Research Design Meets Market Design:
Using Centralized Assignment for Impact Evaluation[*]

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Abstract

A growing number of school districts use centralized assignment mechanisms to allocate school seats in a manner that reflects student preferences and school priorities. Many of these assignment schemes use lotteries to ration seats when schools are oversubscribed. The resulting random assignment opens the door to credible quasi-experimental research designs for the evaluation of school effectiveness. Yet the question of how best to separate the lottery-generated variation integral to such designs from non-random preferences and priorities remains open. This paper develops easily-implemented empirical strategies that fully exploit the random assignment embedded in widely-used mechanisms such as deferred acceptance. We use these new methods to evaluate charter schools in Denver, one of a growing number of districts that integrate charter and traditional public schools in a unified assignment system. The resulting estimates show large achievement gains from charter school attendance. Our approach generates substantial efficiency gains over *ad hoc* methods that fail to exploit the full richness of the lotteries generated by centralized assignment with random tie-breaking.

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1 Introduction

Many children in large urban school districts can now apply for seats at any public school in their district. The fact that some schools are more popular than others, and the need to distinguish between students who have the same priority at a given school, generates a matching problem. Introduced by Gale and Shapley (1962) and Shapley and Scarf (1974), matchmaking via market design allocates scarce resources, such as seats in public schools, in markets where prices cannot be called upon to perform this function. The market-design approach to school choice, pioneered by Abdulkadiroğlu and Sönmez (2003), is used in a long and growing list of public school districts in American, European, and Asian cities. Most of these cities match students to schools using a matching mechanism known as deferred acceptance (DA).

Two benefits of matchmaking schemes like DA are efficiency and fairness: the resulting assignments improve welfare and transparency relative to ad hoc alternatives, while lotteries ensure that students with the same preferences and priorities have the same chance of obtaining highly-sought-after seats. DA and related algorithms also have the virtue of narrowing the scope for strategic behavior that would otherwise give sophisticated families the opportunity to game an assignment system at the expense of less-sophisticated residents (Abdulkadiroğlu et al., 2006; Pathak and Sönmez, 2008). No less important is the fact that centralized assignment generates valuable data for empirical research on schools. In particular, when schools are oversubscribed, lottery-based rationing generates quasi-experimental variation in school assignment that can be used for credible evaluation of individual schools and of school models like charters and magnets.

Previous work exploiting random tie-breaking in DA and related algorithms for student assignment includes studies of schools in Charlotte-Mecklenburg (Hastings et al., 2009; Deming, 2011; Deming et al., 2014) and New York (Bloom and Underman, 2014; Abdulkadiroğlu et al., 2013). A closely related set of studies uses regression-discontinuity (RD)-style tie-breaking as a source of quasi-experimental variation in selective exam school assignment. These studies include evaluations of exam schools in the US and in Chile, Ghana, Kenya, Romania, and Trinidad.[1] Causal effects in these studies are identified by compelling sources of quasi-experimental variation, but the research designs deployed in this work fail to exploit the full power of random or RD-style tie-breaking embedded in centralized assignment schemes.

A stumbling block in the use of market-design for impact evaluation is the elaborate multi-stage nature of many market-design solutions. The most widely used assignment algorithms weave random or running variable tie-breaking into an elaborate tapestry of information on student preferences and school priorities that is far from random. In principle, all features of student preferences and school priorities can shape the probability of assignment to each school. It's only conditional on these features that assignments are independent of potential outcomes. In view of this difficulty, research exploiting centralized assignment has focused either on offers of seats at students' first choices alone, or relied on instrumental variables indicating whether students' lottery numbers fall below the highest number offered a seat at all schools that they've

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[1]Studies of exam school systems that combined centralized assignment with RD include Dobbie and Fryer (2014); Abdulkadiroğlu et al. (2014); Ajayi (2014); Lucas and Mbiti (2014); Pop-Eleches and Urquiola (2013); Jackson (2010); Bergman (2014); Hastings et al. (2013); Kirkeboen et al. (2015)

ranked (we call this a qualification instrument). Both first-choice and qualification instruments discard much of the variation induced by DA.

This paper explains how to recover the full range of quasi-experimental variation embedded in DA. Specifically, we show how DA maps information on preferences, priorities, and school capacities into a conditional probability of random assignment, often referred to as the propensity score. As in other stratified randomized research designs, conditioning on the scalar propensity score eliminates selection bias arising from the association between all conditioning variables and potential outcomes (Rosenbaum and Rubin, 1983). The payoff to propensity-score conditioning turns out to be substantial in our application: naive stratification using all covariate cells reduces empirical degrees of freedom markedly, eliminating many schools and students from consideration, while score-based stratification leaves our research sample largely intact. But the propensity score does more for us than reduce dimensionality. Because all applicants with score values strictly between zero and one contribute variation that can be used for evaluation, the propensity score identifies the maximal set of applicants for whom we have a randomized school-assignment experiment.

The propensity score generated by centralized assignment is a complicated function of many arguments and not easily computed in general. Our theoretical framework therefore revolves around an asymptotic approximation to the general propensity score for a DA assignment mechanism. This *DA propensity score* is a function of a few easily-computed sample statistics. Our analytical formula for the DA propensity score is derived from a large-market sequence that increases the number of students and school capacities in fixed proportion. This approach is validated by comparing empirical estimates using the large-market approximation with estimates based on a brute force simulation, that is, a propensity score generated by drawing lottery numbers many times and computing the resulting average assignment rates across draws.

Both the simulated (brute force) and DA (analytic) propensity scores work well as far as covariate balance goes, but the approximate formula is, of course, much more quickly computed, and highlights specific sources of randomness and confounding in DA-based assignment schemes. In other words, the DA propensity score reveals the nature of the stratified experimental design embedded in a particular match. We can learn, for example, the features of student preferences and school priorities that lead to seats at certain schools being allocated randomly while offer rates elsewhere are degenerate; these facts need not be revealed by simple comparisons of the demand for schools and school capacities. This information in turn suggests ways in which school priorities, capacities, and the instructions given to applicants might be modified so as to increase or supplement the research value of particular assignment schemes.

Our test bed for these tools is an empirical analysis of charter school effects in the Denver Public School (DPS) district, a new and interesting setting for charter school impact evaluation.[2]

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[2]Charter schools operate with considerably more independence than traditional public schools. They are free to structure their curriculum and school environment. Among other things, many charter schools fit more instructional hours into a year by running longer school days and providing instruction on weekends and during the summer. Because few charter schools are unionized, they hire and fire teachers and administrative staff without regard to the collectively bargained seniority and tenure provisions that constrain such decisions in many public schools. About half of Denver charters appear to implement versions of what we've called the *No Excuses* model of urban education. No Excuses charters run a long school day and year, emphasize discipline and comportment

Because DPS assigns seats at traditional and charter schools in a unified match, the population attending DPS charters is less positively selected than the population of charter applicants in other large urban districts (where extra effort is required to participate in decentralized charter lotteries). This descriptive fact makes DPS charter effects relevant for the ongoing debate over charter expansion. As far as we know, ours is the first charter evaluation to exploit an assignment scheme that allocates seats in both the charter and traditional public school sectors.

Our empirical evaluation strategy uses an indicator for DA-generated charter offers to instrument charter school attendance in a two-stage least squares (2SLS) setup. This 2SLS procedure eliminates bias from non-random variation in preferences and priorities by controlling for the offer propensity score with linear and saturated additive models. The step from propensity score-based identification to empirical implementation raises a number of issues that we address in a straightforward manner. The results of this empirical analysis show impressive achievement gains from charter attendance in Denver.

We also compare our propensity-score-based estimates with those generated by first-choice and qualification instruments such as have been employed in earlier school evaluations. Estimation strategies that fully exploit the random assignment embedded in DA yield substantial efficiency gains, while also allowing us to study charter attendance effects at schools for which naive empirical strategies generate little or no variation. Finally, we show how our identification strategy identifies causal effects at different types of schools by using DA-induced offers to jointly estimate the effects of attendance at charters and at DPS's innovation schools, a popular alternative to the charter model (for a descriptive evaluation of innovation schools, see Connors et al. 2013).

The next section uses simple market design examples to explain the problem at hand. Following this explanation, Section 3 uses the theory of market design to characterize and approximate the propensity score in large markets. Section 4 applies these results to estimate charter and innovation school attendance effects in Denver. Finally, Section 5 summarizes our theoretical and empirical findings and outlines an agenda for further work. A theoretical appendix derives propensity scores for the Boston (Immediate Acceptance) mechanism and for DA with multiple tie-breaking.

2 Understanding the DA Propensity Score

We begin by reviewing the basic DA setup for school choice, showing how DA generates probabilities of school assignment that depend on preferences, priorities, and capacities. A total of n students are to be assigned seats at schools of varying capacities.Students report their preferences by ranking schools on an application form or website, while schools rank students by placing them in priority groups. For example, a school may give the highest priority to students with already-enrolled siblings, second highest priority to those who live nearby, with the rest in a third priority group below these two. Each student is also assigned a single random number

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and traditional reading and math skills, and rely heavily on data and teacher feedback in strategies to improve instructions. For more on what distinguishes charters from traditional public schools and No Excuses pedagogy, see Abdulkadiroğlu et al. (2011) and Angrist et al. (2013).

that is used to break ties.

DA assigns students to schools like this:

> Each student applies to his most preferred school. Each school ranks all its applicants first by priority then by random number within priority groups and *tentatively* admits the highest-ranked applicants in this order up to its capacity. Other applicants are rejected.

> Each rejected student applies to his next most preferred school. Each school ranks these new applicants *together with applicants that it admitted tentatively in the previous round* first by priority and then by random number. From this pool, the school tentatively admits those it ranks highest up to capacity, rejecting the rest.

This algorithm terminates when there are no new applications (some students may remain unassigned). DA produces a stable assignment scheme in the following sense: any student who prefers another school to the one he has been assigned must be outranked at that school, either because everyone assigned there has higher priority, or because those who share the student's priority at that school have higher lottery numbers. DA is also strategy-proof, meaning that families do as well as possible by submitting a truthful preference list (for example, there is nothing to be gained by ranking under-subscribed schools highly just because they are likely to yield seats). See Roth and Sotomayor (1990) for a review of these and related theoretical results.

2.1 Propensity Score Pooling

The probability that DA assigns student i a seat at school s depends on many factors: the total number of students, school capacities, the distribution of student preferences, and student priorities at each school. We refer to a student's preferences and priorities as student *type*. For example, a student of one type might rank school b first, school a second, and have sibling priority at b.

Suppose we'd like to estimate the causal effect of attending a particular school, say a, relative to other schools that students who rank a might attend (our application focuses on the causal effect of attendance at groups of schools, specifically, charter schools, but the logic behind such comparisons is similar). DA treats students of the same type symmetrically in that everyone of a given type faces the same probability of assignment to each school. We can therefore eliminate selection bias in comparisons of those who are and aren't offered seats at a simply by conditioning on type, since all that remains to determine assignment is a random number, presumed here to be independent of potential outcomes. As a practical matter, however, we'd like to avoid full type conditioning, since this produces many small and even singleton or empty cells, reducing the sample available for impact analysis dramatically. The following example illustrates this point.

Example 1. Five students $\{1, 2, 3, 4, 5\}$ apply to three schools $\{a, b, c\}$, each with one seat. Student 5 has the highest priority at c and student 2 has the highest priority at b, otherwise the students have the same priority at all schools. We're interested in measuring the effect of an

offer at school a. Student preferences are

$$1 : a \succ b,$$
$$2 : a \succ b,$$
$$3 : a,$$
$$4 : c \succ a,$$
$$5 : c,$$

where $a \succ b$ means that a is preferred to b. Students 3 and 5 find only a single school acceptable.

Note that no two students here have the same preferences and priorities. Therefore, full-type stratification puts each student into a different stratum, a fact that rules out a research design based on full type conditioning. Yet, DA assigns students 1, 2, 3, and 4 to a each with probability 0.25: students 4 and 5 each apply to c and 5 gets it by virtue of priority of his priority there, leaving 1, 2, 3, and 4 all applying to a in the second round and no one advantaged there. Assignment at a can therefore be analyzed in this example in a single stratum of size 4. This stratification scheme is determined by the propensity score, the conditional probability of random assignment to a.Specifically, we can use a dummy indicating offers at a as an instrument for attendance at a in a sample that includes types 1-4: offers among these types are randomly assigned and therefore likely to be independent of potential outcomes (a point discussed in detail, below); we also expect offers of seats at a to boost enrollment there.

The precision of any IV estimate is determined by the precision of the reduced form, in this case the average causal effect of assignment at a. The asymptotic semiparametric efficiency bound for average causal effects is obtained only by full covariate conditioning, meaning, in our case, exhaustive stratification on type (Hahn, 1998). Example 1 highlights the fact that this result fails to hold in small samples or a finite population; more parisimonious conditioning is better. Moreover, the efficiency cost due to full covariate conditioning in finite samples exceeds that due to lost cells. Angrist and Hahn (2004) show that with many small cells, probabilities of assignment close to zero or one, and a modest R-squared for the regression of outcomes on covariates, Hahn (1998)'s large-sample result favoring full conditioning can be misleading even when no cells are lost. Econometric strategies that use propensity score conditioning are likely to enjoy finite sample efficiency gains over full covariate conditioning for a number of reasons that are relevant in practice.[3]

2.2 Further Pooling in Large Markets

Under DA, the propensity score for assignment to school a is determined by a student's failure to win a seat at schools he ranks more highly than a and by the odds he wins a seat at a in competition with those who have also ranked a and similarly failed to find seats at schools they've ranked more highly than a. This structure leads to a large-market approximation that generates pooling beyond that provided by the finite-market propensity score. We illustrate this point via a second simple example.

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[3]Similar arguments for propensity score conditioning appear in Rosenbaum (1987); Rubin and Thomas (1996); Heckman et al. (1998); Hirano et al. (2003).

Example 2. Four students $\{1, 2, 3, 4\}$ apply to three schools $\{a, b, c\}$, each with one seat. There are no school priorities and student preferences are

$$1 : c,$$
$$2 : c \succ b \succ a,$$
$$3 : b \succ a,$$
$$4 : a.$$

As in Example 1, each student is of a different type.

Let $p_a(i)$ for $i = 1, 2, 3, 4$ denote the probability that type i is assigned school a. With four students, $p_a(i)$ comes from $4! = 24$ possible lottery realizations (orders of the four students), all equally likely. Given this modest number of possibilities, $p_a(i)$ is easily calculated by enumeration:

- Not having ranked a, type 1 is never assigned there, so $p_a(1) = 0$.

- Type 2 is seated at a when schools he's ranked ahead of a, schools b and c, are filled by others, and when he also beats type 4 in competition for a seat at a. This occurs for the two realizations of the form $(s, t, 2, 4)$ for $s, t = 1, 3$. Therefore, $p_a(2) = 2/24 = 1/12$.

- Type 3 is seated at a when the schools he's ranked ahead of a–in this case, only b–are filled by others, while he also beats type 4 in competition for a seat at a. b can be filled by type 2 only when 2 loses to 1 in the lottery at c. Consequently, type 3 is seated at a only in sequences of the form $(1, 2, 3, 4)$, which occurs only once. Therefore, $p_a(3) = 1/24$.

- Finally, since type 4 gets the seat at a if and only if the seat does not go to type 2 or type 3, $p_a(4) = 21/24$.

In this example, the propensity score differs for each student. But in larger markets with the same distribution of types, the score is smoother. To see this, consider a large market that replicates the structure of this example n times, so that n students of each type apply to 3 schools, each with n seats.[4] With large n, enumeration of assignment possibilities is a chore. We can, however, simulate the propensity score by repeatedly drawing lottery numbers.

A plot of simulation probabilities of assignment against market size for Example 2, shown in Figure 1, reveals that as the market grows, the distinction between types 2 and 3 disappears. In particular, Figure 1 shows that for large enough n,

$$p_a(2) = p_a(3) = 1/12; \quad p_a(1) = 0; \quad p_a(4) = 10/12 = 5/6,$$

with the probability of assignment at a for types 2 and 3 converging quickly. This convergence is a consequence of a result established in the next section, which explains how, with many students

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[4]Many market-design analysts have found this sort of large-market approximation useful. Examples include Roth and Peranson (1999); Immorlica and Mahdian (2005); Abdulkadiroğlu et al. (2015); Kesten (2009); Kojima and Manea (2010); Kojima and Pathak (2009); Che and Kojima (2010); Budish (2011); Azevedo and Leshno (2014); Lee (2014); Ashlagi et al. (2015); Azevedo and Hatfield (2015); Kesten and Ünver (2015).

and seats, the probability that type 3 is seated at a is determined by failure to qualify at b, just as it is for type 2.

The large-market model leads us to a general characterization of the DA propensity score. This model also reveals why some schools and applicant types are subject to random assignment, while for others, assignment risk is degenerate. A signal feature of the large market characterization is the role played by lottery qualification cutoffs at schools ranked ahead of school a in determining probabilities of assignment at a. This is illustrated by Example 2, which shows that, in the large-market limit, among schools that an applicant prefers to a, we need only be concerned with what happens at the school at which it's easiest to qualify. In general, this *most informative disqualification* (MID) determines how distributions of lottery numbers for applicants of differing types are effectively truncated before entering the competition for seats at a, thereby changing offer rates at a.

3 Score Theory

3.1 Setup

A general school choice problem, which we refer to as an economy, is defined by a set of students, schools, school capacities, student preferences over schools, and student priorities at schools. Let I denote a set of students, indexed by i, and let $s = 1, ..., S$ index schools. We consider markets with a finite number of schools, but with either finite (n) or infinitely many students. The latter setting is referred to as a *continuum economy*. In a continuum economy, $I = [0, 1]$ and school capacities are defined as the fraction of the continuum that can be seated at each school.

Student i's preferences over schools constitute a partial ordering of schools, $\succ_i$, where $a \succ_i b$ means that i prefers school a to school b. Each student is also granted a priority at every school. Let $\rho_{is} \in \{1, ..., K, \infty\}$ denote student i's priority at school s, where $\rho_{is} < \rho_{js}$ means school s prioritizes i over j. For instance, $\rho_{is} = 1$ might encode the fact that student s has sibling priority at school s, while $\rho_{is} = 2$ encodes neighborhood priority, and $\rho_{is} = 3$ for everyone else. Siblings are therefore prioritized ahead of those from the neighborhood, and both are ahead of everybody else. We use $\rho_{is} = \infty$ to indicate that i is ineligible for school s. Many students share priorities at a given school, in which case $\rho_{is} = \rho_{js}$ for some $i \neq j$. Let $\boldsymbol{\rho}_i = (\rho_{i1}, ..., \rho_{iS})$ be a vector of student i's priorities for each school. *Student type* is denoted by $\theta_i = (\succ_i, \boldsymbol{\rho}_i)$. We say that a student of type θ has preferences $\succ_\theta$ and priorities ρ_θ. Θ denotes the set of all possible types.

An economy is also characterized in part by a non-negative capacity vector, $\mathbf{q}$, which is normalized by the total number of students or their measure when students are indexed continuously. In a finite economy, where the set I contains n students and each school s has k_s seats, capacity is defined by $q_s = \frac{k_s}{n}$. In a continuum economy, q_s is the proportion of the set I that can be seated at school s.

We consider school assignment mechanisms that use lotteries to distinguish between students with the same preferences and priorities. Student i's lottery number, r_i, is the realization of a uniformly distributed random variable on $[0, 1]$, independent and identically distributed for all students. In particular, lottery draws are independent of type. In what follows, we consider a centralized assignment system relying on a single lottery number for each student. Extension

to the less-common multiple tie-breaking case, in which a student may have different lottery numbers at different schools, is discussed in the theoretical appendix.

For any set of student types $\Theta_0 \subset \Theta$ and for any number $r_0 \in [0, 1]$, define the set of students in Θ_0 with lottery number less than r_0 to be

$$I(\Theta_0, r_0) = \{i \in I \mid \theta_i \in \Theta_0, r_i \leq r_0\}.$$

We use the shorthand notation $I_0 = I(\Theta_0, r_0)$ for sets of applicants defined by type and lottery number. Also, when $r_0 = 1$, so that I_0 includes all lottery numbers, we omit the second argument and write $I_0 = \{i \in I \mid \theta_i \in \Theta_0\}$ for various choices of Θ_0.

When discussing a continuum economy, we let $F(I_0)$ denote the fraction of students in I_0. By our independence and uniform distribution assumption for lottery numbers in the continuum, this is given by

$$F(I_0) = E[1\{\theta_i \in \Theta_0\}] \times r_0,$$

where the expectation is taken with respect to the distribution of types across students and $E[1\{\theta_i \in \Theta_0\}]$ is the proportion of types in set Θ_0. In a finite economy with n students, the corresponding fraction is computed as

$$F(I_0) = \frac{|I_0|}{n}.$$

Note that, unlike in a continuum economy, the value of $F(I_0)$ in a finite economy depends on particular lottery assignments. Either way, the student side of an economy is fully characterized by the distribution of types and lottery numbers, for which we use the shorthand notation, F. Note also that every finite economy has a continuum analog. This analog can be constructed by replicating the type distribution of the finite economy at increasing scale, while fixing the proportion of seats at school s to be q_s.

Defining DA

We define DA using the notation outlined above, nesting the finite-market and continuum cases. First, combine priority status and lottery realization into a single number for each student and school, which we call *student rank*:

$$\pi_{is} = \rho_{is} + r_i.$$

Since the difference between any two priorities is at least 1 and random numbers are between 0 and 1, student rank is lexicographic in priority and lottery numbers.

DA proceeds in a series of rounds. Denote the evolving vector of *admissions cutoffs* in round t by $\mathbf{c}^t = (c_1^t, ..., c_S^t)$. The *demand* for seats at school s conditional on $\mathbf{c}^t$ is defined as

$$Q_s(\mathbf{c}^t) = \{i \in I \mid \pi_{is} \leq c_s^t \text{ and } s \succ_i \tilde{s} \text{ for all } \tilde{s} \in S \text{ such that } \pi_{i\tilde{s}} \leq c_{\tilde{s}}^t\}.$$

In other words, school s is demanded by students with rank below the school-s cutoff, who prefer school s to any other school for which they are also below the relevant cutoff.

The largest possible value of an eligible student's rank is $K+1$, so we can start with $c_s^1 = K+1$ for all s. Cutoffs then evolve as follows:

$$c_s^{t+1} = \begin{cases} K+1 & \text{if } F(Q_s(\mathbf{c}^t)) < q_s, \\ \max\left\{x \in [0, K+1] \mid F(\{i \in Q_s(\mathbf{c}^t) \text{ such that } \pi_{is} \leq x\}) \leq q_s\right\} & \text{otherwise;} \end{cases}$$

where, because the argument for F can be written in the form $\{i \in I \mid \theta_i \in \Theta_0, r_i \leq r_0\}$, the expression is well-defined. This formalizes the idea that when the demand for seats at s falls below capacity at s, the cutoff is $K+1$. Otherwise, the cutoff at s is the largest value such that demand for seats at s is less than or equal to capacity at s.

The final admissions cutoffs determined by DA for each school s are given by

$$c_s = \lim_{t \to \infty} c_s^t.$$

The set of students that are assigned school s under DA is the demand for seats at the limiting cutoffs: $\{i \in Q_s(\mathbf{c})\}$ where $\mathbf{c} = (c_1, ..., c_S)$. Since $c_s \leq K+1$, an ineligible student is never assigned to school s.

We write the final DA cutoffs as a limiting outcome to accommodate the continuum economy; in finite markets, DA converges after finitely many rounds. Appendix A.1 shows that this description of DA is valid in the sense that: (a) the necessary limits exist for every economy, finite or continuous; (b) for every finite economy, the allocation upon convergence matches that produced by DA as usually described (for example, by Gale and Shapley (1962) and the many market design studies building on their work).

3.2 Characterizing the DA Propensity Score

Let $D_i(s)$ indicate whether student i is assigned school s. $D_i(s)$ depends on lottery realizations. For a market of any size, the *propensity score* $p_s(\theta)$ is

$$p_s(\theta) = \Pr[D_i(s) = 1 | \theta_i = \theta],$$

defined for students who rank s (we think of this as the group of applicants to s). We're interested in the structure of $p_s(\theta)$, specifically, the manner in which this probability (determined by re-randomization of lottery numbers for a fixed set of applicants and schools) depends on preferences and priorities.

A key component in our characterization of $p_s(\theta)$ is the notion of a *marginal priority* group at school s. The marginal priority group consists of applicants with priority status such that the seats assigned to anyone in this group are allocated by lottery if the school is over-subscribed. Formally, the marginal priority, ρ_s, is the integer part of the cutoff, c_s. Conditional on being rejected by all more preferred schools and applying for school s, a student is assigned s with certainty if his $\rho_{is} < \rho_s$, that is, if he clears marginal priority. Applicants with $\rho_{is} > \rho_s$ have no chance of finding a seat at s. Applicants for whom $\rho_{is} = \rho_s$ are marginal: these applicants are seated at s when their lottery numbers fall below a school-specific *lottery cutoff*. The lottery cutoff at school s, denoted τ_s, is the decimal part of the cutoff at s, that is, $\tau_s = c_s - \rho_s$.

These observations motivate a partition determined by marginal priorities at s. Let Θ_s denote the set of student types who rank s and partition Θ_s according to

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i) $\Theta_s^n = \{\theta \in \Theta_s \mid \rho_{\theta s} > \rho_s\}$, (*never seated*)

ii) $\Theta_s^a = \{\theta \in \Theta_s \mid \rho_{\theta s} < \rho_s\}$, (*always seated*)

iii) $\Theta_s^c = \{\theta \in \Theta_s \mid \rho_{\theta s} = \rho_s\}$ (*conditionally seated*)

The set Θ_s^n contains applicant types who have worse-than-marginal priority at s. No one in this group is assigned to s. Θ_s^a contains applicant types that clear marginal priority at s. Some of these applicants may end up seated at a school they prefer to s, but they're assigned s for sure if they fail to find a seat at any school they've ranked more highly. Finally, Θ_s^c is the subset of Θ_s that is marginal at s. These applicants are assigned s when they're not assigned a higher choice *and* have a lottery number that clears the lottery cutoff at s.

A second key component of our score formulation reflects the fact that failure to qualify at schools other than s may truncate the distribution of lottery numbers in the marginal priority group for s. To characterize the distribution of lottery numbers among those at risk of assignment at s, we first define the set of schools ranked above s for which type θ is marginal:

$$M_{\theta s} = \{\tilde{s} \in B_{\theta s} \mid \rho_{\tilde{s}} = \rho_{\theta \tilde{s}}\},$$

where

$$B_{\theta s} = \{s' \in S \mid s' \succ_\theta s\}$$

denotes the set of schools that type θ prefers to s. $M_{\theta s}$ is defined for every θ and may be empty.

An applicant's *most informative disqualification* (MID) at s is defined as a function of $M_{\theta s}$ and cutoffs at schools preferred to s according to

$$MID_{\theta s} \equiv \begin{cases} 0 & \text{if } \rho_{\theta \tilde{s}} > \rho_{\tilde{s}} \text{ for all } \tilde{s} \in B_{\theta s}, \\ \max\{\tau_{\tilde{s}} \mid \tilde{s} \in M_{\theta s}\} & \text{if } \rho_{\theta \tilde{s}} \geq \rho_{\tilde{s}} \text{ for all } \tilde{s} \in B_{\theta s} \text{ with at least one equality}, \\ 1 & \text{if } \rho_{\theta \tilde{s}} < \rho_{\tilde{s}} \text{ for some } \tilde{s} \in B_{\theta s}. \end{cases}$$

$MID_{\theta s}$ tells us how the lottery number distribution among applicants to s is truncated by qualification at schools these applicants prefer to s. $MID_{\theta s}$ is zero when type θ students have worse-than-marginal priority at all higher ranked schools: when no s applicants can be seated at a more preferred school, there's no lottery number truncation among those at risk of assignment to s. On the other hand, when at least one school in $B_{\theta s}$ is under-subscribed, no one of type θ competes for a seat at s. Truncation is therefore complete, and $MID_{\theta s} = 1$.

The definition of $MID_{\theta s}$ also reflects the fact that, among applicants for whom $M_{\theta s}$ is not empty, any student who fail to clear $\tau_{\tilde{s}}$ is surely disqualified at schools with lower cutoffs. For example, applicants who fail to qualify at a school with a cutoff of 0.5 fail to qualify at schools with cutoffs below 0.5. Therefore, to keep track of the truncation induced by disqualification at all schools an applicant prefers to s, we need to record only the most forgiving cutoff that an applicant fails to clear.

In finite markets, $MID_{\theta s}$ varies from one lottery draw to another, but in a continuum economy, $MID_{\theta s}$ is fixed. Consider the large-market analog of Example 2 in which n students of each of four types compete for the n seats at each of three schools. In this example, there's a single priority group, so everyone is marginal. For large n, we can think of realized lottery

numbers as being distributed according to a continuous uniform distribution over $[0, 1]$. Types 2 and 3 rank different schools ahead of a, i.e., $B_{3a} = M_{3a} = \{b\}$ while $B_{2a} = M_{2a} = \{b, c\}$. Nevertheless, because $\tau_c = 0.5 < 0.75 = \tau_b$, we have that $MID_{2a} = MID_{3a} = \tau_b = 0.75$. To see where these cutoffs come from, note first that among the $2n$ type 1 and type 2 students who rank c first in this large market, those with lottery numbers lower (better) than 0.5 are assigned to c since it has a capacity of n: $\tau_c = 0.5$. The remaining type 2 students (half of the original mass of type 2), all of whom have lottery numbers higher (worse) than 0.5, must compete with all type 3 students for seats at b. We therefore have $1.5n$ school-b hopefuls but only n seats at b. All type 3 students with lottery numbers below 0.5 get seated at b (the type 2 students all have lottery numbers above 0.5), but this doesn't fill b. The remaining seats are therefore split equally between type 2 and 3 students in the upper half of the lottery distribution, implying that the highest lottery number seated at b is $\tau_b = 0.75$.

Lottery cutoffs, marginal priority status, and $MID_{\theta s}$ are random, potentially varying from one lottery realization to another. Consequently, the propensity score for a finite economy is not easily computed in general, requiring the sort of enumeration used in the examples in the previous section, or a time-consuming Monte Carlo-style simulation using repeated lottery draws. We therefore turn to the continuum economy for a simple asymptotic approximation of the propensity score in real markets. Specifically, we use the marginal priority and MID concepts to define an easily-computed *DA propensity score* that is a deterministic function of applicant type. This result is given below:

Theorem 1. *Consider a continuum economy populated by applicants of type $\theta \in \Theta$, to be assigned to schools indexed by $s \in S$. For all s and θ in this economy, we have:*

$$\Pr[D_i(s) = 1 | \theta_i = \theta] = \tilde{p}_s(\theta) \equiv \begin{cases} 0 & \text{if } \theta \in \Theta_s^n, \\ (1 - MID_{\theta s}) & \text{if } \theta \in \Theta_s^a, \\ (1 - MID_{\theta s}) \times \max\left\{0, \dfrac{\tau_s - MID_{\theta s}}{1 - MID_{\theta s}}\right\} & \text{if } \theta \in \Theta_s^c. \end{cases} \quad (1)$$

where we also set $\tilde{p}_s(\theta) = 0$ when $MID_{\theta s} = 1$ and $\theta \in \Theta_s^c$.

The proof appears in Appendix A.2.

The case without priorities offers a revealing simplification of this result. Without priorities, DA simplifies to a random serial dictatorship (RSD), that is, a serial dictatorship with applicants ordered by lottery number (see, e.g., Abdulkadiroglu and Sonmez 1998, Svensson 1999, Pathak and Sethuraman 2010).[5] Theorem 1 therefore implies the following corollary, which gives the *RSD propensity score*:

Corollary 1. *Consider a continuum economy with no priorities populated by applicants of type $\theta \in \Theta$, to be assigned to schools indexed by $s \in S$. For all s and θ in this economy, we have:*

$$\Pr[D_i(s) = 1 | \theta_i = \theta] = \tilde{p}_s(\theta) \equiv (1 - MID_{\theta s}) \times \max\left\{0, \frac{\tau_s - MID_{\theta s}}{1 - MID_{\theta s}}\right\} = \max\{0, \tau_s - MID_{\theta s}\}. \quad (2)$$

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[5]Exam school seats are often assigned by a serial dictatorship based on admission test scores instead of random numbers (see, e.g., Abdulkadiroğlu et al. 2014, Dobbie and Fryer 2014). A generalization of RSD, multi-category serial dictatorship, is used for Turkish college admissions (Balinski and Sönmez (1999)).

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Without priorities, Θ_s^n and Θ_s^a are empty. The probability of assignment at s is therefore determined solely by draws from the truncated distribution of lottery numbers remaining after eliminating applicants seated at schools they've ranked more highly. Applicants' whose most informative disqualification exceeds the cutoff at school s cannot be seated at s because disqualification at a more preferred school implies disqualification at s.

In a match with priorities, the DA propensity score also accounts for the fact that random assignment at s occurs partly as a consequence of not being seated a school preferred to s. Applying these principles in the continuum allows us to describe the three cases as follows:

i) Type Θ_s^n applicants have a DA score of zero because these applicants have worse-than-marginal priority at s.

ii) The probability of assignment at s is $1 - MID_{\theta s}$ for applicants in Θ_s^a because these applicants clear marginal priority at s, but not at higher-ranked choices. Applicants who clear marginal priority at s are guaranteed a seat there if they don't do better. Not doing better means failing to clear $MID_{\theta s}$, the most forgiving cutoff to which they're exposed in the set of schools preferred to s. This event occurs with probability $1 - MID_{\theta s}$ by virtue of the uniformity of lottery numbers in the continuum.

iii) Applicants in Θ_s^c are marginal at s but fail to clear marginal priority at higher-ranked choices. For these applicants to be seated at s they must both fail to be seated at a higher-ranked choice and win the competition for seats at s. As for applicants in Θ_s^a, the proportion in Θ_s^c left for consideration at s is $1 - MID_{\theta s}$. Applicants in Θ_s^c are marginal at s, so their status at s is also determined by the lottery cutoff at s. If the cutoff at s, τ_s, falls below the truncation point, $MID_{\theta s}$, no one in this partition finds a seat at s. On the other hand, when τ_s exceeds $MID_{\theta s}$, seats are awarded by drawing from a continuous uniform distribution on $[MID_{\theta s}, 1]$. The assignment probability for those disqualified at more preferred schools for whom $\tau_s > MID_{\theta s}$ is therefore $(\tau_s - MID_{\theta s})/(1 - MID_{\theta s})$.

Applying Theorem 1 to the large-market version of Example 2 explains the convergence in type 2 and type 3 propensity scores seen in Figure 1. With no priorities, both types are in Θ_s^c. As we've seen, $MID_{2a} = MID_{3a} = \tau_b = 0.75$, that is, type 2 and 3 students seated at a must have lottery numbers above 0.75. It remains to compute the cutoff, τ_a. Types 2 and 3 compete only with type 4 at a, and are surely beaten out there by type 4s with lottery numbers below 0.75. The remaining 0.25 seats are shared equally between types 2, 3, and 4, going to the best lottery numbers in $[0.75, 1]$, without regard to type. The lottery cutoff at a, τ_a, is therefore $0.75 + 0.25/3 = 5/6$. Plugging these into equation (1) gives the DA score for types 2 and 3:

$$\tilde{p}_a(\theta) = (1 - MID_{\theta a}) \times \max\left\{0, \frac{\tau_a - MID_{\theta a}}{1 - MID_{\theta a}}\right\}$$
$$= (1 - 0.75) \times \max\left\{0, \frac{5/6 - 0.75}{1 - 0.75}\right\}$$
$$= \frac{1}{12}.$$

The score for type 4 is the remaining probability, $\frac{5}{6} = 1 - (2 \times \frac{1}{12})$.

The DA propensity score is a simple function of a small number of intermediate quantities, specifically, $MID_{\theta s}$, τ_s, and marginal priority status at s and elsewhere. In stylized examples, we can easily compute continuum values for these parameters. In real markets with elaborate preferences and priorities, it's natural to use sample analogs for score estimation. As we show below, the resulting estimated DA propensity score provides an asymptotic approximation to the propensity score for finite markets.

3.3 Asymptotic Properties of the DA Score

We're interested in the limiting behavior of score estimates based on Theorem 1. The asymptotic sequence for our large-market analysis works as follows: randomly sample n students and their lottery numbers from a continuum economy, described by type distribution F and school capacities, $\{q_s\}$. Call the distribution of types and lottery numbers in this sample F_n. Fix the proportion of seats at school s to be q_s and run DA with these students and schools. Compute $MID_{\theta s}$, τ_s, and partition Θ_s by observing cutoffs and assignments in this single realization, then plug these quantities into equation (1). This generates an estimated propensity score, $\hat{p}_{ns}(\theta)$, constructed by treating a size-n sample economy like its continuum analog. The actual propensity score for this finite economy, computed by repeatedly drawing lottery numbers for the sample of students described by F_n and the set of schools with proportional capacities $\{q_s\}$, is denoted $p_{ns}(\theta)$. We consider the gap between $\hat{p}_{ns}(\theta)$ and $p_{ns}(\theta)$ as n grows. The analysis here makes use of a regularity condition:

Assumption 1. *(First choice support) For any $s \in S$ and priority $\rho \in \{1, ..., K\}$ with $F(\{i \in I : \rho_{is} = \rho\}) > 0$, we have $F(\{i \in I : \rho_{is} = \rho, i \text{ ranks } s \text{ first}\}) > 0$.*

This says that in the continuum economy, every school is ranked first by at least some students in every priority group defined for that school.

In this setup, the propensity score estimated by applying Theorem 1 to data drawn from a single sample and lottery realization converges almost surely to the propensity score generated by repeatedly drawing lottery numbers. This result is presented as a theorem:

Theorem 2. *In the asymptotic sequence described by F_n with proportional school capacities fixed at $\{q_s\}$ and maintaining Assumption 1, the DA propensity score $\hat{p}_{ns}(\theta)$ is a consistent estimator of propensity score $p_{ns}(\theta)$ in the following sense: For all $\theta \in \Theta$ and $s \in S$,*

$$|\hat{p}_{ns}(\theta) - p_{ns}(\theta)| \xrightarrow{a.s.} 0.$$

Proof. The proof uses intermediate results, given as lemmas in Appendix A. The first lemma establishes that the vector of cutoffs computed for the sampled economy, $\hat{\mathbf{c}}_n$, converges to the vector of cutoffs in the continuum economy.[6] That is,

$$\hat{\mathbf{c}}_n \xrightarrow{a.s.} \mathbf{c},$$

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[6] Azevedo and Leshno (2014) show something similar for the cutoffs generated by a sequence of stable assignments. Our characterization of the limiting propensity score in Theorem 1 and Theorem 2 does not appear to have an analog in their framework.

where **c** denotes the continuum economy cutoffs (Lemma 1 in Appendix A). This result, together with the continuous mapping theorem, implies

$$\hat{p}_{ns}(\theta) \xrightarrow{a.s.} \tilde{p}_s(\theta).$$

In other words, the propensity score estimated by applying Theorem 1 to a sampled finite economy converges to the DA propensity score for the corresponding continuum economy.

The second step establishes that for all $\theta \in \Theta$ and $s \in S$,

$$p_{ns}(\theta) \xrightarrow{a.s.} p_s(\theta).$$

That is, the actual (re-randomization-based) propensity score in the sampled finite economy also converges to the propensity score in the continuum economy (Lemma 2 in Appendix A). Since $\tilde{p}_s(\theta) = p_s(\theta)$ by Theorem 1, this implies $p_{ns}(\theta) \xrightarrow{a.s.} \tilde{p}_s(\theta)$.

Combining these two results shows that for all $\theta \in \Theta$ and $s \in S$,

$$|\hat{p}_{ns}(\theta) - p_{ns}(\theta)| \xrightarrow{a.s.} |\tilde{p}_s(\theta) - \tilde{p}_s(\theta)| = 0.$$

completing the proof. Since both Θ and S are finite, this also implies uniform convergence, i.e., $\sup_{\theta \in \Theta, s \in S} |\hat{p}_{ns}(\theta) - p_{ns}(\theta)| \xrightarrow{a.s.} 0.$ □

Theorem 2 justifies our use of the formula in Theorem 1 to control for student type in empirical work looking at school attendance effects. Specifically, the theorem explains why, as in example 2, it may be enough to stratify on applicants' most informative disqualification and marginal priority status instead of all possible values of θ when estimating the causal effects of school attendance. Not surprisingly, however, a number of implementation details associated with this strategy remain to be determined. These gaps are filled in the empirical application below.

3.4   Empirical Strategy

We use DPS's conditionally randomized SchoolChoice first-round charter offers to construct an instrument for charter school enrollment. How should the resulting 2SLS estimates be interpreted? Our IV procedure identifies causal effects for applicants treated when DA produces a charter offer but not otherwise; in the local average treatment effects (LATE) framework of Imbens and Angrist (1994) and Angrist et al. (1996), these are charter-offer compliers. IV fails to reveal average causal effects for applicants who decline a first round DA charter offer and are assigned another type of school in round 2 (in the LATE framework, these are never-takers). Likewise, IV methods are not directly informative about the effects of charter enrollment on applicants not offered a charter seat in round 1, but who nevertheless find their way into a charter school in the second round (LATE always-takers).

To flesh out this interpretation and the assumptions it rests on, let C_i be a charter treatment indicator. Causal effects are defined by potential outcomes, indexed against C_i: we see Y_{1i} when

i is treated and Y_{0i} otherwise, though both are assumed to exist for all i. The observed outcome is therefore

$$Y_i = Y_{0i} + (Y_{1i} - Y_{0i})C_i.$$

Average causal effects on charter-offer compliers (LATEs) are described by conditioning on potential treatment status. Potential treatment status (charter enrollment status) is indexed against the DA offer instrument, denoted D_i. In particular, we see C_{1i} when D_i is switched on and C_{0i} otherwise (both of these are also assumed to exist for all i), so that the observed treatment is

$$C_i = C_{0i} + (C_{1i} - C_{0i})D_i.$$

Note that the treatment variable of interest here indicates attendance at *any* charter school, rather than at a specific school (e.g, "school a" in the notation of the previous section). Since the DA mechanism of interest to us produces a single offer, offers of a seat at particular schools are mutually exclusive. We can therefore construct D_i by summing individual charter offer dummies. Likewise, the propensity score for this variable, $p_D(\theta) \equiv E[D_i|\theta]$, is obtained by summing the scores for the individual charter schools in the match.

Conditional on $\theta_i = \theta$, and hence, conditional on $p_D(\theta)$, the offer variable, D_i, is independent of type and therefore likely to be independent of potential outcomes, Y_{1i} and Y_{0i}. More formally, we're asserting the following conditional independence and exclusion restrictions:

$$\{Y_{1i}(d), Y_{0i}(d), C_{1i}, C_{0i}\} \perp\!\!\!\perp D_i|\theta_i; d = 0, 1$$

$$Y_{ji}(1) = Y_{ji}(0) \equiv Y_{ji}; j = 0, 1,$$

where $Y_{ji}(d)$ is potential outcome for applicant i in sector j when the offer instrument, D_i, equals d. In the large market framework, independence of offers and potential outcomes is an immediate consequence of the fact that offers are determined solely by whether an applicant's lottery number clears the relevant constant cutoffs.

The case for the exclusion restriction is less immediate. Here, we might worry that counter-factual outcomes can be affected by lottery numbers even for applicants whose charter status is unchanged (that is, for always-takers and never-takers). Denver's second round allocates any remaining school seats through an *ad hoc* school-by-school application process, unrelated to an applicant's lottery number drawn in the first round. But lottery numbers can nevertheless affect the second round indirectly by changing opportunities. Consider, for example, a skittish charter applicant who chooses a popular non-charter option when $D_i = 0$ in Round 1. Fearing the long charter school day and having applied to charter schools only to satisfy his mother, this applicant also goes non-charter if his $D_i = 1$. But in this case, he must settle for another non-charter option, perhaps less in demand and of lower quality, in Round 2. This violates the exclusion restriction if the skittish charter applicant has $Y_{0i}(1) \neq Y_{0i}(0)$. We must therefore either assume away within-sector differences in potential outcomes, or introduce a finer-grained parameterization of school sector effects. The latter approach is explored in Section 4.6, below, which discusses results from a model with two treatment channels, charter attendance and attendance at one of Denver's many "innovation" schools, a leading charter alternative.

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In addition to the conditional independence and exclusion restrictions, we also assume that, conditional on the propensity score, charter offers cause charter enrollment for at least some students, and that offers can only make enrollment more likely, so that $C_{1i} \geq C_{0i}$ for all i. Given these assumptions, the conditional-on-score IV estimand is a conditional average causal affect for compliers, that is:

$$\frac{E[Y_i|D_i = 1, p_D(\theta_i) = x] - E[Y_i|D_i = 0, p_D(\theta_i) = x]}{E[C_i|D_i = 1, p_D(\theta_i) = x] - E[C_i|D_i = 0, p_D(\theta_i) = x]} = E[Y_{1i} - Y_{0i}|p_D(\theta_i) = x, C_{1i} > C_{0i}], \quad (3)$$

where $p_D(\theta_i)$ is the charter-offer propensity score associated with applicant i's type and x indexes values in the support of $p_D(\theta_i)$.

In view of the fact that cell-by-cell application of (3) generates a separate average causal effect for each score value, it's natural to consider parsimonious models that use data from all propensity-score cells to estimate a single average causal effect. We accomplish this by estimating a 2SLS specification that can be written

$$C_i = \sum_x \gamma(x)d_i(x) + \delta D_i + \nu_i, \quad (4)$$

$$Y_i = \sum_x \alpha(x)d_i(x) + \beta C_i + \epsilon_i, \quad (5)$$

where the $d_i(x)$'s are dummies indicating values of (estimates of) $p_D(\theta_i)$, indexed by x, and $\gamma(x)$ and $\alpha(x)$ are the associated "score effects" in the first and second stages. A formal interpretation of the resulting 2SLS estimates comes from Abadie (2003): With saturated control, the additive model fits the underlying conditional expectation function, $E[D_i|p_D(\theta_i) = x]$, nonparametrically. The 2SLS estimand defined by equations (4) and (5) therefore provides the best (in a minimum mean-squared error sense) additive approximation to the unrestricted local average causal response function associated with this IV setup.[7]

4 School Effectiveness in Denver

Since the 2011 school year, DPS has used DA to assign students to most schools in the district, a process known as SchoolChoice. Denver school assignment involves two rounds, but only the first round uses DA. Our analysis therefore focuses on the initial round.

In the first round of SchoolChoice, parents can rank up to five schools of any type, including traditional public schools, magnet schools, innovation schools, and most charters, in addition to a neighborhood school which is automatically ranked for them. Schools ration seats using a mix of priorities and a single tie breaker used by all schools. Priorities vary across schools and typically involve siblings and neighborhoods. Seats may be reserved for a certain number

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[7]This conclusion is implied by Proposition 5.1 in Abadie (2003). The unrestricted average causal response function is $E[Y_i|X_i, C_i, C_{1i} \geq C_{0i}]$. This conditional mean function describes a causal relationship because treatment is randomly assigned for compliers. In other words,

$$E[Y_i|X_i, C_i, C_{1i} \geq C_{0i}] = E[Y_{Ci}|X_i, C_{1i} \geq C_{0i}].$$

2SLS provides a best additive approximation to this.

of subsidized-lunch students and for children of school staff. Reserved seats are allocated by splitting schools and assigning the highest priority status to students in the reserved group at one of the sub-schools created by a split.Match participants can only qualify for seats in a single grade.

The DPS match distinguishes between programs, known as "buckets", within some schools. Buckets have distinct priorities and capacities. DPS converts preferences over schools into preferences over buckets, splitting off separate sub-schools for each. The upshot for our purposes is that DPS's version of DA assigns seats at sub-schools determined by seat reservation policies and buckets rather than schools, while the relevant propensity score captures the probability of offers at sub-schools. The discussion that follows refers to propensity scores for schools, with the understanding that the fundamental unit of assignment is a bucket, from which assignment rates to schools are constructed.[8]

4.1 Computing the DA Propensity Score

The score estimates used as controls in equations (4) and (5) were constructed three ways. The first is a benchmark: we ran DA for one million lottery draws and recorded the proportion of draws in which applicants of a given type in our fixed DPS sample were seated at each school.[9] By a conventional law of large numbers, this simulated score converges to the actual finite-market score as the number of draws increases. In practice, of course, the number of replications is far smaller than the number of possible lottery draws, so the simulated score takes on more values than we'd expect to see for the actual score. For applicants with a simulated score strictly between zero and one, the simulated score takes on more than 1,100 distinct values (with fewer than 1,300 types in this sample). Because many simulated score values are exceedingly close to one another (or to 0 or 1) our estimators that control for the simulated score use values that have been rounded.

We're particularly interested in taking advantage of the DA score defined in Theorem 1. This theoretical result is used for propensity score estimation in two ways. The first, which we label a "formula" calculation, applies equation (1) directly to the DPS data. Specifically, for each applicant type, school, and entry grade, we identified marginal priorities, and applicants were allocated by priority status to either Θ_s^n, Θ_s^a, or Θ_s^c. The DA score, $\tilde{p}_s(\theta)$ is then estimated by computing the sample analog of $MID_{\theta s}$ and τ_s in the DPS assignment data and plugging these into equation (1).

The bulk of our empirical work uses a second application of Theorem 1, which also starts with marginal priorities, MIDs, and cutoffs in the DPS data. This score estimate, however, is given by the empirical offer rate in cells defined by these variables. This score estimate, which we refer to as a "frequency" calculation, is closer to an estimated score of the sort discussed by

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[8]DPS also modifies the DA mechanism described in Section 2 by recoding the lottery numbers of all siblings applying to the same school to be the best random number held by any of them. This modification (known as "family link") changes the allocation of only about 0.6% of students from that generated by standard DA. Our analysis incorporates family link by defining distinct types for linked students.

[9]Calsamiglia et al. (2014) and Agarwal and Somaini (2015) simulate the Boston mechanism as part of an effort to estimate preferences in a structural model of latent preferences over schools.

Abadie and Imbens (2012) than is the formula score, which ignores realized assignment rates. The large-sample distribution theory in Abadie and Imbens (2012) suggests that conditioning on an estimated score based on realized assignment rates may increase the efficiency of score-based estimates of average treatment effects.

Propensity scores for school offers tell us the number of applicants subject to random assignment at each DPS charter school.[10] These counts, reported in columns 3-5 of Table 1 for the three different score estimators, range from none to over 300. The proportion of applicants subject to random assignment varies markedly from school to school. This can be seen by comparing the count of applicants subject to random assignment with the total applicant count in column 1. The randomized applicant count calculated using frequency and formula score estimates are close, but some differences emerge when a simulated score is used.[11]

Column 5 of Table 1 also establishes the fact that at least some applicants were subject to random assignment at every charter except for the Denver Language School, which offered no seats. In other words, every school besides the Denver Language School had applicants with a simulated propensity score strictly in the unit interval. Three schools for which the simulated score shows ery few randomized applicants (Pioneer, SOAR Oakland, Wyatt) have an empirical offer rate of zero, so the frequency version of the DA propensity score is zero for these schools (applicant counts based on intervals determined by DA frequency and formula scores appear in columns 3 and 4).

DA produces random assignment of seats for students ranking charters *first* for a much smaller set of schools. This can be seen in the last column of Table 1, which reports the number of applicants with a simulated score strictly between zero and one, who also ranked each school first. The reduced scope of first-choice randomization is important for our comparison of strategies using the DA propensity score with previously-employed IV strategies using first-choice instruments. First-choice instruments applied to the DPS charter sector necessarily ignore many schools. Note also that while some schools had only a handful of applicants subject to random assignment, over 1400 students were randomized in the charter sector as a whole.

The number of applicants randomized at particular schools can be understood further using Theorem 1. Why did STRIVE Prep - GVR have 116 applicants randomized, even though Table 1 shows that no applicant with non-degenerate offer risk ranked this school first? Random as-

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[10]The data analyzed here come from files containing the information used for first-round assignment of students applying in the 2011-12 school year for seats the following year (this information includes preference lists, priorities, random numbers, assignment status, and school capacities). School-level scores were constructed by summing scores for all component sub-schools used to implement seat reservation policies and to define buckets. Our empirical work also uses files with information on October enrollment and standardized scores from the Colorado School Assessment Program (CSAP) and the Transitional Colorado Assessment Program (TCAP) tests, given annually in grades 3-10. A data appendix describes these files and the extract we've created from them. For our purposes, "Charter schools" are schools identified as "charter" in DPS *2012-2013 SchoolChoice Enrollment Guide* brochures and not identified as "intensive pathways" schools, which serve students who are much older than typical for their grade.

[11]The gap here is probably due to our treatment of family link. The Blair charter school, where the simulated score randomization count is farthest from the corresponding DA score counts, has more applicants with family link than any other school. Unlike our DA score calculation, which ignores family link, the simulated score accommodates family link by assigning a unique type to every student affected by a link.

signment at GVR is a consequence of the many GVR applicants randomized by admissions offers at schools they'd ranked more highly. This and related determinants of offer risk are detailed in Table 2, which explores the anatomy of the DA propensity score for 6th grade applicants to four middle schools in the STRIVE network. In particular, we see (in column 8 of the table) that all randomized GVR applicants were randomized by virtue of having $MID_{\theta s}$ inside the unit interval, with no one randomized at GVR's own cutoff (column 7 counts applicants randomized at each school's cutoff).

In contrast with STRIVE's GVR school, few applicants were randomized at STRIVE's Highland, Lake, and Montbello campuses. This is a consequence of the fact that most Highland, Lake, and Montbello applicants were likely to clear marginal priority at these schools (having $\rho_{\theta s} < \rho_s$), while having values of $MID_{\theta s}$ mostly equal to zero or 1, eliminating random assignment at schools ranked more highly. Interestingly, the Federal and Westwood campuses are the only STRIVE schools to see applicants randomized around the cutoff in the school's own marginal priority group. We could therefore learn more about the impact of attendance at Federal and Westwood by changing the cutoff there (e.g., by changing capacity), whereas such a change would be of little consequence for evaluations of the other schools.

Table 2 also documents the weak connection between applicant randomization counts and a naive definition of over-subscription based on school capacity. In particular, columns 2 and 3 reveal that four out of six schools described in the table ultimately made fewer offers than they had seats available (far fewer in the case of Montbello). Even so, assignment at these schools was far from certain: they contribute to our score-conditioned charter school impact analysis.

A broad summary of DPS random assignment appears in Figure 2. Panel (a) captures the information in columns 3 and 6 of Table 1 by plotting the number of first-choice applicants subject to randomization as black dots, with the total randomized at each school plotted as an arrow pointing up from these dots (schools are indexed on the x-axis by their capacities). This representation highlights the dramatic gains in the number of schools and the precision with which they can be studied as a payoff to our full-information approach to the DA research design. These benefits are not limited to the charter sector, a fact documented in Panel (b) of the figure, which plots the same comparisons for non-charter schools in the DPS match.

4.2 DPS Data and Descriptive Statistics

The DPS population enrolled in grades 3-9 in the Fall of 2011 is roughly 60% Hispanic, a fact reported in Table 3, along with other descriptive statistics. We focus on grades 3-9 in 2011 because outcome scores come from TCAP tests taken in grades 4-10 in the spring of the 2012-13 school year.[12] The high proportion Hispanic makes DPS an especially interesting and unusual urban district. Not surprisingly in view of this, almost 30 percent of DPS students have limited English proficiency. Consistent with the high poverty rates seen in many urban districts, three quarters of DPS students are poor enough to qualify for a subsidized lunch. Roughly 20% of the DPS students in our data are identified as gifted, a designation that qualifies them for differentiated instruction and other programs.

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[12]Grade 3 is omitted from the outcome sample because 3rd graders have no baseline test.

Nearly 11,000 of the roughly 40,000 students enrolled in grades 3-9 in Fall 2011 sought to change their school for the following year by participating in the assignment, which occurs in the spring. The sample participating in the assignment, described in column 2 of Table 3, contains fewer charter school students than appear in the total DPS population, but is otherwise demographically similar. It's also worth noting that our impact analysis is limited to students enrolled in DPS in the baseline (pre-assignment) year. The sample described in column 2 is therefore a subset of that described in column 1. The 2012 school assignment, which also determines the propensity score, includes the column 2 sample plus new entrants.

Column 3 of Table 3 shows that of the nearly 11,000 DPS-at-baseline students included in the assignment, almost 5,000 ranked at least one charter school. We refer to these students as charter applicants; the estimated charter attendance effects that follow are for subsets of this applicant group. DPS charter applicants have baseline achievement levels and demographic characteristics broadly similar to those seen district-wide. The most noteworthy feature of the charter applicant sample is a reduced proportion white, from about 19% in the centralized assignment to a little over 12% among charter applicants. It's also worth noting that charter applicants have baseline test scores close to the DPS average. This contrasts with the modest positive selection of charter applicants seen in Boston (reported in Abdulkadiroğlu et al. 2011).

A little over 1,400 charter applicants have a frequency estimate of the probability of charter assignment between zero and one; the count of applicants subject to random assignment rises to about 1,500 when the score is estimated by simulation. Charter applicants subject to random assignment are described in columns 4 and 6 of Table 3. Although only about 30% of charter applicants were randomly assigned a charter seat, these students look much like the full charter applicant pool. The main difference is a higher proportion of applicants of randomized applicants originating at a charter school (that is, already enrolled at a charter at the time they applied for seats elsewhere). Columns 5 and 7, which report statistics for the subset of the randomized group that enrolls in a charter school, show slightly higher baseline scores among charter students.

4.3   Score-Based Balance

Conditional on the propensity score, applicants offered a charter seat should look much like those not offered a seat. Moreover, because offers are randomly assigned conditional on the score, we expect to see conditional balance in all applicant characteristics and not just for the variables that define an applicant's type. We assess the balancing properties of the DA propensity score using simulated expectations. Specifically, drawing lottery numbers 400 times, we ran DA and computed the DA propensity score each time, and then computed average covariate differences by offer status. The balance analysis begins with uncontrolled differences in means, followed by regression-adjusted differences that put applicant characteristics on the left-hand side of regression models like equation (4).

Uncontrolled comparisons by offer status, reported in columns 1 and 2 of Table 4, show large differences in average student characteristics, especially for variables related to preferences. For instance, across 400 lottery draws, those not offered a charter seat ranked an average of 1.4 charters, but this figure increases by almost half a school for applicants who were offered a charter seat. Likewise, while fewer than 30% of those not offered a charter seat had ranked a

charter school first, the probability applicants ranked a charter first increases to over 0.9 (that is, 0.29+0.62) for those offered a charter seat. Column 2 also reveals important demographic differences by offer status; Hispanic applicants, for example, are substantially over-represented among those offered a charter seat.[13]

Conditioning on frequency estimates of the DA propensity score reduces differences by offer status markedly. This can be seen in columns 3-5 of Table 4. The first set of conditional results, which come from models adding the propensity score as a linear control, show virtually no difference by offer status in the odds a charter is ranked first or that an applicant is Hispanic. Offer gaps in other application and demographic variables are also much reduced in this specification. Columns 4 and 5 of the table show that non-parametric control for the DA propensity score (implemented by dummying all score values in the unit interval; an average of 39 across simulations when rounded to nearest hundredth and an average 47 without rounding) reduces offer gaps even further. This confirms that a single DPS applicant cohort is large enough for the DA propensity score to eliminate selection bias. It's also important to note that the analysis to follow shows the imbalance left after conditioning on the DA propensity score matter little for the 2SLS estimate we're ultimately after.

Columns 6-8 of Table 4, which report estimated offer gaps conditional on a simulated propensity score, show that the simulated score does a better job of balancing treatment and control groups than does the DA score. Differences by offer status conditional on the simulated score, whether estimated linearly or with nonparametric controls, appear mostly in the third decimal place. This reflects the fact that simulation recovers the actual finite-market propensity score (up to simulation error), while the DA propensity score is an asymptotic approximation that should be expected to provide perfect treatment-control balance only in the limit. It's worth noting, however, that the simulated score starts with 1,148 unique values. As a practical matter, the simulated score must be smoothed to accommodate non-parametric control. Rounding to the nearest hundredth leaves us with 51 points of support, close to the number of support points seen for the DA score. Rounding to the nearest ten-thousandth leaves 120 points of support. Finer rounding produces noticeably better balance for the number-of-schools-ranked variable.

Because the balancing properties of the DA propensity score are central to our methodological agenda, we explore this further in Table 5. This table provides a computational proof of Theorem 2 by reporting offer gaps of the sort shown in Table 4 for scaled-up versions of the DPS economy. As a reference point, the first two columns of Table 5 repeat the actual-market offer gaps estimated with no controls and the gaps estimated with saturated (nonparametric) controls for the DA propensity score (repeated from columns 2 and 5 of Table 4). Column 3 shows that doubling the number of applicants and seats at each school in the DPS market pushes the gaps down sharply (conditional on the DA propensity score). Market sizes of $4n$ and $8n$ make most of these small gaps even smaller. In fact, as with the estimates that condition on the simulated score in Table 4, most of the gaps here are zero out to the third decimal place.

Our exploration of score-based balance is rounded out with the results from a traditional balance analysis such as would be seen in analyses of a randomized trial. Specifically, Table 6

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[13]Table 4 omits standard errors because the only source of uncertainty here is the modest simulation error arising from the fact that we've drawn lottery numbers only 400 times.

documents balance for the DPS match by reporting the usual t and F statistics for offer gaps in covariate means. Again, we look at balance conditional on propensity scores for applicants with scores strictly between 0 and 1. As can be seen in Table 6a, application covariates are well-balanced by non-parametric control for either DA or simulated score estimates (linear control for the DA propensity score leaves a significant gap in the number of charter schools ranked).[14]

Table 6a also demonstrates that full control for type leaves us with a much smaller sample than does control for the propensity score: models with full type control are run on a sample of size 301, a sample size reported in the last column of the table. Likewise, the fact that saturated control for the simulated score requires some smoothing can be see in the second last column showing the reduced sample available for estimation of models that control fully for a simulated score rounded to the nearest ten-thousandth.

Not surprisingly, a few significant imbalances emerge in balance tests for the longer list of baseline covariates, reported in Table 6b. Here, the simulated score seems to balance characteristics somewhat more completely than does the DA score, but the F-statistics (reported at the bottom of the table) that jointly test balance of all baseline covariates fail to reject the null hypothesis of conditional balance for any specification reported.

Baseline score gaps as large as $-.1\sigma$ appear in some of the comparisons at the bottom of the table. The fact that these gaps are not mirrored in the comparisons in Table 4 tells suggests the differences in Table 6 are due to chance. Still, we can mitigate the effect of chance differences on 2SLS estimates of charter effects by adding baseline score controls (and other covariates) to our empirical models. The inclusion of these additional controls also has the salutary effect of making the 2SLS estimates of interest considerably more precise (baseline score controls are responsible for most of the resulting precision gain).

Modes of Inference

An important question in this context concerns the appropriate mode of inference when interpreting statistical results like that reported in tables like 6a and 6b. Econometric inference typically tries to quantify the uncertainty due to *random sampling*. Here, we might imagine that the 2012 DPS applicants we happen to be studying constitute a random sample from some larger population or stochastic process. At the same time, its clear that the uncertainty in our empirical work can also be seen as a consequence of *random assignment*: we see only a single lottery draw for each applicant, one of many possibilities. The population of 2012 applicants, on the other hand, is fixed, and therefore not a source of uncertainty.

In an effort to determine whether the distinction between sampling inference and randomization inference matters for our purposes, we computed randomization p-values by repeatedly drawing lottery numbers and calculating offer gaps in covariates conditional on the simulated propensity score. Conditioning on the simulated score produces near-perfect balance in Table 4 so this produces an appropriate null distribution. Randomization p-values are given by quantiles of the t-statistics in the distribution resulting from these repeated draws.

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[14]Table 6 reports the results controlling for frequency estimates of the DA propensity score and the simulated propensity score. Balance results using formula estimates of the score appear in Appendix Table B3.

The p-values associated with the t-statistics for covariate balance computed from the realized DPS data turn out to be close to the randomization p-values (for the number of charter schools ranked, for example, the conventional p-value for balance is 0.885 while the corresponding sampling p-value is 0.850). This is consistent with the the theorem from mathematical statistics which says that randomization and sampling p-values for differences in means are asymptotically equivalent (see Lehmann and Romano 2005 chapter 15).

Apart from a small simulation error, the simulated score can be seen as a "known" or population score. Our empirical strategy conditions on formula and frequency estimates of the propensity score as well as the known (simulated) score. As noted by Hirano et al. (2003) and Abadie and Imbens (2012), conditioning on an estimated score may affect sampling distributions. We therefore checked conventional large-sample p-values against randomization p-values for the reduced-form charter offer effects associated with the various sorts of 2SLS estimates discussed in the next section. Conventional asymptotic sampling formulas generate p-values close to a randomization-inference benchmark, regardless of how the score behind these estimates was constructed. In view of these findings, we rely on the usual asymptotic (robust) standard errors and test statistics for inference about treatment effects.[15]

4.4   Effects of Charter Enrollment

A DA-generated charter offer boosts charter middle school attendance rates by about 0.4. These first-stage estimates, computed by estimating equation (4), are reported in the first row of Table 7. First stage estimates of around 0.68 computed without score controls, shown in column 4 of the table, are clearly biased upwards. These estimates and those that follow are from models that also control for baseline test scores and a few other covariates described in a footnote. The extra controls are not necessary for consistent causal inference but their inclusion increases precision (Estimates without covariates appear in the appendix).[16]

2SLS estimates of charter attendance effects on test scores, reported below the first-stage estimates in Table 7, show remarkably large gains in math, with smaller effects on reading. The math gains reported here are similar to those found for charter students in Boston (see, for example, Abdulkadiroğlu et al. 2011). Previous lottery-based studies of charter schools likewise report substantially larger gains in math than in reading. Here, we also see large and statistically significant gains in writing scores.[17]

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[15]Appendix table B2 reports conditional-on-score estimates of attrition differentials by offer status. Here, we see marginally significant gaps on the order of 4-5 points when estimated conditional on the DA propensity score. Attrition differentials fall to a statistically insignificant 3 points when estimated conditional on a simulated score. The estimated charter attendance effects discussed below are similar when computed using either type of score control, so it seems unlikely that differential attrition is a source of bias in our 2SLS estimates.

[16]Results here are for scores in grades 4-10. The pattern of results in an analysis that separates high schools from middle and elementary schools is similar. The sample used for IV estimation is limited to charter applicants with the relevant propensity score in the unit interval, for which score cells have offer variation in the data at hand (these restrictions amount to the same thing for the frequency score). The OLS estimation sample includes charter applicants, ignoring score- and cell-variation restrictions. Estimates in this and the following tables also control for grade tested, gender, origin school charter status, race, gifted status, bilingual status, subsidized price lunch eligibility, special education, limited English proficient status, and test scores at baseline.

[17]Standard errors for the 2SLS estimates reported here ignore the fact that the DA propensity score is estimated.

Importantly for our methodological agenda, charter impact estimates reported in Table 7 are largely invariant to whether the propensity score is estimated by simulation or by a frequency or formula calculation that uses Theorem 1. Compare, for example, math impact estimates of 0.496, 0.524, and 0.543 using frequency-, formula-, and simulation-based score controls, all estimated with similar precision. This alignment validates the use of Theorem 1 to control for applicant type.

Estimates that omit propensity score controls highlight the risk of selection bias in a naive 2SLS empirical strategy. This is documented in column 4 of Table 7, which shows that 2SLS estimates of math and writing effects constructed using DA offer instruments while omitting propensity score controls are too small by about half. A corresponding set of OLS estimates without propensity score controls, reported in column 5 of the table, also comes in too small. The results in column 6 show that adding score controls to the OLS model pulls the estimates up a little, but a substantial gap between between these and the corresponding set of 2SLS estimates remains.

Finally, an appendix table (B5) compares the 2SLS estimates reported inTable 7 with semi-parametric estimates of average treatment effects constructed using propensity score weighting instead of additive linear models. The semiparametric estimates are remarkably similar to the corresponding 2SLS estimates. Compare, for example, frequency-score-controlled 2SLS estimates of effects on math and reading of 0.496 and 0.127 with semiparametric estimates around 0.44 and 0.11, respectively. At the same time, standard errors for the semiparametric estimates are markedly higher than those for 2SLS (precision is estimated using a Bayesian bootstrap that randomly reweights observations; see (Shao and Tu, 1995) for an introduction).[18]

This is probably conservative: Abadie and Imbens (2012) show that the correction for conditioning on an estimated score reduces the asymptotic variance matrix for an estimated average treatment effect. Strictly speaking, our 2SLS procedure estimates a population average treatment effect only under additional assumptions (like constant effects). Still, as noted in our discussion of modes of inference, the p-values implied by conventional (robust) 2SLS standard errors ignoring score estimation come out close to randomization p-values, suggesting any adjustment for score estimation in this context is small.

[18]The score weighting estimators are the sample analogs of

$$\frac{1}{P[C_{1i} > C_{0i}]} \times \frac{E[(D_i - p_D(\theta))Y_i]}{E[p_\theta(1 - p_D(\theta))]},$$

where $p_D(\theta)$ is the propensity score estimated using frequency or formula, and the first stage, $P[C_{1i} > C_{0i}]$, is estimated two ways. The "Abadie" version uses

$$E\left\{1 - \frac{C_i(1 - D_i)}{1 - p_D(\theta)} - \frac{(1 - C_i)D_i}{p_D(\theta)}\right\},$$

as suggested by (Abadie, 2003). The "Hirano" version uses

$$\frac{E[(D_i - p_\theta)C_i]}{E[p_D(\theta)(1 - p_\theta)]},$$

as suggested by (Hirano et al., 2003) and paralleling the estimand for the reduced form. See (Angrist and Rokkanen, 2015) for details and an application of both estimators.

4.5 Alternative Identification Results

We're interested in comparing 2SLS estimates constructed using a DA offer dummy as an instrument while controlling for the DA propensity score with suitably-controlled estimates constructed using first-choice and qualification instruments. As noted in Section 3.4, we expect DA-offer instruments to yield a precision gain and to increase the number of schools represented in the estimation sample relative to these two previously-employed IV strategies.[19]

Let $Q(\theta_i)$ be a variable that uniquely identifies the charter school that applicant i ranks first, along with his priority status at this school, defined for applicants whose first choice is indeed a charter school. $Q(\theta_i)$ ignores other schools that might have been ranked. The first-choice strategy is implemented by the following 2SLS setup:

$$Y_i = \sum_x \alpha(x)q_i(x) + \beta C_i + \epsilon_i, \tag{6}$$

$$C_i = \sum_x \gamma(x)q_i(x) + \delta D_i^f + \nu_i, \tag{7}$$

where the $d_i(x)$'s are dummies indicating values of $Q(\theta_i)$, indexed by x, and $\gamma(x)$ and $\alpha(x)$ are the associated "qualification risk set effects" in the first and second stages. The first-choice instrument, D_i^f, is a dummy variable indicating i's qualification at his or her first-choice school. In other words,

$$D_i^f = 1[\pi_{is} \le c_s \text{ for charter } s \text{ that } i \text{ has ranked first}].$$

First choice qualification is the same as first choice offer since under DA, applicants who rank a first are offered a seat there if and only if they qualify at a.

The qualification strategy expands the sample to include all charter applicants, with $Q(\theta_i)$ identifying the set of all charter schools that i ranks, along with his or her priority status at each of these schools. $Q(\theta_i)$ ignores the order in which schools are ranked, coding only their identities, but priorities are associated with schools.[20] The qualification instrument, D_i^q, indicates qualification at *any* charter he or she has ranked. In other words,

$$D_i^q = 1[\pi_{is} \le c_s \text{ for at least one charter } s \text{ that } i \text{ has ranked}].$$

In large markets, the instruments D_i^f and D_i^q are independent of type conditional on $Q(\theta_i)$; see Appendix A.4 for details.

A primary source of inefficiency in the first-choice and qualification strategies is apparent in Panel A of Table 8. This panel reports two sorts of first stage estimates for each instrument: the first of these regresses a dummy indicating any charter *offer*–that is, our DA charter offer instrument, D_i–on each of the three instruments under consideration. A regression of D_i on itself necessarily produces a coefficient of one. By contrast, a first-choice offer boosts the probability

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[19]First-choice applications include Abdulkadiroğlu et al. (2013), Deming (2011), Deming et al. (2014), and Hastings et al. (2009). First-choice instruments have also been used with decentralized assignment mechanisms (Cullen et al. (2006), Abdulkadiroğlu et al. (2011), Dobbie and Fryer (2011), and Hoxby et al. (2009)). Dobbie and Fryer (2014), Lucas and Mbiti (2014), and Pop-Eleches and Urquiola (2013) use qualification instruments.

[20]For example, an applicant who ranks A and B with marginal priority only at A is distinguished from an applicant who ranks A and B with marginal priority only at B.

of any charter offer by only around 0.77 in the sample of those who've ranked a charter first. This reflects the fact that, while anyone receiving a first choice charter offer has surely been offered a charter seat, roughly 23% of the sample ranking a charter first is offered a charter seat at schools other than their first choice. The relationship between D_i^q and charter offers is even weaker, at around 0.48. This reflects the fact that for schools below the one ranked first, charter qualification is not sufficient for a charter offer.

The diminished impact of the two alternative instruments on charter offers translates into a weakened first stage for charter *enrollment*. The best case scenario, using all DA-generated offers (that is, D_i) as a source of quasi-experimental variation, produces a first stage of around 0.41 (this differs from the first-stage estimates reported in Table 7 in that it uses a pooled elementary-middle-high-school sample). But first-choice offers boost charter enrollment by just 0.32, while qualification anywhere yields a charter enrollment gain of only 0.18. As always with comparisons of IV strategies, the size of the first stage is a primary determinant of relative precision.

At 0.071, the standard error of the DA-offer estimate is markedly lower than the standard error of 0.102 yielded by a first-choice strategy and well below the standard error of 0.149 generated by qualification instruments. In fact, the precision loss here is virtually the same as the decline in the intermediate first stages recorded in the first row of the table (compare 0.774 with $0.071/0.102 = 0.696$ and 0.476 with $0.071/0.149 = 0.477$). The loss here is substantial: columns 4 and 5 show the sample size increase needed to undo the damage done by a smaller first stage for each alternative instrument.[21]

Only half as many schools are represented in the first-choice analysis sample as in the DA sample (At 24, the number of schools in the qualification sample is closer to the full complement of 30 schools available for study with DA offers). First-choice analyses lose schools because many lotteries fail to randomize first-choice applicants (as seen in Table 1). It's therefore interesting to note that the first-choice estimate of effects on math and reading scores are noticeably larger than the estimates generated using DA offer and qualification instruments (compare the estimate of 0.5 using DA offers with estimates of 0.6 and 0.41 using first-choice and qualification instruments). This finding may reflect an advantage for those awarded a seat at their first choice school (Hastings et al. 2009; Deming 2011; Deming et al. 2014 find a general "first choice advantage" in analyses of school attendance effects.) By contrast, the DA offer instrument yields an estimand that's more representative of the full complement of charter schools in the match.

4.6 Charter School Effects with a Mixed Counterfactual

The 2SLS estimates in Tables 7 and 8 contrast charter outcomes with potential outcomes generated by attendance at a mix of traditional public schools and schools from other non-charter sectors. We'd like to simplify this mix so as to produce something closer to a pure sector-to-sector comparison. Allowance for more than one treatment channel also addresses concerns about charter-offer-induced changes in counterfactual outcomes that might cause violations of the exclusion restriction.

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[21]The sample used to construct the estimates in columns 1-3 of Table 8 is limited to those who have variation in the instrument at hand conditional on the relevant risk sets controls.

Our first step in this effort is to describe the distribution of non-charter school choices for applicants who were and weren't offered a charter seat in the DPS assignment. We then identify the distribution of counterfactual (non-charter) school sectors for the group of charter-lottery compliers. Finally, we use the DA mechanism to jointly estimate causal effects of attendance at schools in different sectors, thereby making the non-charter counterfactual in our 2SLS estimates more homogeneous.

The analysis here builds on a categorical variable, W_i, capturing school sector in which i is enrolled. Important DPS sectors besides the charter sector are traditional public schools, innovation schools, magnet schools, and alternative schools. Innovation and magnet schools are managed by DPS. Innovation schools design and implement innovative practices meant to improve student outcomes (for details and a descriptive evaluation of innovation schools, see Connors et al. 2013). Magnet schools serve students with particular styles of learning. Alternative schools serve mainly older students struggling with factors that may prevent them from succeeding in a traditional school environment. Smaller school sectors include a single charter middle school outside the centralized DPS assignment process (now closed) and a private school contracted to serve DPS students.

The distribution of enrollment sectors for students who do and don't receive a charter offer are described in the first two columns of Table 9. These columns show a charter enrollment rate of 87% in the group offered a charter seat, along with substantial but much smaller charter enrollment in the non-offered group.[22] Perhaps surprisingly, only around 41% of those not offered a charter seat enroll in a traditional public schools, with the rest of the non-offered group distributed over a variety of school types. Innovation schools are the leading non-charter alternative to traditional public schools. Innovation schools operate under an innovation plan that waives some provisions of the relevant collective bargaining agreements (for a descriptive evaluation of these schools, see Connors et al. 2013).[23]

The sector distribution for non-offered applicants with non-trivial charter risk appears in column 3 of Table 9, alongside the sum of the non-offered mean and a charter-offer treatment effect on enrollment in each sector in column 4. These extended first-stage estimates, computed by putting indicators $1(W_i = j)$ on the left-hand side of equation (4), control for the DA propensity score and therefore have a causal interpretation. The number of applicants not offered a seat who end up in a charter school is higher for those with non-trivial charter offer risk than in the full applicant sample, as can be seen by comparing columns 3 and 1. The charter enrollment first stage that's implicit in the column 4-vs-3 comparison matches the first stage in Table 7. First stages for other sectors show charter offers sharply reduce innovation school enrollment as well as reducing enrollment in traditional public schools.

The 2SLS estimates reported in Table 7 capture causal effect for charter lottery compliers. We

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[22]Applicants unhappy with the offer they've receive in the first round of the Denver assignment may apply to schools individually in a second round. This process produces charter offers for those not offered a charter seat initially.

[23]Innovation waivers are subject to approval by the Denver Classroom Teachers Association (which organizes Denver public school teachers' bargaining unit), and they allow, for example, increased instruction time. DPS innovation schools appear to have much in common with Boston's pilot schools, a model examined in Abdulkadiroğlu et al. (2011).

describe the distribution of school sectors for compliers by defining *potential* sector enrollment variables, W_{1i} and W_{0i}, indexed against charter offers, D_i. Potential and observed sector variables are related by

$$W_i = W_{0i} + (W_{1i} - W_{0i})D_i.$$

In the population of charter-offer compliers, $W_{1i} = charter$ for all i: by definition, charter-offer compliers attend a charter school when the DPS assignment offers them the opportunity to do so. Here, we're interested in $E[1(W_{0i} = j)|C_{1i} > C_{0i}]$, that is, the sector distribution for charter-offer compliers in the scenario where they aren't offered a charter seat. We refer to this distribution as describing "enrollment destinies" for compliers.

Enrollment destinies are marginal potential outcome distributions for compliers. As shown by Abadie (2002), these are identified by a simple 2SLS estimand. The details of our implementation of this identification strategy follow those in Angrist et al. (2015), with the modification that instead of estimating marginal potential outcome densities for a continuous variable, the outcomes of interest here are Bernoulli.[24]

Column 5 of Table 9 reveals that, among middle- and elementary-school applicants, only about half of charter lottery compliers are destined to end up in a traditional public school if they aren't offered a charter seat. The second most-likely counterfactual destiny for the younger applicant group is an innovation school, with nearly a third of non-offered compliers enrolling in one of these. The likelihood of an enrollment destiny outside the charter, traditional, and innovation sectors is much smaller.

Isolating an Innovation School Effect

The outsize role of innovation schools in counterfactual destinies motivates an empirical strategy that allows for distinct charter and innovation school treatment effects. By pulling innovation schools out of the non-charter counterfactual, we capture charter effects driven mainly by the contrast between charter and traditional public schools. Comparisons with a more homogeneous counterfactual also mitigates bias that might arise from violations of the exclusion restriction (discussed in Section 3.4). And, of course, the innovation treatment effect is also of interest in its own right.

For the purposes of this discussion, we code the sector variable, W_i, as taking on the value 2 for innovation schools, the value 1 for charters, and 0 otherwise. The corresponding potential outcomes are Y_{2i}, Y_{1i}, and Y_{0i}. At this point, we also adopt a constant-effects causal framework; the question of how to identify average causal effects in instrumental variables models with multiple treatments raises issues that go beyond the scope of this paper.[25] Defining constant

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[24]Briefly, our procedure puts $(1-C_i)1(W_i = j)$ on the left hand side of a version of equation (5) with endogenous variable $1 - C_i$. The coefficient on this endogenous variable is an estimate of $E[1(W_{0i} = j)|C_{1i} > C_{0i}, X_i]$. The covariates and sample used here are the same as used to construct the 2SLS impact estimates reported in column 1 of Table 7.

[25]See Behaghel et al. (2013) and Blackwell (2015) for recent progress on this issue.

effects with the notation

$$Y_{2i} - Y_{0i} = \beta_2,$$
$$Y_{1i} - Y_{0i} = \beta_1,$$

our two-sector identification strategy can be motivated by the conditional independence assumption,

$$Y_{0i} \perp\!\!\!\perp Z_i | \theta_i, \tag{8}$$

where Z_i is a categorical variable that records DA-generated offers in each sector sector (charter, innovation, other).

The instruments here are indicators for charter and innovation-sector offers, $D_i^1 = 1[Z_i = 1]$ and $D_i^2 = 1[Z_i = 2]$. These dummy instruments are used in a 2SLS procedure with two endogenous variables, C_i^1 for charter school enrollment and C_i^2 for innovation school enrollment. Propensity score conditioning is justified by the fact that conditional independence relation (8) implies

$$Y_{0i} \perp\!\!\!\perp Z_i \mid p_1(\theta), p_2(\theta), \tag{9}$$

where $p_1(\theta) = E[D_i^1|\theta]$ and $p_2(\theta) = E[D_i^2|\theta]$.

The 2SLS setup in this case consists of

$$Y_i = \sum_x \alpha_1(x)d_i^1(x) + \sum_x \alpha_2(x)d_i^2(x) + \beta_1 C_i^1 + \beta_2 C_i^2 + \epsilon_i, \tag{10}$$

$$C_i^1 = \sum_x \gamma_{11}(x)d_i^1(x) + \sum_x \gamma_{12}(x)d_i^2(x) + \delta_{11}D_i^1 + \delta_{12}D_i^2 + \nu_i, \tag{11}$$

$$C_i^2 = \sum_x \gamma_{21}(x)d_i^1(x) + \sum_x \gamma_{22}(x)d_i^2(x) + \delta_{21}D_i^1 + \delta_{22}D_i^2 + \eta_i, \tag{12}$$

where the dummy control variables, $d_i^1(x)$ and $d_i^2(x)$, saturate estimates of the scores for each treatment, $\hat{p}_1(\theta_i)$ and $\hat{p}_2(\theta_i)$, with corresponding score effects denoted by γ's and α's in the first and second stage models. The sample used for this analysis is the union of charter and innovation school applicants.

As noted by Imbens (2000) and Yang et al. (2014), the key conditional independence relation in this context (equation (9)) suggests we should choose a parameterization that fixes the conditional probability of assignment for all treatment levels *jointly*. Joint score control replaces the additive score controls in equations (10), (11), and (12) with score controls of the form

$$d_i^{12}(x^1, x^2) = 1[\hat{p}_1(\theta_i) = x^1, \hat{p}_2(\theta_i) = x^2],$$

where hats denote score estimates and the indices, x^1 and x^2, run independently over all values in the support for each score. This model generates far more score fixed effects than does equation (10).[26] Fortunately, however, the algebra of 2SLS obviates the need for joint score control; additive is enough.

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[26] When $p_1(\theta)$ takes on k_1 values and $p_2(\theta)$ takes on k_2 values, the additive model has k_1+k_2-2 score parameters, while the joint model has $k_1k_2 - 1$.

To see why additive control is adequate for 2SLS models that exploit (9), note first that 2SLS estimates of the second stage equation, (10), can be obtained by first regressing each offer dummy on the full set of $d_i^1(x)$ and $d_i^2(x)$ and then using the residuals from this regression as instruments after dropping these controls from the model (see, e.g., Angrist and Pischke 2009). Note also that both sets of regressors in this auxiliary first-step model are functions of type. A regression of D_i^j on the full set of $d_i^1(x)$ and $d_i^2(x)$ therefore returns fitted values given by $E[D^j|\theta] = p_j(\theta_i)$.

Suppose now that we replace additive controls, $d_i^1(x)$ and $d_i^2(x)$, with the full set of dummies, $d_i^{12}(x^1, x^2)$, parameterizing the jointly-controlled model. Since the model here is saturated, a regression of D_i^j on the full set of $d_i^{12}(x^1, x^2)$ dummies recovers the conditional expectation function of offers given both scores. By the law of iterated expectations, however, this is

$$E[D_i^j|p_1(\theta_i), p_2(\theta_i)] = E\{E[D_i^j|\theta_i]|p_1(\theta_i), p_2(\theta_i)]\} = p_j(\theta_i).$$

From this we conclude that the IV equivalent of 2SLS is the same in the additive and jointly-controlled models.[27]

As a benchmark, columns 1-2 of Table 10 compare charter-only and innovation-only estimates computed using DA (frequency) score controls. Each sample is limited to applicants to the relevant sector.[28] Like the estimates in Tables 7 and 8, these pool all grades. A parallel set of single-sector estimates using simulated score controls appears in columns 5 and 6. The innovation first stage (the effect of an innovation school offer on innovation school enrollment) is around 0.35. The pooled single-sector charter estimates in Table 10 are the same as those in Table 7. Not surprisingly in view of the substantially reduced number of applicants with non-trivial innovation offer risk (546 in column 2 and 613 in column 6 of Table 10), and the smaller innovation first stage, the innovation attendance effects are relatively imprecise. This imprecision notwithstanding, the innovation-only model generates a large negative and marginally significant effect on reading when estimated with the DA score.

2SLS estimates of equation (10) appear in columns 3 and 7 of Table 10. Charter school effects change little in this specification, but (insignificant) negative innovation estimates for math flip to positive when estimated using a model that also isolates charter treatment effects. The negative innovation school effects on reading seen in columns 2 and 6 also become smaller in the two-endogenous-variables models. Most interestingly, perhaps, the marginally significant positive charter school effect on reading (when estimating using DA score controls) also disappears. While charter students' reading performance exceeds what we can expect to see were these students to enroll in a mix of traditional and (low-performing) innovation schools, the reading gap between charters and traditional public schools is a little smaller.

Finally, as the theoretical discussion above leads us to expect, the results of estimation with joint score controls, shown in columns 4 and 8 of Table 10, differ little from the estimates

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[27]This conclusion holds in the population, but need not hold exactly in our data (because scores here are estimated by something more elaborate than a sample mean conditional on type) or for models that include additional covariates beyond saturated score controls.

[28]Appendix Table B6 lists innovation schools and describes the random assignment pattern at these schools along the lines of Table 1 for charter schools. Covariate balance and differential attrition results for innovation schools are reported in Appendix Table B7.

constructed using additive score controls reported in columns 3 and 7 (a marginally significant though still imprecisely estimate positive innovation effect on math scores emerges in column 4). Overall, it seems fair to say that the findings on charter effectiveness in Table 7 stand when charter effects are estimated using a procedure that removes the innovation sector from the charter enrollment counterfactual.

5 Summary and Directions for Further Work

We investigate empirical strategies that use the random tie-breaking embedded in market design solutions to school matching problems as a research tool. The most important fruit of this inquiry is the DA propensity score, an easily-computed formula for the conditional probability of assignment to particular schools as a function of type. The DA propensity score reveals the nature of the experimental design generated as a by-product of market design and suggests directions in which match parameters might be modified so as to boost the research value of school assignment and other matching schemes. We also show how the DA score can be used to simultaneously evaluate attendance effects in multiple sectors or schools.

A score-based analysis of data from Denver's unified school match reveals substantial gains from attendance at one of Denver's many charter schools. The resulting charter effects are similar to those computed using single-school lottery strategies for charters in Boston. At the same time, as with previously reported results for Boston Pilot schools, Denver's Innovation model does not appear to generate substantial achievement gains. Our analysis focuses on defining and estimating the DA propensity score, giving less attention to the problem of how best to use the score for estimation. Still, simple 2SLS procedures seem to work well, and the resulting estimates of DPS charter effects differ little from those generated by semiparametric alternatives.

The methods developed here should be broadly applicable to markets using the DA family of mechanisms for centralized assignment. There's nothing special about markets that match schools and students, except, perhaps, their accessible high-quality data. At the same time, some markets and matches use mechanisms not covered by the DA framework. Most important on this list is the top trading cycles (TTC) mechanism (Shapley and Scarf, 1974; Abdulkadiroğlu and Sönmez, 2003), which allows students to trade priorities rather than treating priorities as fixed. We expect to have theoretical results on the TTC propensity score soon, along with an application to New Orleans Recovery School District, which has experimented with TTC matching.

Many matching problems, such as the Boston and New York exam schools analyzed by Abdulkadiroğlu et al. (2014), use some kind of non-randomly-assigned tie-breaker rather than a lottery. These schemes embed a regression discontinuity design inside a market design rather than a randomized trial. The question of how best to define and exploit a DA propensity score for markets that combine regression-discontinuity designs with market design matchmaking is also a natural next step for our research design and market design agenda.

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Figure 1: Propensity Scores and Market Size in in Example 2

Probability of Assignment to School *a*
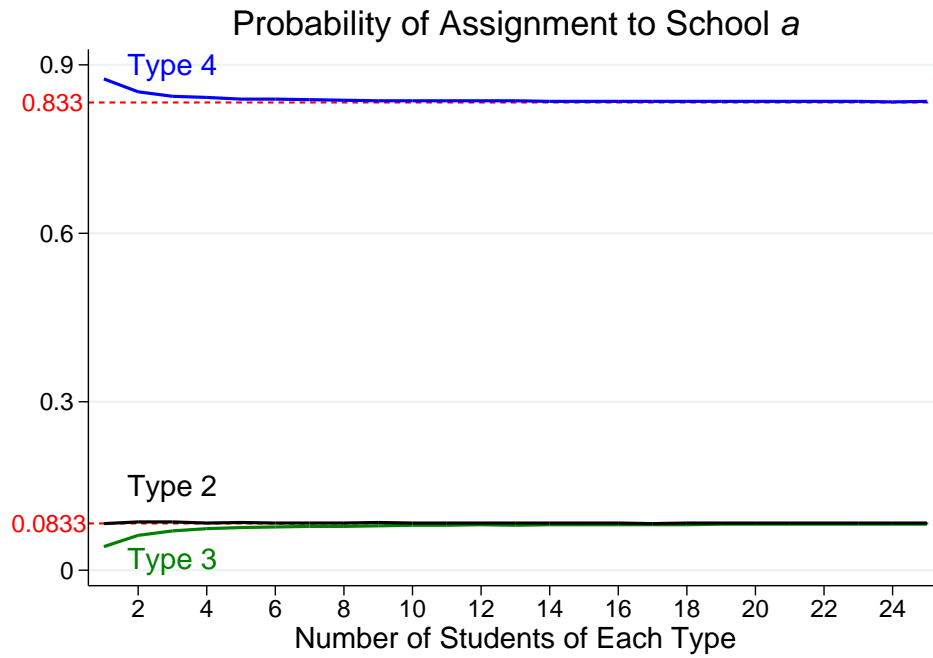
Notes: This figure plots finite-market propensity scores for expansions of Example 2 in Section 2.2. For each value of the x axis, we consider an expansion of the example with x students of each type. The propensity scores plotted here were computed by drawing lottery numbers 100,000 times.

Figure 2: Sample Size Gains from the Propensity Score Strategy

Sample Size Gains: Charter Schools

(a)

Sample Size Gains: Non Charter Schools
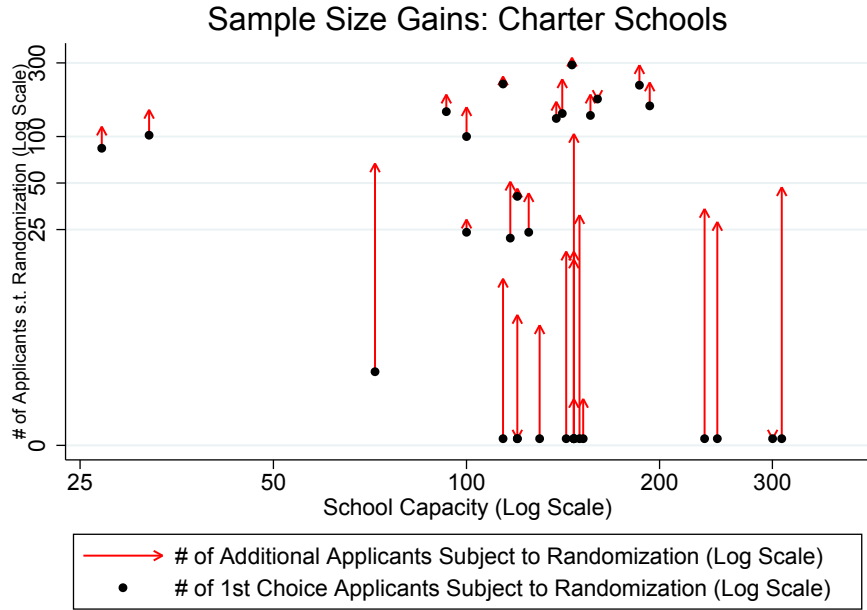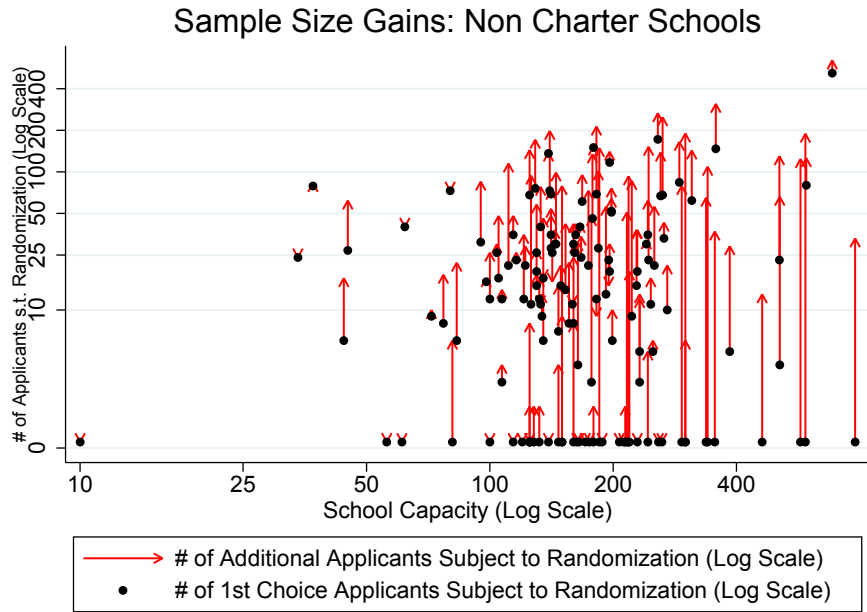
(b)

Notes: These figures compare the sample size under our DA propensity score strategy to that under the first choice strategy. Down arrows mean the two empirical strategies produce the same number of applicants subject to randomization at the corresponding schools. We say a student is subject to randomization at a school if the student has the DA propensity score (frequency) of assignment to that school that is neither 0 nor 1.

Table 1: DPS charter schools

School	Total applicants (1)	Applicants offered seats (2)	Propensity score in (0,1)			
			DA score (frequency) (3)	DA score (formula) (4)	Simulated score (5)	Simulated score (first choice) (6)
Elementary and middle schools						
Cesar Chavez Academy Denver	62	9	7	9	8	3
Denver Language School	4	0	0	0	0	0
DSST: Cole	281	129	31	40	44	0
DSST: College View	299	130	47	67	68	0
DSST: Green Valley Ranch	1014	146	324	344	357	291
DSST: Stapleton	849	156	180	189	221	137
Girls Athletic Leadership School	221	86	18	40	48	0
Highline Academy Charter School	159	26	69	78	84	50
KIPP Montbello College Prep	211	39	36	48	55	20
KIPP Sunshine Peak Academy	389	83	41	42	44	36
Odyssey Charter Elementary	215	6	20	21	22	14
Omar D. Blair Charter School	385	114	135	141	182	99
Pioneer Charter School	25	5	0	2	2	0
SIMS Fayola International Academy Denver	86	37	7	18	20	0
SOAR at Green Valley Ranch	85	9	41	42	43	37
SOAR Oakland	40	4	0	9	7	2
STRIVE Prep - Federal	621	138	170	172	175	131
STRIVE Prep - GVR	324	112	104	116	118	0
STRIVE Prep - Highland	263	112	2	21	18	0
STRIVE Prep - Lake	320	126	18	26	26	0
STRIVE Prep - Montbello	188	37	16	31	35	0
STRIVE Prep - Westwood	535	141	235	238	239	141
Venture Prep	100	50	12	17	17	0
Wyatt Edison Charter Elementary	48	4	0	3	2	0
High schools						
DSST: Green Valley Ranch	806	173	290	343	330	263
DSST: Stapleton	522	27	116	117	139	96
Southwest Early College	265	76	34	47	55	0
Venture Prep	140	39	28	42	45	0
KIPP Denver Collegiate High School	268	60	29	37	40	24
SIMS Fayola International Academy Denver	71	15	6	22	22	0
STRIVE Prep - SMART	383	160	175	175	175	175

Notes: This table shows application patterns at charter schools. Column 1 is the number of applicants ranking each school. Columns 3-6 are restricted to applicants with propensity score values that are neither zero (i.e. ineligible for a seat) nor one (i.e. guaranteed a seat) according to different score computation methods. Column 6 is the number of applicants in column 5 who rank each school as their first choice. Elementary and middle schools are those serving grades 4-8. High schools are those serving grades 9 and 10.

STRIVE Prep School	Eligible applicants	Capacity	Offers	DA Score = 0			DA Score in (0,1)		DA Score = 1
				Θ_s^n	Θ_s^c	Θ_s^a	Θ_s^c	Θ_s^a	Θ_s^a
				$(\rho_{\theta s} > \rho_s)$	$(\rho_{\theta s} = \rho_s)$	$(\rho_{\theta s} < \rho_s)$	$(\rho_{\theta s} = \rho_s)$	$(\rho_{\theta s} < \rho_s)$	$(\rho_{\theta s} < \rho_s)$
				$0 \leq MID \leq 1$	$MID \geq \tau_s$	$MID = 1$	$MID < \tau_s$	$0 < MID < 1$	$MID = 0$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
GVR	324	147	112	0	0	159	0	116	49
Lake	274	147	126	0	0	132	0	26	116
Highland	244	147	112	0	0	121	0	21	102
Montbello	188	147	37	0	0	128	0	31	29
Federal	574	138	138	78	284	3	171	1	37
Westwood	494	141	141	53	181	4	238	0	18

Notes: This table shows how formula scores are determined for STRIVE school seats in grade 6 (all 6th grade seats at these schools are assigned in a single bucket; ineligible applicants, who have a score of zero, are omitted). Column 3 records offers made to these applicants. Columns 4-6 show the number of applicants in partitions with a score of zero. Likewise, columns 7 and 8 show the number of applicants subject to random assignment. Column 9 shows the number of applicants with certain offers.

Table 3: Baseline characteristics of DPS students

| | Denver students (1) | SchoolChoice applicants (2) | Charter applicants (3) | Propensity score in (0,1) | | | |
| | | | | DA score (frequency) | | Simulated score | |
				All applicants (4)	Enroll at charter (5)	All applicants (6)	Enroll at charter (7)
Origin school is charter	0.133	0.080	0.130	0.259	0.371	0.230	0.357
Female	0.495	0.502	0.518	0.488	0.496	0.506	0.511
Race							
Hispanic	0.594	0.593	0.633	0.667	0.713	0.636	0.711
Black	0.141	0.143	0.169	0.181	0.161	0.192	0.168
White	0.192	0.187	0.124	0.084	0.062	0.098	0.059
Asian	0.034	0.034	0.032	0.032	0.039	0.033	0.037
Gifted	0.171	0.213	0.192	0.159	0.152	0.165	0.149
Bilingual	0.039	0.026	0.033	0.038	0.042	0.032	0.037
Subsidized lunch	0.753	0.756	0.797	0.813	0.818	0.800	0.823
Limited English proficient	0.285	0.290	0.324	0.343	0.378	0.337	0.380
Special education	0.119	0.114	0.085	0.079	0.068	0.083	0.070
Baseline scores							
Math	0.000	0.015	0.021	0.037	0.089	0.037	0.062
Reading	0.000	0.016	0.005	-0.011	0.007	0.008	-0.002
Writing	0.000	0.010	0.006	0.001	0.039	0.016	0.035
N	40,143	10,898	4,964	1,436	828	1,523	781

Notes: This table reports demographic characteristics of Denver 3rd-9th graders in 2011-2012, the baseline (and application) year. Column 1 includes all charter and non-charter students. Column 2 restricts the sample to students who submitted an application to the SchoolChoice system for a seat in grades 4-10 at another DPS school in 2012-2013. Column 3 reports values for students ranking any charter school. Columns 4-7 are restricted to applicants with propensity score values that are neither zero (i.e. ineligible for a seat) nor one (i.e. guaranteed a seat). Test scores are standardized to have mean zero and standard deviation one within each grade based on all Denver students.

Table 4: Average covariate balance across 400 offer simulations

| | Non-offered mean (1) | No controls (2) | Propensity score controls | | | | | |
| | | | DA score (frequency) | | | Simulated score | | |
			Linear (3)	Hundredths (4)	Saturated (5)	Linear (6)	Hundredths (7)	Ten Thousandths (8)
			A. Application covariates					
Number of schools ranked	4.374	-0.340	0.112	0.067	0.054	0.018	0.017	0.001
Number of charter schools ranked	1.426	0.474	0.111	0.071	0.056	0.008	0.008	0.002
First school ranked is charter	0.290	0.617	0.005	0.005	0.002	0.001	0.001	-0.001
			B. Baseline covariates					
Origin school is charter	0.083	0.114	-0.020	-0.005	-0.002	-0.002	-0.002	0.000
Female	0.520	-0.007	0.004	0.005	0.005	0.004	0.003	0.000
Race								
Hispanic	0.595	0.094	-0.010	-0.004	-0.005	0.003	0.001	0.001
Black	0.182	-0.031	0.005	0.001	0.002	-0.001	0.001	0.000
Gifted	0.200	-0.021	0.000	-0.001	-0.003	0.003	0.003	0.003
Bilingual	0.025	0.020	0.001	0.001	0.001	0.001	0.001	0.001
Subsidized lunch	0.767	0.074	0.004	0.003	0.003	0.003	0.000	0.000
Limited English proficient	0.289	0.085	-0.001	-0.002	-0.002	0.003	0.001	-0.002
Special education	0.087	-0.004	-0.004	-0.005	-0.004	0.000	-0.001	-0.001
Baseline scores								
Math	0.017	0.010	-0.023	-0.019	-0.020	-0.003	-0.003	0.005
Reading	0.034	-0.071	-0.019	-0.017	-0.017	-0.005	-0.004	0.001
Writing	0.029	-0.056	-0.019	-0.016	-0.016	-0.002	-0.001	0.003
Average risk set points of support			87	39	47	1,148	51	120

Notes: This table shows the average of covariate balance coefficients across 400 charter offer simulations. We first simulate the real Denver mechanism 400 times and run the covariate balance regressions under each simulation. Coefficients are estimated from regressions of each variable on the left on an any-charter simulated offer dummy, controlling for the variables indicated in each column header. Only applicants to 2012-2013 charter seats in grades 4-10 who were enrolled in Denver at baseline grade are included. Test scores are standardized to have mean zero and standard deviation one within each grade based on all Denver students. Charter offer equals one if a student is accepted into any charter school, excluding alternative charters. Column 1 reports baseline characteristics of charter applicants who did not receive a charter offer. For columns 3 and 6, risk set points of support report the average number (across 400 offer simulations) of unique values in the support of the respective propensity scores. For columns 4-5 and 7-8, risk set points of support report the average number of cells (as defined by dummies for each value of the saturated score) with offer instrument variation. Coefficients in columns 3-8 control for the probability of assignment to a charter school according to different functional forms and probability computation methods, and exclude applicants with propensity score equal to zero (i.e. ineligible) or one (i.e. guaranteed). Controls in columns 4 and 7 are dummies for rounded values in the propensity score support, rounding to the hundredths. Controls in column 5 are dummies for every value in the propensity score support. Column 8 are dummies for rounded values of the simulated score, rounding to the ten thousandths.

Table 5: Average covariate balance by market size

		DA score (frequency) controls (saturated)			
	No controls	Actual market size	Twice the market size	Four times the market size	Eight times the market size
	(1)	(2)	(3)	(4)	(5)
Number of schools ranked	-0.340	0.054	0.022	0.009	0.003
Number of charter schools ranked	0.474	0.056	0.019	0.006	-0.001
First school ranked is charter	0.617	0.002	0.001	0.001	0.001
Origin school is charter	0.114	-0.002	0.000	0.001	0.001
Female	-0.007	0.005	0.001	0.000	0.000
Race					
Hispanic	0.094	-0.005	0.001	0.003	0.003
Black	-0.031	0.002	-0.002	-0.002	-0.001
Gifted	-0.021	-0.003	-0.002	-0.002	-0.001
Bilingual	0.020	0.001	0.001	0.002	0.001
Subsidized lunch	0.074	0.003	0.002	0.000	0.000
Limited English proficient	0.085	-0.002	0.000	-0.001	-0.002
Special education	-0.004	-0.004	-0.003	-0.003	-0.002
Baseline scores					
Math	0.010	-0.020	-0.013	-0.010	-0.009
Reading	-0.071	-0.017	-0.011	-0.007	-0.005
Writing	-0.056	-0.016	-0.008	-0.006	-0.006
Average sample size	4,964	1,417	2,631	5,432	11058

Notes: This table shows the average of covariate balance coefficients across 400 charter offer simulations using replicas of students in the realized market to increase the market size. Coefficients are estimated from regressions of each variable on the left on an any-charter simulated offer dummy, controlling for the variables indicated in each column header. All estimates are based on a sample of charter applicants to grades 4 through 10 with probability of assignment into a charter school greater than zero and less than one. Average sample size reports the average number of students (across 400 offer simulations) with propensity score values that are neither zero (i.e. ineligible for a seat) nor one (i.e. guaranteed a seat), and whose DA score (frequency) control (saturated) cell has variation in instrument.

Table 6a: Application covariate balance

		Propensity score controls						Full applicant type controls
		DA score (frequency)			Simulated score			
	No controls	Linear	Hundredths	Saturated	Linear	Hundredths	Ten Thousandths	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of schools ranked	-0.341***	0.097	0.059	0.028	0.014	0.001	-0.061	-0.015
	(0.046)	(0.103)	(0.095)	(0.094)	(0.102)	(0.095)	(0.125)	(0.042)
Number of charter schools ranked	0.476***	0.143***	0.100**	0.074	0.020	-0.017	0.009	0.007
	(0.024)	(0.052)	(0.047)	(0.047)	(0.048)	(0.043)	(0.061)	(0.010)
First school ranked is charter	0.612***	0.012	0.002	-0.001	-0.030	-0.042*	0.012	0.000
	(0.011)	(0.025)	(0.022)	(0.020)	(0.027)	(0.022)	(0.027)	(0.000)
N	4,964	1,436	1,289	1,247	1,523	1,290	681	301
Risk set points of support		88	40	47	1,148	51	126	61
Robust F-test for joint significance	1190	2.70	1.70	1.09	0.49	1.26	0.31	0.34
p-value	0.000	0.044	0.165	0.352	0.688	0.287	0.817	0.710

Notes: This table reports coefficients from regressions of the application characteristics on each row on an any-charter school offer receivership dummy. Only applicants to 2012-2013 charter seats in grades 4-10 who were enrolled in Denver at baseline grade are included. Columns 1-7 are as defined in Table 3. Column 8 controls for fully saturated applicant types (that is, unique combinations of applicant preferences over school programs and school priorities in those programs). Robust standard errors are reported in parentheses. P-values for robust joint significance tests are estimated by stacking outcomes and clustering standard errors at the student level.
*significant at 10%; **significant at 5%; ***significant at 1%

Table 6b: Baseline covariate balance

		Propensity score controls					
		DA score (frequency)			Simulated score		
	No controls	Linear	Hundredths	Saturated	Linear	Hundredths	Ten Thousandths
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Origin school is charter	0.108***	-0.051**	-0.037**	-0.029*	-0.039*	-0.036**	-0.037*
	(0.010)	(0.024)	(0.017)	(0.017)	(0.023)	(0.017)	(0.022)
Female	-0.005	0.024	0.021	0.019	0.016	0.030	0.010
	(0.014)	(0.034)	(0.034)	(0.034)	(0.033)	(0.034)	(0.054)
Race							
Hispanic	0.095***	-0.022	-0.013	-0.007	0.005	-0.001	-0.018
	(0.014)	(0.031)	(0.028)	(0.028)	(0.031)	(0.029)	(0.042)
Black	-0.033***	-0.002	-0.005	-0.007	-0.012	-0.012	0.011
	(0.011)	(0.026)	(0.025)	(0.025)	(0.026)	(0.026)	(0.039)
Gifted	-0.028**	-0.026	-0.028	-0.030	-0.032	-0.035	-0.037
	(0.011)	(0.026)	(0.026)	(0.026)	(0.025)	(0.026)	(0.042)
Bilingual	0.023***	0.016	0.014	0.015	0.012	0.014	0.011
	(0.005)	(0.014)	(0.013)	(0.014)	(0.014)	(0.014)	(0.021)
Subsidized lunch	0.073***	-0.003	-0.004	0.001	0.001	-0.005	0.024
	(0.011)	(0.027)	(0.025)	(0.025)	(0.027)	(0.026)	(0.037)
Limited English proficient	0.086***	-0.002	-0.002	0.001	0.011	0.001	0.004
	(0.014)	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)	(0.053)
Special education	0.004	0.034**	0.032*	0.032*	0.043**	0.044**	0.035
	(0.008)	(0.017)	(0.017)	(0.017)	(0.017)	(0.018)	(0.028)
N	4,964	1,436	1,289	1,247	1,523	1,290	681
Baseline scores							
Math	-0.002	-0.087	-0.083	-0.082	-0.068	-0.078	-0.053
	(0.027)	(0.061)	(0.060)	(0.061)	(0.061)	(0.061)	(0.094)
Reading	-0.085***	-0.096*	-0.100*	-0.108*	-0.081	-0.086	-0.070
	(0.026)	(0.057)	(0.056)	(0.056)	(0.056)	(0.056)	(0.087)
Writing	-0.072***	-0.097*	-0.096*	-0.101*	-0.085	-0.094*	-0.053
	(0.026)	(0.056)	(0.054)	(0.055)	(0.055)	(0.054)	(0.083)
N	4,889	1,420	1,275	1,234	1,504	1,275	675
Robust F-test for joint significance	19.1	1.20	1.13	0.99	1.04	1.35	0.71
p-value	0.000	0.278	0.329	0.454	0.408	0.183	0.743

Notes: This table reports coefficients from regressions of the baseline characteristics on each row on an any-charter school offer receivership dummy. Only applicants to 2012-2013 charter seats in grades 4-10 who were enrolled in Denver at baseline grade are included. Test scores are standardized to have mean zero and standard deviation one within each grade based on all Denver students. Columns 1-7 are as defined in Table 3. Robust standard errors are reported in parentheses. P-values for robust joint significance tests are estimated by stacking outcomes and clustering standard errors at the student level.

*significant at 10%; **significant at 5%; ***significant at 1%

Table 7: DPS Charter attendance effects on standardized scores

| | 2SLS estimates | | | | | |
| | DA score | | | | | |
	Frequency (saturated) (1)	Formula (saturated) (2)	Simulated score (hundredths) (3)	No score controls (4)	OLS (5)	OLS with score controls (6)
First stage	0.410***	0.389***	0.377***	0.683***		
	(0.031)	(0.032)	(0.032)	(0.012)		
Math	0.496***	0.524***	0.543***	0.306***	0.304***	0.386***
	(0.071)	(0.076)	(0.079)	(0.021)	(0.015)	(0.034)
Reading	0.127**	0.120*	0.106	0.093***	0.103***	0.093***
	(0.065)	(0.069)	(0.071)	(0.020)	(0.014)	(0.029)
Writing	0.325***	0.356***	0.324***	0.183***	0.180***	0.202***
	(0.077)	(0.080)	(0.080)	(0.023)	(0.015)	(0.036)
N	1,102	1,083	1,137	4,317	4,317	1,102

Notes: This table reports 2SLS and OLS estimates of charter attendance effects on 2012-2013 TCAP scores of Denver 4th-10th graders using the SchoolChoice any-charter offer instrument. Test scores are standardized to have mean zero and standard deviation one within each grade based on all Denver students. Column 6 estimates OLS using DA score (frequency) controls (saturated). Columns 1-6 control for grade tested, gender, origin school charter status, race, gifted status, bilingual status, subsidized price lunch eligibility, special education, limited English proficient status, and test scores at baseline. Robust standard errors are reported in parentheses.

*significant at 10%; **significant at 5%; ***significant at 1%

Table 8: Other IV strategies

	Charter attendance effect			Sample size increase for equivalent gain (col 2 vs col 1)	Sample size increase for equivalent gain (col 3 vs col 1)
	Offer instrument with DA score (frequency) controls (saturated)	First choice charter offer with risk set controls	Qualification instrument with risk set controls		
	(1)	(2)	(3)	(4)	(5)

A. First stage estimates

First stage for charter offers	1.000	0.774***	0.476***		
	--	(0.026)	(0.024)		
First stage for charter enrollment	0.410***	0.323***	0.178***		
	(0.031)	(0.035)	(0.027)		

B. 2SLS estimates

Math	0.496***	0.596***	0.409***		
	(0.071)	(0.102)	(0.149)	2.0	4.4
Reading	0.127**	0.227**	0.229		
	(0.065)	(0.102)	(0.144)	2.5	4.9
Writing	0.325***	0.333***	0.505***		
	(0.077)	(0.119)	(0.162)	2.4	4.5
N (students)	1,102	1,125	1,969		
N (schools)	30	15	24		

Notes: This table compares 2SLS attendance effects on 2012-2013 TCAP scores of Denver 4th-10th graders estimated using DA score (frequency) controls (saturated) to other IV strategies. First stage for charter offers reports the regression coefficient of the any-charter offer dummy (the instrument used in column 1) on other instruments, conditioning on the same controls used in the corresponding first stage estimates for charter enrollment. Column 2 reports charter attendance effects using a first-choice charter offer instrument. Column 3 reports charter attendance effects using an any-charter qualification instrument. Columns 2 and 3 control for risk sets making the first-choice and qualification instruments conditionally random; see the main text for details. Columns 2 and 3 exclude 10th graders because there were no 10th grade applicants in risk sets with variation in either first choice offer or any-charter qualification. Test scores are standardized to have mean zero and standard deviation one within each grade based on all Denver students. Columns 4 and 5 report the multiples of the first-choice offer sample size and qualification sample size needed, respectively, in order to achieve a precision gain equivalent to the gain from using the any-charter offer instrument, and is calculated as one minus the square of ratio of the standard error in column 3 to the standard error in column 1. Coefficients in columns 1-3 control for grade tested, gender, origin school charter status, race, gifted status, subsidized price lunch eligibility, special education, bilingual status, limited English proficient status, and test scores at baseline. The last row counts the number of schools for which we observe in-sample variation in the assignment to each school conditional on the cell controls included in the model.

*significant at 10%; **significant at 5%; ***significant at 1%

Table 9: Enrollment destinies among charter applicants

	All charter applicants		Charter applicants with DA score (frequency) in (0,1)			
			All applicants		Compliers	
	No charter offer (Z = 0)	Charter offer (Z = 1)	Non-offered mean	First stage + col 3	No charter offer (Z = 0)	Charter offer (Z = 1)
	(1)	(2)	(3)	(4)	(5)	(6)
Enrolled in a study charter	0.147	0.865	0.347	0.757	--	1.000
… in a traditional public	0.405	0.081	0.257	0.054	0.497	--
… in an innovation school	0.234	0.023	0.241	0.107	0.328	--
… in a magnet school	0.192	0.021	0.116	0.078	0.094	--
… in an alternative school	0.009	0.005	0.018	0.006	0.030	--
… in a contract school	0.012	0.004	0.018	-0.001	0.047	--
… in a non-study charter	0.001	0.000	0.002	0.000	0.005	--
N	2,555	1,833	498	1,102	--	--

Notes: This table describes school enrollment for charter applicants. The sample in this table is identical to that in Table 6. Columns 1-2 describe enrollment for applicants with a charter offer (Z=1) and without a charter offer (Z=0) across all charter applicants, while columns 3-4 and 5-6 show the same for applicants with DA score (frequency) greater than zero and less than one, and for compliers, respectively. Columns 3-6 are restricted to applicants whose DA score (frequency) control (saturated) cells have variation in charter offer. Column 4 adds the non-offered mean in column 3 to the first stage estimate of a regression of a dummy for each school type on the left on an any-charter offer dummy, conditional on the same DA score (frequency) controls and demographic controls as used to construct the estimates in Table 6. School classification is conducted at the grade level, since some schools run magnet programs for a subset of grades served only. Innovation and magnet schools are managed by DPS. Innovation schools design and implement innovative practices to improve student outcomes. Magnet schools serve students with particular styles of learning. Alternative schools include "intensive pathways" and "multiple pathways" schools. The former serve students struggling with academics, behavior, attendance, or other factors that may prevent them from succeeding in a traditional school environment; the latter offer faster pathways toward high school graduation, such as GED preparation and technical education. Non-study charter corresponds to Northeast Academy, a K-8 school that was not included in the SchoolChoice mechanism and closed in May 2013. Contract school corresponds to Escuela Tlatelolco, a private school contracted to serve DPS students. Complier means are estimated by 2SLS following Abadie (2002), using the same DA score (frequency) controls and demographic controls as used to construct the estimates in Table 6. The coefficient for contract school in column 5 was rounded down to zero from -0.002.

Table 10: DPS charter and innovation school attendance effects

	DA score (frequency) controls (saturated)				Simulated score controls (hundredths)			
			Both dummies				Both dummies	
	Charter-only dummy	Innovation-only dummy	Additive score controls	Joint score controls	Charter-only dummy	Innovation-only dummy	Additive score controls	Joint score controls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Charter First Stage	0.410***	--	0.405***	0.398***	0.377***	--	0.437***	0.417***
	(0.031)	--	(0.034)	(0.035)	(0.032)	--	(0.032)	(0.035)
Innovation First Stage	--	0.348***	0.347***	0.348***	--	0.345***	0.301***	0.300***
	--	(0.042)	(0.042)	(0.044)	--	(0.041)	(0.040)	(0.043)
				A. Math				
Charter	0.496***	--	0.534***	0.517***	0.543***	--	0.618***	0.550***
	(0.071)	--	(0.077)	(0.082)	(0.079)	--	(0.073)	(0.082)
Innovation	--	-0.035	0.177	0.286*	--	-0.180	0.199	0.146
	--	(0.136)	(0.134)	(0.147)	--	(0.137)	(0.159)	(0.174)
				B. Reading				
Charter	0.127**	--	0.076	0.072	0.106	--	0.105	0.089
	(0.065)	--	(0.078)	(0.084)	(0.071)	--	(0.075)	(0.085)
Innovation	--	-0.285**	-0.231	-0.190	--	-0.203	-0.074	-0.162
	--	(0.141)	(0.153)	(0.165)	--	(0.136)	(0.161)	(0.185)
				C. Writing				
Charter	0.325***	--	0.357***	0.334***	0.324***	--	0.348***	0.393***
	(0.077)	--	(0.087)	(0.094)	(0.080)	--	(0.079)	(0.087)
Innovation	--	-0.119	0.115	0.052	--	-0.057	0.063	0.004
	--	(0.136)	(0.148)	(0.156)	--	(0.132)	(0.153)	(0.167)
N	1,102	546	1,418	1,274	1,137	613	1,583	1,274

Notes: This table reports 2SLS estimates of charter and innovation attendance effects on 2012-2013 TCAP scores of Denver 4th-10th graders. Columns 1 and 5 use the any-charter offer instrument and condition on charter-specific DA score (frequency) controls (saturated). Columns 2 and 6 use the any-innovation offer instrument and condition on innovation-specific saturated score controls. Columns 3 and 7 report coefficients from a two-endogenous, two-instrument 2SLS model for the attendance effects of charters and innovations, conditioning additively on charter-specific and innovation-specific saturated score controls. Columns 4 and 8 present similar estimates, but conditioning on interactions of charter-specific and innovation-specific saturated score controls ("joint score controls"). Test scores are standardized to have mean zero and standard deviation one within each grade based on all Denver students. All columns control for grade tested, gender, origin school charter and innovation statuses, race, gifted status, bilingual status, subsidized price lunch eligibility, special education, limited English proficient status, and test scores at baseline. Robust standard errors are reported in parentheses.
*significant at 10%; **significant at 5%; ***significant at 1%

A   Theory Appendix

A.1   Defining DA: Details

Our general formulation defines the DA allocation as determined by cutoffs found in the limit of a sequence. Recall that these cutoffs evolve according to

$$
c_s^{t+1} = \begin{cases} K+1 & \text{if } F(Q_s(\mathbf{c}^t)) < q_s, \\ \max\left\{x \in [0, K+1] \mid F(\{i \in Q_s(\mathbf{c}^t) \text{ such that } \pi_{is} \leq x\}) \leq q_s\right\} & \text{otherwise,} \end{cases}
$$

where $Q_s(\mathbf{c}^t)$ is the demand for seats at school s for a given vector of cutoffs $\mathbf{c}^t$ and is defined as

$$
Q_s(\mathbf{c}^t) = \{i \in I \mid \pi_{is} \leq c_s^t \text{ and } s \succ_i \tilde{s} \text{ for all } \tilde{s} \in S \text{ such that } \pi_{i\tilde{s}} \leq c_{\tilde{s}}^t\}.
$$

The following result confirms that these limiting cutoffs exist, i.e., that the sequence $\mathbf{c}^t$ converges.

Proposition 1. *Consider an economy described by a distribution of students F and school capacities as defined in Section 3.1. Construct a sequence of cutoffs, c_s^t, for this economy as described above. Then, $\lim_{t \to \infty} c_s^t$ exists.*

Proof. c_s^t is well-defined for all $t \geq 1$ and all $s \in S$ since it is either $K+1$ or the maximizer of a continuous function over a compact set. We will show by induction that $\{c_s^t\}$ is a decreasing sequence for all s.

For the base case, $c_s^2 \leq c_s^1$ for all s since $c_s^1 = K+1$ and $c_s^2 \leq K+1$ by construction.

For the inductive step, suppose that $c_s^t \leq c_s^{t-1}$ for all s and all $t = 1, ..., T$. For each s, if $c_s^T = K+1$, then $c_s^{T+1} \leq c_s^T$ since $c_s^t \leq K+1$ for all t by construction. Otherwise, suppose to the contrary that $c_s^{T+1} > c_s^T$. Since $c_s^T < K+1$, $F(\{i \in Q_s(\mathbf{c}^{T-1}) \text{ such that } \pi_{is} \leq c_s^T\}) = q_s$. Then,

$$
F(\{i \in Q_s(\mathbf{c}^T) \text{ such that } \pi_{is} \leq c_s^{T+1}\})
$$
$$
= F(\{i \in Q_s(\mathbf{c}^T) \text{ such that } \pi_{is} \leq c_s^T\}) + F(\{i \in Q_s(\mathbf{c}^T) \text{ such that } c_s^T < \pi_{is} \leq c_s^{T+1}\})
$$
$$
\geq F(\{i \in Q_s(\mathbf{c}^{T-1}) \text{ such that } \pi_{is} \leq c_s^T\}) + F(\{i \in Q_s(\mathbf{c}^T) \text{ such that } c_s^T < \pi_{is} \leq c_s^{T+1}\}) \tag{13}
$$
$$
\geq q_s + F(\{i \in Q_s(\mathbf{c}^T) \text{ such that } c_s^T < \pi_{is} \leq c_s^{T+1}\}) \tag{14}
$$
$$
> q_s. \tag{15}
$$

Expression (13) follows because

$$
\{i \in Q_s(\mathbf{c}^T) \text{ such that } \pi_{is} \leq c_s^T\}
$$
$$
= \{i \in I \mid \pi_{is} \leq c_s^T \text{ and } s \succ_i \tilde{s} \text{ for all } \tilde{s} \in S \text{ such that } \pi_{i\tilde{s}} \leq c_{\tilde{s}}^T\}
$$
$$
\supseteq \{i \in I \mid \pi_{is} \leq c_s^T \text{ and } s \succ_i \tilde{s} \text{ for all } \tilde{s} \in S \text{ such that } \pi_{i\tilde{s}} \leq c_{\tilde{s}}^{T-1}\} \quad \text{(by } c_{\tilde{s}}^T \leq c_{\tilde{s}}^{T-1})
$$
$$
= \{i \in Q_s(\mathbf{c}^{T-1}) \text{ such that } \pi_{is} \leq c_s^T\}.
$$

Expression (14) follows by the inductive assumption and since $c_s^T < K+1$.
Expression (15) follows since if $F(\{i \in Q_s(\mathbf{c}^T) \text{ such that } c_s^T < \pi_{is} \leq c_s^{T+1}\}) = 0$, then

$$
F(\{i \in Q_s(\mathbf{c}^{T-1}) \text{ such that } \pi_{is} \leq c_s^{T+1}\}) = F(\{i \in Q_s(\mathbf{c}^{T-1}) \text{ such that } \pi_{is} \leq c_s^T\}) \leq q_s,
$$

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while $c_s^{T+1} > c_s^T$, contradicting the definition of c_s^T.

Expression (15) contradicts the definition of c^{T+1} since the cutoff at step $T + 1$ results in an allocation that exceeds the capacity of school s. This therefore establishes the inductive step that $c_s^{T+1} \leq c_s^T$.

To complete the proof of the proposition, observe that since $\{c_s^t\}$ is a decreasing sequence in the compact interval $[0, K + 1]$, c_s^t converges by the monotone convergence theorem. $\square$

Note that this result applies to the cutoffs for both finite and continuum economies. In finite markets, at convergence, these cutoffs produce the allocation we get from the usual definition of DA (e.g., as in Gale and Shapley (1962)). This can be seen by noting that

$$\max\{x \in [0, K + 1] \mid F(\{i \in Q_s(\mathbf{c}^t) \text{ such that } \pi_{is} \leq x\}) \leq q_s\}$$
$$= \max\{x \in [0, K + 1] \mid |\{j \in Q_s(\mathbf{c}^t) : \pi_{js} \leq x\}| \leq k_s\},$$

implying that the tentative cutoff at school s in step t in our DA formulation, which is determined by the left hand side of this equality, is the same as that in Gale and Shapley (1962)'s DA formulation, which is determined by the right hand side of the equality. Our DA formulation and the Gale and Shapley (1962) formulation therefore produce the same cutoff at each step. This also implies that, in finite markets, our DA cutoffs are found in a finite number of iterations, since DA as described by Gale and Shapley (1962) converges in a finite number of steps.

A.2 Proof of Theorem 1

Admissions cutoffs $\mathbf{c}$ in a continuum economy are invariant to lottery outcomes (r_i): DA in the continuum depends on (r_i) only through $F(I_0)$ for sets $I_0 = \{i \in I \mid \theta_i \in \Theta_0\}$ with various choices of Θ_0. In particular, $F(I_0)$ doesn't depend on lottery realizations. Likewise, marginal priority $\rho_{\tilde{s}}$ is uniquely determined for every school $\tilde{s}$.

Consider the propensity score for school s. If student i does not rank s, i.e. $\theta_i \in (\Theta \backslash \Theta_s)$, then he is not assigned s. Every student of type $\theta \in \Theta_s^n$ is not assigned s because $\rho_{\theta s} > \rho_s$. Therefore $p_s(\theta) = 0$ for every $\theta \in \Theta_s^n \cup (\Theta \backslash \Theta_s)$.

Students of type $\theta \in \Theta_s^a \cup \Theta_s^c$ may be assigned $\tilde{s} \in M_{\theta s}$. The ratio of type θ students that are assigned some $\tilde{s} \in M_{\theta s}$ is given by $MID_{\theta \tilde{s}}$ since lottery numbers are distributed uniformly randomly, an assumption that we will use repeatedly in this proof without referring to it. In other words, the probability of not being assigned any $\tilde{s} \in M_{\theta s}$ for a type θ student is $1 - MID_{\theta s}$. Every student of type $\theta \in \Theta_s^a$ who is not assigned a higher choice is assigned s because $\rho_{\theta s} < \rho_s$, and therefore

$$p_s(\theta) = (1 - MID_{\theta s}) \text{ for all } \theta \in \Theta_s^a.$$

Finally, consider students of type $\theta \in \Theta_s^c$ who are not assigned a higher choice. The fraction of students $\theta \in \Theta_s^c$ who are not assigned a higher choice is $1 - MID_{\theta s}$. Also, the random numbers of these students is larger than $MID_{\theta s}$. If $\tau_s < MID_{\theta s}$, then no such student is assigned s. If $\tau_s \geq MID_{\theta s}$, then the ratio of students that are assigned s within this set is given by $\frac{\tau_s - MID_{\theta s}}{1 - MID_{\theta s}}$. Hence, conditional on $\theta \in \Theta_s^c$ and not being assigned a choice higher than s, the probability of

being assigned s is given by $\max\{0, \frac{\tau_s - MID_{\theta s}}{1 - MID_{\theta s}}\}$. Therefore,

$$p_s(\theta) = (1 - MID_{\theta s}) \times \max\left\{0, \frac{\tau_s - MID_{\theta s}}{1 - MID_{\theta s}}\right\} \text{ for all } \theta \in \Theta_s^c.$$

A.3   Proof of Theorem 2

We complete the proof of Theorem 2 in Section 3.3 by proving the following two intermediate results.

Lemma 1. *(Cutoff almost sure convergence)* $\hat{\mathbf{c}}_n \xrightarrow{a.s.} \mathbf{c}$.

Lemma 2. *(Propensity score almost sure convergence)* For all $\theta \in \Theta$ and $s \in S$, $p_{ns}(\theta) \xrightarrow{a.s.} p_s(\theta)$.

A.3.1   Proof of Lemma 1

We use the Extended Continuous Mapping Theorem (Theorem 19.1 in van der Vaart (2000)) to prove the lemma. We first show deterministic convergence of cutoffs in order to verify the assumptions of the theorem.

Modify the definition of F to describe the distribution of lottery numbers as well types: For any set of student types $\Theta_0 \subset \Theta$ and for any numbers $r_0, r_1 \in [0, 1]$ with $r_0 < r_1$, define the set of students of types in Θ_0 with random numbers worse than r_0 and better than r_1 as

$$I(\Theta_0, r_0, r_1) = \{i \in I \mid \theta_i \in \Theta_0, r_0 < r_i \le r_1\}. \tag{16}$$

In a continuum economy,

$$F(I(\Theta_0, r_0, r_1)) = E[1\{\theta_i \in \Theta_0\}] \times (r_1 - r_0),$$

where the expectation is assumed to exist. In a finite economy with n students,

$$F(I(\Theta_0, r_0, r_1)) = \frac{|I(\Theta_0, r_0, r_1)|}{n}.$$

Let $\mathcal{F}$ be the set of possible F's defined above. For any two distributions F and F', the supnorm metric is defined by

$$d(F, F') = \sup_{\Theta_0 \subset \Theta, r_0, r_1 \in [0,1]} |F(I(\Theta_0, r_0, r_1)) - F'(I(\Theta_0, r_0, r_1))|.$$

The notation is otherwise as in the text.

Proof. Consider a sequence of economies described by a sequence of distributions $\{f_n\}$ over students, together with associated school capacities, so that for all n, $f_n \in \mathcal{F}$ is a potential realization produced by randomly drawing n students and their lottery numbers from F. Assume that $f_n \to F$ in metric space $(\mathcal{F}, d)$. Let $\mathbf{c}_n$ denote the admissions cutoffs in f_n. Note the $\mathbf{c}_n$ is constant because this is the cutoff for a particular realized economy f_n.

The proof first shows deterministic convergence of cutoffs for any convergent subsequence of f_n. Let $\{\tilde{f}_n\}$ be a subsequence of realized economies $\{f_n\}$. The corresponding cutoffs are denoted $\{\tilde{\mathbf{c}}_n\}$. Let $\tilde{\mathbf{c}} \equiv (\tilde{c}_s)$ be the limit of $\tilde{\mathbf{c}}_n$. The following two claims establish that $\tilde{\mathbf{c}}_n \to \mathbf{c}$, the cutoff associated with F.

Claim 1. $\tilde{c}_s \geq c_s$ for every $s \in S$.

Proof of Claim 1. This is proved by contradiction in 3 steps. Suppose to the contrary that $\tilde{c}_s < c_s$ for some s. Let $S' \subset S$ be the set of schools the cutoffs of which are strictly lower under $\tilde{\mathbf{c}}$. For any $s \in S'$, define $I_n^s = \{i \in I | \tilde{c}_{ns} < \pi_{is} \leq c_s$ and i ranks s first$\}$ where I is the set of students in F, which contains the set of students in f_n for all n. In other words, I_n^s are the set of students ranking school s first who have a student rank in between $\tilde{c}_{ns}$ and c_s.

Step (a): We first show that for our subsequence, when the market is large enough, there must be some students who are in I_n^s. That is, there exists N such that for any $n > N$, we have $\tilde{f}_n(I_n^s) > 0$ for all $s \in S'$.

To see this, we begin by showing that for all $s \in S'$, there exists N such that for any $n > N$, we have $F(I_n^s) > 0$. Suppose, to the contrary, that there exists $s \in S'$ such that for all N, there exists $n > N$ such that $F(I_n^s) = 0$. When we consider the subsequence of realized economies $\{\tilde{f}_n\}$, we find that

$$\tilde{f}_n(\{i \in Q_s(\mathbf{c}_n) \text{ such that } \pi_{is} \leq c_s\})$$
$$= \tilde{f}_n(\{i \in Q_s(\mathbf{c}_n) \text{ such that } \pi_{is} \leq \tilde{c}_{ns}\}) + \tilde{f}_n(\{i \in Q_s(\mathbf{c}_n) \text{ such that } \tilde{c}_{ns} < \pi_{is} \leq c_s\})$$
$$= \tilde{f}_n(\{i \in Q_s(\mathbf{c}_n) \text{ such that } \pi_{is} \leq \tilde{c}_{ns}\}) \tag{17}$$
$$\leq q_s. \tag{18}$$

Expression (17) follows from Assumption 1 by the following reason. (17) does not hold, i.e., $\tilde{f}_n(\{i \in Q_s(\mathbf{c}_n) \text{ such that } \tilde{c}_{ns} < \pi_{is} \leq c_s\}) > 0$ only if $F(\{i \in I | \tilde{c}_{ns} < \pi_{is} \leq c_s\}) > 0$. This and Assumption 1 imply $F(\{i \in I | \tilde{c}_{ns} < \pi_{is} \leq c_s$ and i ranks s first$\}) \equiv F(I_n^s) > 0$, a contradiction to $F(I_n^s) = 0$. Since $\tilde{f}_n$ is realized as n iid samples from F, $\tilde{f}_n(\{i \in I | \tilde{c}_{ns} < \pi_{is} \leq c_s\}) = 0$. Expression (18) follows by our definition of DA, which can never assign more students to a school than its capacity for each of the n samples. We obtain our contradiction since $\tilde{c}_{ns}$ is not maximal at s in $\tilde{f}_n$ since expression (18) means it is possible to increase the cutoff $\tilde{c}_{ns}$ to c_s without violating the capacity constraint.

Given that we've just shown that for each $s \in S'$, $F(I_n^s) > 0$ for some n, it is possible to find an n such that $F(I_n^s) > \epsilon > 0$. Since $f_n \to F$ and so $\tilde{f}_n \to F$, there exists N such that for all $n > N$, we have $\tilde{f}_n(I_n^s) > F(I_n^s) - \epsilon > 0$. Since the number of schools is finite, such N can be taken uniformly over all $s \in S$. This completes the argument for Step (a).

Step (a) allows us to find some N such that for any $n > N$, $\tilde{f}_n(I_n^s) > 0$ for all $s' \in S'$. Let $\tilde{s}_n \in S$ and t be such that $\tilde{c}_{ns}^{t-1} \geq c_s$ for all $s \in S$ and $\tilde{c}_{n\tilde{s}_n}^t < c_{\tilde{s}_n}$. That is, $\tilde{s}_n$ is one of the first schools the cutoff of which falls strictly below $c_{\tilde{s}_n}$ under the DA algorithm in $\tilde{f}_n$, which happens in round t of the DA algorithm. Such $\tilde{s}_n$ and t exist since the choice of n guarantees $\tilde{f}_n(I_n^s) > 0$ and so $\tilde{c}_{ns} < c_s$ for all $s \in S'$.

Step (b): We next show that there exist infinitely many values of n such that the associated $\tilde{s}_n$ is in S' and $\tilde{f}_n(I_n^s) > 0$ for all $s \in S'$. It is because otherwise, by Step (a), there exists N such that for all $n > N$, we have $\tilde{s}_n \notin S'$. Since there are only finitely many schools, $\{\tilde{s}_n\}$ has a subsequence $\{\tilde{s}_m\}$ such that $\tilde{s}_m$ is the same school outside S' for all m. By definition of $\tilde{s}_n$, $\tilde{c}_{m\tilde{s}_m} \leq \tilde{c}_{m\tilde{s}_m}^t < c_{\tilde{s}_m}$ for all m and so $\tilde{c}_{\tilde{s}_m} < c_{\tilde{s}_m}$, a contradiction to $\tilde{s}_m \notin S'$. Therefore, we have our desired conclusion of Step (b).

Fix some n such that the associated $\tilde{s}_n$ is in S' and $\tilde{f}_n(l_n^s) > 0$ for all $s \in S'$. Step (b) guarantees that such n exists. Let $\tilde{A}_{n\tilde{s}_n}$ and $A_{\tilde{s}_n}$ be the sets of students assigned $\tilde{s}_n$ under $\tilde{f}_n$ and F, respectively. All students in $I_n^{\tilde{s}_n}$ are assigned $\tilde{s}_n$ in F and rejected by $\tilde{s}_n$ in $\tilde{f}_n$. Since these students rank $\tilde{s}_n$ first, there must exist a positive measure (with respect to $\tilde{f}_n$) of students outside $I_n^{\tilde{s}_n}$ who are assigned $\tilde{s}_n$ in $\tilde{f}_n$ and some other school in F; denote the set of them by $\tilde{A}_{n\tilde{s}_n} \backslash A_{\tilde{s}_n}$. $\tilde{f}_n(\tilde{A}_{n\tilde{s}_n} \backslash A_{\tilde{s}_n}) > 0$ since otherwise, for any n such that Step (b) applies,

$$\tilde{f}_n(\tilde{A}_{n\tilde{s}_n}) \leq \tilde{f}_n(A_{\tilde{s}_n} \backslash I_n^{\tilde{s}_n}) = \tilde{f}_n(A_{\tilde{s}_n}) - \tilde{f}_n(I_n^{\tilde{s}_n}),$$

which by Step (a) converges to something strictly smaller than $F(A_{\tilde{s}_n})$ since $\tilde{f}_n(A_{\tilde{s}_n}) \to F(A_{\tilde{s}_n})$ and $\tilde{f}_n(I_n^{\tilde{s}_n}) > 0$ for all large enough n by Step (a). Note that $F(A_{\tilde{s}_n})$ is weakly smaller than $q_{\tilde{s}_n}$. This implies that for large enough n, $\tilde{f}_n(\tilde{A}_{n\tilde{s}_n}) < q_{\tilde{s}_n}$, a contradiction to $\tilde{A}_{n\tilde{s}_n}$'s being the set of students assigned $\tilde{s}_n$ at a cutoff strictly smaller than the largest possible value $K + 1$. For each $i \in \tilde{A}_{n\tilde{s}_n} \backslash A_{\tilde{s}_n}$, let s_i be the school to which i is assigned under F.

Step (c): To complete the argument for Claim 1, we show that some $i \in \tilde{A}_{n\tilde{s}_n} \backslash A_{\tilde{s}_n}$ must have been rejected by s_i in some step $\tilde{t} \leq t - 1$ of the DA algorithm in $\tilde{f}_n$. That is, there exists $i \in \tilde{A}_{n\tilde{s}_n} \backslash A_{\tilde{s}_n}$ and $\tilde{t} \leq t - 1$ such that $\pi_{is_i} > \tilde{c}_{ns_i}^{\tilde{t}}$. Suppose to the contrary that for all $i \in \tilde{A}_{n\tilde{s}_n} \backslash A_{\tilde{s}_n}$ and $\tilde{t} \leq t - 1$, we have $\pi_{is_i} \leq \tilde{c}_{ns_i}^{\tilde{t}}$. Each such student i must prefer s_i to $\tilde{s}_n$ because i is assigned $s_i \neq \tilde{s}_n$ under F though $\pi_{i\tilde{s}_n} \leq \tilde{c}_{n\tilde{s}_n} < c_{\tilde{s}_n}$, where the first inequality holds because i is assigned $\tilde{s}_n$ in $\tilde{F}_n$ while the second inequality does because $\tilde{s}_n \in S'$. This implies none of $\tilde{A}_{n\tilde{s}_n} \backslash A_{\tilde{s}_n}$ is rejected by s_i, applies for $\tilde{s}$, and contributes to decreasing $\tilde{c}_{n\tilde{s}_n}^t$ at least until step t and so $\tilde{c}_{n\tilde{s}_n}^t < c_{\tilde{s}_n}$ cannot be the case, a contradiction. Therefore, we have our desired conclusion of Step (c).

Claim 1 can now be established by showing that Step (c) implies there are $i \in \tilde{A}_{n\tilde{s}_n} \backslash A_{\tilde{s}_n}$ and $\tilde{t} \leq t - 1$ such that $\pi_{is_i} > \tilde{c}_{ns_i}^{\tilde{t}} \geq \tilde{c}_{ns_i}$, where the last inequality is implies by the fact that in every economy, for all $s \in S$ and $t \geq 0$, we have $c_s^{t+1} \leq c_s^t$. Also, they are assigned s_i in F so that $\pi_{is_i} \leq c_{s_i}$. These imply $c_{s_i} > \tilde{c}_{ns_i}^{\tilde{t}} \geq \tilde{c}_{ns_i}$. That is, the cutoff of s_i falls below c_{si} in step $\tilde{t} \leq t - 1 < t$ of the DA algorithm in $\tilde{f}_n$. This contradicts the definition of $\tilde{s}_n$ and t. Therefore $\tilde{c}_s \geq c_s$ for all $s \in S$, as desired. $\square$

Claim 2. *By a similar argument, $\tilde{c}_s \leq c_s$ for every $s \in S$.*

Since $\tilde{c}_s \geq c_s$ and $\tilde{c}_s \leq c_s$ for all s, it must be the case that $\tilde{\mathbf{c}}_n \to \mathbf{c}$. The following claim uses this to show that $\mathbf{c}_n \to \mathbf{c}$.

Claim 3. *If $\tilde{\mathbf{c}}_n \to \mathbf{c}$ for every convergent subsequence $\{\tilde{\mathbf{c}}_n\}$ of $\{\mathbf{c}_n\}$, then $\mathbf{c}_n \to \mathbf{c}$.*

Proof of Claim 3. Since $\{\mathbf{c}_n\}$ is bounded in $[0, K+1]^{|S|}$, it has a convergent subsequence by the Bolzano-Weierstrass theorem. Suppose to the contrary that for every convergent subsequence $\{\tilde{\mathbf{c}}_n\}$, we have $\tilde{\mathbf{c}}_n \to \mathbf{c}$, but $\mathbf{c}_n \not\to \mathbf{c}$. Then there exists $\epsilon > 0$ such that for all $k > 0$, there exists $n_k > k$ such that $||\mathbf{c}_{n_k} - \mathbf{c}|| \geq \epsilon$. Then the subsequence $\{\mathbf{c}_{n_k}\}_k \subset \{\mathbf{c}_n\}$ has a convergent subsequence that does not converge to $\mathbf{c}$ (since $||\mathbf{c}_{n_k} - \mathbf{c}|| \geq \epsilon$ for all k), which contradicts the supposition that every convergent subsequence of $\{\mathbf{c}_n\}$ converges to $\mathbf{c}$. $\square$

The last step in the proof of Lemma 1 relates this fact to stochastic convergence.

Claim 4. $\mathbf{c}_n \to \mathbf{c}$ *implies* $\hat{\mathbf{c}}_n \xrightarrow{a.s.} \mathbf{c}$

Proof of Claim 4. This proof is based on two off-the-shelf asymptotic results from mathematical statics. First, let F_n be the distribution over $I(\Theta_0, r_0, r_1)$'s generated by randomly drawing n students from F. Note that F_n is random since is involves randomly drawing n students. $F_n \xrightarrow{a.s.} F$ by the Glivenko-Cantelli theorem (Theorem 19.1 in van der Vaart (2000)). Next, since $F_n \xrightarrow{a.s.} F$ and $\mathbf{c}_n \to \mathbf{c}$, the Extended Continuous Mapping Theorem (Theorem 18.11 in van der Vaart (2000)) implies that $\hat{\mathbf{c}}_n \xrightarrow{a.s.} \mathbf{c}$, completing the proof of Lemma 1. $\square$

A.3.2 Proof of Lemma 2

Proof. Consider any sequence of economies $\{f_n\}$ such that $f_n \in \mathcal{F}$ for all n and $f_n \to F$ in the $(\mathcal{F}, d)$ metric space. Let $p_{ns}(\theta)$ be the (finite-market, deterministic) propensity score for a particular f_n. Note that this subtly modifies the definition of $p_{ns}(\theta)$ from that in the text. The change here is that the propensity score for f_n is not a random quantity, because economy f_n is viewed as fixed.

For Lemma 2, it is enough to show deterministic convergence of this finite-market score, that is, $p_{ns}(\theta) \longrightarrow p_s(\theta)$ as $f_n \to F$. To see this, let F_n be the distribution over $I(\Theta_0, r_0, r_1)$'s induced by randomly drawing n students from F. Note that F_n is random and that $F_n \xrightarrow{a.s.} F$ by the Glivenko-Cantelli theorem (Theorem 19.1 in van der Vaart (2000)). $F_n \xrightarrow{a.s.} F$ and $p_{ns}(\theta) \to p_s(\theta)$ allow us to apply the Extended Continuous Mapping Theorem (Theorem 18.11 in van der Vaart (2000)) to obtain $\tilde{p}_{ns}(\theta) \xrightarrow{a.s.} p_s(\theta)$.

We prove convergence of $p_{ns}(\theta) \longrightarrow p_s(\theta)$ as follows. Let $\tilde{c}_{ns}$ and $\tilde{c}_{ns'}$ be the random cutoffs at s and s', respectively, in f_n, and

$$\tau_{\theta s} \equiv c_s - \rho_{\theta s},$$
$$\tau_{\theta s_-} \equiv \max_{s' \succ_\theta s}\{c_{s'} - \rho_{\theta s'}\},$$
$$\tilde{\tau}_{n\theta s} \equiv \tilde{c}_{ns} - \rho_{\theta s}, \text{ and}$$
$$\tilde{\tau}_{n\theta s_-} \equiv \max_{s' \succ_\theta s}\{\tilde{c}_{ns'} - \rho_{\theta s'}\}.$$

We can express $p_s(\theta)$ and $p_{ns}(\theta)$ as follows.

$$p_s(\theta) = \max\{0, \tau_{\theta s} - \tau_{\theta s_-}\}$$
$$p_{ns}(\theta) = P_n(\tilde{\tau}_{n\theta s} \geq R > \tilde{\tau}_{n\theta s_-})$$

where P_n is the probability induced by randomly drawing lottery numbers given f_n, and R is any type θ student's random lottery number distributed according to $U[0,1]$. By Lemma 1, with probability 1, for all $\epsilon_1 > 0$, there exists N_1 such that for all $n > N_1$,

$$|\tilde{c}_{ns'} - c_{s'}| < \epsilon_1 \text{ for all } s',$$

which implies that with probability 1,

$$
\begin{aligned}
&|\tilde{\tau}_{n\theta s_-} - \tau_{\theta s_-}| \\
=&|\{\tilde{c}_{ns_1} - \rho_{\theta s_1}\} - \{c_{s_2} - \rho_{\theta s_2}\}| \\
<&\begin{cases} |\{\tilde{c}_{ns_1} - \rho_{\theta s_1}\} - (\{\tilde{c}_{ns_1} - \rho_{\theta s_2}\} + \epsilon_1)| \text{ if } c_{s_2} - \rho_{\theta s_2} \geq \tilde{c}_{ns_1} - \rho_{\theta s_1} \\ |\{\tilde{c}_{ns_1} - \rho_{\theta s_1}\} - (\{\tilde{c}_{ns_1} - \rho_{\theta s_2}\} - \epsilon_1)| \text{ if } c_{s_2} - \rho_{\theta s_2} < \tilde{c}_{ns_1} - \rho_{\theta s_1} \end{cases} \\
=&\epsilon_1
\end{aligned}
$$

where in the first equality, $s_1 \equiv \arg\max_{s' \succ_\theta s}\{\tilde{c}_{ns'} - \rho_{\theta s'}\}$ and $s_2 \equiv \arg\max\{c_{s'} - \rho_{\theta s'}\}$. The inequality is by $|\tilde{c}_{ns'} - c_{s'}| < \epsilon_1$ for all s'. For all $\epsilon > 0$, the above argument with setting $\epsilon_1 < \epsilon/2$ implies that there exists N such that for all $n > N$,

$$
\begin{aligned}
&p_{ns}(\theta) \\
=& P_n(\tilde{\tau}_{n\theta s} \geq R > \tilde{\tau}_{n\theta s_-}) \\
\in& (\max\{0, \tau_{\theta s} - \tau_{\theta s_-} - \epsilon, \max\{0, \tau_{\theta s} - \tau_{\theta s_-} + \epsilon) \\
\in& (p_s(\theta) - \epsilon, p_s(\theta) + \epsilon),
\end{aligned}
$$

where the second-to-last inclusion is because with probability 1, there exists N such that for all $n > N$ such that $|\tilde{\tau}_{n\theta s} - \tau_{\theta s}|, |\tilde{\tau}_{n\theta s_-} - \tau_{\theta s_-}| < \epsilon_1$ and $R \sim U[0,1]$. This means $p_{ns}(\theta) \longrightarrow p_s(\theta)$, completing the proof of Lemma 2. $\qquad \square$

A.4 First Choice and Qualification Instruments: Details

Let D_i^f be the first choice instrument defined in section 4.5 and let $\tilde{s}_i$ be i's first choice school. The first choice risk set is $Q(\theta_i) \equiv (\tilde{s}_i, \rho_{i\tilde{s}})$.

Proposition 2. *In any continuum economy, D_i^f is independent of θ_i conditional on $Q(\theta_i)$.*

Proof. In general,
$$
\begin{aligned}
&\Pr(D_i^f = 1|\theta_i = \theta) \\
=& \Pr(\pi_{i\tilde{s}_i} \leq c_{\tilde{s}_i}|\theta_i = \theta) \\
=& \Pr(\rho_{i\tilde{s}_i} + r_i \leq c_{\tilde{s}_i}|\theta_i = \theta) \\
=& \Pr(r_i \leq c_{\tilde{s}_i} - \rho_{i\tilde{s}_i}|\theta_i = \theta) \\
=& c_{\tilde{s}_i} - \rho_{i\tilde{s}_i},
\end{aligned}
$$
which depends on θ_i only through $Q(\theta_i)$ because cutoffs are fixed in the continuum.. $\qquad \square$

Let D_i^q and $Q(\theta_i)$ be the qualification instrument and the associated risk set defined in section 4.5. The latter is given by the list of schools i ranks and his priority status at each, that is, $Q(\theta_i) \equiv (S_i, (\rho_{is})_{s \in S_i})$ where S_i is the set of charter schools i ranks.

Proposition 3. *In any continuum economy, D_i^q is independent of θ_i conditional on $Q(\theta_i)$.*

Proof. In general, we have
$$\Pr(D_i^q = 1 | \theta_i = \theta)$$
$$= \Pr(\pi_{is} \leq c_s \text{ for some } s \in S_i | \theta_i = \theta)$$
$$= \Pr(\rho_{is} + r_i \leq c_s \text{ for some } s \in S_i | \theta_i = \theta)$$
$$= \Pr(r_i \leq c_s - \rho_{is} \text{ for some } s \in S_i | \theta_i = \theta)$$
$$= \Pr(r_i \leq \max_{s \in S_i}(c_s - \rho_{is}) | \theta_i = \theta)$$
$$= \max_{s \in S_i}(c_s - \rho_{is}),$$
which depends on θ_i only through $Q(\theta_i)$ because cutoffs are fixed in the continuum. $\qquad \square$

A.5 Extension to a General Lottery Structure

Washington DC, New Orleans, and Amsterdam use DA with multiple lottery numbers, one for each school (see, for example, de Haan et al. (2015)). Washington, DC uses a version of DA that uses a mixture of shared and individual school lotteries. This section derives the DA propensity score for a mechanism with any sort of multiple tie-breaking.

Let a random variable R_{is} denote student i's lottery number at school s. Assume that each R_{is} is drawn from $U[0,1]$, independently with schools. We consider a general lottery structure where $R_{is} \neq R_{is'}$ for some (not necessarily all) $s, s' \in S$ and $i \in I$.

Partition $B_{\theta s}$ into $\bar{m}$ disjoint sets $B_{\theta s}^1, ..., B_{\theta s}^{\bar{m}}$, so that s' and s'' use the same lottery if and only if $s', s'' \in B_{\theta s}^m$ for some m. (Recall $B_{\theta s}$ is defined as $\{s' \in S \mid s' \succ_\theta s\}$). Partition $M_{\theta s}$ similarly, so that $M_{\theta s}^m \subset B_{\theta s}^m$ for all m. (Recall $M_{\theta s}$ is defined as $\{\tilde{s} \in B_{\theta s} \mid \rho_{\tilde{s}} = \rho_{\theta \tilde{s}}\}$). Note that these partitions are specific to type θ. With single-school lotteries, $\bar{m}$ simplifies to $|B_{\theta s}|$, the number of schools type θ ranks ahead of s.

Most informative disqualification, $MID_{\theta s}^m$, is defined for each m as

$$MID_{\theta s}^m \equiv \begin{cases} 0 & \text{if } \rho_{\theta \tilde{s}} > \rho_{\tilde{s}} \text{ for all } s' \in B_{\theta s}^m, \\ \max\{\tau_{\tilde{s}} \mid \tilde{s} \in M_{\theta s}^m\} & \text{if } \rho_{\theta \tilde{s}} \geq \rho_{\tilde{s}} \text{ for all } s' \in B_{\theta s}^m \text{ with at least one equality,} \\ 1 & \text{if } \rho_{\theta \tilde{s}} < \rho_{\tilde{s}} \text{ for some } s' \in B_{\theta s}^m. \end{cases}$$

Let m^* be the value of m for schools in the partition that use the same lottery as s. Denote the associated MID by $MID_{\theta s}^*$. We define $MID_{\theta s}^* = 0$ when the lottery at s is unique and there is no m^*. The following result extends Theorem 1 to a general lottery structure. The proof is omitted.

Theorem 1 (Generalization). *For all s and θ in any continuum economy, we have:*

$$\Pr[D_i(s) = 1 | \theta_i = \theta] = \tilde{p}_s(\theta) \equiv \begin{cases} 0 & \text{if } \theta \in \Theta_s^n, \\ \Pi_{m=1}^{\bar{m}}(1 - MID_{\theta s}^m) & \text{if } \theta \in \Theta_s^a, \\ \Pi_{m=1}^{\bar{m}}(1 - MID_{\theta s}^m) \times \max\left\{0, \dfrac{\tau_s - MID_{\theta s}^*}{1 - MID_{\theta s}^*}\right\} & \text{if } \theta \in \Theta_s^c. \end{cases}$$
(19)

where we also set $\tilde{p}_s(\theta) = 0$ when $MID_{\theta s}^* = 1$ and $\theta \in \Theta_s^c$.

Note that in the single tie breaker case, the expression for $\tilde{p}_s(\theta)$ reduces to that in Theorem 1 since $\bar{m} = 1$ in that case.

A.6   The Boston (Immediate Acceptance) Mechanism

Studies by Hastings-Kane-Staiger (2009), Hastings-Neilson-Zimmerman (2012), and Deming-Hastings-Kane-Staiger (2013)), among others, use data generated from versions of the Boston mechanism. Given strict preferences of students and schools, the Boston mechanism is defined as follows:

- Step 1: Each student applies to her most preferred acceptable school (if any). Each school accepts its most-preferred students up to its capacity and rejects every other student.

In general, for any step $t \geq 2$,

- Step t: Each student who has not been accepted by any school applies to her most preferred acceptable school that has not rejected her (if any). Each school accepts its most-preferred students up to its remaining capacity and rejects every other student.

This algorithm terminates at the first step in which no student applies to a school. Boston assignments differ DA in that any offer at any step is fixed; students receiving offers cannot be displaced later.

This important difference notwithstanding, the Boston mechanism can be represented as a special case of DA by redefining priorities as follows:

Proposition 4. *(Ergin and Sönmez (2006)) The Boston mechanism applied to $(\succ_i)_i$ and $(\succ_s)_s$ produces the same assignment as DA applied to $(\succ_i)_i$ and $(\succ_s^*)_s$ where $\succ_s^*$ is defined as follows:*

1. *For $k = 1, 2...,$ $\{$students who rank s k-th$\} \succ_s^* \{$students who rank s $k + 1$-th$\}$*

2. *Within each category, $\succ_s^*$ ranks the students in the same order as original $\succ_s$.*

This equivalence allows us to construct a Boston propensity score by redefining priorities so that priority groups at a given school consists of applicants who (i) share the same original priority status at the school and (ii) give the same rank to the school.

B Data Appendix and Additional Empirical Results

B.1 Data Appendix

The Denver Public Schools (DPS) analysis file is constructed using application, school assignment, enrollment, demographic, and outcome data provided by DPS for school years 2011-2012 and 2012-2013. All files are de-identified, but students can be matched across years and files. Applicant data are from the 2012-2013 SchoolChoice assignment file and test score data are from the CSAP (Colorado Student Assessment Program) and the TCAP (Transitional Colorado Assessment Program) files. The CSAP was discontinued in 2011, and was replaced by the TCAP beginning with the 2012-2013 school year. Enrollment, demographic, and outcome data are available for students enrolled in DPS only; enrollment data are for October.

Applications and assignment: The SchoolChoice file

The 2012-2013 SchoolChoice assignment file contains information on applicants' preferences over schools (school rankings), school priorities over applicants, applicants' school assignments (offers) and lottery numbers, a flag for whether the applicant is subject to the family link policy described in the main text and, if so, to which sibling the applicant is linked. Each observation in the assignment file corresponds to an applicant applying for a seat in programs within schools known as a bucket.[29] Each applicant receives at most one offer across all buckets at a school. Information on applicant preferences, school priorities, lottery numbers, and offers are used to compute the DA propensity score and the simulated propensity score.

Appendix Table B1 describes the construction of the analysis sample starting from all applicants in the 2012-2013 SchoolChoice assignment file. Out of a total of 25,687 applicants seeking a seat in DPS in the academic year 2012-2013, 5,669 applied to any charter school seats in grades 4 through 10. We focus the analysis on applicants to grades 4 through 10 because baseline grade test scores are available for these grades only. We further limit the sample to 4,964 applicants who were enrolled in DPS in the baseline grade (the grade prior to the application grade) in the baseline year (2011-2012), for whom baseline enrollment demographic characteristics are available.

Enrollment and demographic characteristics

Each observation in the enrollment files describes a student enrolled in a school in a year, and includes information on grade attended, student sex, race, gifted status, bilingual status, special education status, limited English proficiency status, and subsidized lunch eligibility.[30] Demo-

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[29]Since applicants' rankings are at the school-level but seats are assigned at the bucket level, the SchoolChoice assignment mechanism translates school-level rankings into bucket-level rankings. For example, if an applicant ranked school A first and school B second, and if all seats at both A and B are split into two categories, one for faculty children ("Faculty") and one for any type of applicant ("Any"), then the applicant's ranking of the programs at A and B would be listed as 10 for Faculty at A, 11 for Any at A, 20 for Faculty at B, 21 for Any at B where numbers code preferences (smaller is more preferred).

[30]Race is coded as black, white, asian, hispanic, and other. In DPS these are mutually-exclusive categories.

graphic and enrollment information are from the first calendar year a student spent in each grade.

Applicant outcomes: CSAP/TCAP

Test scores and proficiency levels for the CSAP/TCAP math, reading, and writing exams are available for grades 3 through 10. Each observation in the CSAP/TCAP data file corresponds to a student's test results in a particular subject, grade, and year. For each grade, we use scores from the first attempt at a given subject test, and exclude the lowest obtainable scores as outliers. As a result, 41 observed math scores, 19 observed reading scores, and 1 observed writing score are excluded from the sample of charter applicants that are in DPS in baseline year. After outlier exclusion, score variables are standardized to have mean zero and unit standard deviation in a subject-grade-year in the DPS district.

School classification: Parent Guide

We classify schools as charters, traditional public schools, magnet schools, innovation schools, contract schools, or alternative schools (i.e. intensive pathways and multiple pathways schools) according to the 2012-2013 Denver SchoolChoice Parent Guides for Elementary and Middle Schools and High Schools. School classification is by grade, since some schools run magnet programs for a few grades only. Schools not included in the Parent Guide (i.e. SIMS Fayola International Academy Denver) were classified according to information from the school's website.

Table B1: SchoolChoice application records

	All applicants		In DPS at baseline	
	Applicants	Types	Applicants	Types
	(1)	(2)	(3)	(4)
All applicants	25,687	16,087	15,487	9,564
Applicants to grades 4 through 10	12,507	7,480	10,898	6,642
Applicants to any charters (grades 4 through 10)	5,669	4,833	4,964	4,282

Notes: All applications are for the 2012-2013 academic year. Columns 1 and 2 include all applicants in the SchoolChoice assignment file (see Data Appendix for details). Columns 3 and 4 exclude applicants who were not in DPS at the baseline grade (the grade prior to application grade) and baseline year (2011-2012). Applicants to grade "EC" (early childhood, or pre-kindergarten) are excluded from columns 3 and 4 because there is no baseline grade for those applicants. Columns 2 and 4 count unique combinations of applicant preferences over school programs and school priorities in those programs.

Table B2: Differential attrition

	Non-offered mean (1)	No controls (2)	Propensity score controls			
			Linear (3)	Hundredths (4)	Saturated (5)	Ten Thousandths (6)
A. DA score (frequency)						
Enrolls in Denver in follow-up year	0.905	0.029***	0.041**	0.040**	0.038**	
		(0.008)	(0.019)	(0.019)	(0.019)	
Has scores in follow-up year	0.881	0.032***	0.050**	0.049**	0.048**	
		(0.009)	(0.020)	(0.020)	(0.021)	
N	2,939	4,964	1,436	1,289	1,247	
B. DA score (formula)						
Enrolls in Denver in follow-up year	0.905	0.029***	0.036**	0.027	0.031	
		(0.008)	(0.017)	(0.018)	(0.020)	
Has scores in follow-up year	0.881	0.032***	0.032*	0.026	0.038*	
		(0.009)	(0.018)	(0.020)	(0.022)	
N	2,939	4,964	1,508	1,472	1,224	
C. Simulated score						
Enrolls in Denver in follow-up year	0.905	0.029***	0.037**	0.040**		0.037*
		(0.008)	(0.018)	(0.019)		(0.022)
Has scores in follow-up year	0.881	0.032***	0.040**	0.043**		0.046**
		(0.009)	(0.020)	(0.021)		(0.023)
N	2,939	4,964	1,523	1,290		1,112

Notes: This table reports coefficients from regressions of the availability of follow-up variables on each row on an any-charter school offer receivership dummy. Follow-up variables are observed for academic year 2012-2013. Only applicants to 2012-2013 charter seats in grades 4-10 who were enrolled in Denver at baseline grade are included. Charter offer equals one if a student is accepted into any charter school, excluding alternative charters. Column 1 reports baseline characteristics of charter applicants who did not receive a charter offer. Coefficients in columns 3-6 control for the probability of assignment to a charter school according to different functional forms and probability computation methods, and exclude applicants with propensity score equal to zero (i.e. ineligible) or one (i.e. guaranteed). Controls in column 4 are dummies for rounded values in the propensity score support, rounding to the hundredth. Controls in column 5 are dummies for rounded values of the simulated score, rounding to the thousandth. Controls in column 6 are dummies for every value in the propensity score support. Column 7 controls for fully saturated student types (that is, the profile of all ranked schools and student priorities at the ranked schools). Robust standard errors are reported in parentheses.
*significant at 10%; **significant at 5%; ***significant at 1%

Table B3a: Application covariate balance

	Non-offered mean (1)	No controls (2)	DA score (formula)		
			Linear (3)	Hundredths (4)	Saturated (5)
Number of schools ranked	4.375	-0.341***	-0.317***	-0.056	-0.001
		(0.046)	(0.093)	(0.086)	(0.094)
Number of charter schools ranked	1.425	0.476***	0.062	0.016	0.002
		(0.024)	(0.043)	(0.041)	(0.044)
First school ranked is charter	0.291	0.612***	0.003	-0.005	-0.007
		(0.011)	(0.023)	(0.020)	(0.019)
N	2,939	4,964	1,508	1,472	1,224
Risk set points of support			156	43	58
Robust F-test for joint significance		1190	8.06	0.47	0.05
p-value		0.000	0.000	0.701	0.986

Notes: This table reports coefficients from regressions of the application characteristics on each row on an any-charter school offer receivership dummy. Only applicants to 2012-2013 charter seats in grades 4-10 who were enrolled in Denver at baseline grade are included. Charter offer equals one if a student is accepted into any charter school, excluding alternative charters. For column 3, risk set points of support count unique values in the support of the respective propensity scores. For column 4, risk set points of support count the number of bins or cells (as defined by dummies for each value of the saturated score or as dummies for student types) with offer instrument variation. Column 1 reports baseline characteristics of charter applicants who did not receive a charter offer. Coefficients in columns 3-5 control for the DA score (formula) of being assigned to a charter school according to different functional forms, and exclude applicants with propensity score equal to zero (i.e. ineligible) or one (i.e. guaranteed). Controls in columns 4 are dummies for rounded values in the propensity score support, rounding to the hundredth. Controls in column 5 are dummies for every value in the propensity score support. Robust standard errors are reported in parentheses. P-values for robust joint significance tests are estimated by stacking outcomes and clustering standard errors at the student level.

*significant at 10%; **significant at 5%; ***significant at 1%

Table B3b: Baseline covariate balance

		Non-offered mean	No controls	DA score (formula)		
				Linear	Hundredths	Saturated
		(1)	(2)	(3)	(4)	(5)
Origin school is charter		0.086	0.108***	0.085***	-0.012	-0.037**
			(0.010)	(0.022)	(0.017)	(0.017)
Female		0.520	-0.005	0.014	0.041	0.020
			(0.014)	(0.030)	(0.032)	(0.035)
Race						
Hispanic		0.595	0.095***	-0.004	-0.031	0.003
			(0.014)	(0.028)	(0.028)	(0.029)
Black		0.183	-0.033***	-0.008	0.008	-0.009
			(0.011)	(0.024)	(0.025)	(0.027)
Gifted		0.203	-0.028**	-0.047**	-0.040*	-0.036
			(0.011)	(0.023)	(0.024)	(0.027)
Bilingual		0.289	0.086***	0.021	-0.002	0.010
			(0.014)	(0.029)	(0.030)	(0.033)
Subsidized lunch		0.767	0.073***	-0.007	0.011	0.002
			(0.011)	(0.024)	(0.023)	(0.026)
Limited English proficient		0.289	0.086***	0.021	-0.002	0.010
			(0.014)	(0.029)	(0.030)	(0.033)
Special education		0.084	0.004	0.027*	0.036**	0.033*
			(0.008)	(0.016)	(0.017)	(0.018)
	N	2,939	4,964	1,508	1,472	1,224
Baseline scores						
Math		0.022	-0.002	0.018	-0.049	-0.080
			(0.027)	(0.056)	(0.057)	(0.063)
Reading		0.040	-0.085***	-0.023	-0.067	-0.100*
			(0.026)	(0.053)	(0.053)	(0.057)
Writing		0.035	-0.072***	-0.039	-0.068	-0.108*
			(0.026)	(0.051)	(0.051)	(0.055)
	N	2,891	4,889	1,491	1,455	1,213
Robust F-test for joint significance			19.1	2.38	1.16	1.18
p-value			0.000	0.005	0.309	0.290

Notes: This table reports coefficients from regressions of the baseline characteristics on each row on an any-charter school offer receivership dummy. Only applicants to 2012-2013 charter seats in grades 4-10 who were enrolled in Denver at baseline grade are included. Test scores are standardized to have mean zero and standard deviation one within each grade based on all Denver students. Charter offer equals one if a student is accepted into any charter school, excluding alternative charters. Column 1 reports baseline characteristics of charter applicants who did not receive a charter offer. Coefficients in column 3-5 control for the DA score (formula) of being assigned to a charter school according to different functional forms, and exclude applicants with propensity score equal to zero (i.e. ineligible) or one (i.e. guaranteed). Controls in columns 4 are dummies for rounded values in the propensity score support, rounding to the hundredth. Controls in column 5 are dummies for every value in the propensity score support. Robust standard errors are reported in parentheses. P-values for robust joint significance tests are estimated by stacking outcomes and clustering standard errors at the student level.

*significant at 10%; **significant at 5%; ***significant at 1%

Table B4: DPS Charter attendance effects on standardized scores without covariate controls

| | 2SLS estimates | | | | | |
| | DA score | | | | | |
	Frequency (saturated) (1)	Formula (saturated) (2)	Simulation (hundredths) (3)	No score controls (4)	OLS (5)	OLS with score controls (6)
First stage	0.399***	0.376***	0.367***	0.734***		
	(0.032)	(0.032)	(0.032)	(0.011)		
Math	0.339**	0.363**	0.409**	0.239***	0.285***	0.457***
	(0.148)	(0.158)	(0.162)	(0.039)	(0.028)	(0.067)
Math: Reduced form	0.135**	0.137**	0.150**	0.176***		
	(0.062)	(0.063)	(0.063)	(0.029)		
Reading	-0.102	-0.108	-0.091	-0.050	0.039	0.158***
	(0.136)	(0.144)	(0.150)	(0.038)	(0.027)	(0.059)
Reading: Reduced form	-0.041	-0.041	-0.034	-0.037		
	(0.056)	(0.056)	(0.057)	(0.028)		
Writing	0.116	0.134	0.140	0.052	0.127***	0.282***
	(0.137)	(0.144)	(0.150)	(0.039)	(0.027)	(0.059)
Writing: Reduced form	0.046	0.051	0.052	0.038		
	(0.057)	(0.056)	(0.057)	(0.028)		
N	1,102	1,083	1,137	4,317	4,317	1,102

Notes: This table reports 2SLS and OLS estimates of charter attendance effects on 2012-2013 TCAP scores of Denver 4th-10th graders using the SchoolChoice any-charter offer instrument. Test scores are standardized to have mean zero and standard deviation one within each grade based on all Denver students. Column 6 estimates OLS using DA score (frequency) controls (saturated). Columns 1-6 control for no covariates but grade tested. Robust standard errors are reported in parentheses.
*significant at 10%; **significant at 5%; ***significant at 1%

Table B5: Comparison of 2SLS and semiparametric estimates of charter effects

	DA score					
	Frequency (saturated)			Formula (saturated)		
	2SLS	Abadie	Hirano	2SLS	Abadie	Hirano
	(1)	(2)	(3)	(4)	(5)	(6)
First stage	0.410***	0.357***	0.352***	0.389***	0.340***	0.334***
	(0.032)	(0.034)	(0.036)	(0.031)	(0.032)	(0.036)
	{0.031}			{0.032}		
Math	0.496***	0.437***	0.443***	0.524***	0.478***	0.486***
	(0.076)	(0.117)	(0.120)	(0.071)	(0.117)	(0.119)
	{0.071}			{0.076}		
Reading	0.127*	0.104	0.106	0.120*	0.116	0.118
	(0.065)	(0.129)	(0.131)	(0.073)	(0.130)	(0.132)
	{0.065}			{0.069}		
Writing	0.325***	0.322***	0.326***	0.356***	0.359***	0.364***
	(0.079)	(0.116)	(0.119)	(0.082)	(0.123)	(0.127)
	{0.077}			{0.080}		
N	1,102	1,093	1,093	1,083	1,081	1,081

Notes: This table compares 2SLS estimates with estimates computed using semi-parametric propensity score weighting, based on Abadie (2002) and Hirano, Imbens, and Ridder (2003). For details, see Angrist and Rokkanen (2015). Standard errors in parentheses use a Bayesian bootstrap. The table also shows conventional robust standard errors for 2SLS estimates in braces.
*significant at 10%; **significant at 5%; ***significant at 1%

Table B6: DPS innovation schools

School	Total applicants (1)	Applicants offered seats (2)	DA score (frequency) (3)	DA score (formula) (4)	Simulated (5)
			Propensity score in (0,1)		
Elementary and middle schools					
Cole Arts and Science Academy	31	15	11	9	10
DCIS at Ford	16	0	0	0	1
DCIS at Montbello	412	125	163	156	170
Denver Green School	153	62	29	46	52
Godsman Elementary	10	8	0	0	0
Green Valley Elementary	53	15	3	23	35
Martin Luther King Jr. Early College	427	177	117	120	121
McAuliffe International School	406	165	91	115	112
McGlone	14	2	1	4	3
Montclair Elementary	15	11	2	1	1
Noel Community Arts School	288	108	92	97	105
Valdez Elementary	6	3	0	1	1
Whittier K-8 School	47	8	1	3	4
High schools					
Collegiate Preparatory Academy	433	125	173	158	153
DCIS at Montbello	506	125	208	169	174
High-Tech Early College	481	125	209	193	214
Manual High School	390	130	152	159	187
Martin Luther King Jr. Early College	515	144	179	151	162
Noel Community Arts School	334	78	112	112	107

Notes: This table shows application patterns at innovation schools. Column 1 is the number of applicants ranking each school. Columns 3-5 are restricted to applicants with propensity score values that are neither zero (i.e. ineligible for a seat) nor one (i.e. guaranteed a seat) according to different score computation methods. Elementary and middle schools are those serving grades 4-8. High schools are those serving grades 9 and 10.

Table B7: Covariate balance and differential attrition for DPS innovation schools

	Non-offered mean (1)	No controls (2)	DA score (frequency) Linear (3)	Hundredths (4)	Saturated (5)	Simulated score Linear (6)	Hundredths (7)	Ten Thousandths (8)
			A. Application covariates					
Number of schools ranked	4.657	-0.142**	0.164	0.012	0.034	0.135	0.132	0.190
		(0.058)	(0.119)	(0.107)	(0.106)	(0.114)	(0.110)	(0.158)
Number of innovation schools ranked	1.279	0.710***	0.192**	0.086	0.035	0.121	0.092	0.097
		(0.035)	(0.079)	(0.062)	(0.059)	(0.076)	(0.069)	(0.118)
First school ranked is innovation	0.052	0.611***	-0.003	-0.007	-0.005	-0.030	-0.030	-0.043
		(0.015)	(0.036)	(0.022)	(0.018)	(0.032)	(0.027)	(0.037)
			B. Baseline covariates					
Origin school is innovation	0.116	0.125***	0.032	0.045	0.044	0.010	0.040	0.100*
		(0.015)	(0.034)	(0.036)	(0.036)	(0.033)	(0.034)	(0.053)
Female	0.526	-0.011	0.030	0.028	0.028	0.063	0.060	0.077
		(0.020)	(0.046)	(0.047)	(0.049)	(0.044)	(0.048)	(0.087)
Race								
Hispanic	0.491	0.136***	0.028	0.015	-0.001	0.037	0.043	0.039
		(0.020)	(0.045)	(0.044)	(0.046)	(0.044)	(0.044)	(0.077)
Black	0.262	-0.064***	0.018	0.018	0.030	0.003	0.009	0.023
		(0.017)	(0.038)	(0.039)	(0.041)	(0.036)	(0.040)	(0.071)
Gifted	0.198	-0.056***	-0.019	-0.028	-0.041	0.017	0.020	0.008
		(0.015)	(0.034)	(0.035)	(0.036)	(0.033)	(0.035)	(0.062)
Bilingual	0.018	0.007	-0.025	-0.027*	-0.029*	-0.020	-0.014	-0.006
		(0.006)	(0.016)	(0.016)	(0.015)	(0.015)	(0.015)	(0.029)
Subsidized lunch	0.763	0.047***	0.029	0.034	0.016	0.011	0.013	-0.044
		(0.016)	(0.037)	(0.036)	(0.037)	(0.037)	(0.036)	(0.061)
Limited English proficient	0.253	0.047***	0.016	0.032	0.031	0.007	-0.001	-0.030
		(0.018)	(0.041)	(0.042)	(0.043)	(0.041)	(0.043)	(0.085)
Special education	0.092	0.004	-0.021	-0.031	-0.036	-0.026	-0.037	-0.050
		(0.012)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.062)
N	1,176	2,483	769	717	623	888	705	279
Baseline scores								
Math	-0.017	-0.186***	-0.032	-0.018	-0.057	0.023	0.042	0.030
		(0.040)	(0.091)	(0.087)	(0.088)	(0.088)	(0.091)	(0.158)
Reading	0.036	-0.220***	-0.066	-0.047	-0.047	-0.013	0.002	0.015
		(0.038)	(0.084)	(0.082)	(0.084)	(0.080)	(0.083)	(0.153)
Writing	0.000	-0.163***	0.025	0.041	0.030	0.079	0.081	0.119
		(0.038)	(0.085)	(0.082)	(0.084)	(0.082)	(0.084)	(0.165)
N	1,158	2,434	752	704	614	869	689	273
Robust F-test for joint significance		143	1.10	0.99	0.91	0.80	0.92	1.52
p-value		0.000	0.354	0.457	0.548	0.669	0.535	0.102
			C. Differential attrition					
Enrolls in Denver in follow-up year	0.920	-0.001	-0.017	-0.012	-0.011	-0.015	-0.020	-0.008
		(0.011)	(0.026)	(0.027)	(0.029)	(0.024)	(0.027)	(0.044)
Has scores in follow-up year	0.897	-0.011	-0.019	-0.014	-0.018	-0.008	-0.017	0.018
		(0.012)	(0.027)	(0.029)	(0.030)	(0.026)	(0.029)	(0.051)
N	1,176	2,483	769	717	623	888	705	279

Notes: This table reports coefficients from regressions of the baseline characteristics and follow-up variables on each row on an any-innovation school offer receivership dummy. Only applicants to 2012-2013 innovation seats in grades 4-10 who were enrolled in Denver at baseline grade are included. Test scores are standardized to have mean zero and standard deviation one within each grade based on all Denver students. Innovation offer equals one if a student is accepted into any innovation school. Column 1 reports baseline characteristics of innovation applicants who did not receive an innovation offer. Coefficients in columns 3-8 control for the probability of assignment to an innovation school according to different functional forms and probability computation methods, and exclude applicants with propensity score equal to zero (i.e. ineligible) or one (i.e. guaranteed). Controls in columns 4 and 7 group observations into 200 bins of 0.5 percentage-point size each. Coefficients in columns 5 and 8 are estimated by fully saturating propensity score values, with scores in column 8 rounded to the nearest ten-thousandth decimal point. Robust standard errors are reported in parentheses. P-values for robust joint significance tests are estimated by stacking outcomes and clustering standard errors at the student level.
*significant at 10%; **significant at 5%; ***significant at 1%