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 families in large urban school districts now have the option to send their children to any public school in their district. The fact that some schools are more popular than others, and the need to distinguish students who have higher priority at a given school (perhaps because a sibling is enrolled) from those with lower priority generates a two-sided matching problem. Cities increasingly use deferred acceptance and other centralized matchmaking algorithms to assign students to schools. Originally proposed by Gale and Shapley (1962) and Shapley and Scarf (1974), economic matchmaking via market design allocates valuable resources in markets where prices cannot be relied upon to perform this function (Abdulkadiroğlu and Sönmez, 2003; Roth, 2015). As of this writing, Boston, Charlotte, Denver, New Orleans, Newark, New York City, San Francisco, Washington DC, and many European and Asian cities use market design to assign students to schools.

Two benefits of matchmaking schemes like DA are efficiency and fairness: the resulting assignments improve welfare 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 and Sönmez, 2003; 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 over-subscribed, 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, magnets, and special programs.

Earlier 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 the regression-discontinuity (RD)-style tie-breaking that distinguishes between applicants who apply to selective exam schools. These studies include evaluations of exam schools in the US and in Chile, Ghana, Kenya, Romania, Trinidad, and Tobago.[1] Although causal effects in these studies are identified by compelling sources of quasi-experimental variation, the research designs deployed in this work fail to exploit the full potential of random or RD-style tie-breaking embedded in market design matchmaking.

A stumbling block in the use of market-design for impact evaluation is the elaborate multi-stage nature of market-design solutions. 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 assuredly independent of potential outcomes. In view of this diffi-

[1] References here include Dobbie and Fryer (2014); Abdulkadiroğlu et al. (2014); Ajayi (2013); Lucas and Mbiti (2014); Pop-Eleches and Urquiola (2013); Jackson (2010); Bergman (2014); Hastings et al. (2013); Kirkeboen et al. (2015)

culty, 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 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 (the propensity score). As in other stratified randomized research designs, we can then condition on this scalar propensity score to eliminate 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 from consideration, while score-based stratification leaves the 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. We therefore develop an analytic 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 an asymptotic approximation based on 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 offers elsewhere are degenerate; these facts need not be revealed by naive 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]

[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

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 combines charters with traditional public schools in a joint match.

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 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. (2014)).

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 under a lottery structure that involves 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.[3] Students report their preferences by ranking schools on an application form or website, while schools rank students by

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 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. (2011).

[3] Most students in a district stay at the school where they've been enrolled previously. Those participating in the assignment are either new entrants to the district, students facing a transition such as from middle to high school, or students interested in switching schools.

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 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 is no new application and all tentative assignments are finalized (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 effective sample size available for impact analysis. 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

$$\begin{aligned}
&1 : a \succ b, \\
&2 : a \succ b, \\
&3 : a, \\
&4 : c \succ a, \\
&5 : c,
\end{aligned}$$

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 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.[4]

An important asymptotic result in the econometric theory of the estimation of treatment effects is that with discrete covariates, 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 cannot be true in small samples or a finite population. Moreover, the efficiency cost due to full covariate conditioning in finite samples exceeds that due to the data lost to singleton 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.[5]

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

[4] As a originally defined by Rosenbaum and Rubin (1983), the propensity score is a population probability, determined by an experimental design and unrelated to the particular sampling scheme by which data might be collected. The probabilities we have in mind are defined by repeatedly drawing lottery numbers and running DA in a fixed population of students and schools.

[5] Closely related discussions include Rosenbaum (1987); Rubin and Thomas (1996); Heckman et al. (1998); Hirano et al. (2003).

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 stylized example.

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

$$\begin{aligned} &1 : c, \\ &2 : c \succ b \succ a, \\ &3 : b \succ a, \\ &4 : a. \end{aligned}$$

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 a, 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.[6] With large n, enumeration of assignment possibilities is a chore. We can, however, simulate the propensity score by repeatedly drawing lottery numbers.

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

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

[6] An increasing number of 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. (2009); 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).

with the probability of assignment at a for types 2 and 3 converging quickly. This convergence is a consequence of a result we prove in the next section, which shows how, with many students 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 randomization, while for others, assignment risk is degenerate. A single feature of the large market characterization is the central 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, we need only be concerned with what happens at the school at which it's easiest to qualify among those schools that an applicant prefers to a. 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 determining 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, $\rho_{is} = 2$ might encode the fact that student s has sibling priority at school s, while $\rho_{is} = 3$ encodes neighborhood priority, and applicants with siblings are prioritized ahead of those from the neighborhood. We use $\rho_{is} = \infty$ to indicate that i is ineligible for school s. Students often 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 model a school assignment mechanism using 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 $E[1\{\theta_i \in \Theta_0\}]$ means 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 these distribution functions, 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. Ultimately, we will show that conditioning on the propensity score for a continuum analog of a finite economy (the 2012 DPS match) eliminates omitted variables bias as effectively as would conditioning on the relevant finite-market propensity score. This provides an empirical illustration of our principal theoretical claim, which is that the propensity score for the continuous analog of a finite economy offers an easily computed and empirical useful asymptotic approximation to the relevant finite-market score.

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 and 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 $K+1$ and cleared by all applicants eligible for the school. 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 (Gale and Shapley (1962)).

3.2 Characterizing the 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 the fraction of type θ students assigned s in the continuum economy, or induced by re-randomization of lottery numbers for a fixed set of applicants and schools in a finite economy) 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 and potentially subject to random assignment at s.

The propensity score defines the set of students who have some chance of being seated at s, but are not certain to end up there. In addition to falling into the marginal priority group, these students have failed to qualify for seats at schools they prefer to s. Failure to qualify at these schools can be described as a function of school-specific *lottery cutoffs*. The lottery cutoff at school s, denoted τ_s, is the decimal part of the cutoff at s, that is, $\tau_s = c_s - \rho_s$. Lottery cutoffs are defined within marginal priority groups for each school.

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 as follows:

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

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 lottery number according to

$$MID_{\theta s} \equiv \begin{cases} 1 & \text{if } c_{s'} = K+1 \text{ for some } s' \in B_{\theta s}, \\ \max\{\tau_{\tilde{s}} \mid \tilde{s} \in M_{\theta s}\} & \text{if } c_{s'} < K+1 \text{ for all } s' \in B_{\theta s} \text{ and } M_{\theta s} \neq \emptyset, \\ 0 & \text{otherwise.} \end{cases}$$

$MID_{\theta s}$ tells us where the lottery number distribution among applicants to s is cut off by qualification at schools these applicants prefer to s. For example, $MID_{\theta s}$ is zero when the priority status of type θ students is worse-than-marginal at all higher ranked schools, because there's no lottery number truncation in this case. On the other hand, when at least one school in $B_{\theta s}$ is under-subscribed, the cutoff at that school is $K + 1$, so the lottery cutoff is 1 and $MID_{\theta s} = 1$.

We call $MID_{\theta s}$ the *most informative* disqualification because 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 they've ranked ahead of s with a cutoff of 0.5 fail to qualify at schools they prefer to s with cutoffs below 0.5. To keep track of the truncation induced by disqualification at all schools an applicant prefers to s, we need record only the most forgiving cutoff that an applicant fails to clear.

In finite markets, $MID_{\theta s}$ varies from realization to realization, 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. 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 $\tau_b = 0.75$.

It remains to integrate the truncation captured by $MID_{\theta s}$ with marginal priorities. To that end, let Θ_s denote the set of student types who rank s and partition Θ_s according to

i) $\Theta_s^- = \{\theta \in \Theta_s \mid \rho_{\theta s} > \rho_s \text{ or } \rho_{\theta\tilde{s}} < \rho_{\tilde{s}} \text{ for some } \tilde{s} \in B_{\theta s}\}$,

ii) $\Theta_s^+ = \{\theta \in \Theta_s \mid \rho_{\theta s} < \rho_s \text{ and } \rho_{\theta\tilde{s}} \geq \rho_{\tilde{s}} \text{ for every } \tilde{s} \in B_{\theta s}\}$, and

iii) $\Theta_s^* = \{\theta \in \Theta_s \mid \rho_{\theta s} = \rho_s \text{ and } \rho_{\theta\tilde{s}} \geq \rho_{\tilde{s}} \text{ for every } \tilde{s} \in B_{\theta s}\}$

The set Θ_s^- contains applicant types who either have worse-than-marginal priority at s or who clear marginal priority at a higher ranked choice. No one in this group is assigned to s. Θ_s^+ contains applicant types that clear marginal priority at s, while failing to clear marginal priority at higher ranked choices. Because some of these applicants may be marginal at higher ranked choices, they are assigned s only if they fail to find a seat at any school they've ranked more highly. Finally, Θ_s^* is the subset of Θ_s that fails to clear marginal priority at higher ranked choices and is marginal at s. In this group, too, some maybe be marginal at higher ranked choices. These applicants are therefore assigned s only if they are not assigned a higher choice and have a random number that clears the lottery cutoff at s.

In finite markets, lottery cutoffs, marginal priority status, and $MID_{\theta s}$ are all random, 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 for an 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:*

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

The proof appears in Appendix A.2. This formulation aligns with the usual notion of a propensity score: $\tilde{p}_s(\theta)$ describes fixed features of the conditional-on-type assignment distribution independent of lottery numbers.

By way of intuition for $\tilde{p}_s(\theta)$, note first that the DA propensity score reflects the two key components described above: (1) random assignment at s occurs partly as a consequence of not being seated a school preferred to s; (2) for those not seated at a more preferred school, assignment at s is determined by draws from the truncated distribution of lottery numbers remaining after eliminating applicants seated at schools they've ranked more highly. Applying these principles in the continuum allows us to simplify as follows:

i) Type Θ_s^- applicants have a DA score of zero because these applicants have either worse-than-marginal priority at s or they clear marginal priority at a more highly-ranked school.

ii) The probability of assignment at s is $1 - MID_{\theta s}$ for applicants in Θ_s^+ 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. Note that no one in Θ_s^+ has $MID_{\theta s} = 1$, because this implies clearance of marginal priority at a higher-ranked choice and hence membership in Θ_s^-.

iii) Applicants in Θ_s^* 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^+, the proportion in Θ_s^* left for consideration at s is $1 - MID_{\theta s}$ (again, no one here has $MID_{\theta s} = 1$). Applicants in Θ_s^* 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 $\frac{\tau_s - MID_{\theta s}}{1 - MID_{\theta s}}$.

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_a^*. 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), we have

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

The DA propensity score is a simple function of a small number of intermediate quantities, specifically, $MID_{\theta s}, \tau_s$, 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. Our 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 $\{q_s\}$, 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:*

$$\sup_{\theta \in \Theta, s \in S} |\hat{p}_{ns}(\theta) - p_{ns}(\theta)| \xrightarrow{a.s.} 0.$$

Proof. The proof use two intermediate results, given as lemmas in Appendix A. The first step 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.[7] That is,

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

where $\mathbf{c}$ denotes the continuum economy cutoffs (Lemma 1 in Appendix A). This result, together with the continuous mapping theorem, implies that each $\hat{MID}_{\theta s,n}$ computed in a sampled finite economy converges to the corresponding $MID_{\theta s}$ in the continuum economy as n grows, since the sampled $\hat{MID}_{\theta s,n}$ are continuous functions of $\mathbf{c}_n$. Moreover, since $\hat{p}_{ns}(\theta)$ is an almost-everywhere continuous function of the $\hat{MID}_{\theta s,n}$ and cutoffs $\hat{\mathbf{c}}_n$,

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

In other words, the DA 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 $s \in S$,

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

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). Hence, the propensity score estimated using Theorem 1 approaches the actual propensity score in the limit. □

Theorem 2 justifies our use of the sample analog of the formula in Theorem 1 to control for student type in empirical work looking at school attendance effects. 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 Strategies

Because DA-generated offers of school seats are generated by randomly assigned lottery numbers conditional on type, they provide compelling instruments for the causal effects of school attendance. Specifically, we use DPS SchoolChoice first-round charter offers as instruments for eventual charter 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

[7] 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.

are not directly informative about charter enrollment effects on applicants not offered a charter seat in round 1, but who nevertheless find their way into a charter in the second round (LATE always-takers).

To flesh out the nature of this interpretation and the assumptions it rests on, let C_i be a charter attendance 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–local average treatment effects–are described by conditioning on potential treatment status. Potential treatment 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, however, 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}.[8]

In addition to conditional independence of charter offers and potential outcomes, we also assume that, conditional on the propensity score, offers cause 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:

$$\frac{E[Y_i|D_i = 1, p_D(\theta_i) = x] - E[Y_i|D_i = 0, p_D(\theta_i) = x]}{E[C_i|D_i = 1, p_D(\theta_i) = x] - E[C_i|D_i = 0, p_D(\theta_i) = x]} = E[Y_{1i} - Y_{0i}|p_D(\theta_i) = x, C_{1i} > C_{0i}], \quad (2)$$

[8] More formally, we're asserting the following joint conditional independence/exclusion restriction:

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

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 (randomly assigned) lottery number clears the relevant constant cutoffs. The case for the exclusion restriction is less immediate because, under centralized assignment with a single random tie-breaker, the same lottery number can affect opportunities at schools other than those where a seat is offered. Suppose, for example, that applicants take the time to research a school only after learning they've been offered a seat there. Those offered a seat at a investigate it and learn they don't like it, but by this time a second choice, b, is full, and only an under-subscribed fallback, c, has empty seats in the second round. In this scenario, no one goes to a and offers at a induce some to switch from b to c. This violates charter offer exclusion if applicants have heterogenous potential outcomes at b and c. Implicitly, therefore, the exclusion restriction requires us to rule out such consequential preference reversals, or to limit heterogeneity among control schools. The later point is addressed in Section 4.6 by allowing for an additional treatment channel.

where x indexes values in the support of the score.

Given the proliferation of treatment effects generated by cell-by-cell application of (2), 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

$$C_i = \sum_x \gamma(x) d_i(x) + \delta D_i + \nu_i, \tag{3}$$

$$Y_i = \sum_x \alpha(x) d_i(x) + \beta C_i + \epsilon_i, \tag{4}$$

where the $d_i(x)$'s are dummies indicating values of the DA propensity score, 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): Because the covariate parameterization here is saturated, $E[D_i|p_D(\theta_i) = x]$ is linear in x. The 2SLS estimand defined by equations (3) and (4) 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.[9]

Alternative Estimation Strategies

We compare 2SLS estimates computed using DA offer instruments to the results generated by two IV strategies employed in earlier studies that use centralized assignment mechanisms to identify causal effects of school attendance. The first exploits information on offers at schools that students have ranked first, ignoring lower-ranked schools.[10] The second uses an instrument determined solely by qualification, that is, whether a student has a lottery number below the highest number offered a seat at any school he or she has ranked, ignoring more specific information on preferences and offers received.[11]

In our large-market framework, both of these instruments provide conditional random assignment. To see this for first-choice instruments, note that outside the marginal priority group at a, the probability of an offer at a is zero or one. In the marginal priority group, DA offers a seat to those who rank a first with probability τ_a, without regard to the identity of schools applicants have ranked lower. Because cutoffs are fixed and unrelated to applicant type in large markets, empirical strategies using first-choice offers as an instrument need only condition on the identity of the school ranked first in samples of applicants in the marginal priority group.

[9] This conclusion is implied by Proposition 5.1 in Abadie (2003). The unrestricted local 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.

[10] This strategy is used in 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)).

[11] Dobbie and Fryer (2014), Lucas and Mbiti (2014), and Pop-Eleches and Urquiola (2013) use qualification instruments.

The argument for qualification instruments proceeds similarly. As with first-choice instruments, the qualification strategy for the effect of attendance at a discards students outside the marginal priority group for a. In the marginal priority group, student i is said to be qualified at school a when he has ranked a and if $\rho_{ia} + r_{ia} \leq c_a$, that is, when the sum of his priority and random number at a is less than or equal to the admissions cutoff at a. Because qualification is necessary (though not sufficient) for assignment to a, an indicator for qualification at a is likely to be correlated with offers and attendance there. Moreover, because cutoffs are fixed under repeated lottery draws in the large market, the qualification propensity score is the same for all types, regardless of how a is ranked by applicants in the relevant marginal priority group.[12]

Although valid for causal inference, we expect both first-choice and qualification instruments to generate second-stage estimates that are markedly less precise than those produced by the DA offer-IV strategy described by equations (3) and (4). Both qualification and first-choice instruments are compromised by the fact that they fail to isolate the subsample where compliance rates with offers are highest, that is, the sample of applicants at risk of assignment at a who have failed to qualify at schools they've ranked higher. Moreover, because first-choice instruments necessarily fail to exploit information on random assignment at school a when a is ranked second or lower, they may fail to identify causal effects at schools for which our strategy produces a useful first stage.

Multiple Treatments

We use this same 2SLS framework to estimate the effects of attendance at different types of schools. This extension is motivated by the fact that the DPS district enrolls students in a number of different types of schools besides traditional and charter schools. Most importantly, in addition to a large charter sector, DPS includes many "innovation" schools. These are schools that operate under an innovation plan that waives some provisions of the relevant collective bargaining agreements. These 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.[13] The details of our two-treatments analysis are outlined following the presentation of the estimated charter treatment effects arising from a single-treatment model.

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 first round.[14]

[12]The question of whether the first-choice and qualification instruments are valid in finite markets is more subtle; In finite markets, realized cutoffs may be correlated with type.

[13]DPS innovation schools appear to have much in common with Boston's pilot schools, a model examined in Abdulkadiroğlu et al. (2011).

[14]The second round allows families unhappy with first-round assignment to apply directly to schools. In practice, few submit second-round applications. Information for parents is posted online at `http://schoolchoice.dpsk12.org`.

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 of subsidized-lunch students and for children of school staff.[15] The DA assignment is also specific to grades.

DPS 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 and buckets rather than schools, while the relevant propensity score captures the probability of offers at these sub-schools.[16] 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 must be constructed.

4.1 Computing the DA Propensity Score

The score estimates used as controls in first- and second-stage equations (3) and (4) 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.[17] 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 score use simulated score 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^-, Θ_s^+, or Θ_s^*. The DA score, $\tilde{p}_s(\theta)$ is then estimated by

[15] 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. For example, a school that reserves seats for staff children is treated as two sub-schools, one with priorities that ignore this consideration and one giving highest priority to staff children.

[16] For more on DA with slot-specific priorities, see Kominers and Sönmez (2014) and Dur et al. (2014). 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.

[17] 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.

computing the sample analog of $MID_{\theta s}$ and τ_s in the DPS assignment 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, which we refer to as a "frequency" calculation, is given by the offer rate in cells defined by these variables. The frequency score is closer to an estimated score of the sort discussed by 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.[18] 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.[19]

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 with very 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* at a much smaller set of schools. This can be seen in the last column of Table 1, which reports the number of applicants who ranked the school first and have a simulated score implying a risk of assignment strictly between zero and one. The reduction in randomization scope 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

[18] 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. "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.

[19] 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.

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. For example, STRIVE Prep - GVR had 116 applicants randomized, even though Table 1 shows that no one with non-degenerate offer risk ranked this school first. Random assignment at GVR is a consequence of the many GVR applicants found in Θ_s^+, the group for which random assignment is determined solely by failure to obtain an offer at schools 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 columns 9 and 10 of the table) that all randomized GVR applicants fall into Θ_s^+, with no one randomized at the GVR cutoff.

In contrast with STRIVE's GVR school, few applicants were randomized at STRIVE's Highland and Lake campuses because applicants ranking these schools were likely to clear marginal priority at schools they had ranked more highly (thereby falling into Θ_s^-, producing a score of zero) or at the schools themselves (falling into Θ_s^+ and with $MID_{\theta s} = 0$, producing a score of one). Interestingly, STRIVE's Federal campus is the only STRIVE school to lose randomized applicants to a low cutoff in the marginal priority group: 110 Federal applicants in Θ_s^*, 40% of the Θ_s^* partition, are lost to random assignment because $MID_{\theta s} \geq \tau_s$. We could learn more about the impact of attendance at STRIVE Federal by changing the cutoff there (e.g., by changing capacity), whereas such a change would be of little consequence for evaluations of Highland and Lake.

A broad picture of DPS random assignment appears in Figure 2. Panel (a) captures the information in columns 3 and 6 in 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 assignment.

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.[20] 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.

[20]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 (3).

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.[21]

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 found in published analyses of a conventional randomized

[21] 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.

trial. Specifically, Table 6 documents balance for the realized DPS assignment by reporting the usual t and F statistics for offer gaps.[22] 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).

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.

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 legitimately viewed as fixed, and therefore not a source of uncertainty.[23]

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.

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).

[22]Table 6 reports the results controlling for the DA propensity score (frequency) and the simulated propensity score. The results under the DA propensity score (formula) are in Appendix Table B3.

[23]See Rosenbaum (2002) for more on the distinction between these modes of inference.

Aside from tiny simulation error, the simulated score can be viewed 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. Here too, 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.[24]

4.4 Effects of Charter Enrollment

A DA-generated charter offer boosts charter middle school attendance rates by 0.42-0.45, depending on whether formula or frequency estimates are used to control for the propensity score. These first-stage estimates (computed with saturated controls for the score included in equation (3)) are reported in the first row of Table 7, which also shows a middle school first stage estimate of 0.43 constructed using a simulated score (rounded to the nearest hundredth). At around one-third, the first stage for high school charter applicants, reported in the first row of Panel B in the table, is noticeably smaller than that for middle school. Here too, first stage estimates are similar across alternative score estimators. First stage estimates of around 0.67 computed without score controls, shown in column 4 of the table, are clearly biased upwards.[25]

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 that aren't significantly different from zero. The math gains reported here are similar to those found for charter middle school students in Boston (see, for example, Angrist et al. (2015), Angrist et al. (2012)). 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 middle school writing scores. Across subjects, 2SLS estimates for high school are markedly less precise than those for middle school, a natural consequence of the smaller high school applicant sample

[24] 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. As the estimated charter attendance effects discussed below are similar when computed using either type of score control, it seems unlikely that differential attrition is a source of bias in our 2SLS estimates.

[25] The middle school sample for Table 7 includes DPS 4th-8th graders enrolled in 2012-13; the high school sample includes 9th and 10th graders. The middle and high school samples used for IV estimation are limited to charter applicants with propensity scores in the unit interval, for which score cells have offer variation (this is not necessarily implied by the unit interval restriction for formula and simulated scores since it is possible that applicants with a nontrivial propensity score, say 0.1, experience no offer variation under realized offers). The OLS estimation sample includes charter applicants, ignoring score- and cell-variation restrictions. First- and second-stage estimates in this table 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.

and first stage.[26]

Importantly for our methodological agenda, Table 7 shows charter impact estimates that are largely invariant to whether the score is estimated by simulation or by a frequency or formula calculation that uses Theorem 1. Compare, for example, math middle school impact estimates of 0.525, 0.527, and 0.559 using frequency-, formula-, and simulation-based score controls, all estimated with similar precision. This alignment validates the use of Theorem 1 to construct score controls.

Estimates that omit propensity score controls entirely highlight the risk of selection bias in a naive empirical strategy. This is documented in column 4 of Table 7, which shows that 2SLS estimates constructed using DA offer instruments without control for the propensity score are too small by about half. A corresponding set of OLS estimates without propensity score controls, reported in column 5 of the table, are also too small. On the other hand, including score controls in the OLS model for high school students pulls the middle school estimate back up, close to the corresponding 2SLS estimates. This suggests that the primary source of selection bias in OLS estimates for high school applicants is omitted preferences and priorities rather than unobserved differences in potential achievement between those who do and don't enroll at a charter school.

4.5 Other IV Strategies

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 both a precision gain and an increase in the number of schools represented in the estimation sample relative to these two previously-employed IV strategies.

Let $Q(\theta_i)$ be a variable that uniquely identifies the charter 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:

$$Y_i = \sum_x \alpha(x) 1\{Q(\theta_i) = x\} + \beta C_i + \epsilon_i \tag{5}$$

$$C_i = \sum_x \gamma(x) 1\{Q(\theta_i) = x\} + \delta D_i^f + \nu_i, \tag{6}$$

where the summation index, x, runs over all possible values of $Q(\theta_i)$. The first-choice instrument, D_i^f, is a dummy variable indicating i's qualification at the first-choice school. In other words,

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

[26]Standard errors for the 2SLS estimates reported here ignore the fact that the DA propensity score is estimated. 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 Table 7, 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.

First choice qualification is the same as first choice offer since under DA, applicants who have ranked a first are offered a seat at a if and only if they qualify at a.[27]

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 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.[28] The qualification instrument, D_i^q, indicates qualification at *any* charter he or she has ranked. In other words,

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

Under the same sequence used to establish Theorem 1, the instruments D_i^f and D_i^q can be shown to be asymptotically independent of type conditional on $Q(\theta_i)$.[29]

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 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). Note also that 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.[30]

[27] Specifically, all applicants who clear marginal priority qualify and are offered a seat; applicants with less than marginal priority are unqualified and not offered a seat; applicants having marginal priority are offered a seat with probability determined by τ_s, the school-specific lottery cutoff in the marginal priority group.

[28] 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.

[29] See Appendix A.4 for details.

[30] 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.

Interestingly, the first-choice estimate of the effect of charter enrollment 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 likely reflects the fact that 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 do not produce randomization for first-choice applicants alone (as seen in Table 1 and Figure 2).

School-impact heterogeneity here 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 captures causal effects across all schools applied to, yielding an estimand that would appear to be more representative of "typical charter attendance effects."

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.

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 multinomial variable, W_i, which denotes the school sector student i enrolls in. 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 to improve student outcomes (for details and a descriptive evaluation of innovation schools, see Connors et al. (2014)). 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 DPS assignment (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 over 85% in the group offered a charter seat, along with substantial but much smaller charter enrollment in the non-offered group.[31] Perhaps surprisingly, only around 40% of those

[31] Applicants unhappy with the offer they've receive in the first round of the Denver assignment may apply to

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 alternative to traditional public schools.

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 (3), control for the charter offer 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 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 enrollment in traditional public schools.

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

Counterfactual 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.[32]

Column 5 of Table 9 shows that, among middle- and elementary-school applicants, 64% of charter lottery compliers 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 a little over 20% of non-offered compliers enrolling in one of these.

Innovation schools dominate the destiny distribution for high school applicants: two thirds of high school compliers wind up in an innovation school when not offered a charter seat, while only 13% of this group ends up in a traditional public school. This probably reflects the fact that even for non-traditional sectors, choices are heavily neighborhood-based. In particular, the non-charter high school options nearest to the largest charter high school, DSST: Green Valley

schools individually in a second round. This process produces charter offers for those not offered a charter seat initially.

[32] Briefly, our procedure puts $(1 - C_i)1(W_i = j)$ on the left hand side of a version of equation (4) 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.

Ranch, are innovation schools. Disappointed Green Valley applicants are therefore likely to have ranked an innovation school highly as well.

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, which we label β_1 and β_2. By pulling innovation schools out of the non-charter achievement outcome, β_1 is driven mainly by the contrast between charter and traditional public schools. Of course, the innovation treatment effect, β_2, is also of interest in its own right.

Innovation and charter enrollment effects are separately identified by a 2SLS procedure with two endogenous variables, C_i^1 for charter school enrollment and C_i^2 for innovation school enrollment. Specifically, we estimate

$$Y_i = \sum_x [\alpha_1(x) d_i^1(x) + \alpha_2(x) d_i^2(x)] + \beta_1 C_i^1 + \beta_2 C_i^2 + \epsilon_i, \tag{7}$$

$$C_i^1 = \sum_x [\gamma_{11}(x) d_i^1(x) + \gamma_{12}(x) d_i^2(x)] + \delta_{11} D_i^1 + \delta_{12} D_i^2 + \nu_i, \tag{8}$$

$$C_i^2 = \sum_x [\gamma_{21}(x) d_i^1(x) + \gamma_{22}(x) d_i^2(x)] + \delta_{21} D_i^1 + \delta_{22} D_i^2 + \eta_i, \tag{9}$$

where the dummy control variables, $d_i^1(x)$ and $d_i^2(x)$, saturate the estimated propensity scores, $\hat{p}_1(\theta_i)$ and $\hat{p}_2(\theta_i)$, for each treatment. In other words, $\hat{p}_1(\theta_i)$ and $\hat{p}_2(\theta_i)$ estimate $E[D_i^1|\theta_i]$ and $E[D_i^2|\theta_i]$, and

$$d_i^1(x) = 1[\hat{p}_1(\theta_i) = x], \tag{10}$$

$$d_i^2(x) = 1[\hat{p}_2(\theta_i) = x], \tag{11}$$

with the index, x, running over all possible values in the union of the supports for the two scores. The sample used for this analysis is the union of charter and innovation school applicants.

As a benchmark, columns 1-2 of Table 10 compare charter-only and innovation-only estimates computed using DA (frequency) score controls, computed in the relevant applicant sub-samples.[33] Like the estimates in Table 8, these differ from the 2SLS estimates of charter effects reported in Table 7 in that they pool all grades. A parallel set of benchmark 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 8. 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), the estimated effects of innovation school attendance are much less precise than are the corresponding charter estimates. This imprecision notwithstanding, the innovation-only models generate a large negative and marginally significant innovation school effect on reading when estimated with the DA score.

[33] Appendix Table B4 lists innovation schools and describes the rand om assignment pattern at these schools in the same format as Table 1 for charter schools. Covariate balance and differential attrition results for innovation schools are reported in Appendix Table B5.

2SLS estimates of equation (7) 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 model. Most interestingly, perhaps, the marginally significant positive charter school effect on reading 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 somewhat smaller.

Finally, its worth noting that the use of additive propensity score controls in equation (7) is justified by an additive causal model. Specifically, the additive setup presumes that $Y_i - \beta_1 C_i^1$ is independent of D_i^2 conditional on applicant type and vice versa. This is a simplifying assumption motivated by constant causal effects. In general, propensity score conditioning with a multinomial treatment should fix the conditional probability of assignment for all treatment levels jointly (Imbens, 2000). We therefore also report results from an estimation strategy that replaces additive score controls in equations (7), (8), and (9) with joint 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 the indices, x^1 and x^2, run independently over all values in the support for each score.

The results of estimation with joint score controls, reported in columns 4 and 8 of Table 10, differ little from estimates constructed using additive controls 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 while removing 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 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 effect estimates 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.

The methods developed here should be broadly applicable to markets in which allocations use the DA family of mechanisms for centralized assignment. There's nothing special about markets that match schools and students, except, perhaps, accessible high-quality applicant and outcome data. At the same time, other markets and matches use mechanisms not covered by the DA framework. Most important on this list of extensions 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 OneApp, which has experimented with TTC matching.

Our analysis leaves a number of other important areas unexplored. In particular, we've focused here on defining and estimating the DA propensity score, giving less attention to the problem of how best to use the score for estimation. Simple 2SLS procedures seem to work well, but it's natural to integrate the score with more modern semiparametric IV strategies such as have been detailed by Abadie (2003). The analysis here also avoids the complications of non-random tie-breaking. Many markets involving DA break ties with a non-random running variable. The question of how best to define and exploit a DA propensity score for markets that combine regression-discontinuity designs with market design is a natural next step on our research design and market design agenda.

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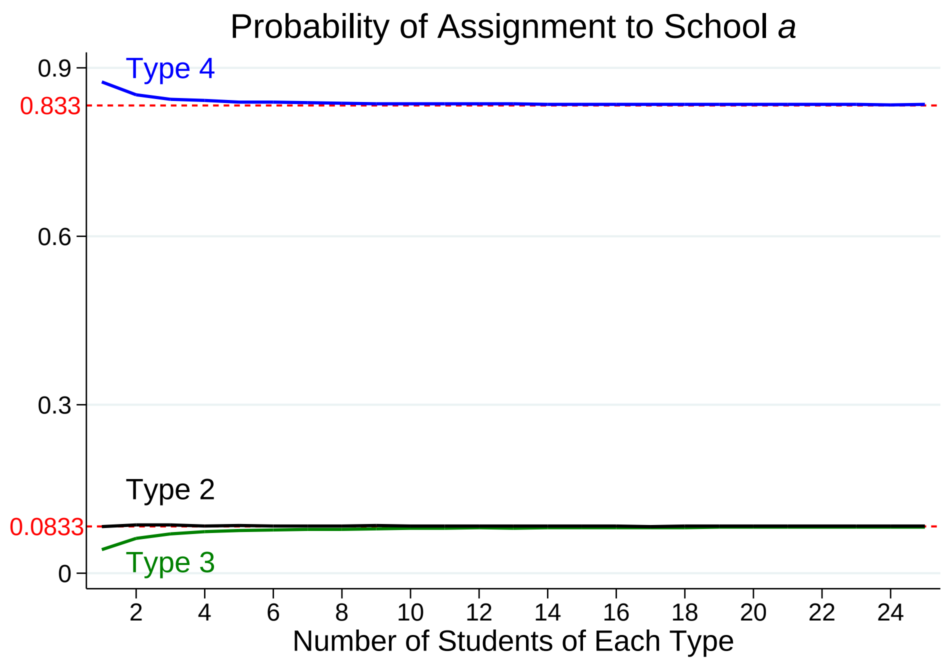
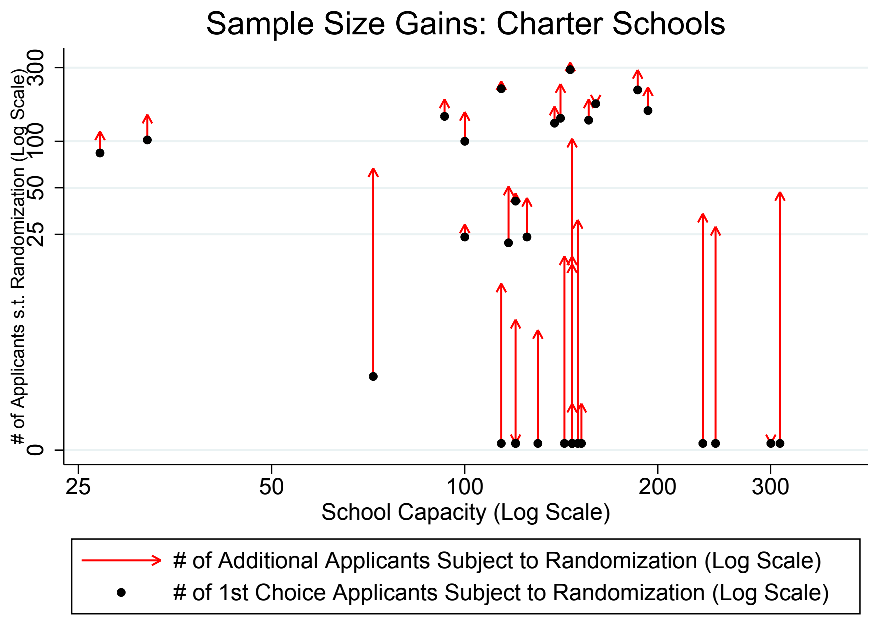
Figure 1: Propensity Scores and Market Size in in Example 2

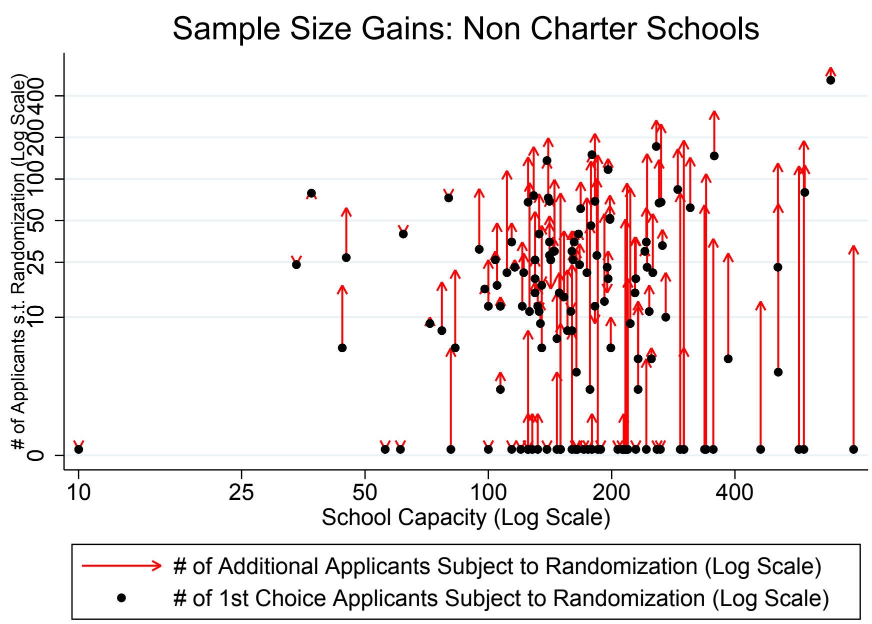


Notes: This figure plots finite-market propensity scores for expansions of Example 2 in Section 2.2. For each value of the x axis, we consider an expansion of the example with x students of each type. The propensity scores plotted here were computed by drawing lottery numbers 100,000 times.

Figure 2: Sample Size Gains from the Propensity Score Strategy



(a)



(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	Applicants offered seats	Propensity score in (0,1): DA score (frequency)	DA score (formula)	Simulated score	Simulated score (first choice)
	(1)	(2)	(3)	(4)	(5)	(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.

Table 2: DA score anatomy

School	Eligible applicants (1)	Capacity (2)	Offers (3)	Randomized seats (4)	DA Score = 1: In Θ_s^+, MID = 0 (5)	DA Score = 0: In Θ_s^-, $\rho_{\theta \acute{s}} < \rho_{\acute{s}}$ (6)	DA Score = 0: In Θ_s^-, $\rho_{\theta s} > \rho_s$ (7)	DA Score = 0: In Θ_s^*, MID $\geq \tau_s$ (8)	DA Score in (0,1): In Θ_s^+, 0 < MID < 1 (9)	DA Score in (0,1): In Θ_s^*, MID < τ_s (10)
STRIVE Prep - GVR	324	147	112	63	49	159	0	0	116	0
STRIVE Prep - Highland	244	147	112	10	102	121	0	0	21	0
STRIVE Prep - Lake	274	147	126	10	116	132	0	0	26	0
STRIVE Prep - Federal	574	138	138	101	37	222	33	110	1	171

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 and column 4 describes the number of seats that were randomly assigned. Total offers equals randomized seats plus certain offers, i.e., offers made to applicants with a DA score of one (reported in column 5). Columns 6-8 show the number of applicants in partitions with a score of zero, as detailed in the formal statement of Theorem 1. Likewise, columns 9 and 10 show the number of applicants subject to random assignment, distinguishing between those randomized in Θ_s^+ and those randomized in Θ_s^-.

Table 3: Baseline characteristics of DPS students

	Denver students	SchoolChoice applicants	Charter applicants	Propensity score in (0,1): DA score (frequency): All applicants	Propensity score in (0,1): DA score (frequency): Enroll at charter	Propensity score in (0,1): Simulated score: All applicants	Propensity score in (0,1): Simulated score: Enroll at charter
	(1)	(2)	(3)	(4)	(5)	(6)	(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	No controls	Propensity score controls					
			DA score (frequency)			Simulated score		
			Linear	Hundredths	Saturated	Linear	Hundredths	Ten Thousandths
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(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						
		DA score (frequency)			Simulated score			
	No controls	Linear	Hundredths	Saturated	Linear	Hundredths	Ten Thousandths	Full applicant type controls
	(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	1189.785	2.699	1.699	1.091	0.492	1.259	0.311	0.343
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	19.139	1.199	1.133	0.992	1.041	1.351	0.709
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)	Formula (saturated)	Simulated score (hundredths)	No score controls	OLS	OLS with score controls
	(1)	(2)	(3)	(4)	(5)	(6)
	A. Elementary and middle school					
First stage	0.442***	0.422***	0.425***	0.688***		
	(0.038)	(0.038)	(0.039)	(0.014)		
Math	0.525***	0.527***	0.559***	0.329***	0.302***	0.347***
	(0.077)	(0.080)	(0.081)	(0.023)	(0.016)	(0.037)
Reading	0.163**	0.098	0.094	0.133***	0.134***	0.125***
	(0.074)	(0.077)	(0.077)	(0.023)	(0.016)	(0.035)
Writing	0.386***	0.356***	0.348***	0.221***	0.204***	0.254***
	(0.088)	(0.092)	(0.090)	(0.027)	(0.018)	(0.045)
N	705	708	732	2,991	2,991	705
	B. High school					
First stage	0.342***	0.308***	0.311***	0.673***		
	(0.054)	(0.056)	(0.056)	(0.026)		
Math	0.456***	0.509***	0.510***	0.241***	0.312***	0.478***
	(0.160)	(0.187)	(0.193)	(0.046)	(0.033)	(0.078)
Reading	0.055	0.157	0.044	-0.002	0.010	0.009
	(0.130)	(0.152)	(0.151)	(0.039)	(0.027)	(0.053)
Writing	0.114	0.281*	0.181	0.083**	0.099***	0.042
	(0.147)	(0.161)	(0.160)	(0.041)	(0.029)	(0.060)
N	397	375	405	1,326	1,326	397

Notes: This table reports 2SLS and OLS estimates of charter attendance effects on 2012-2013 TCAP scores of Denver 4th-8th graders (panel A) and 9th-10th graders (panel B) 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				
	Offer instrument with DA score (frequency) controls (saturated)	First choice charter offer with risk set controls	Qualification instrument with risk set controls	Sample size increase for equivalent gain (col 2 vs col 1)	Sample size increase for equivalent gain (col 3 vs col 1)
	(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)
			A. Elementary and middle school			
Enrolled in a study charter	0.178	0.868	0.359	0.801	--	1.000
… in a traditional public	0.394	0.078	0.317	0.036	0.636	--
… in an innovation school	0.219	0.024	0.178	0.086	0.208	--
… in a magnet school	0.180	0.024	0.108	0.070	0.085	--
… in an alternative school	0.008	0.003	0.006	0.006	0.002	--
… in a contract school	0.019	0.004	0.029	0.000	0.064	--
… in a non-study charter	0.001	0.000	0.003	0.001	0.005	--
N	1,631	1,397	315	705	--	--
			B. High school			
Enrolled in a study charter	0.091	0.858	0.328	0.670	--	1.000
… in a traditional public	0.423	0.092	0.153	0.107	0.134	--
… in an innovation school	0.261	0.021	0.350	0.123	0.663	--
… in a magnet school	0.213	0.014	0.131	0.095	0.104	--
… in an alternative school	0.010	0.014	0.038	0.004	0.101	--
… in a contract school	0.000	0.002	0.000	0.001	0.000	--
N	924	436	183	397	--	--

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,

$$\begin{aligned}
&F(\{i \in Q_s(\mathbf{c}^T) \text{ such that } \pi_{is} \leq c_s^{T+1}\}) \\
&\quad = F(\{i \in Q_s(\mathbf{c}^T) \text{ such that } \pi_{is} \leq c_s^T\}) + F(\{i \in Q_s(\mathbf{c}^T) \text{ such that } c_s^T < \pi_{is} \leq c_s^{T+1}\}) \\
&\quad \geq F(\{i \in Q_s(\mathbf{c}^{T-1}) \text{ such that } \pi_{is} \leq c_s^T\}) + F(\{i \in Q_s(\mathbf{c}^T) \text{ such that } c_s^T < \pi_{is} \leq c_s^{T+1}\}) && (12) \\
&\quad \geq q_s + F(\{i \in Q_s(\mathbf{c}^T) \text{ such that } c_s^T < \pi_{is} \leq c_s^{T+1}\}) && (13) \\
&\quad > q_s. && (14)
\end{aligned}$$

Expression (12) follows because

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

Expression (13) follows by the inductive assumption and since $c_s^T < K+1$.
Expression (14) 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

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

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

Expression (14) 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. □

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 $\theta \in \Theta_s^-$ is either assigned a school that he ranks higher than s because $\rho_{\theta\tilde{s}} < \rho_{\tilde{s}}$ for some $\tilde{s} \in B_{\theta s}$ or is not assigned s because $\rho_{\theta s} > \rho_s$. Therefore $p_s(\theta) = 0$ for every $\theta \in \Theta_s^- \cup (\Theta \backslash \Theta_s)$.

Students of type $\theta \in \Theta_s^* \cup \Theta_s^+$ 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 students is $1 - MID_{\theta s}$. Every student of type $\theta \in \Theta_s^+$ who is not assigned a higher choice is assigned s because $\rho_{\theta s} < \rho_s$, and therefore

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

Finally, consider students of type $\theta \in \Theta_s^*$ who are not assigned a higher choice. The fraction of students $\theta \in \Theta_s^*$ 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 $\theta \in \Theta_s^*$ 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,

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

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

$$I(\Theta_0, r_0, r_1) = \{i \in I \mid \theta_i \in \Theta_0, r_0 < r_i \leq r_1\}. \tag{15}$$

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 $I_n^s = \{i \in I | \tilde{c}_{ns} < \pi_{is} \leq c_s \text{ and } i \text{ ranks } s \text{ first}\}$ 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

$$\begin{aligned}
&\tilde{f}_n(\{i \in Q_s(\mathbf{c}_n) \text{ such that } \pi_{is} \leq c_s\}) \\
&\quad = \tilde{f}_n(\{i \in Q_s(\mathbf{c}_n) \text{ such that } \pi_{is} \leq \tilde{c}_{ns}\}) + \tilde{f}_n(\{i \in Q_s(\mathbf{c}_n) \text{ such that } \tilde{c}_{ns} < \pi_{is} \leq c_s\}) \\
&\quad = \tilde{f}_n(\{i \in Q_s(\mathbf{c}_n) \text{ such that } \pi_{is} \leq \tilde{c}_{ns}\}) && (16) \\
&\quad \leq q_s. && (17)
\end{aligned}$$

Expression (16) follows from Assumption 1 by the following reason. (16) 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 $F(\{i \in I | \tilde{c}_{ns} < \pi_{is} \leq c_s \text{ and } i \text{ ranks } s \text{ first}\}) \equiv F(I_n^s) > 0$, 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 (17) 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 (17) 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 $\tilde{s}_n$, $\tilde{c}_{m\tilde{s}_m} \leq \tilde{c}^t_{m\tilde{s}_m} < c_{\tilde{s}_m}$ 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,

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

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 $i \in \tilde{A}_{n\tilde{s}_n} \setminus A_{\tilde{s}_n}$, 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 $i \in \tilde{A}_{n\tilde{s}_n} \setminus A_{\tilde{s}_n}$ 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 $i \in \tilde{A}_{n\tilde{s}_n} \setminus A_{\tilde{s}_n}$ and $\tilde{t} \leq t-1$ such that $\pi_{is_i} > \tilde{c}^{\tilde{t}}_{ns_i}$. Suppose to the contrary that for all $i \in \tilde{A}_{n\tilde{s}_n} \setminus A_{\tilde{s}_n}$ and $\tilde{t} \leq t-1$, we have $\pi_{is_i} \leq \tilde{c}^{\tilde{t}}_{ns_i}$. 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 $\tilde{A}_{n\tilde{s}_n} \setminus A_{\tilde{s}_n}$ is rejected by s_i, applies for $\tilde{s}$, and contributes to decreasing $\tilde{c}^t_{n\tilde{s}_n}$ at least until step t and so $\tilde{c}^t_{n\tilde{s}_n} < c_{\tilde{s}_n}$ 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 $i \in \tilde{A}_{n\tilde{s}_n} \setminus A_{\tilde{s}_n}$ and $\tilde{t} \leq t - 1$ such that $\pi_{is_i} > \tilde{c}^{\tilde{t}}_{ns_i} \geq \tilde{c}_{ns_i}$, 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 $c_{s_i} > \tilde{c}^{\tilde{t}}_{ns_i} \geq \tilde{c}_{ns_i}$. 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. □

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 $\mathbf{c}$. □

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. $F_n \overset{a.s.}{\to} F$ by the Glivenko-Cantelli theorem (Theorem 19.1 in van der Vaart (2000)). Next, since $F_n \overset{a.s.}{\to} F$ 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. □

A.3.2 Proof of Lemma 2

Proof. We use the Extended Continuous Mapping Theorem to prove Lemma 2. 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. With a slight abuse of notation, let $p_{ns}(\theta)$ be the propensity score in f_n; $p_{ns}(\theta)$ is deterministic because it is the propensity score for a particular economy f_n. For Lemma 2, it is enough to show $p_{ns}(\theta) \longrightarrow p_s(\theta)$: 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. $F_n \overset{a.s.}{\to} F$ by the Glivenko-Cantelli theorem (Theorem 19.1 in van der Vaart (2000)). $F_n \overset{a.s.}{\to} F$ 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 the desired convergence $p_{ns}(\theta) \longrightarrow p_s(\theta)$ 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, there exists event Ω_1 with $\Pr(\omega \in \Omega_1) = 1$ in the underlying probability space such that for all $\omega \in \Omega_1$ and all $\epsilon_1 > 0$, there exists N_1 such that for all $n > N_1$,

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

which implies

$$\begin{aligned}
&|\tilde{\tau}_{n\theta s_-}(\omega) - \tau_{\theta s_-}| \\
&(\text{where } \tilde{\tau}_{n\theta s_-}(\omega) \text{ is the realization of } \tilde{\tau}_{n\theta s_-} \text{ under } \omega) \\
&= |\{\tilde{c}_{ns_1}(\omega) - \rho_{\theta s_1}\} - \{c_{s_2} - \rho_{\theta s_2}\}| \\
&(\text{where } s_1 \equiv \arg\max_{s' \succ_\theta s}\{\tilde{c}_{ns'}(\omega) - \rho_{\theta s'}\} \text{ and } s_2 \equiv \arg\max\{c_{s'} - \rho_{\theta s'}\}|) \\
&< \begin{cases} |\{\tilde{c}_{ns_1}(\omega) - \rho_{\theta s_1}\} - (\{\tilde{c}_{ns_1}(\omega) - \rho_{\theta s_2}\} + \epsilon_1)| \text{ if } c_{s_2} - \rho_{\theta s_2} \geq \tilde{c}_{ns_1}(\omega) - \rho_{\theta s_1} \\ |\{\tilde{c}_{ns_1}(\omega) - \rho_{\theta s_1}\} - (\{\tilde{c}_{ns_1}(\omega) - \rho_{\theta s_2}\} - \epsilon_1)| \text{ if } c_{s_2} - \rho_{\theta s_2} < \tilde{c}_{ns_1}(\omega) - \rho_{\theta s_1} \end{cases} \\
&= \epsilon_1
\end{aligned}$$

where the inequality is by $|\tilde{c}_{ns'}(\omega) - c_{s'}| < \epsilon_1$ 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 $n > N$,

$$\begin{aligned}
&p_{ns}(\theta) \\
&= P_n(\tilde{\tau}_{n\theta s} \geq R > \tilde{\tau}_{n\theta s_-}\}) \\
&= P_n(\tilde{\tau}_{n\theta s} \geq R > \tilde{\tau}_{n\theta s_-}|\omega \in \Omega_1) \\
&\in (\max\{0, \tau_{\theta s} - \tau_{\theta s_-} - \epsilon, \max\{0, \tau_{\theta s} - \tau_{\theta s_-} + \epsilon) \\
&\in (p_s(\theta) - \epsilon, p_s(\theta) + \epsilon),
\end{aligned}$$

where the second-to-last inclusion is because for any $\omega \in \Omega_1$, there exists N such that for all $n > N$ such that $|\tilde{\tau}_{n\theta s}(\omega) - \tau_{\theta s}|, |\tilde{\tau}_{n\theta s_-}(\omega) - \tau_{\theta s_-}| < \epsilon_1$ and $R \sim U[0, 1]$. This means $p_{ns}(\theta) \longrightarrow p_s(\theta)$, thus completing the proof of Lemma 2. □

A.4 Qualification Instrument: Details

Proposition 3. *In any continuum economy, D_i^q is independent of type θ_i conditional on $Q(\theta_i)$, where D_i^q and $Q(\theta_i)$ are the qualification instrument and the associated risk set (conditioning variable), respectively, defined in section 4.5.*

Proof. The key to this argument is that school cutoffs, c_s, are constant any continuum economy (though not in general finite economies). We therefore have that

$$\begin{aligned}
&\Pr(D_i^q = 1|\theta_i = \theta) \\
&= \Pr(\pi_{is} \leq c_s \text{ for some charter } s \text{ ranked by } i|\theta_i = \theta) \\
&= \Pr(\rho_{is} + r_i \leq c_s \text{ for some charter } s \text{ ranked by } i|\theta_i = \theta) \\
&= \Pr(r_i \leq c_s - \rho_{is} \text{ for some charter } s \text{ ranked by } i|\theta_i = \theta) \\
&= \Pr(r_i \leq \max_{s \text{ ranked by } i}(c_s - \rho_{is})|\theta_i = \theta)
\end{aligned}$$

$= \max_{s \text{ ranked by } i}(c_s - \rho_{is}\ ),$

which depends on θ_i only through $Q(\theta_i)$. $\square$

A.5 Extension to the General Lottery Structure

While section 3 assumes a student has a single lottery number common to all schools, some cities such as Washington DC, New Orleans, and Amsterdam (see, for example, de Haan et al. (2015)) use a distinct student lottery number for each school. In this section, we extend our theory to the case with multiple tie breakers.

Let a random variable R_{is} denote student i's lottery number at school s. The only assumption we need is that R_{is} is iid according to $U[0,1]$ across students *within each school*. Schools may use independent lotteries and a student's lottery numbers at different schools may be independent. This setting allows not only for the single tie breaker we consider in section 3 but also for any *multiple tie breaker*, i.e., a lottery structure where $R_{is} \neq R_{is'}$ for some $s, s' \in S$ and $i \in I$.

Partition $M_{\theta s}$ into disjoint sets $M^1_{\theta s}, ..., M^{\bar{m}}_{\theta s}$ such that (1) if $s', s'' \in M^m_{\theta s}$ for some m, then s' and s'' use the same lottery and (2) for any $m' \neq m''$, $s' \in M^{m'}_{\theta s}$ and $s'' \in M^{m''}_{\theta s}$, s and s' use different lotteries. Partition $B_{\theta s}$ in the same way so that $M^m_{\theta s} \subset B^m_{\theta s}$ for all m. Here, $\bar{m}$ denotes the number of distinct lotteries to which a student might be exposed. When each school conducts its own lottery, $\bar{m}$ simply equals the number of schools a student ranks ahead of s. Define the *most informative disqualification cutoff within* $M^m_{\theta s}$ as

$$MID^m_{\theta s} \equiv \begin{cases} 1 & \text{if } c_{s'} = K+1 \text{ for some } s' \in B^m_{\theta s}, \\ \max\{\tau_{\tilde{s}} \mid \tilde{s} \in M^m_{\theta s}\} & \text{if } c_{s'} < K+1 \text{ for all } s' \in B^m_{\theta s} \text{ and } M^m_{\theta s} \neq \emptyset, \\ 0 & \text{otherwise.} \end{cases}$$

Define $M^{m^*}_{\theta s} \equiv \{\tilde{s} \in M_{\theta s} : \rho_{\tilde{s}} = \rho_{\theta \tilde{s}},\ s \text{ and } \tilde{s} \text{ use the same lottery}\}$, $B^{m^*}_{\theta s} \equiv \{\tilde{s} \in B_{\theta s} : s \text{ and } \tilde{s}$ use the same lottery$\}$, and the *most informative disqualification cutoff within* $M^{m^*}_{\theta s}$ as

$$MID^{m^*}_{\theta s} \equiv \begin{cases} 1 & \text{if } c_{s'} = K+1 \text{ for some } s' \in B^{m^*}_{\theta s}, \\ \max\{\tau_{\tilde{s}} \mid \tilde{s} \in M^{m^*}_{\theta s}\} & \text{if } c_{s'} < K+1 \text{ for all } s' \in B^{m^*}_{\theta s} \text{ and } M^{m^*}_{\theta s} \neq \emptyset, \\ 0 & \text{otherwise.} \end{cases}$$

The following extends Theorem 1 to the general lottery structure. We omit the proof since it is almost the same as the proof of Theorem 1 in Appendix A.2.

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

$$\Pr[D_i(s) = 1 | \theta_i = \theta] = \tilde{p}_s(\theta) \equiv \begin{cases} 0 & \text{if } \theta \in \Theta^-_s, \\ \Pi^{\bar{m}}_{m=1}(1 - MID^m_{\theta s}) & \text{if } \theta \in \Theta^+_s, \\ \Pi^{\bar{m}}_{m=1}(1 - MID^m_{\theta s}) \times \max\left\{0, \dfrac{\tau_s - MID^{m^*}_{\theta s}}{1 - MID^{m^*}_{\theta s}}\right\} & \text{if } \theta \in \Theta^*_s. \end{cases} \tag{18}$$

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. Everything else in our analysis remains the same with this modified definition of the DA propensity score $\tilde{p}_s(\theta)$.

A.6 Extension to the Boston (Immediate Acceptance) Mechanism

The Boston (Immediate Acceptance) mechanism has been as popular in practice as DA. Several papers (e.g. Hastings-Kane-Staiger (2009), Hastings-Neilson-Zimmerman (2012), Deming-Hastings-Kane-Staiger (2013)) use data generated from versions of the Boston mechanism. It is thus natural to consider our research question for the Boston mechanism.

Given strict preferences of students and schools, the *Boston mechanism* is defined through the following algorithm.

- 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.

The algorithm terminates at the first step in which no student applies to a school. Each student accepted by a school during some step of the algorithm is allocated a seat in that school. The Boston algorithm differs from the deferred acceptance algorithm in that when a school accepts a student at a step, in the Boston algorithm, the student is guaranteed a seat at that school, while in the deferred acceptance algorithm, that student may be later displaced by another student to whom the school gives a higher ex post priority.

It is easy to modify empirical strategies in section 3.4 and apply them to the Boston mechanism. To see this, take any strict preferences of students $(\succ_i)_i$ and schools $(\succ_s)_s$ as fixed, and use the following existing result.

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 enables us to consider the Boston mechanism as a version of DA with priorities where a priority group at a school consists of those who (i) share a same original priority status at the school and (ii) give a same preference rank to the school. We can directly apply empirical strategies in section 3.4 to the equivalent DA representation of the Boston mechanism.

B Data Appendix and Additional 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 across files through a fake ID number. Applicant data are from the 2012-2013 SchoolChoice assignment file,[34] 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 from the 2012-2013 school year. Enrollment, demographic, and outcome data are available for students enrolled in DPS only, and enrollment files report enrollment as of October.

Applications and assignment: SchoolChoice

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 a capacity program (i.e. bucket explained in the main text) within a ranked school.[35] Each applicant receives at most one offer across all capacity programs. 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.

[34] SchoolChoice is the name of DPS's centralized enrollment process, introduced in 2011-2012. See `https://www.dpsk12.org/`.

[35] Since applicants' rankings are at the school-level but seats are assigned at the capacity-program level, the SchoolChoice assignment mechanism translates school-level rankings into capacity-program-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).

Applicant enrollment and demographics

DPS enrollment data are anonymized (i.e. no information on student names and dates of birth), but students are uniquely identified across years through a fake ID. Each observation in the enrollment files is 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.[36] We construct a panel dataset using the unique student IDs capturing demographic and enrollment information for every student in each grade, keeping information from the first calendar year spent in each grade. A student is counted as attending a charter if the school in which she is enrolled according to the October enrollment files is a charter school.

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 the outlier exclusion is implemented, The raw test score variables are standardized to have mean zero and standard deviation one within 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 conducted at the grade level, since some schools run magnet programs for a subset of grades served only. See footnotes in Table 9 for details. Schools not included in the Parent Guide (i.e. SIMS Fayola International Academy Denver) were classified according to information from the school's website.

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

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. Panel A includes all applicants in the SchoolChoice assignment file (see Data Appendix for details). Panels B-E 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 panels B-E because there is no baseline grade for those applicants. Panels C-E 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). Columns 2, 4, and 6 count unique combinations of applicant preferences over school programs and school priorities in those programs.

Table B2: Differential attrition

	Non-offered mean	No controls	Propensity score controls: Linear	Hundredths	Saturated	Ten Thousandths
	(1)	(2)	(3)	(4)	(5)	(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	No controls	DA score (formula)		
			Linear	Hundredths	Saturated
	(1)	(2)	(3)	(4)	(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		1189.785	8.058	0.473	0.048
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		19.139	2.382	1.158	1.183
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 innovation schools

School	Total applicants	Applicants offered seats	Propensity score in (0,1)		
			DA score (frequency)	DA score (formula)	Simulated
	(1)	(2)	(3)	(4)	(5)
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 B5: Covariate balance and differential attrition for DPS innovation schools

	Non-offered mean	No controls	Propensity score controls: DA score (frequency) Linear	DA score (frequency) Hundredths	DA score (frequency) Saturated	Simulated score Linear	Simulated score Hundredths	Simulated score Ten Thousandths
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(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		142.846	1.100	0.995	0.909	0.801	0.921	1.523
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%