outcomes

Outcome:	# Years Enrolled in Postsecondary 1 Year After HS Graduation	# Years Enrolled in 2-Year College 1 Year After HS Graduation	# Years Enrolled in 4-Year College 1 Year After HS Graduation	# Years Enrolled in Postsecondary 4 Years After HS Graduation	# Years Enrolled in 2-Year College 4 Years After HS Graduation	# Years Enrolled in 4-Year College 4 Years After HS Graduation
DIFFERENCES (APPLIED−LOCAL) IN:						
Reading, Z-score				+		
Math, Z-score[a]			−			
School value-added, Reading				+		
School value-added, Math[a]						
Mean Grade Point Average						
Proportion on Track in Grade 9						
Proportion Graduating HS on Time						
% Days Absent						
Mean Citizenship Grade						
Proportion Students White					+	
Proportion Students Black						
Proportion Students Asian						
Proportion Students Hispanic						
Proportion Parent Ed $\geq$ Bachelor's						

a — Restricted to middle schools and grades 6–7

Table 13, Continued

Panel C: Postsecondary Graduation Outcomes

Outcome:	Associate or Other 2-Year College Cert. 4 Years After HS Graduation	Associate or Other 2-Year College Cert. 5 Years After HS Graduation	Associate or Other 2-Year College Cert. 6 Years After HS Graduation	Bachelor's 4 Years After HS Graduation	Bachelor's 5 Years After HS Graduation	Bachelor's 6 Years After HS Graduation
DIFFERENCES (APPLIED−LOCAL) IN:						
Reading, Z-score						
Math, Z-score[a]						
School value-added, Reading						
School value-added, Math[a]						
Mean Grade Point Average						
Proportion on Track in Grade 9						
Proportion Graduating HS on Time						
% Days Absent					−	
Mean Citizenship Grade						
Proportion Students White						
Proportion Students Black						
Proportion Students Asian					+	
Proportion Students Hispanic						
Proportion Parent Ed ≥ Bachelor's						

a — Restricted to middle schools and grades 6–7

Table 14: Significance of Interactions Between Winning a Lottery and Student Characteristics

Panel A: High School Outcomes

Outcome:	On Track in Grade 9	Passed Exit Exam in Grade 10	% Days Attended in Grade 12	Grade 12 Citizenship Grade	GPA in Grade 12	Graduated on Time
INTERACTION VARIABLE:						
Lagged Reading Z-score						
Lagged Math Z-Score						
Male						−
Grade to Which Applied				−−	++	++
Ever an English Learner						
Ever Received Special Education Services						
Race/Ethnicity			*			*
Parental Education Level						

Panel B: Postsecondary Enrollment Outcomes

Outcome:	# Years Enrolled in Postsecondary 1 Year After HS Graduation	# Years Enrolled in 2-Year College 1 Year After HS Graduation	# Years Enrolled in 4-Year College 1 Year After HS Graduation	# Years Enrolled in Postsecondary 4 Years After HS Graduation	# Years Enrolled in 2-Year College 4 Years After HS Graduation	# Years Enrolled in 4-Year College 4 Years After HS Graduation
INTERACTION VARIABLE:						
Lagged Reading Z-score					+	−−
Lagged Math Z-Score					+	
Male						
Grade to Which Applied	−		**	**		**
Ever an English Learner						
Ever Received Special Education Services					−	
Race/Ethnicity						
Parental Education Level						

Notes: +, ++, − and −− indicate that the interaction between winning a lottery and the difference in the school characteristic listed was positive and significant at the 5% level, positive and significant at the 1% level, negative and significant at the 5% level, and negative and significant at the 1% level, respectively. Blank cells indicate the interaction was not significant. For grade, race and parental education, significance is from an F-test for lack of joint significance of individual interactions. ** or * indicates that an F-test rejected the null with a p-value of < 0.01 or < 0.05 but the impact did not vary monotonically with the given variable, or the variable itself is not an ordered variable.

Table 14, Continued

Panel C: Postsecondary Graduation Outcomes

Outcome:	Associate or Other 2-Year College Cert. 4 Years After HS Graduation	Associate or Other 2-Year College Cert. 5 Years After HS Graduation	Associate or Other 2-Year College Cert. 6 Years After HS Graduation	Bachelor's 4 Years After HS Graduation	Bachelor's 5 Years After HS Graduation	Bachelor's 6 Years After HS Graduation
INTERACTION VARIABLE:						
Lagged Reading Z-score	+	+	+	−	−	
Lagged Math Z-Score			+			
Male						
Grade to Which Applied						**
Ever an English Learner	−					
Ever Received Special Education Services					+	+
Race/Ethnicity						
Parental Education Level		*				

Appendix

Table A1: The Relationship Between Winning a Lottery and Attrition

	Magnet	VEEP	Choice	Pooled
On Track in Grade 9	0.007	-0.015	0.000	-0.001
	(0.010)	(0.013)	(0.013)	(0.007)
Passed Exit Exam in Grade 10	0.002	-0.006	-0.013	-0.004
	(0.009)	(0.015)	(0.011)	(0.007)
% Days Attended in Grade 12	-0.010	-0.016	-0.004	-0.010
	(0.015)	(0.024)	(0.020)	(0.011)
Grade 12 Citizenship Grade	-0.001	-0.004	-0.016	-0.006
	(0.015)	(0.022)	(0.019)	(0.011)
GPA in Grade 12	-0.004	-0.008	-0.011	-0.007
	(0.016)	(0.021)	(0.019)	(0.011)
Graduated on Time	0.002	-0.015	0.007	-0.002
	(0.009)	(0.020)	(0.016)	(0.008)
# Years Postsecondary 1 Year after Graduation	-0.021	-0.025	0.005	-0.015
	(0.012)	(0.017)	(0.016)	(0.008)
# Years in 2-Year College 1 Year after Graduation	-0.021	-0.025	0.005	-0.015
	(0.012)	(0.017)	(0.016)	(0.008)
# Years in 4-Year College 1 Year after Graduation	-0.021	-0.025	0.005	-0.015
	(0.012)	(0.017)	(0.016)	(0.008)
# Years Postsecondary 4 Years after Graduation	-0.021	-0.025	0.005	-0.015
	(0.012)	(0.017)	(0.016)	(0.008)
# Years in 2-Year College 4 Years after Graduation	-0.021	-0.025	0.005	-0.015
	(0.012)	(0.017)	(0.016)	(0.008)
# Years in 4-Year College 4 Years after Graduation	-0.021	-0.025	0.005	-0.015
	(0.012)	(0.017)	(0.016)	(0.008)
Associate's or Other 2-Year College Certification	-0.029*	-0.021	-0.004	-0.020*
4 Years after Graduation	(0.013)	(0.017)	(0.016)	(0.009)
Associate's or Other 2-Year College Certification	-0.027*	-0.015	-0.012	-0.020*
5 Years after Graduation	(0.013)	(0.017)	(0.014)	(0.008)
Associate's or Other 2-Year College Certification	-0.023	-0.014	-0.007	-0.016*
6 Years after Graduation	(0.013)	(0.017)	(0.013)	(0.008)
Bachelor's Degree 4 Years after Graduation	-0.024	-0.019	-0.000	-0.016
	(0.014)	(0.017)	(0.014)	(0.009)
Bachelor's Degree 5 Years after Graduation	-0.022	-0.013	-0.007	-0.016
	(0.013)	(0.017)	(0.012)	(0.008)
Bachelor's Degree 6 Years after Graduation	-0.019	-0.012	-0.003	-0.013
	(0.012)	(0.017)	(0.012)	(0.008)

Notes: For each outcome of interest, an indicator for "missing" is used as the outcome and regressed on a dummy for winning the lottery plus lottery fixed effects. This table reports the coefficients on 'win lottery' from these regressions, along with their standard errors. Positive significant results indicate that lottery winners have greater attrition for the outcome of interest, while negative results indicate that lottery losers have greater attrition. Robust standard errors are in parentheses, clustered by lottery. ** $p<0.01$, * $p<0.05$.

Table A2: More Detailed Version of Intent to Treat Estimates, High School Outcomes

Magnet

	(1) On Track in Grade 9	(2) Passed Exit Exam in Grade 10	(3) % Days Attended in Grade 12	(4) Grade 12 Citizenship Grade	(5) GPA in Grade 12	(6) Graduated on Time
Won Lottery	-0.00758	0.000665	-0.174	-0.0198	-0.0160	0.0254
	(0.0123)	(0.0174)	(0.314)	(0.0372)	(0.0327)	(0.0228)
Observations	3,476	3,885	2,865	2,979	3,062	3,662
R-squared	0.160	0.298	0.026	0.089	0.126	0.058
Number of Clusters	78	90	96	98	99	99

VEEP

	(1) On Track in Grade 9	(2) Passed Exit Exam in Grade 10	(3) % Days Attended in Grade 12	(4) Grade 12 Citizenship Grade	(5) GPA in Grade 12	(6) Graduated on Time
Won Lottery	-0.0114	-0.0136	-0.0250	0.0839	0.0276	0.0136
	(0.0264)	(0.0235)	(0.526)	(0.0553)	(0.0400)	(0.0237)
Observations	2,092	2,296	1,502	1,538	1,608	2,002
R-squared	0.134	0.337	0.011	0.068	0.089	0.091
Number of Clusters	41	49	49	49	50	50

Notes: Robust standard errors are in parentheses, clustered by lottery. ** p<0.01, * p<0.05.

Table A2, Continued

Choice

	(1) On Track in Grade 9	(2) Passed Exit Exam in Grade 10	(3) % Days Attended in Grade 12	(4) Grade 12 Citizenship Grade	(5) GPA in Grade 12	(6) Graduated on Time
Won Lottery	0.0241	0.0148	-0.460	-0.0472	0.00494	-0.00602
	(0.0344)	(0.0224)	(0.318)	(0.0428)	(0.0379)	(0.0176)
Observations	1,654	1,888	1,411	1,471	1,509	1,744
R-squared	0.133	0.239	0.017	0.123	0.178	0.051
Number of Clusters	34	39	46	46	46	46

Pooled

	(1) On Track in Grade 9	(2) Passed Exit Exam in Grade 10	(3) % Days Attended in Grade 12	(4) Grade 12 Citizenship Grade	(5) GPA in Grade 12	(6) Graduated on Time
Won Lottery	-0.000268	0.00207	-0.201	-0.000635	0.00104	0.0151
	(0.0127)	(0.0120)	(0.220)	(0.0264)	(0.0215)	(0.0134)
Observations	7,222	8,069	5,778	5,988	6,179	7,408
R-squared	0.142	0.287	0.019	0.088	0.126	0.063
Number of Clusters	153	178	191	193	195	195

Notes: Robust standard errors are in parentheses, clustered by lottery. ** p<0.01, * p<0.05.

Table A3: More Detailed Version of Intent to Treat Estimates, Postsecondary Enrollment Outcomes

Magnet

	(1) # Years Enrolled in Postsecondary 1 Year After HS Graduation	(2) # Years Enrolled in 2-Year College 1 Year After HS Graduation	(3) # Years Enrolled in 4-Year College 1 Year After HS Graduation	(4) # Years Enrolled in Postsecondary 4 Years After HS Graduation	(5) # Years Enrolled in 2-Year College 4 Years After HS Graduation	(6) # Years Enrolled in 4-Year College 4 Years After HS Graduation
Won Lottery	-0.000545	-0.0108	-0.00427	-0.0164	0.0244	-0.0804
	(0.0212)	(0.0246)	(0.0228)	(0.0590)	(0.0506)	(0.0661)
Observations	2,978	2,978	2,978	2,978	2,978	2,978
R-squared	0.034	0.011	0.097	0.058	0.010	0.122
Number of Clusters	96	96	96	96	96	96

VEEP

	(1) # Years Enrolled in Postsecondary 1 Year After HS Graduation	(2) # Years Enrolled in 2-Year College 1 Year After HS Graduation	(3) # Years Enrolled in 4-Year College 1 Year After HS Graduation	(4) # Years Enrolled in Postsecondary 4 Years After HS Graduation	(5) # Years Enrolled in 2-Year College 4 Years After HS Graduation	(6) # Years Enrolled in 4-Year College 4 Years After HS Graduation
Won Lottery	0.0185	0.0437	-0.0449	0.00397	0.131	-0.0786
	(0.0253)	(0.0322)	(0.0250)	(0.0772)	(0.0952)	(0.0882)
Observations	1,516	1,516	1,516	1,516	1,516	1,516
R-squared	0.038	0.003	0.095	0.063	0.005	0.113
Number of Clusters	46	46	46	46	46	46

Notes: Robust standard errors are in parentheses, clustered by lottery. ** p<0.01, * p<0.05.

Table A3, Continued

Choice

	(1) # Years Enrolled in Postsecondary 1 Year After HS Graduation	(2) # Years Enrolled in 2-Year College 1 Year After HS Graduation	(3) # Years Enrolled in 4-Year College 1 Year After HS Graduation	(4) # Years Enrolled in Postsecondary 4 Years After HS Graduation	(5) # Years Enrolled in 2-Year College 4 Years After HS Graduation	(6) # Years Enrolled in 4-Year College 4 Years After HS Graduation
Won Lottery	0.0294	0.0471	-0.0216	0.120	0.145	-0.0276
	(0.0167)	(0.0253)	(0.0237)	(0.0829)	(0.0793)	(0.0708)
Observations	1,564	1,564	1,564	1,564	1,564	1,564
R-squared	0.029	0.063	0.150	0.060	0.060	0.176
Number of Clusters	44	44	44	44	44	44

Pooled

	(1) # Years Enrolled in Postsecondary 1 Year After HS Graduation	(2) # Years Enrolled in 2-Year College 1 Year After HS Graduation	(3) # Years Enrolled in 4-Year College 1 Year After HS Graduation	(4) # Years Enrolled in Postsecondary 4 Years After HS Graduation	(5) # Years Enrolled in 2-Year College 4 Years After HS Graduation	(6) # Years Enrolled in 4-Year College 4 Years After HS Graduation
Won Lottery	0.0122	0.0181	-0.0185	0.0256	0.0838*	-0.0656
	(0.0125)	(0.0155)	(0.0144)	(0.0416)	(0.0397)	(0.0438)
Observations	6,058	6,058	6,058	6,058	6,058	6,058
R-squared	0.033	0.014	0.111	0.058	0.012	0.135
Number of Clusters	186	186	186	186	186	186

Notes: Robust standard errors are in parentheses, clustered by lottery. ** p<0.01, * p<0.05.

Table A4: More Detailed Version of Intent to Treat Estimates, Postsecondary Graduation Outcomes

Magnet

	(1) Associate or Other 2-Year College Cert. 4 Years After HS Graduation	(2) Associate or Other 2-Year College Cert. 5 Years After HS Graduation	(3) Associate or Other 2-Year College Cert. 6 Years After HS Graduation	(4) Bachelor's 4 Years After HS Graduation	(5) Bachelor's 5 Years After HS Graduation	(6) Bachelor's 6 Years After HS Graduation
Won Lottery	0.0163*	0.0166*	0.0131	-0.00234	-0.0196	-0.0505*
	(0.00701)	(0.00716)	(0.0106)	(0.0107)	(0.0148)	(0.0213)
Observations	2,343	2,248	1,805	2,363	2,268	1,829
R-squared	0.014	0.017	0.011	0.064	0.101	0.114
Number of Clusters	89	80	69	89	80	74

VEEP

	(1) Associate or Other 2-Year College Cert. 4 Years After HS Graduation	(2) Associate or Other 2-Year College Cert. 5 Years After HS Graduation	(3) Associate or Other 2-Year College Cert. 6 Years After HS Graduation	(4) Bachelor's 4 Years After HS Graduation	(5) Bachelor's 5 Years After HS Graduation	(6) Bachelor's 6 Years After HS Graduation
Won Lottery	0.0226**	0.0338**	0.0170	0.0171	-0.00737	0.0106
	(0.00786)	(0.0120)	(0.0144)	(0.0105)	(0.0172)	(0.0234)
Observations	1,219	1,127	1,076	1,232	1,140	1,089
R-squared	0.008	0.016	0.014	0.049	0.080	0.089
Number of Clusters	44	44	41	44	44	41

Notes: Robust standard errors are in parentheses, clustered by lottery. ** p<0.01, * p<0.05.

Table A4, Continued

Choice

	(1) Associate or Other 2-Year College Cert. 4 Years After HS Graduation	(2) Associate or Other 2-Year College Cert. 5 Years After HS Graduation	(3) Associate or Other 2-Year College Cert. 6 Years After HS Graduation	(4) Bachelor's 4 Years After HS Graduation	(5) Bachelor's 5 Years After HS Graduation	(6) Bachelor's 6 Years After HS Graduation
Won Lottery	0.0138	0.0217	0.0183	-0.0437*	-0.00793	0.00905
	(0.0177)	(0.0199)	(0.0278)	(0.0196)	(0.0282)	(0.0273)
Observations	1,186	1,081	1,000	1,213	1,108	1,027
R-squared	0.004	0.007	0.004	0.131	0.141	0.152
Number of Clusters	41	40	38	41	40	38

Pooled

	(1) Associate or Other 2-Year College Cert. 4 Years After HS Graduation	(2) Associate or Other 2-Year College Cert. 5 Years After HS Graduation	(3) Associate or Other 2-Year College Cert. 6 Years After HS Graduation	(4) Bachelor's 4 Years After HS Graduation	(5) Bachelor's 5 Years After HS Graduation	(6) Bachelor's 6 Years After HS Graduation
Won Lottery	0.0170**	0.0219**	0.0157	-0.00657	-0.0135	-0.0193
	(0.00587	(0.00643)	(0.00879)	(0.00799)	(0.0105)	(0.0142)
Observations	4,748	4,456	3,881	4,808	4,516	3,945
R-squared	0.007	0.011	0.008	0.080	0.108	0.117
Number of Clusters	174	164	148	174	164	153

Notes: Robust standard errors are in parentheses, clustered by lottery. ** p<0.01, * p<0.05.

Table A5: Intent to Treat Estimates on Postsecondary Graduation Outcomes with 2-Year and 4-Year Degrees Combined

		(1) Bachelor's or Associate's or Other 2-Year College Cert. 4 Years After HS Graduation	(2) Bachelor's or Associate's or Other 2-Year College Cert. 5 Years After HS Graduation	(3) Bachelor's or Associate's or Other 2-Year College Cert. 6 Years After HS Graduation
Magnet	Win	0.0138	-0.00243	-0.0337
		(0.0142)	(0.0159)	(0.0239)
VEEP	Win	0.0370**	0.0218	0.0226
		(0.0110)	(0.0180)	(0.0265)
Choice	Win	-0.0288	0.00704	0.0180
		(0.0167)	(0.0247)	(0.0291)
Pooled	Win	0.00992	0.00601	-0.00546
		(0.00927)	(0.0107)	(0.0155)

Notes: Robust standard errors are in parentheses, clustered by lottery. ** p<0.01, * p<0.05.