RESEARCH DESIGN MEETS MARKET DESIGN: USING CENTRALIZED ASSIGNMENT FOR IMPACT EVALUATION

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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 a wide class of mechanisms, while also revealing why seats are randomized at one school but not another. We use these methods to evaluate charter schools in Denver, one of a growing number of districts that combine charter and traditional public schools in a unified assignment system. The resulting estimates show large achievement gains from charter school attendance. Our approach generates efficiency gains over *ad hoc* methods, such as those that focus on schools ranked first, while also identifying a more representative average causal effect. We also show how to use centralized assignment mechanisms to identify causal effects in models with multiple school sectors.

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1 Introduction

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

Two benefits of centralized matching schemes like DA are efficiency and fairness: the resulting match improves welfare and transparency relative to ad hoc alternatives, while lotteries ensure that students with the same preferences and priorities have the same chance of obtaining highly-sought-after seats. The latter is sometimes called the "equal treatment of equals" (ETE) property. DA and related algorithms also have the virtue of narrowing the scope for strategic behavior that would otherwise give sophisticated families the opportunity to manipulate an assignment system at the expense of less-sophisticated participants (Abdulkadiroğlu et al., 2006; Pathak and Sönmez, 2008). In addition to these economic considerations, centralized assignment generates valuable data for empirical research on schools. In particular, when schools are oversubscribed, lottery-based rationing generates quasi-experimental variation in school assignment that can be used for credible evaluation of individual schools and of school reform models like charters.

Previous research using the lotteries embedded in centralized assignment schemes include studies of schools in Charlotte-Mecklenburg (Hastings et al., 2009; Deming, 2011; Deming et al., 2014) and New York (Bloom and Unterman, 2014; Abdulkadiroğlu et al., 2013). Causal effects in these studies are convincingly identified by quasi-experimental variation, but the research designs deployed in this work fail to exploit the full power of the random assignment embedded in centralized assignment schemes. A major stumbling block is the elaborate multi-stage nature of market-design matching. Market design weaves random assignment into an elaborate tapestry of information on student preferences and school priorities. In principle, all features of student preferences and school priorities can shape the probability of assignment to each school. Families tend to prefer schools located in their neighborhoods, for example, while schools may grant priority to children poor enough to qualify for a subsidized lunch. Conditional on preferences and priorities, however, mechanism-generated assignments are independent of potential outcomes.

This paper explains how to recover the full range of quasi-experimental variation embedded in centralized assignment. Specifically, we show how mechanisms that satisfy ETE map information on preferences, priorities, and school capacities into a conditional probability of random assignment, often referred to as the propensity score. As in other stratified randomized research designs, conditioning on the propensity score eliminates selection bias arising from the association between conditioning variables and potential outcomes (Rosenbaum and Rubin, 1983). The payoff to propensity-score conditioning turns out to be substantial in our application: full stratification on preferences and priorities reduces degrees of freedom markedly, eliminating many schools and students from consideration, while score-based stratification leaves our research sample largely intact.

The propensity score does more for us than reduce the dimensionality of preference and priority conditioning. 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 nature of this sample is not easily seen otherwise. We show, for example, that the quasi-experimental sample covers many schools that are undersubscribed, that is, schools that have fewer applicants than seats. Intuitively, applicants are randomly assigned to undersubscribed schools when they are rejected by oversubscribed schools that they've ranked more highly. As we show here, random assignment of this sort occurs frequently.

The propensity score for any mechanism that satisfies ETE is easily estimated by simulation, that is, by repeatedly drawing lottery numbers and computing the resulting average assignment rates across draws. This amounts to sampling from the relevant permutation distribution, a natural and highly general initial solution to the problem of score estimation. At the same time, while any stochastic mechanism can be simulated, simulation fails to illuminate the path producing random assignment. For example, the simulated score for a given school does not reveal the proportion of applicants randomly assigned due to oversubscription of that school and the proportion randomly assigned due to over-subscription of other schools that this school's applicants have ranked more highly. We therefore develop an analytic formula for the propensity score for a broad class of DA-type mechanisms. This formula explains how and why random assignment emerges. Our formula also provides a natural smoother for estimated scores. Because the relevant covariates are discrete, unsmoothed simulated scores fail to provide the sort of dimension reduction that gives the propensity score its practical appeal (Hirano et al. (2003)).

The propensity score generated by DA-type mechanisms does not typically have a general closed form solution. As a result, our analytic framework uses an asymptotic "large market" approximation to derive a simple formula for the score. The resulting *DA propensity score* is a function of a few easily-computed sample statistics. Both the simulated and DA (analytic) propensity scores work well as far as covariate balance goes, a result that emerges in our empirical application. Importantly, however, the DA score 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. The DA score is also quickly and easily computed, and can be used without the rounding or functional form restrictions (such as linear controls) required when using a simulated score.

Our test bed for the DA propensity score 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.[1] Because DPS assigns seats at traditional and charter schools in a unified match, the

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

population attending DPS charters is less positively selected than in large urban districts with decentralized charter lotteries. As far as we know, ours is the first charter evaluation to exploit an assignment scheme that simultaneously allocates seats in both the charter and traditional public school sectors.

The next section details the general class of assignment mechanisms of interest to us and describes the central role of the propensity score. Following this bit of context, Section 3 uses the theory of market design to characterize the propensity score for DA offers in large markets. Section 4 uses these results to estimate charter effects. Specifically our empirical evaluation strategy uses an indicator for DA-generated charter offers as an instrument for 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 DA propensity score. This section also shows how to estimate effects for multiple sectors, an important extension when school effects are potentially heterogeneous. Finally, Section 5 summarizes our theoretical and empirical findings and outlines an agenda for further work.

2 Centralized Assignment: In General and In Denver

2.1 Conditional Independence

A school choice problem is an economy defined by a set of applicants, schools, and school capacities. Applicants have strict preferences over schools while schools have priorities over applicants. Let I denote a set of applicants, indexed by i, and let $s = 0, 1, ..., S$ index schools, where $s = 0$ represents an outside option. Let n be the number of applicants. Seats at schools are constrained by a capacity vector, $\mathbf{q} = (q_0, q_1, q_2, ..., q_S)$; we assume $q_0 > n$.

Applicant 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 applicant is also granted a priority at every school. Let $\rho_{is} \in \{1, ..., K, \infty\}$ denote applicants i's priority at school s, where $\rho_{is} < \rho_{js}$ means school s prioritizes i over j. For instance, $\rho_{is} = 1$ might encode the fact that applicant i has sibling priority at school s, while $\rho_{is} = 2$ encodes neighborhood priority, and $\rho_{is} = 3$ for everyone else. We use $\rho_{is} = \infty$ to indicate that i is ineligible for school s. Many applicants 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 the vector of applicant i's priorities for each school.

Applicant type is defined as $\theta_i = (\succ_i, \boldsymbol{\rho}_i)$, that is, the combination of this applicant's preference and priorities at all schools. We say that an applicant of type θ has preferences $\succ_\theta$ and priorities ρ_θ. Θ denotes the set of possible types.

An *assignment* is a vector $\boldsymbol{\mu} = (\mu_1, \mu_2, \ldots, \mu_I)$ specifying each applicant's assigned school or assignment to the outside option. School s is assigned at most q_s applicants. A mechanism is a set of rules determining $\boldsymbol{\mu}$ as a function of preferences, priorities, and a possible tie-breaking variable that might be randomly assigned. The mechanism known as *serial dictatorship*, for example, orders applicants by the tie-breaker, assigning the first in line his or her top choice, the second in line his or her top choice among schools with seats remaining, and so on.

comportment and traditional reading and math skills, and rely heavily on data and teacher feedback to improve instruction. See Abdulkadiroğlu et al. (2011) and Angrist et al. (2013) for related evidence on charter effects.

Many mechanisms use randomization to break ties, inducing a distribution of assignments. Such mechanisms are said to be *stochastic*. When applicants in a serial dictatorship are ordered randomly, for example, the mechanism is called *random serial dictatorship* (RSD). Randomizers drawn independently from a uniform distribution for each applicant are called *lottery numbers*. The distribution of assignments associated with RSD is the permutation distribution generated by all possible lottery draws.

Formally, any stochastic mechanism maps economies characterized by $(I, S, \mathbf{q}, \Theta)$ into a distribution of possible assignments. This distribution is described by a matrix with generic element p_{is} satisfying (i) $0 \leq p_{is} \leq 1$ for all i and s, (ii) $\Sigma_s p_{is} = 1$ for all i, and (iii) $\Sigma_i p_{is} \leq q_s$ for all s. The value of p_{is} is the probability that applicant i is assigned to school s. These are collected in a vector $\boldsymbol{p}_i = (p_{i0}, p_{i1}, ..., p_{iS})$ recording the probability i finds a seat at all schools. This notation covers deterministic mechanisms in which p_{is} equals either 0 or 1 and, for each i, $p_{is} = 1$ for at most one s.

We say mechanism φ satisfies the *equal treatment of equals* (ETE) property when applicants with the same preferences and priorities at all schools have the same assignment probability at each school.[2] That is, for any school choice problem and any applicants i and j with $\theta_i = \theta_j$, we have that $\boldsymbol{p}_i = \boldsymbol{p}_j$.

ETE allows us to use stochastic mechanisms to estimate causal effects. Specifically, we'd like to estimate the causal effect of attendance at a particular school or group of schools relative to one or more alternative schools. This task is complicated by the fact that school assignment reflects preferences and priorities and these variables in turn are related to outcomes like test scores. ETE allows us to solve this problem: the distribution of offers generated by a stochastic assignment mechanism is viewed here as a stratified randomized trial, where the "strata" are defined by type.

As the notion of a stratified randomized trial suggests, ETE makes offers conditionally independent of all possible confounding variables that might otherwise generate omitted variables bias in econometric analyses of school attendance effects. To see this, pick any school s as a treatment school and let $D_i(s)$ be a dummy variable indicating when applicant i is assigned to school s by stochastic mechanism φ. For any random variable or vector of characteristics W_i, which can include covariates like race or outcome variables like test scores, let W_{0i} be the potential value of W_i that is revealed when $D_i(s) = 0$ and let W_{1i} be the potential value revealed when $D_i(s) = 1$. These two potential values might be the same, as for covariates (race is unchanged by school assignment) or for test scores when assignment has no effect on achievement. In cases where they differ, as for outcomes affected by treatment, only one is seen in a given assignment realization. Potential variables are attributes and therefore non-stochastic in a fixed applicant population, that is, they are unchanged by school assignments (see, e.g., Rosenbaum (2002); Imbens and Rubin (2015)). We therefore say that the observed characteristic W_i is *fixed under re-randomization* if $W_{0i} = W_{1i}$ for all i.

Although applicant characteristics are almost certainly associated with differences in assignment probabilities, ETE restricts this variation to be independent of characteristics conditional

[2]ETE is widely studied in allocation problems, see, e.g. Moulin (2003). Shapley (1953)'s axiomatization of the Shapley value appears to be the first formal statement of this concept.

on type:

Proposition 1. *Consider the conditional assignment probability* $P[D_i(s) = 1|W_i = w, \theta_i = \theta]$ *for all applicants* i *with* $W_i = w$ *and* $\theta_i = \theta$. *The probability* P *is the assignment rate to school* s *induced by stochastic mechanism* φ *and* w *is a particular value of* W_i. *If* φ *satisfies ETE and* W_i *is fixed under re-randomization, we have that*

$$P[D_i(s) = 1|W_i = w, \theta_i = \theta] = P[D_i(s) = 1|\theta_i = \theta]$$

for all values w.

Proof. Since W_i is fixed under re-randomization,

$$P[D_i(s) = 1|W_i = w, \theta_i = \theta] = P[D_i(s) = 1|W_{0i} = w, \theta_i = \theta]$$

Moreover, since knowledge of an individual applicant's identity implies knowledge of his type and W_i, the law of iterated expectations implies

$$P[D_i(s) = 1|W_{0i} = w, \theta_i = \theta] = E[p_{is}|W_{0i} = w, \theta_i = \theta] = E[p_{is}|\theta_i = \theta],$$

where the second equality follows from ETE: p_{is} is the same for applicants of the same type. We've therefore shown

$$P[D_i(s) = 1|W_i = w, \theta_i = \theta] = P[D_i(s) = 1|\theta_i = \theta].$$

□

Although elementary, Proposition 1 is the foundation of our analysis: it shows how centralized assignment schemes induce a stratified randomized trial. In particular, this proposition defines the conditional independence assumption that provides the foundation for causal analysis of DA-generated assignments.

Conditional Independence for DA

Many U.S. school districts implement versions of DA with a single tie-breaking lottery number. As we discuss below, most widely-used centralized assignment mechanisms can be cast as a version of DA. Single tie-breaking DA for school assignment works like this:

> Draw an independently and identically distributed lottery number for each applicant.
>
> Each applicant applies to his most preferred school. Each school ranks these 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 applicant applies to his next most preferred school. Each school ranks these new applicants *together with applicants that it admitted tentatively in the previous round*, first by priority and then by random number. From this pool, the school tentatively admits those it ranks highest up to capacity, rejecting the rest.

This algorithm terminates when there are no new applications (some applicants may remain unassigned).[3]

DA with single tie-breaking is easily seen to satisfy ETE. Stochastic assignments in this case are determined by the type distribution, which is fixed, and a particular lottery draw. Assignment differences from one realization to the next are therefore generated solely by differences in lottery draws. In particular, if we swap the lottery numbers for two applicants with the same type, DA swaps their assignments, leaving other assignments unchanged. Since all draws are equally likely for all applicants, the probability of assignment must be equal for two applicants of the same type, satisfying ETE. This argument is made formally in Appendix A.1, which also shows that other mechanisms satisfying ETE include:

- DA with multiple tie-breakers (that is, different lottery numbers at different schools)
- The immediate acceptance ("Boston") mechanism with single or multiple tie-breakers
- Random serial dictatorship
- Top trading cycles with single or multiple tie-breakers

Top trading cycles, rarely seen in school choice applications, is the only mechanism on this list that cannot also be written as DA with suitably modified inputs.[4] Most centralized school assignment schemes currently in use can be expressed as modifications of DA.

Where does ETE fail? Some English towns use DA with distance-based tie-breaking (Burgess et al., 2014). In this case, distance plays the role otherwise played by lottery numbers. DA with distance-based tie-breaking fails to satisfy ETE because applicants of the same type need not face the same assignment probability, while expanding the definition of type to include distance makes the mechanism non-stochastic.

2.2 Propensity Score Pooling

Proposition 1 implies that for applicant i of type θ and W_i fixed under re-randomization,

$$P[D_i(s) = 1 | W_i = w, \theta_i = \theta] = P[D_i(s) = 1 | \theta_i = \theta].$$

That is, for any mechanism that treats equals equally, conditioning on θ_i eliminates any selection bias arising from the association between type and potential outcomes. Since θ_i takes on many values, however, full-type conditioning reduces the sample available for impact evaluation. We, therefore, consider schemes that compare applicants while pooling types.

[3] DA produces a stable allocation in the following sense: any applicant 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 applicant'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.

[4] Appendix A.10 describes how to modify school priorities to compute the outcome of the immediate acceptance mechanism using DA.

Rosenbaum and Rubin's (1983) propensity score theorem tells us how this pooling can be accomplished while still eliminating omitted variables bias. The *propensity score* for a market of any size, denoted $p_s(\theta)$, is the scalar function of type defined by

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

Rosenbaum and Rubin (1983) show that propensity score conditioning is enough to ensure that offers are independent of W_i. In other words,

$$P[D_i(s) = 1|W_i = w, p_s(\theta_i) = p] = P[D_i(s) = 1|p_s(\theta_i) = p] = p. \quad (1)$$

Equation (1) implies that propensity score conditioning eliminates the possibility of omitted variables bias due to the dependence of offers on type.[5] The following simple example illustrates propensity score pooling.

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

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

Applicants 3 and 5 rank only one school.

Note that no two applicants here have the same preferences and priorities. Consequently, full-type conditioning puts each applicant into a different stratum. This rules out research strategies that rely on full type conditioning to eliminate selection bias. But full-type conditioning is unnecessary in this case because DA assigns each of applicants 1, 2, 3, and 4 to school a with probability (propensity score) 0.25. This calculation reflects the fact that 5 beats 4 at c by virtue of his priority there, leaving 1, 2, 3, and 4 all applying to a with no one advantaged there. The impact of assignment to a can therefore be analyzed in a single stratum containing four applicants with a common score value of 0.25.[6]

The simple structure of this example allows us to infer the propensity score. In real assignment problems, the score is not easily computed, but can be simulated by repeatedly drawing lottery numbers and running DA. By a conventional law of large numbers, this (estimated) simulated score converges to the actual finite-market score as the number of draws increases. We illustrate this by using data from the Denver Public School (DPS) district to compute the propensity score for offers of a charter school seat.

[5] Rosenbaum and Rubin (1983) also show that the propensity score is the coarsest balancing score, which in this case means that no coarser function of type ensures conditional independence of $D_i(s)$ and W_i. Hahn (1998), Hirano et al. (2003), and Angrist and Hahn (2004) discuss the efficiency consequences of conditioning on the score.

[6] Applicants not assigned to a are assigned to either b or c or are unassigned.

2.3 DPS Data and Descriptive Statistics

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

In the first round of SchoolChoice, parents rank up to five schools of any type, including traditional public schools, magnet schools, innovation schools, and most charters. A neighborhood school is also ranked automatically (the district adds a neighborhood school to applicant rankings as the last choice). Schools ration seats using a mix of priorities and a single lottery number. Priorities vary across schools and typically involve siblings and neighborhoods. Seats may be reserved for a certain number of subsidized-lunch applicants and for children of school staff. Reserved seats are allocated by splitting schools and assigning the highest priority status to applicants in the reserved group at one of the sub-schools created by a split.[7] Match participants can only qualify for seats in a single grade.

The DPS match distinguishes between groups of seats at a given school, known as "buckets." Buckets in the same school have distinct priorities and capacities. DPS converts applicants' 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 the sub-schools determined by seat reservation policies and buckets rather than schools, while the relevant propensity score captures the probability of offers at sub-schools. The discussion that follows refers to propensity scores for schools, with the understanding that the fundamental unit of assignment is a bucket, from which assignment rates to schools have been constructed.[8]

The data analyzed here come from files containing the information used for first-round assignment of students applying in the 2011-12 and 2012-13 school year for seats the following year (this information includes preference lists, priorities, random numbers, assignment status, and school capacities). We focus on applicants for grades 4-10, who are in grades 3-9 in the application year. Most of our applicants are applying for a middle school grade 6 school or a high school grade 9 seat. School-level scores were constructed by summing scores for all component sub-schools used to implement seat reservation policies and to define buckets. Our empirical work also uses files with information on October enrollment and standardized scores from the Colorado School Assessment Program (CSAP) and the Transitional Colorado Assessment Program (TCAP) tests, given annually in grades 3-10. A data appendix describes these files and the extract we've created from them. For our purposes, "Charter schools" are schools identified as "charter" in DPS *2012-2013 and 2013-2014 SchoolChoice Enrollment Guide* brochures and not identified as "intensive pathways" schools, which serve applicants who are much older than is typical for their grade.

Our application involves data from two years of data from DPS. The DPS population enrolled

[7]For more details on reserve implementation via school-seat splitting, see Dur et al. (2014) and Dur et al. (2016).

[8]DPS modifies the traditional DA mechanism 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 applicants from that generated by standard DA. Our analysis incorporates family link by defining distinct types for linked applicants.

in grades 3-9 is roughly 60% Hispanic, a fact reported in Table 1, along with other descriptive statistics. The outcome scores of applicants in grades 3-9 come from TCAP tests taken in grades 4-10 in the spring of the following year.[9] The high Hispanic proportion 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.

In the two years covered in Table 1, roughly 22,000 students enrolled in grades 3-9 sought to change their school for the following year by participating in SchoolChoice in the spring. We drop applicants from 2013-14 who also participated in the previous year's match. The sample participating in the assignment, described in column 2 of Table 1, contains fewer charter school students than appear in the total DPS population, but is otherwise similar. It's also worth noting that our impact analysis is limited to students enrolled in DPS in the baseline (pre-assignment) years. The sample described in column 2 is therefore a subset of that described in column 1.

Column 3 of Table 1 shows that of the 22,000 DPS-at-baseline applicants participating in SchoolChoice, about 10,000 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 18% among SchoolChoice applicants 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).

We computed simulated scores by running DA for one million lottery draws for each year. Simulated scores are the proportion of draws in which applicants of a given type were seated at each school. The propensity score for charter offers is the sum of the scores for each individual charter school (a consequence of the fact that SchoolChoice produces a single offer for each applicant).[10] Applicants subject to random charter assignment are those with charter propensity scores (probabilities of assignment) between zero and one. Column 4 shows that nearly 3,500 charter applicants are subject to random assignment. This group looks like the full charter applicant pool on most dimensions, though randomized applicants are more likely to have already been enrolled at a charter at the time they entered the match. Column 5 reports statistics for the subset of the randomized group that enrolls in a charter school; these show slightly higher baseline scores among charter students.

2.4 DPS Schools Randomized

Table 2 lists charter schools in the sample, along with the number of applicants, capacities, offers, and counts of those subject to random assignment for each school in 2013. Three char-

[9]Grade 3 is omitted from our outcome sample because 3rd graders have no baseline test to gauge balance.

[10]Calsamiglia et al. (2014) and Agarwal and Somaini (2015) simulate variants of the Boston mechanism as part of a study estimating preferences over schools.

ter management organizations (CMOs), the Denver School of Science and Technology (DSST), STRIVE Preparatory Schools and the Knowledge is Power Program (KIPP), contribute 16 of the 31 charters listed.

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 in column 5 with the total applicant count in column 2. Column 5 shows random assignment at every charter in 2013, except for the Denver Language School, which offered no seats. With the exception of Venture Prep, this was also true in 2014 (see Appendix Table B5 for details).

DA randomizes seats for applicants ranking charters *first* for a smaller set of schools. This can be seen in the last column of Table 2, which reports the number of applicants with a simulated charter score strictly between zero and one who also ranked each school first. The reduced scope of first-choice randomization is important for our comparison of strategies using the DA propensity score with previously-employed IV strategies using first-choice instruments. First-choice instruments applied to the DPS charter sector necessarily ignore many schools (in 2013, 10 schools had no first-choice random assignment.)

A broad picture of DPS random assignment appears in Figure 1. Panel (a) captures the information in columns 5 and 6 of Table 2 by plotting the number of first-choice applicants subject to randomization as black dots, with the total number 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 empirical payoff to our score-based approach to the DA research design. These benefits are not limited to the charter sector, a fact documented in Panel (b) of the figure, which plots the same comparisons for non-charter schools in the DPS match.

Table 2 reveals a few surprising features of the assignment distribution. We see, for example, that only 112 applicants were offered seats at STRIVE Prep-GVR, a school with a capacity of 147. In spite of the fact that this school was under-subscribed, some of the seats there were randomly assigned. The simulated score shows that this happens, without explaining why. This motivates a large market approximation to $p_s(\theta)$ that reveals the sources of random assignment in a large class of mechanisms satisfying equal treatment of equals. The large market score also provides a natural smoother of the unrestricted empirical score.

3 Score Theory

3.1 A Large Market Approximation

Our analysis of single tie-breaking DA provides a theoretical foundation for a wide class of mechanisms. Extension to the most important of these other mechanisms is discussed in the appendix, following proof and further illustration of our main theoretical results.

The probability of assignment to school a under DA is determined both by an applicant's failure to win a seat at schools he ranks more highly than a and by the odds he wins a seat at a in competition with those who have also ranked a and similarly failed to find seats at schools they've ranked more highly. This structure leads to a simple formula expressing these two sources of risk.[11] The following example shows this structure:

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

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

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

Let p_{ia} for $i = 1, 2, 3, 4$ denote the probability that type i is assigned to school a. With four applicants, p_{ia} comes from $4! = 24$ possible lottery draws, all equally likely. Given this modest number of possibilities, p_{ia} is easily calculated by enumeration:

- Not having ranked a, type 1 is never assigned there, so $p_{1a} = 0$.
- 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, $p_{2a} = 2/24 = 1/12$.
- 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 a sequence of the form $(1, 2, 3, 4)$, which occurs only once. Therefore, $p_{3a} = 1/24$.
- Finally, since type 4 gets the seat at a if and only if the seat does not go to type 2 or type 3, $p_{4a} = 21/24$.

In this example, the propensity score differs for each applicant. But in larger markets with the same distribution of types, the score is smoother. To see this, consider a market that replicates the structure of this example n times, so that n applicants of each type apply to up to 3 schools, each with n seats.

The relationship between simulated probabilities of assignment and market size for Example 2, plotted in Figure 2, reveals that as the market grows, the distinction between types 2 and 3 disappears. In particular, Figure 2 shows that for large enough n,

$$p_{2a} = p_{3a} = 1/12; \quad p_{1a} = 0; \quad p_{4a} = 10/12 = 5/6,$$

with the probability of assignment at a for types 2 and 3 converging quickly. This convergence is a consequence of a result established in the next subsection, which shows that the large-market probabilities that types 2 and 3 are seated at a are both determined by failure to win a seat at b. The fact that applicant 2 ranks c ahead of b is irrelevant.

[11] Other applications of large-market approximations include Abdulkadiroğlu et al. (2015); Azevedo and Leshno (2016); Budish (2011); Che and Kojima (2010); Kesten and Ünver (2015).

Why is the difference in preferences between applicant 2 and 3 ultimately irrelevant? Among schools that an applicant prefers to a, large market risk is determined solely by failure to qualify–that is, by having a lottery number above the cutoff–at the school at which it is easiest to qualify. In general, this *most informative disqualification* (MID) determines how distributions of lottery numbers for applicants of differing types are effectively truncated before entering the competition for seats at a. As we show below, the fact that the large market score depends on type only through a set of constructs like MID allows us to replace full type conditioning with something much smoother.

As a formal matter, the large market model is built on the notion of a continuum of applicants as in Abdulkadiroğlu et al. (2015) and Azevedo and Leshno (2016). A *continuum economy* sets $I = [0, 1]$, with school capacities, q_s, defined as the proportion of I that can be seated at school s. Applicant i's lottery number r_i, is drawn from a standard uniform distribution, independently for all applicants. In particular, lottery draws are independent of type. With single tie-breaking, all schools look at the same lottery number. Extension to the less-common multiple tie-breaking case, in which applicants have different lottery numbers at different schools, is discussed in the theoretical appendix.

For any set of applicant types $\Theta_0 \subset \Theta$ and for any number $r_0 \in [0, 1]$, define the set of applicants 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 $I_0 = I(\Theta_0, r_0)$.

In a finite economy with n applicants, denote the fraction of applicants in I_0 by

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

$F(I_0)$ for a finite economy depends on the realized lottery draw. In a continuum economy, $F(I_0)$ is defined as

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

where $E[1\{\theta_i \in \Theta_0\}]$ is the proportion of types in set Θ_0. Either way, the applicant side of an economy is fully characterized by the distribution of types and lottery numbers, for which we sometimes use the shorthand notation, F.

Defining DA

We define DA using the notation above, nesting the finite-market and continuum cases. First, combine priority status and lottery realization into a single number for each applicant and school, called *applicant 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, rank is lexicographic in priority and lottery numbers.

DA proceeds in a series of rounds, indexed here by t. 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 applicants with rank below the school-s cutoff, who prefer school s to any other school for which they are also below the relevant cutoff.

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

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

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

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

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

The set of applicants that are assigned school s under DA is the demand for seats at the limiting cutoffs: $\{i \in Q_s(\mathbf{c})\}$ where $\mathbf{c} = (c_1, ..., c_S)$.[12] Since $c_s \leq K + 1$, an ineligible applicant is never assigned to school s.

We write the final DA cutoffs as a limiting outcome to accommodate the continuum economy; in real markets, DA converges in a finite number of rounds. Appendix A.2 shows that the characterization of DA in this section is valid in the sense that: (a) the necessary limits exist for every economy, finite or continuum; (b) for every finite economy, the allocation upon convergence matches that produced by DA as usually described (for example, by Gale and Shapley (1962) and the many studies building on their work).

3.2 Characterizing the DA Propensity Score

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 for whom seats are allocated by lottery when a school is over-subscribed. Formally, 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, an applicant is assigned s with certainty if $\rho_{is} < \rho_s$, that is, if he clears marginal priority. Applicants with $\rho_{is} > \rho_s$ have no chance of finding a seat at s. Applicants for whom $\rho_{is} = \rho_s$ are marginal: these applicants are seated at s when their lottery numbers fall below a school-specific *lottery cutoff*. The lottery cutoff at school s, denoted τ_s, is the decimal part of the cutoff at s, that is, $\tau_s = c_s - \rho_s$.

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

[12] The characterization of DA via cutoffs has proven valuable in other studies of matching markets. See, e.g., Abdulkadiroğlu et al. (2015), Azevedo and Leshno (2016) and Agarwal and Somaini (2015).

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

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

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

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

A second key component of our score formulation reflects the fact that failure to qualify at schools other than s may truncate the distribution of lottery numbers in the marginal priority group for s. To characterize the distribution of lottery numbers among those at risk of assignment at s, we introduce notation for the set of schools ranked above s. Specifically, applicants of type θ view the following set of schools as better than s:

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

Type θ's *most informative disqualification* (MID) at s is defined as a function of the cutoffs at schools in $B_{\theta s}$

$$MID_{\theta s} \equiv \begin{cases} 0 & \text{if } \rho_{\theta \tilde{s}} > \rho_{\tilde{s}} \text{ for all } \tilde{s} \in B_{\theta s}, \\ 1 & \text{if } \rho_{\theta \tilde{s}} < \rho_{\tilde{s}} \text{ for some } \tilde{s} \in B_{\theta s}, \\ \max\{\tau_{\tilde{s}} \mid \tilde{s} \in B_{\theta s} \text{ and } \rho_{\theta \tilde{s}} = \rho_{\tilde{s}}\} & \text{if } \rho_{\theta \tilde{s}} = \rho_{\tilde{s}} \text{ for some } \tilde{s} \in B_{\theta s} \text{ and } \rho_{\theta \tilde{s}} > \rho_{\tilde{s}} \text{ otherwise.} \end{cases}$$

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

The definition of $MID_{\theta s}$ also reflects the fact that, among applicants for whom $\rho_{\theta \tilde{s}} = \rho_{\tilde{s}}$ for some $\tilde{s} \in B_{\theta s}$, anyone who fails to clear $\tau_{\tilde{s}}$ is surely disqualified at schools with lower (less forgiving) cutoffs. For example, applicants who fail to qualify at a school with a cutoff of 0.5 fail to qualify at schools with cutoffs below 0.5. Consequently, to keep track of the truncation induced by disqualification at all schools an applicant prefers to s, we need to record only the most forgiving cutoff that an applicant fails to clear.

The following theorem uses the marginal priority and MID concepts to define an easily-computed *DA propensity score*, which coincides with the true propensity score $p_s(\theta)$ in any continuum economy:

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:*

$$p_s(\theta) = \varphi_s(\theta) \equiv \begin{cases} 0 & \text{if } \theta \in \Theta_s^n, \\ (1 - MID_{\theta s}) & \text{if } \theta \in \Theta_s^a, \\ (1 - MID_{\theta s}) \times \max\left\{0, \dfrac{\tau_s - MID_{\theta s}}{1 - MID_{\theta s}}\right\} & \text{if } \theta \in \Theta_s^c, \end{cases} \quad (2)$$

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

The proof appears in Appendix A.3.

The case without priorities offers a revealing simplification of this result. Without priorities, DA is the same as a random serial dictatorship (RSD), that is, a serial dictatorship with applicants ordered by lottery number (see, e.g., Abdulkadiroğlu and Sönmez 1998, Svensson 1999, Pathak and Sethuraman 2010).[13] Theorem 1 therefore implies the following corollary, which gives the *RSD propensity score*:

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

$$p_s(\theta) = \varphi_s(\theta) \equiv (1 - MID_{\theta s}) \times \max\left\{0, \frac{\tau_s - MID_{\theta s}}{1 - MID_{\theta s}}\right\} = \max\{0, \tau_s - MID_{\theta s}\}.$$

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

In a match with priorities, the DA propensity score also accounts for the fact that random assignment at s occurs partly as a consequence of not being seated a school preferred to s. Using the language and notation introduced in this section, we can explain the DA propensity score as follows:

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

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

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

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

The DA propensity score is a simple function of a small number of intermediate quantities, specifically, $MID_{\theta s}$, τ_s, and marginal priority status at s and elsewhere. It's common to find that different types have the same marginal priority status and $MID_{\theta s}$, coarsening or smoothing the score in a manner that facilitates empirical work. In stylized examples, we can easily compute continuum values for these parameters.[14] In real markets with elaborate preferences and priorities, it's natural to use sample analogs for score estimation. As we show below, this generates a consistent estimator of the propensity score for finite markets.[15]

3.3 Estimating the DA Propensity Score

We're interested in the asymptotic behavior of propensity score estimates based on Theorem 1. In particular, we show here that a sample analog of the DA score converges (almost surely) uniformly in market size to the propensity score for the limiting economy. It's noteworthy that convergence emerges under an asymptotic sequence driven by overall market size rather than the number of applicants per type. This explains in part why we expect the sample analog of the DA score to produce ignorable offers in real markets with few applicants per type. Our empirical application validates this conjectured good performance: applicant characteristics are balanced conditional on sample analogs of the DA propensity score. Other factors contributing to the empirical success of Theorem 1 are discussed briefly after documenting this balance.

The asymptotic sequence for the estimated score works as follows: randomly sample n applicants 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 in the sampled economy to be q_s and run DA with these applicants and schools. Compute $MID_{\theta s}$, τ_s, and partition Θ_s by observing cutoffs $\hat{\mathbf{c}}_n$ and assignments in this single realization, then plug these quantities into equation (2). 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 applicants 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.

[14] Appendix A.5 explains how Theorem 1 explains the convergence of type 2 and type 3 propensity scores seen in Figure 1.

[15] Appendices A.9 and A.10 show how to extend Theorem 1 to DA using school-specific tie-breaking and the Boston mechanism. Theorem 1 also applies to First Preference First mechanisms (Pathak and Sönmez, 2013), Chinese Parallel mechanisms (Chen and Kesten, 2016), and Deduction point mechanisms (Pathak et al., 2016).

The analysis here makes use of a regularity condition:

Assumption 1. *(Rich 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 applicants in every non-empty priority group defined for that school.

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

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

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

Moreover, since θ has finite support, this convergence is uniform in θ.

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

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

where $\mathbf{c}$ denotes the continuum economy cutoffs. This result, together with the continuous mapping theorem, implies

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

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

A second lemma establishes that for all $\theta \in \Theta$ and $s \in S$,

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

since φ_s is an almost-everywhere continuous function of cutoffs. That is, the actual (re-randomization-based) propensity score in the sampled finite economy also converges to the propensity score in the continuum economy.[16]

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

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

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

[16] See also Azevedo and Leshno (2016), who provide convergence results for the cutoffs and conditional-on-type probabilities of assignment generated by a sequence of stable matchings, showing that the empirical assignment rates for types in a finite market converge to the continuum probability of assignment. The two lemmas in the appendix differ from Azevedo and Leshno (2016)'s results in that they use Assumption 1 and are proved using the extended continuous mapping theorem. The characterization of the DA propensity score in Theorem 1 does not appear to have an analog in the Azevedo and Leshno (2016) framework.

Theorem 2 justifies our use of the formula in Theorem 1 to control for applicant type in empirical work estimating school attendance effects. This theoretical result is used for propensity score estimation in two ways. The first, which we label a "formula" calculation, applies equation (2) directly to the DPS data (as described in the preamble to Theorem 2). Specifically, for each applicant type, school, and entry grade, we identify marginal priorities, and applicants were allocated by priority status to either Θ_s^n, Θ_s^a, or Θ_s^c. The DA score is then estimated by computing the sample analog of $MID_{\theta s}$ and τ_s in the DPS assignment data and plugging these into equation (2).

Much 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 takes cells defined by constant values of MID, Θ^c, Θ^a and Θ^n estimated as in the formula calculation and tabulates the empirical offer rate in these cells. This score estimate, which we refer to as a "frequency" calculation, is closer to an estimated score of the sort discussed by Abadie and Imbens (2016) than is the formula score, which looks only at cutoffs. The large-sample distribution theory in Abadie and Imbens (2016) suggests that conditioning on an estimated score may increase the efficiency of score-based estimates of average treatment effects.[17]

3.4 Explaining Random Assignment

Earlier, we noted that STRIVE Prep - GVR had 119 applicants randomized in 2013, in spite of the fact that no applicant with non-degenerate offer risk ranked this school first. Random assignment at GVR is a consequence of the many GVR applicants randomized by admissions offers at schools they'd ranked more highly. This and related determinants of offer risk are detailed in Table 3, which explores the anatomy of the DA propensity score for 6th grade applicants to six middle schools in the STRIVE network in 2013. Columns 6 and 8 of the table count the number of randomized applicants.[18] We see, for example, that all randomized GVR applicants were randomized by virtue of having $MID_{\theta s}$ inside the unit interval (shown in column 8), with no one randomized at GVR's own cutoff (shown in column 6).

In contrast with STRIVE's GVR school, few 2013 applicants were randomized at STRIVE's Highland, Lake, and Montbello campuses. This is a consequence of the fact that most Highland, Lake, and Montbello applicants were likely to clear marginal priority at these schools (having $\rho_{\theta s} < \rho_s$), while having values of $MID_{\theta s}$ mostly equal to zero or 1, eliminating random assignment at schools ranked more highly. Interestingly, the Federal and Westwood campuses are the only STRIVE schools to see applicants randomized around the cutoff in the school's own marginal priority group. We might therefore learn more about the impact of attendance at Federal and Westwood by changing the cutoff there (e.g., by changing capacity), whereas such a change is likely to be of little consequence for evaluations of the other schools.

Table 3 also documents the surprisingly weak connection between applicant randomization counts and a naive definition of over-subscription based on school capacity. In particular, columns 2 and 3 reveal that four out of six schools described in the table ultimately made fewer offers

[17] See also Section 2.3 of Rosenbaum (1987) for a similar argument.

[18] 116 applicants have a non-degenerate DA formula score compared to 119 applicants with a non-degenerate simulated score.

than they had seats available (far fewer in the case of Montbello). Even so, assignment at these schools was far from certain. They therefore contribute to our score-conditioned charter school impact analysis.

3.5 DA Score Balancing Tests

Theorems 1 and 2 provide asymptotic approximations, the quality of which should be judged in real markets. The goal of propensity score conditioning is to eliminate omitted variables bias induced by covariates associated with treatments. Covariate balance is therefore an important measure of the score-based empirical strategy's success (a standard applied elsewhere; see, e.g., the applications summarized in Dehejia and Wahba (1999) and Chapter 14 of Imbens and Rubin (2015)).

The balance measures reported in Table 4 compare uncontrolled differences in average applicant characteristics by charter offer status with estimates that control for the score. The latter put applicant characteristics on the left-hand side of regression models with charter offer status and controls for the propensity score on the right. Balance in this case is measured by

$$E[W_i|D_i = 1, \hat{p}_D(\theta_i)] - E[W_i|D_i = 0, \hat{p}_D(\theta_i)],$$

where W_i is a vector of applicant characteristics, including some in θ_i, and D_i indicates the offer of any charter seat. $\hat{p}_D(\theta_i)$ is an estimate of the propensity score of getting any charter seat ($D_i = 1$) computed using simulation or Theorems 1 and 2. These expectations are estimated by averaging conditional expectations in 400 runs of DA. Specifically, for each of these 400 draws, we regress W_i on a saturated model for the estimated (or simulated) propensity score along with a dummy for charter offers. By a standard argument (Yang et al., 2016), $\hat{p}_D(\theta_i)$ is supposed to have the balance property pushing down the above expectation difference to zero.

Table 4 reports the average coefficient on offer in models that dummy all score values inside the unit interval. Uncontrolled comparisons by offer status, reported in column 2 of Table 4, show large differences in average applicant characteristics, especially for variables related to preferences. On average, those not offered a charter seat ranked an average of 1.4 charter schools, but this 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, 0.28+0.64) 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.

Not surprisingly, control for the simulated propensity score balances covariates almost perfectly. This can be seen in columns 3 and 4 of Table 4, which report balance conditional on the simulated score using two rounding schemes. Rounding reflects the fact that, for example, the simulated score starts with 1,229 unique values. Rounding to the nearest hundredth leaves us with 77 points of support while rounding to the nearest thousandth leaves 153 points of support.

Conditioning on frequency and formula estimates of the DA propensity score also reduces differences by offer status markedly, and almost as completely as does conditioning on the simulated score. This can be seen in columns 5 and 6 of Table 4. The first set of conditional results, which come from regression models with non-parametric control for the DA frequency score, show only small differences by offer status. Column 6 shows that control for the formula score reduces offer gaps for most covariates even further. This evidence of balance means that the estimated DA score succeeds in its mission of eliminating selection bias. This is in spite of the fact that the DA propensity score is an asymptotic approximation that can be expected to provide perfect treatment-control balance only in a large market limit.

We also report traditional statistical balance tests such as would typically be reported for a randomized trial. Specifically, Table 5 documents balance for the realized SchoolChoice match by reporting t and F-statistics for offer gaps in covariate means. Again, we look at balance conditional on propensity scores for applicants with scores strictly between 0 and 1. Measured by statistical tests, covariates are about equally well-balanced by both the simulated score and the estimated DA scores. Not surprisingly, a few marginally significant imbalances pop up. 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. In this case, conditioning on the frequency score produces a slight improvement in balance over the formula score.

As can be seen in the last column of Table 5, full control for type reduces the sample available for estimation considerably. Models with full type control are run on a sample of size 462. Likewise, the fact that saturated control for the simulated score requires some smoothing can be seen in the reduced sample available for estimation of models that control fully for a simulated score rounded to the nearest thousandth (the sample size here falls from 2,678 to 2,263).

A few marginally significant baseline score gaps appear in some of the score-controlled comparisons at the bottom of the table. The F-test results and the fact that these gaps are not mirrored in the comparisons in Table 4 suggests the differences in Table 5 are due to chance. Still, we can mitigate the effect of chance differences on 2SLS estimates of charter effects by adding baseline score controls (and other covariates) to empirical models. The inclusion of these additional controls also has the salutary effect of making the 2SLS estimates of interest considerably more precise (covariates include dummies for grade tested, gender, origin school charter status, race, gifted status, bilingual status, subsidized lunch eligibility, special education, limited English proficient status, and baseline test scores; baseline score controls are responsible for most of the resulting precision gain).[19]

The DA score provides effective control for covariates in spite of the fact that the DPS SchoolChoice market includes almost as many types as applicants. This is consistent with Theorem 2, which establishes uniform almost sure convergence at a rate determined by overall market size. The good performance of the estimated DA score is also in line with earlier evidence on the accuracy of large market approximations in matching markets from Azevedo and Leshno (2016), who use simulation to show the rapid convergence of DA cutoffs to large-market values. The Azevedo-Leshno results are relevant because our DA score is determined entirely by cutoffs.

[19]Appendix Table B2 reports score-controlled estimates of differential attrition by offer status. Applicants who receive charter offers are 3-4 percent more likely to have follow-up scores, a modest difference that seems unlikely to bias the 2SLS charter estimates reported below. This is confirmed by an analysis that omits the 5% of applicants for whom conditional-on-score imbalance is greatest. Results in this trimmed sample are virtually unchanged from those in the full sample.

We conclude this section by noting that when lottery numbers are independent of cutoffs, the DA score described by Theorems 1 and 2 is both unbiased and sufficient for type. This too helps explain the success of an empirical strategy based on these theoretical results. Formally, we have the following finite-sample result:

Proposition 2. *Consider any finite economy and let $\tilde{p}_s(\theta)$ be the estimated DA propensity score obtained by computing $MID_{\theta s}$, τ_s, and Θ_s for each lottery realization and then plugging these quantities into equation (2).*[20] *Suppose that individual lottery numbers are independent of the DA cutoffs estimated in each realization, i.e., $r_i \perp\!\!\!\perp (c_1, ..., c_S)$ for every applicant i. Then the estimated DA propensity score is unbiased for the true propensity score, i.e.,*

$$E(\tilde{p}_s(\theta)) = P(D_i(s) = 1|\theta_i = \theta)$$

for every applicant type θ, where P denotes the probability induced by DA with random lottery numbers. Moreover, assignment is independent of type conditional on the estimated DA propensity score:

$$P(D_i(s) = 1|\tilde{p}_s(\theta_i), \theta_i) = P(D_i(s) = 1|\tilde{p}_s(\theta_i)).$$

These unbiasedness and conditional independence properties also hold for the frequency version of $\tilde{p}_s(\theta)$.

This result (proved in the appendix) applies to the continuum since continuum cutoffs are constant. In finite economies, cutoffs are correlated with individual lottery numbers, so the premise of the proposition is false. Even so, our simulations of DPS SchoolChoice show that lottery numbers are close to uniformly distributed conditional on cutoffs, suggesting the premise is a reasonable approximation for this market. Proposition 2 therefore provides a finite-sample rationale for the use of the estimated DA propensity score in our application, and may apply more generally, for example, in other large market models like Kojima and Pathak (2009); Lee (2014); Ashlagi et al. (2015). The near-unbiasedness of DPS charter score estimates constructed using Theorems 1 and 2 is documented in Figure 3. This figure plots the average frequency and formula scores across 2,000 lottery draws against the corresponding values of the simulated score (computed using one million draws and rounded to 0.01). The figure shows a close fit.

4 Using the Score

4.1 Empirical Strategies

We use DPS's first-round charter offers to construct instrumental variables estimates of the effects of charter enrollment on achievement. How should the resulting IV estimates be interpreted? Our IV procedure identifies causal effects for applicants enrolling in a charter 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

[20] Here, $\tilde{p}_s(\theta)$ is the estimated DA score for a fixed finite economy. In contrast, $\hat{p}_{ns}(\theta)$ in Section 3.3 is the estimated DA score for random finite economies sampled from a continuum economy.

to reveal average causal effects for applicants who decline a first round DA charter offer and are assigned another type of school in round 2 (in the LATE framework, these are never-takers). Likewise, IV methods are not directly informative about the effects of charter enrollment on applicants not offered a charter seat in round 1, but who nevertheless find their way into a charter school in the second round (LATE always-takers).

To flesh out this interpretation and the assumptions on which it rests, let C_i be a charter enrollment indicator and let D_i indicate the offer of a charter seat. These variables indicate attendance and offers at *any* charter school, rather than at a specific school. Since DA produces a single offer, offers of seats 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, $p_D(\theta) \equiv E[D_i|\theta_i = \theta]$, is obtained by summing the scores for type θ for all charter schools.

The population of charter-offer compliers is defined by *potential* treatment status. Potential treatment status (charter enrollment status) is indexed against the charter offer instrument, denoted D_i. In particular, we see potential treatment C_{1i} when D_i is switched on and potential treatment C_{0i} otherwise (both of these are also assumed to exist for all i). Observed treatment is therefore

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

Compliers have $C_{1i} - C_{0i} = 1$, an event that happens when $C_{1i} = 1$ and $C_{0i} = 0$.

Causal effects are determined by potential outcomes, indexed against C_i. These are written as Y_{1i} and Y_{0i}. When $C_i = c$, we see Y_{ci}, so the observed outcome (a test score) is

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

Proposition 1 makes it likely that, conditional on type, the offer variable, D_i, is independent of potential assignments. Given an exclusion restriction, the conditional random assignment of D_i also makes D_i conditionally independent of potential outcomes. The exclusion restriction in this case means that charter offers have no effect on outcomes other than by boosting charter attendance. The conceptual distinction between random assignment and exclusion is discussed in Angrist et al. (1996) and, for the case of charter offers, in our working paper (Abdulkadiroglu et al., 2015). As a practical matter, the exclusion restriction fails when charter offers change school quality *within* charter and non-charter sectors. This most likely occurs when charter offers change the type of school attended on a margin other than charter attendance. We therefore explore multi-sector models that identify the causal effects of attendance at different types of charter and non-charter schools. Estimates of multi-sector models are reported following 2SLS estimates of overall charter effect.

As with the conditional independence of single-school offers described by Proposition 1, the conditional independence and exclusion assumptions motivating 2SLS can be written:

$$P[D_i = 1|\{Y_{1i}, Y_{0i}, C_{1i}, C_{0i}\}, \theta_i = \theta] = P[D_i = 1|\theta_i = \theta] = p_D(\theta) \quad (3)$$

where the vector of potentials, $\{Y_{1i}, Y_{0i}, C_{1i}, C_{0i}\}$, plays the role of W_i. Likewise, as for single-school offers in equation (1), the propensity score theorem implies

$$P[D_i = 1|\{Y_{1i}, Y_{0i}, C_{1i}, C_{0i}\}, p_D(\theta_i)] = p_D(\theta_i) \quad (4)$$

where $p_D(\theta_i)$ is the charter-offer propensity score associated with applicant i's type.

Equations (3) and (4) allow us to estimate causal effects of charter *offers*, that is, the effect of D_i. In practice, however, we're interested in the effects of charter *attendance*, the treatment indicated by C_i. To complete the causal chain from charter offers to charter enrollment and finally to outcomes, we assume that charter offers cause charter enrollment for at least some applicants, and that charter offers can only make charter enrollment more likely, so that $C_{1i} \geq C_{0i}$ for all i. With these first-stage and monotonicity assumptions supplementing (4), the conditional-on-score IV estimand is a conditional average causal affect for compliers at that score value.[21] That is, for all θ_i with $p_D(\theta_i) = x \in (0, 1)$,

$$\frac{E[Y_i|D_i = 1, p_D(\theta_i) = x] - E[Y_i|D_i = 0, p_D(\theta_i) = x]}{E[C_i|D_i = 1, p_D(\theta_i) = x] - E[C_i|D_i = 0, p_D(\theta_i) = x]} = E[Y_{1i} - Y_{0i}|p_D(\theta_i) = x, C_{1i} > C_{0i}], \quad (5)$$

where x indexes values in the support of $p_D(\theta)$.

In view of the fact that (5) generates a distinct causal effect for each score value, it's natural to consider parsimonious models that use data from all propensity-score cells to estimate a single average causal effect. We marginalize conditional effects by estimating a 2SLS specification with first and second stage equations that can be written:

$$C_i = \sum_x \gamma(x) d_i(x) + \delta D_i + X_i' \lambda + \nu_i \quad (6)$$

$$Y_i = \sum_x \alpha(x) d_i(x) + \beta C_i + X_i' \mu + \epsilon_i, \quad (7)$$

where the $d_i(x)$'s are dummies indicating values of the estimated score, $\hat{p}_D(\theta_i)$, indexed by x, and $\gamma(x)$ and $\alpha(x)$ are the associated "score effects" in the first and second stages. The coefficient δ in (6) is the first-stage effect of charter offers on charter enrollment, while the coefficient β in (7) is the causal effect of interest. These first and second stage equations include baseline covariates, X_i, to increase precision and adjust for any chance imbalances in applicant characteristics.

As a check on the 2SLS specification, we also report semiparametric estimates of $E[Y_{1i} - Y_{0i}|C_{1i} > C_{0i}]$. In contrast with the additive 2SLS setup, the semiparametric procedure requires only correct specification of the propensity score to generate a single average causal effect for all compliers. Our semiparametric strategy uses Abadie (2003)'s observation that the conditional independence and exclusion restrictions imply:

$$E[Y_{0i}|C_{1i} > C_{0i}] = \frac{1}{Pr(C_{1i} > C_{0i})} E\left[\frac{C_i Y_i (D_i - p_D(\theta_i))}{(1 - p_D(\theta_i)) p_D(\theta_i)}\right]$$

$$E[Y_{1i}|C_{1i} > C_{0i}] = \frac{1}{Pr(C_{1i} > C_{0i})} E\left[\frac{(1 - C_i) Y_i ((1 - D_i) - (1 - p_D(\theta_i)))}{(1 - p_D(\theta_i)) p_D(\theta_i)}\right].$$

Subtracting and rearranging, we have:

$$E[Y_{1i} - Y_{0i}|C_{1i} > C_{0i}] = \frac{1}{Pr(C_{1i} > C_{0i})} E\left[\frac{Y_i (D_i - p_D(\theta))}{(1 - p_D(\theta_i)) p_D(\theta_i)}\right]. \quad (8)$$

[21]Monotonicity is plausible because noncompliance of any sort arises through post-match appeals. Specifically, applicants can appeal SchoolChoice offers after the match, no matter what they're offered in SchoolChoice. But the offer of a charter seat through SchoolChoice produces a charter option that remains available regardless of the result of the appeal. The appeals process therefore seems unlikely to reduce charter enrollment for applicants effectively guaranteed a charter seat.

The first stage in this case, $P[C_{1i} > C_{0i}]$, is constructed using

$$P[C_{1i} > C_{0i}] = E\left[\frac{C_i(D_i - p_D(\theta_i))}{(1 - p_D(\theta_i))p_D(\theta_i)}\right]. \quad (9)$$

The semi-parametric IV estimator used here is the sample analog of the right hand side of (8) divided by the sample analog of (9).

4.2 Effects of Charter Enrollment

As can be seen in Table 6, 2SLS estimates of charter attendance effects are similar to the corresponding semiparametric estimates. Compare, for instance, the semiparametric estimates of effects on math and writing scores of 0.37 and 0.22 in column 1 with the 2SLS estimates of 0.35 and 0.18 in column 2. Both of these control for simulated scores. The standard errors for the semiparametric estimates using the simulated score are higher than those for 2SLS (semiparametric precision is estimated using a Bayesian bootstrap that randomly reweights observations; see, e.g. Shao and Tu (1995)). There are further substantial precision gains in 2SLS estimates using models that control for covariates beyond the score, reported in column 3. The similarity of 2SLS and semiparametric estimates and the relative simplicity of 2SLS estimation leads us to focus on 2SLS estimates with covariates in what follows.[22] It's also worth noting that 2SLS can be interpreted as a "doubly robust" variation on the semiparametric IV strategy; see, e.g., Robins (2000) and Okui et al. (2012).[23]

A DA-generated charter offer boosts charter school attendance rates by about 0.4. These first stage estimates, shown in the first row of Table 6, are computed by estimating equation (6). The first stage of 0.4 reflects the fact that many charter applicants who are not offered a seat in the SchoolChoice first round ultimately find their way into a charter school by applying to schools directly in the second round (specifically, 44% of the charter applicants analyzed in Table 6 are always-takers who enroll in charters even without a first-round charter offer, while fewer than 20% of the analysis sample are never-takers who decline charter offers). First-stage estimates of around 0.56 computed without score controls, shown in column 6 of the table, are clearly biased upwards.

2SLS estimates of charter attendance effects on test scores, reported below the first-stage estimates in Table 6, show remarkably large gains in math, with smaller effects on reading. The math gains reported here are similar to those found for charter students in Boston (see, for example, Abdulkadiroğlu et al. 2011). Previous lottery-based studies of charter schools likewise report substantially larger gains in math than in reading. Here, however, we also see large and statistically significant gains in writing scores.

[22] Controls include baseline test scores and the covariates described earlier. Estimates are for scores on exams taken in grades 4-10. The sample used for IV estimation is limited to charter applicants with the relevant propensity score in the unit interval, for which score cells have offer variation in the data at hand (these restrictions amount to the same thing for the frequency score).

[23] 2SLS also obviates the need for judgements regarding bootstrap methods or implementation. We found, for example, that a conventional nonparametric bootstrap for the semiparametric estimators requires trimming or tuning to eliminate the influence of occasional small first stage estimates.

Importantly for our methodological agenda, the estimated charter attendance effects reported in Table 6 are largely invariant to whether the propensity score is estimated by simulation or by a frequency or formula calculation that uses Theorem 1. Compare, for example, math impact estimates of 0.415, 0.417, and 0.415 using simulation-, frequency-, or formula–based score controls, all estimated with similar precision (these appear in columns 3-5). This alignment further validates the use of Theorem 1 to control for applicant type.

Estimates that omit propensity score controls highlight the risk of selection bias in a naive 2SLS empirical strategy. This is documented in column 6 of Table 6, which shows that 2SLS estimates of math and writing effects constructed using DA offer instruments while omitting propensity score controls are too small by about half. A corresponding set of OLS estimates without propensity score controls, reported in column 7 of the table, also tends to underestimate the gains from charter attendance attendance.[24]

4.3 Unbundling Hetereogeneity

Earlier empirical work emphasizes charter sector heterogeneity (see, e.g., Angrist et al. (2013)). It's therefore of interest to estimate separate character attendance effects for different sorts of schools. Since just over half of the schools listed in Table 2 belong to one of three Denver Charter Management Organizations (CMOs), it's natural to split the charter sector by CMO affiliation. Charters run by CMOs implement common practices across school sites, and CMOs similar to those operating in Denver appear have produced especially large achievement gains (Teh et al., 2010; Gleason et al., 2014; Angrist et al., 2012).

The 2SLS estimates in Table 6 also 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 unbundle 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 changes in counterfactual outcomes that might cause violations of the exclusion restriction.

The first step in our effort to unpack school sectors is to describe the distribution of charter and non-charter school choices for applicants who were and weren't offered a charter seat in the SchoolChoice match. We then identify the distribution of 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 treatment and counterfactuals in our 2SLS strategy more homogeneous.

Important DPS sectors besides charters are traditional public schools, innovation schools, magnet schools, and alternative schools. Innovation and magnet schools are managed by DPS. Innovation schools design and implement innovative practices meant to improve student outcomes. Innovation schools operate under an innovation plan that waives some provisions of the relevant collective bargaining agreements (for a descriptive evaluation of these schools, see Connors et al. 2013).[25] Magnet schools serve students with particular styles of learning. Alternative

[24]The OLS estimation sample includes charter applicants, ignoring score- and cell-variation restrictions.

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

schools serve older students who have struggled in a traditional school environment. Smaller school sectors include a single charter middle school outside the centralized DPS assignment process (now closed) and a private school contracted to serve DPS students.

The distribution of enrollment sectors for applicants who do and don't receive a charter offer is described in the first two columns of Table 7. These columns show a charter enrollment rate of 88% in the group offered a charter seat, with roughly 76% enrolling in a CMO charter. Perhaps surprisingly, only around 38% of those not offered a charter seat enroll in a traditional public schools, with the rest of the non-offered group distributed over a variety of sectors. Innovation schools are the leading non-charter alternative to traditional public schools.

The sector distribution for non-offered applicants with non-trivial charter risk (meaning a charter offer score strictly between zero and one) appears in column 3 of Table 7, alongside the sum of the non-offered mean and a charter-offer treatment effect on enrollment in each sector in column 4. These first-stage estimates, computed by putting indicators $1(S_i = j)$ on the left-hand side of equation (6), control for the DA propensity score and therefore have a causal interpretation. The number of applicants not offered a seat who end up in a charter school is higher for those with non-trivial charter offer risk than in the full applicant sample, as can be seen by comparing columns 3 and 1. The charter enrollment first stage that is implicit in the column 4-vs-3 comparison matches the first stage in column 4 of Table 6. The distinction between CMO and non-CMO categories, however, shows that the charter offer instrument mostly moves applicants into CMO charters. First stages for other sectors show that charter offers reduce innovation and traditional public school enrollment.

The 2SLS estimates reported in Table 6 capture causal effect for charter lottery compliers. We describe the distribution of school sectors for compliers by defining *potential* school sectors, S_{1i} and S_{0i}, indexed against charter offers, D_i. Potential and observed school sectors are related by

$$S_i = S_{0i} + (S_{1i} - S_{0i})D_i.$$

In the population of charter-offer compliers, $S_{1i} = charter$ for all i: by definition, charter-offer compliers attend a charter school when the DPS assignment offers them the opportunity to do so. The top panel of Table 7 reports the breakdown of charter sector for charter-offer compliers, showing (in the last column) that 96% of offered compliers attend CMO charters. We're also interested in $E[1(S_{0i} = k)|C_{1i} > C_{0i}]$ for sectors indexed by k, that is, the sector type distribution for charter-offer compliers in the scenario where they aren't offered a charter seat. We refer to this distribution as describing counterfactual *enrollment destinies* for compliers.

Enrollment destinies are marginal potential outcome distributions for compliers. As shown by Abadie (2002), these are identified by a simple 2SLS estimand. The details of our implementation of this identification strategy follow those in Angrist et al. (2016b), with the modification that instead of estimating marginal potential outcome densities for a continuous variable, the outcomes of interest here are Bernoulli.[26]

et al. (2011).

[26] Briefly, our procedure puts $(1-C_i)1(S_i = k)$ on the left hand side of a version of equation (7) with endogenous variable $1 - C_i$. The coefficient on this endogenous variable is an estimate of $E[1(S_{0i} = k)|C_{1i} > C_{0i}, X_i]$. The covariates and sample used here are the same as those used to construct the 2SLS impact estimates reported in column 4 of Table 6.

Column 5 of Table 7 reveals that only about 41% charter lottery compliers are destined to end up in a traditional public school if they aren't offered a charter seat. Moreover, an innovation school enrollment destiny is just as likely as a traditional public school. By contrast, the likelihood of an enrollment destiny outside the charter, traditional, and innovation sectors is much smaller.

4.4 Additional School Sector Effects

The importance of Denver's CMO charters in our first stage and the outsize role of innovation schools in counterfactual destinies motivates an empirical strategy that distinguishes the effects of CMO and non-CMO charters and allows for separate innovation school treatment effects. By pulling innovation schools out of the non-charter counterfactual, we capture charter treatment effects driven mainly by the contrast between charter and traditional public schools. Models with a more homogeneous non-charter counterfactual also mitigate bias that might arise from violations of the exclusion restriction (discussed in Section 4.1). The innovation treatment effect is also of interest in its own right.

To facilitate the causal analysis of multiple school sectors, we write the potential outcome for sector k as Y_{ki}, representing the latent outcome when $S_i = k$, for school sectors coded by $k \in \{0, 1, ...K\}$. This leads to $K-1$ heterogeneous causal effects: $Y_{Ki} - Y_{0i}$, ..., $Y_{2i} - Y_{0i}$, and $Y_{1i} - Y_{0i}$. Identification of multiple LATEs with unrestricted heterogeneity is challenging and raises issues that go beyond the scope of this paper.[27] We therefore assume constant effects for each sector:

$$Y_{ki} - Y_{0i} = \beta_k. \tag{10}$$

With constant effects, a multi-sector identification strategy can be motivated by the simple conditional independence assumption,

$$Y_{0i} \perp\!\!\!\perp Z_i | \theta_i, \tag{11}$$

where $Z_i = k \in \{0, 1, ...K\}$ is a categorical variable that records DA-generated offers in each sector.

The instruments for the multi-sector model are the full set of indicators for offers in sector ℓ: $\{D_i^\ell = 1[Z_i = \ell]; \ell = 1, ...K\}$. These dummy instruments are used in a 2SLS procedure with endogenous variables $C_i^k = 1[S_i = k]$ for sector k enrollment. As in Imbens (2000)'s extension of the propensity score method to multiple treatments, propensity score conditioning to make the instruments ignorable is justified by the fact that (11) implies

$$Y_{0i} \perp\!\!\!\perp Z_i \mid p_1(\theta), p_2(\theta), ..., p_{K-1}(\theta); \tag{12}$$

where $p_\ell(\theta) = E[D_i^\ell | \theta]$ for $\ell = 1, ...K$.[28]

[27] See Behaghel et al. (2013), Blackwell (2015), and Hull (2016) for recent progress on multi-treatment IV models with heterogeneous effects.

[28] Imbens (2000) and Yang et al. (2016) call the score for indicators of values of a multi-level treatment the *generalized propensity score*.

The 2SLS setup in this case consists of the second and first stage equations,

$$Y_i = \sum_{\ell=1}^{K} \sum_{x} \alpha_\ell(x) d_i^\ell(x) + \sum_{k=1}^{K} \beta_k C_i^k + \epsilon_i \tag{13}$$

$$C_i^k = \sum_{\ell=1}^{K} \sum_{x} \gamma_{\ell k}(x) d_i^\ell(x) + \sum_{\ell=1}^{K} \delta_{\ell k} D_i^\ell + \nu_{ik}, \quad \text{for } k = 1, ..., K \tag{14}$$

where the dummy control variables, $d_i^\ell(x)$, saturate estimates of the propensity scores for each offer dummy, D_i^ℓ, with corresponding score effects denoted by the γ's and α's in the first and second stage models. Note that there are as many first stages as there are sectors (minus one) and that each offer dummy appears in each first stage equation, with an associated set of score controls for that offer. The sample used for this analysis contains the union of the sets of charter and innovation school applicants, including all applicants with assignment risk in any sector in the model.

The conditional independence relation (12) suggests we should control for conditional probabilities of assignment for all treatment levels jointly. Joint score control replaces the additive score controls in equations (13) and (14) with score controls of the form

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

where hats denote score estimates and the indices, $(x^1, x^2, ..., x^K)$ run independently over all values in the support for each score. This model generates far more score fixed effects than appear in equation (13). Fortunately, however, the algebra of 2SLS obviates the need for joint score control; additive control as in (13) is enough, a conclusion that follows from the following proposition.

Proposition 3. *Let $q(\theta_i)$ be an arbitrary function of type. Consider the 2SLS estimator β_k^q for $k = 1, ..., K$ constructed by estimating (13) and (14) with additional controls $q(\theta_i)$ in each equation. Then,*

$$\beta_k^q = \beta_k.$$

Proof. Note first that 2SLS estimates of (13)-(14) can be obtained by regressing first stage fitted values on the controls in these two equations (the full set of score dummies and any other covariates included in these equations) and then using the residuals from this regression as instruments for a model that omits the score dummies and additional covariates (see, e.g., Section 4.1 in Angrist and Pischke 2009). Equivalently, since first-stage fitted values are a linear combination of offer dummies, we can regress each of the offer dummies on these same controls and use the resulting residuals as instruments.

Now consider the auxiliary regressions that produce these residualized instruments: they have D_i^k on the left hand side, with a saturated model for $p_k(\theta_i)$ and a vector of additional controls, $q(\theta_i)$, on the right. By the law of iterated expectations, the conditional expectation function (CEF) associated with this auxiliary regression is therefore

$$E[D_i^k | p_k(\theta_i), q(\theta_i)] = E\{E[D_i^k|\theta_i] | p_k(\theta_i), q(\theta_i)] = p_k(\theta_i).$$

In other words, having conditioned on $p_k(\theta_i)$, other functions of θ_i drop out of the CEF (this is just a restatement of the propensity score theorem). Moreover, because our 2SLS procedure uses a saturated model for the own-score $p_k(\theta_i)$, the CEF $E[D_i^k|p_k(\theta_i), q(\theta_i)]$ is linear in regressors, so it and the associated auxiliary regression function coincide. □

The argument for additive control is completed by observing that both the additive and joint models implicitly control for a full set of own-score dummies and additional functions of θ_i. In particular, for both of these choices of $q(\theta_i)$, as for any other, the auxiliary regression that generates the instruments used by 2SLS has residual $D_i^k - p_k(\theta_i)$.[29]

Multi-Sector Estimates

As a benchmark, columns 1-3 of Table 8 report three sets of single-sector 2SLS estimates, comparing CMO charter-only, non-CMO charter-only, and innovation-only estimates computed using DA (frequency) score controls. Each sample is limited to applicants to the relevant sector.[30] The CMO-charter first stage (the effect of a CMO-charter offer on CMO-charter enrollment) is around 0.49. The non-CMO charter first stage is 0.33. It's worth noting that these two column use different instruments, one capturing CMO charter offers and one indicating non-CMO offers. This fact makes it possible to separately identify a non-CMO charter effect, even though the any charter instrument shifts most applicants to charter schools, as we saw in Table 7. The innovation school first stage (the effect of an innovation school offer on innovation school enrollment) is around 0.37. Not surprisingly in view of the substantially reduced number of applicants with non-trivial non-CMO and innovation offer risk (401 and 942) and the corresponding smaller first stages, both the non-CMO and Innovation attendance effects are less precise than the CMO effects. Even so, it's noteworthy that the effects of non-CMO and innovation school attendance are negative or close to zero.

2SLS estimates of equation (13) appear in columns 4 and 5 of Table 8. The large CMO-charter school effects reported in column 1 remain substantial in this specification, but (insignificant) negative innovation estimates for math flip to positive when estimated using a model that also isolates the two charter treatment effects. The negative innovation school effects on reading seen in column 3 also become smaller in the three-endogenous-variables models. Most interestingly, perhaps, the significant positive CMO-charter school effect on reading in column 1 is smaller and marginally significant in both column 4 and 5. While charter applicants' reading performance exceeds what we can expect to see were these applicants to enroll in a mix of traditional and (low-performing) innovation schools, the reading gap between CMO-charters and traditional public schools appears to be a little smaller.

As the theoretical discussion above leads us to expect, the results of estimation with joint score controls, reported in column 5 of Table 8, differ little from the estimates constructed using

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

[30] Appendix Table B3 lists innovation schools and describes the random assignment pattern at these schools along the lines of Table 1 for charter schools. Covariate balance and differential attrition results for innovation schools are reported in Appendix Table B4.

additive score controls. Overall, it seems fair to say that the findings showing substantial charter effectiveness in Table 6 are driven entirely by CMO charters, and that these findings hold up when effects are estimated using a procedure that removes the innovation sector from the charter enrollment counterfactual.

4.5 Alternative IV Strategies

Previous research eliminates the selection bias that arises from the dependence of assignments on preferences and priorities by focusing either on offers of seats at applicants' first choice schools, or using instrumental variables (IVs) indicating whether an applicant's lottery number falls below the highest number offered a seat at all schools he's ranked (we call this a qualification instrument). The first choice strategy conditions on the identity of the school ranked first, while qualification instruments condition on the set of schools ranked. These IV strategies are likely to produce estimates of school attendance free of omitted variables bias. At the same time, both first-choice and qualification instruments discard much of the variation induced by centralized assignment.

We're interested in comparing 2SLS estimates constructed using offer dummies as instruments while controlling for the DA propensity score with suitably-controlled estimates constructed using first-choice and qualification instruments. We expect DA-offer instruments to yield a precision gain while also increasing the number of schools represented in the estimation sample relative to these two previously-employed IV strategies.[31]

Let $X(\theta_i)$ be a variable identifying the charter school that applicant i ranks first, along with his priority status at this school, defined for applicants whose first choice is indeed a charter school. $X(\theta_i)$ ignores other schools that might have been ranked.[32] The first-choice strategy is implemented by the following 2SLS setup:

$$Y_i = \sum_x \alpha(x) d_i(x) + \beta C_i + \epsilon_i$$
$$C_i = \sum_x \gamma(x) d_i(x) + \delta D_i^f + \nu_i,$$

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

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

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

[31] Studies using first-choice instruments to evaluate schools in districts with centralized assignment include Abdulkadiroğlu et al. (2013), Deming (2011), Deming et al. (2014), and Hastings et al. (2009). First-choice instruments have also been used with decentralized assignment mechanisms (Abdulkadiroğlu et al. (2011), Cullen et al. (2006), Dobbie and Fryer (2011), and Hoxby et al. (2009)). Dobbie and Fryer (2014), Lucas and Mbiti (2014), and Pop-Eleches and Urquiola (2013) use qualification instruments.

[32] DPS divides each school into buckets, as explained in Section 2.3. Our first-choice risk set therefore identifies applicants in all buckets at the first-choice school along with priority status for each of these buckets.

The qualification strategy expands the sample to include all charter applicants, with the risk sets for qualification instruments identifying the set of all charter schools that i ranks, along with his or her priority status at each of these schools (again, these risk sets are denoted $X(\theta_i)$). In this case, $X(\theta_i)$ ignores the order in which schools are ranked, coding only their identities, but priorities are associated with schools.[33] The qualification instrument, D_i^q, indicates qualification at *any* charter he or she has ranked. In other words,

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

In large markets, the instruments D_i^f and D_i^q are independent of type conditional on $X(\theta_i)$; see Appendix A.8 for details.

A primary source of inefficiency in the first-choice and qualification strategies is apparent in Panel A of Table 9. 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 0.73 in the sample of those who have 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 27% 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 0.46. This reflects the fact that for schools below the one ranked first, charter qualification is insufficient 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 0.44. But first-choice offers boost charter enrollment by only 0.35, while qualification at any charter yields a charter enrollment gain of only 0.23. As always, the size of the first stage is a primary determinant of the precision of an IV estimate.

At 0.050, the standard error of the DA-offer estimate is lower than the standard error of 0.064 yielded by a first-choice strategy and well below the standard error of 0.092 generated by qualification instruments. The precision loss here is similar to the decline in the intermediate first stages recorded in the first row of the table (compare 0.73 with 0.050/0.064 = 0.78 and 0.46 with 0.050/0.092 = 0.54). The precision 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.[34][35]

[33] 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. Also, precisely speaking, "schools" in the above description of the qualification risk set are buckets.

[34] The sample used to construct the estimates in columns 1-3 of Table 9 is limited to those who have variation in the instrument at hand conditional on the relevant risk sets controls.

[35] This pattern is consistent with theoretical econometric results in Newey (1990) and Hong and Nekipelov (2010), who show that the semiparametric efficiency bound for LATE-type estimates is proportional to the number of compliers, i.e., to the size of the first stage. Hong and Nekipelov (2010) also show (p.294) that the efficient estimator of marginal-over-covariates LATE ($E[Y_1 - Y_0|D_1 > D_0] = E\{E[Y_1 - Y_0|D_1 > D_0, X]|D_1 > D_0\}$) is a

The precision loss from alternative IV strategies is even starker for multi sector models. For example, when estimating sector effects jointly, with additive score controls (as in column 4 of Table 8) the innovation school math effect estimated using a first-choice offer instrument has a standard error of 0.737, while the corresponding standard error using a qualification instrument is 1.879. These can be compared with the standard error of 0.097 using DA offers and the DA score. It seems fair to say that multi-sector estimators with these other IV strategies are uninformative.

First-choice analyses lose schools because many lotteries fail to randomize first-choice applicants (as seen in Table 2). It's therefore interesting to note that the first-choice estimate of effects on math and reading scores are noticeably larger than the estimates generated using DA offer and qualification instruments (compare the estimate of 0.42 using DA offers with estimates of 0.52 and 0.38 using first-choice and qualification instruments). This finding may reflect an advantage for those awarded a seat at their first choice school (Hastings et al. 2009; Deming 2011; Deming et al. 2014 find a general "first choice advantage" in analyses of school attendance effects.) By contrast, the DA offer instrument yields an estimand that is more representative of the full complement of charter schools in the match, as suggested by the listing of schools in Table 2 with DA offer instrument variation vs. first-choice instrument variation.

Motivated by the possibility of a "first-choice" advantage, we conclude our empirical analysis by using our multi-sector methodology to estimate models with separate effects for first-choice charter and other-choice charters. As for the estimates in Table 8, the instruments for enrollment in more narrowly defined sectors are dummies indicating offers of seats in these sectors, controlling the corresponding narrow-sector propensity score. For first-choice charters, for example, the instrument indicates offers at a charter ranked first and the propensity score is the probability of receiving an offer at a charter ranked first.

Consistent with the hypothesis of "first-choice" advantage, the estimates in Table 10 suggest first-choice charters generate achievement effects beyond those of charters ranked lower. Compare, for example, the 0.40 math estimate for first-choice charters with the 0.25 estimate for other-choice charters, both of which are reported in column 1. Likewise, for reading, the estimates in column 3 show a gain around 0.15 at first-choice charters, with an estimated zero reading effect elsewhere. As can be seen in column 5, estimates of effects on writing are similar at both types of schools.

We've seen that CMO charters drive positive overall charter effects. It's natural, therefore, to ask whether the first choice advantage is visible within CMO and non-CMO sectors. The estimates reported in even numbered columns in Table 10 are from a model with four endogenous variables, distinguishing first-choice and other charters by their CMO status. The impression of CMO charter quality remains in this parameterization. We see, for example (in column 2), that among charters ranked first, CMO charters boost math scores by 0.43, while non-CMO charters ranked first have essentially no effect. A similar contrast in favor of CMOs appears for other subjects as well, though it should be noted that the non-CMO estimates here are not very

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weighted average of empirical covariate-specific Wald estimators, with weights proportional to the corresponding covariate-specific first stage. For reasons outlined in Section 4.2, we prefer conventional 2SLS, which is efficient for constant effects and implicitly weights by an increasing function of the first stage as well. See also Frölich (2007).

precise.

5 Summary and Directions for Further Work

We've shown here how to analyze the stratified randomized trial induced by any centralized assignment mechanism satisfying the equal treatment of equals property. DA is the leading mechanism in the ETE class. Our main theoretical result is an analytical formula for the DA propensity score, derived using a large market approximation. This approximation works well in our DPS application in the sense of producing the covariate balance promised by the propensity score theorem.

The theoretical results developed here extend to other widely-using matching schemes, including immediate acceptance and random serial dictatorship, as well as to matches using multiple tie-breakers. The DA propensity score also reveals the nature of the experimental design embedded in DA, as well as suggesting how parameters of market design (say, priorities) might be modified so as to boost the research value of school assignment or other matching schemes. Finally, as a theoretically matter, the DA score provides a natural data-dependent automatic smoother for the finite-market propensity score. In ongoing work, we're extending the framework in this paper to cover centralized assignment schemes using top trading cycles.

A score-based analysis of data from Denver's unified school match reveals substantial gains from attendance at one of Denver's many charter schools. The resulting charter effects are similar to those computed using single-school lottery strategies for Boston's charters reported in Abdulkadiroğlu et al. (2011) and to estimates computed using charter takeovers in New Orleans (Abdulkadiroğlu et al., 2016). At the same time, as with previously reported results for Boston Pilot schools, Denver's Innovation school model does not appear to boost achievement. As always, econometric estimates need not predict the effects of policy changes. But the track record for charter lottery estimates is encouraging. In addition to the fact that lottery estimates have been replicated in many large urban districts, Cohodes et al. (2016) show that a recent wave of Boston charter expansions, a policy innovation prompted by a legislative change, produced achievement gains very much in line earlier lottery estimates.

Our analysis focuses on defining and estimating the DA propensity score, giving less attention to the problem of how best to use the score for estimation. Still, simple 2SLS procedures seem to work well, and the resulting estimates of DPS charter effects differ little from those generated by semiparametric alternatives. Estimates using DA offer instruments also generate noteworthy precision gains relative to qualification and first-choice instruments, mostly as a consequence of an increased first stage though also (in the case of first choice) by exploiting randomization at a larger set of schools.

The DPS application shows how centralized assignment schemes can be used to unbundle school heterogeneity. We see, for example, that CMO-affiliated charters are much stronger than others in DPS, and achievement gains are larger when applicants are offered seats at charters they rank first. In principle, the empirical strategy demonstrated here can be used to construct single-school value-added estimates, though the VAM agenda raises unique challenges. In a related paper, Angrist et al. (2016a) show how lottery estimates can be embedded in an empirical

Bayes framework that identifies value added for schools that are under-subscribed or for which there's no randomization. A natural direction for future work is the combination of the strategy outlined here with the empirical Bayes VAM framework.

Finally, it's worth noting that matching schemes for selective exam schools (analyzed by Jackson (2010); Dobbie and Fryer (2014); Abdulkadiroğlu et al. (2014); Lucas and Mbiti (2014); Pop-Eleches and Urquiola (2013)) and the US medical match use non-randomly-assigned tie-breakers rather than a lottery. These schemes embed regression discontinuity designs inside a market design rather than embedding a randomized trial. The question of how best to define and exploit the DA propensity score for markets that combine regression-discontinuity tie-breaking with market design matchmaking is an important next step on the market-design-meets-research-design agenda. Abdulkadiroğlu et al. (2017) report initial results on this agenda.

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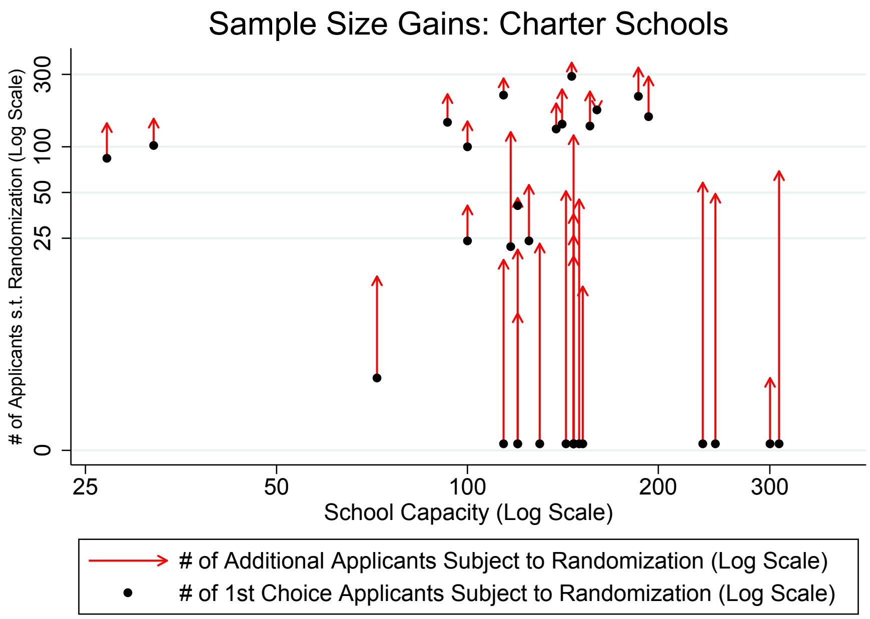
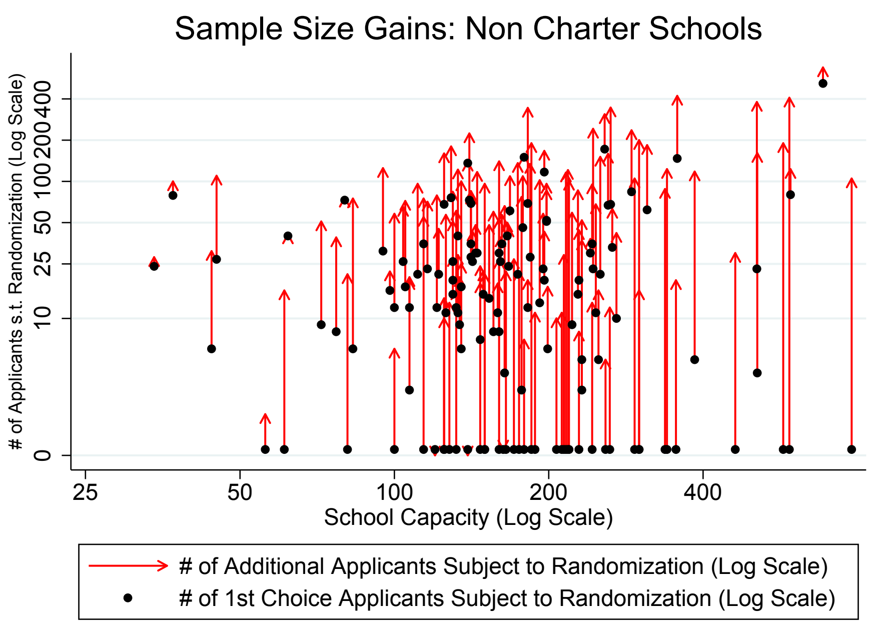
Figure 1: Sample Size Gains from the Propensity Score Strategy



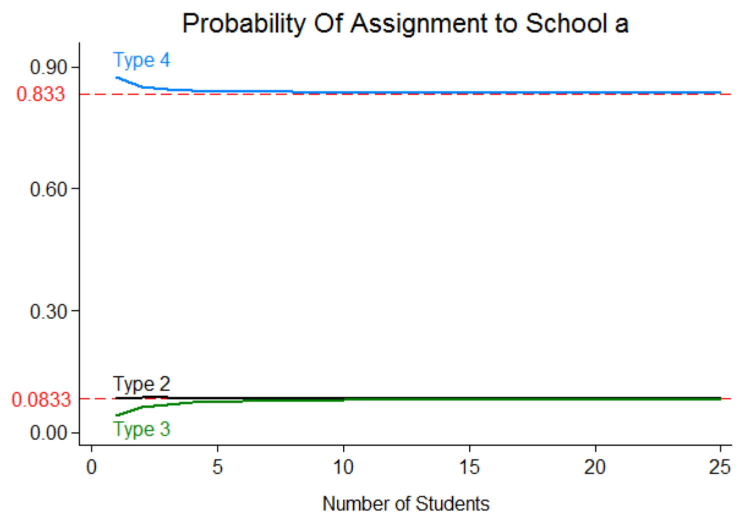
(a)



(b)

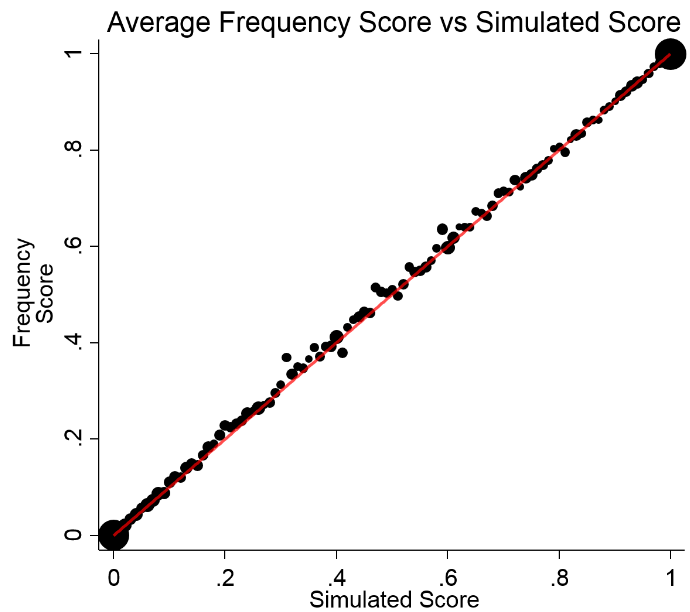
Notes: These figures compare the sample size under our simulated score strategy to that under the first choice strategy. Down arrows mean the two empirical strategies produce the same number of applicants subject to randomization at the corresponding schools. We say an applicant is subject to randomization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

Figure 2: Propensity Scores and Market Size in Example 2

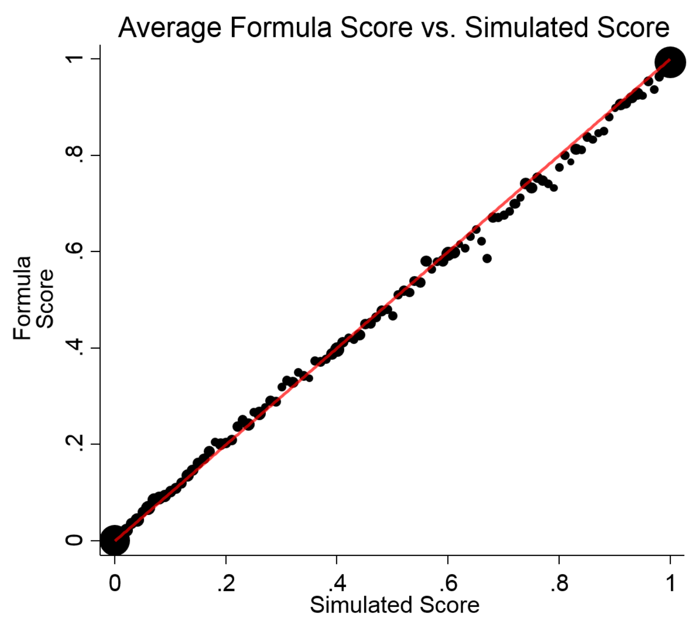


Notes: This figure plots finite-market propensity scores for expansions of Example 2. For each value of the x axis, we consider an expansion of the example with x applicants of each type. The propensity scores plotted here were computed by drawing lottery numbers 100,000 times and rerunning the DA algorithm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

Figure 3: Comparison of Frequency and Formula Score to Simulated Score



(a)



(b)

Notes: This figure plots average frequency and formula score across 2000 simulations for 2013 against the average simulated score computed from 1,000,000 lottery draws for each school bucket. Simulated scores are rounded to 0.01 bins. The red line is the 45 degree line. The circles are proportional to the square root of the number students in each 0.01 bin.

Table 1: Baseline characteristics for DPS applicants

	Denver students	SchoolChoice applicants	Charter applicants	Simulated score in (0,1) Charter applicants	Simulated score in (0,1) Charter students
	(1)	(2)	(3)	(4)	(5)
Origin school is charter	0.151	0.088	0.144	0.192	0.273
Female	0.494	0.495	0.505	0.500	0.495
Race					
Hispanic	0.594	0.600	0.640	0.645	0.698
Black	0.140	0.143	0.167	0.186	0.161
White	0.193	0.184	0.126	0.102	0.082
Asian	0.034	0.033	0.028	0.027	0.029
Applied in 2013	0.490	0.488	0.487	0.445	0.445
Gifted	0.180	0.214	0.203	0.180	0.175
Bilingual	0.040	0.030	0.038	0.037	0.038
Subsidized lunch	0.752	0.763	0.804	0.815	0.827
Limited English proficient	0.297	0.301	0.344	0.364	0.419
Special education	0.119	0.122	0.091	0.087	0.084
Baseline scores					
Math	0.000	-0.003	0.008	-0.009	0.043
Reading	0.000	-0.003	-0.009	-0.032	-0.022
Writing	0.000	-0.008	-0.005	-0.012	0.019
N	51,325	22,311	10,203	3,466	1,769

Notes: This table decribes the population of Denver 3rd-9th graders in 2011-2012 and 2012-2013, the baseline years. Statistics in column 1 are for charter and non-charter students. Column 2 describes the subset that submitted an application to the SchoolChoice system for a seat in grades 4-10 at another DPS school in 2013 or 2014. Column 3 reports values for applicants ranking any charter school. A charter student is an applicant that enrolled in a charter school. Columns 4 and 5 show statistics for charter applicants and students with simulated score values strictly between zero and one. The simulated score is rounded to 0.001. Columns 5 and 6 show statistics for charter applicants with a frequency score strictly between 0 and 1. Test scores are standardized to the population in column 1.

Table 2: DPS charter schools (2013 only)

School	CMO	Total applicants	Capacity	Applicants offered seats	Simulated score in (0,1): Total applicants	Simulated score in (0,1): Applicants (first choice)
	(1)	(2)	(3)	(4)	(5)	(6)
Elementary and middle schools						
Cesar Chavez Academy Denver		62	72	9	8	3
Denver Language School		4	100	0	0	0
DSST: Cole	Yes	281	150	129	45	0
DSST: College View	Yes	299	310	130	69	0
DSST: Green Valley Ranch	Yes	1014	146	146	358	291
DSST: Stapleton	Yes	849	156	156	231	137
Girls Athletic Leadership School		221	143	86	51	0
Highline Academy Charter School		159	93	26	84	50
KIPP Montbello College Prep	Yes	211	125	39	56	21
KIPP Sunshine Peak Academy	Yes	389	120	83	46	36
Odyssey Charter Elementary		215	32	6	22	14
Omar D. Blair Charter School		385	193	114	182	99
Pioneer Charter School		25	152	5	2	0
SIMS Fayola International Academy Denver		86	120	37	21	0
SOAR at Green Valley Ranch		85	114	9	44	37
SOAR Oakland		40	117	4	7	2
STRIVE Prep - Federal	Yes	621	138	138	193	131
STRIVE Prep - GVR	Yes	324	147	112	119	0
STRIVE Prep - Highland	Yes	263	147	112	19	0
STRIVE Prep - Lake	Yes	320	147	126	26	0
STRIVE Prep - Montbello	Yes	188	147	37	36	0
STRIVE Prep - Westwood	Yes	535	141	141	239	141
Venture Prep		100	114	50	18	0
Wyatt Edison Charter Elementary		48	300	4	2	0
High schools						
DSST: Green Valley Ranch	Yes	806	186	173	332	263
DSST: Stapleton	Yes	522	27	27	143	96
KIPP Denver Collegiate High School	Yes	268	100	60	41	24
SIMS Fayola International Academy Denver		71	130	15	23	0
Southwest Early College		265	235	76	58	0
STRIVE Prep - SMART	Yes	383	160	160	175	175
Venture Prep		140	246	39	49	0

Notes: This table describes DPS charter applications for the academic year 2012-2013. Column 1 lists all CMO schools. CMO stands for Charter Management Organization, and these schools are comprised of the DSST, STRIVE and KIPP networks. Column 2 reports the number of applicants ranking each school. Column 3 reports each school's capacity. Column 4 counts the number of applicants who received an offer from the school. Column 5 counts applicants with simulated score values strictly between zero and one. The simulated score is rounded to 0.001. Column 6 shows the subset of applicants from column 5 who rank each school as their first choice.

Table 3: DA score anatomy at STRIVE Prep Schools (2013 only)

Campus	Eligible applicants	Capacity	Offers	Θ_s^n	Θ_s^c		Θ_s^a		
				0≤MID≤1	MID ≥ τ_s	MID < τ_s	MID = 1	0<MID<1	MID = 0
				Score = 0	Score = 0	0 < Score < 1	Score = 0	0 < Score < 1	Score = 1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
GVR	324	147	112	0	0	0	159	116	49
Lake	274	147	126	0	0	0	132	26	116
Highland	244	147	112	0	0	0	121	21	102
Montbello	188	147	37	0	0	0	128	31	29
Federal	574	138	138	78	284	171	3	1	37
Westwood	494	141	141	53	181	238	4	0	18

Notes: This table shows how formula scores are determined for STRIVE school seats in grade 6 (all 6th grade seats at these schools are assigned in a single bucket; ineligible applicants are omitted) for academic year 2012-2013. Column 3 records offers made to these applicants. Columns 4, 5 and 7 show the number of applicants in partitions with a score of zero. Columns 6 and 8 show the number of applicants subject to random assignment. Column 9 shows the number of applicants with certain offers.

Table 4: Expected balance

			Simulated score controls		DA score controls	
	Non-offered mean	No controls	Rounded (hundredths)	Rounded (thousandths)	Frequency (saturated)	Formula (saturated)
Covariate	(1)	(2)	(3)	(4)	(5)	(6)
			A. Application covariates			
Number of schools ranked	4.431	-0.533	0.013	0.005	0.049	0.008
Number of charter schools ranked	1.451	0.445	0.005	0.002	0.061	0.001
First school ranked is charter	0.276	0.641	0.000	-0.001	0.001	-0.001
			B. Baseline covariates			
Origin school is charter	0.085	0.124	-0.003	-0.003	0.002	-0.001
Female	0.513	-0.021	0.003	0.003	0.002	0.003
Hispanic	0.596	0.105	0.001	0.001	-0.002	0.004
Black	0.189	-0.053	0.000	0.001	0.001	0.000
Gifted	0.207	-0.009	0.003	0.002	-0.002	-0.001
Bilingual	0.033	0.016	0.000	0.000	0.001	0.001
Subsidized lunch	0.781	0.054	0.001	0.000	0.000	0.002
Limited English proficient	0.306	0.090	0.001	0.001	-0.002	0.002
Special education	0.094	-0.007	-0.001	0.000	-0.003	-0.001
Baseline scores						
Math	-0.028	0.078	-0.003	-0.002	-0.007	-0.005
Reading	-0.001	-0.024	-0.002	-0.004	-0.007	-0.007
Writing	-0.001	-0.011	-0.002	-0.003	-0.008	-0.005
Average risk set points of support			77	153	93	111

Notes: This table reports average covariate balance by charter offer status across 400 lottery draws, with DA rerun each time. The sample includes applicants for 2012-13 and 2013-14 charter seats in grades 4-10 who were enrolled in Denver at baseline. Balance is estimated by regressing each covariate on an any-charter simulated offer dummy, controlling for the propensity score variables indicated in each column header. We also include dummy controls for the year in which the applicant applied. The table reports averages of these balance coefficients. The charter offer variable indicates an offer at any charter school for a given lottery draw (excluding alternative charters). Column 1 reports the baseline characteristics of charter applicants who did not receive a charter offer. Column 2 reports the average coefficient when no propensity score controls are used. The estimates in columns 3, 4, 5 and 6 use score values as indicated in the column header. The average risk set points of support reported at the bottom of the table count the average number of unique values found in the support of the relevant propensity score. These exclude values of 0 and 1 for the propensity score. For applicants who applied in both years, we only consider their first time application.

Table 5: Statistical tests for balance in application and baseline covariates

	Non-offered mean	No controls	Simulated score controls: Rounded (hundredths)	Simulated score controls: Rounded (thousandths)	DA score controls: Frequency (saturated)	DA score controls: Formula (saturated)	Full applicant type controls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Application covariates							
Number of schools ranked	4.438	-0.544***	-0.076	-0.084	-0.091	-0.115	-0.046
		(0.031)	(0.070)	(0.073)	(0.070)	(0.070)	(0.041)
Number of charter schools ranked	1.450	0.443***	-0.027	-0.029	0.030	-0.028	0.001
		(0.018)	(0.035)	(0.037)	(0.037)	(0.035)	(0.018)
First school ranked is charter	0.275	0.639***	-0.026	-0.022	-0.003	-0.006	0.000
		(0.007)	(0.017)	(0.015)	(0.015)	(0.014)	(0.000)
B. Baseline applicant characteristics							
Origin school is charter	0.087	0.118***	-0.027*	-0.029**	-0.029**	-0.039***	0.024
		(0.007)	(0.014)	(0.014)	(0.012)	(0.012)	(0.015)
Female	0.512	-0.017*	0.030	0.023	0.021	0.024	0.027
		(0.010)	(0.025)	(0.027)	(0.026)	(0.027)	(0.055)
Hispanic	0.597	0.102***	-0.011	-0.013	-0.013	-0.006	0.025
		(0.010)	(0.021)	(0.023)	(0.021)	(0.022)	(0.034)
Black	0.188	-0.052***	0.004	0.000	0.005	0.008	-0.020
		(0.007)	(0.019)	(0.020)	(0.019)	(0.020)	(0.028)
Subsidized lunch	0.782	0.052***	-0.007	-0.003	0.003	0.018	0.031
		(0.008)	(0.018)	(0.019)	(0.018)	(0.018)	(0.031)
Limited English proficient	0.305	0.089***	0.006	0.017	0.001	0.021	0.007
		(0.010)	(0.023)	(0.026)	(0.024)	(0.025)	(0.051)
Special education	0.093	-0.005	0.014	0.009	0.006	0.010	0.036
		(0.006)	(0.014)	(0.016)	(0.014)	(0.015)	(0.023)
N	5,674	9,879	2,714	2,291	2,436	2,335	464
Baseline scores							
Math	-0.025	0.073***	-0.038	-0.035	-0.040	-0.047	-0.205**
		(0.019)	(0.044)	(0.049)	(0.045)	(0.047)	(0.093)
Reading	0.003	-0.032*	-0.069	-0.079*	-0.080*	-0.082*	-0.140
		(0.019)	(0.043)	(0.048)	(0.045)	(0.046)	(0.086)
Writing	0.002	-0.016	-0.055	-0.049	-0.058	-0.064	-0.122
		(0.018)	(0.043)	(0.046)	(0.044)	(0.045)	(0.089)
N	5,586	9,743	2,678	2,263	2,406	2,306	462
Risk set points of support			75	141	81	97	89
F-test for joint significance (mvreg)		483.3	0.78	0.82	1.07	1.35	0.92
p-value		0.000	0.702	0.660	0.375	0.163	0.538

Notes: This table reports coefficients from regressions of the application variables and baseline covariates in each row on a dummy for charter offers. The sample includes applicants for 2012-14 charter seats in grades 4-10 who were enrolled in Denver at baseline. Columns 2-5 are from regressions like those used to construct expected balance in Table 4, except that the tests reported here use the realized DA offers, with test statistics and standard errors computed in the usual way. Column 7 reports the balance test generated by a regression with saturated controls for applicant type (that is, unique combinations of applicant preferences over school programs and school priorities in those programs). In columns 3-7, N is the number of applicants in a non degenerate risk set subject to random assignment with a propensity score between 0 and 1. Robust standard errors are reported in parentheses. P-values for joint significance tests are estimated with stata's mvreg command. For applicants who applied in both years, we only consider their first time application.

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

Table 6: Charter effects using alternative score controls

	Simulated score controls (Rounded hundredths)			DA score controls (with covariates)		No score controls (with covariates)	
	Semiparametric	2SLS (no covariates)	2SLS (with covariates)	Frequency (saturated)	Formula (saturated)	2SLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
First stage	0.389***	0.415***	0.420***	0.444***	0.428***	0.561***	
	{0.053}	(0.024)	(0.024)	(0.024)	(0.024)	(0.016)	
Math	0.372***	0.351***	0.415***	0.417***	0.415***	0.231***	0.230***
	{0.133}	(0.108)	(0.052)	(0.050)	(0.052)	(0.030)	(0.010)
Reading	0.180	0.083	0.166***	0.178***	0.162***	0.066**	0.094***
	{0.162}	(0.108)	(0.053)	(0.050)	(0.053)	(0.029)	(0.010)
Writing	0.217	0.184*	0.274***	0.302***	0.309***	0.141***	0.171***
	{0.136}	(0.105)	(0.058)	(0.056)	(0.059)	(0.032)	(0.011)
N	2,229	2,308	2,308	2,092	1,999	2,947	8,528

Notes: This table compares semiparametric, 2SLS, and OLS estimates of charter attendance effects on the 2013 and 2014 TCAP scores of Denver 4th-10th graders. For columns 2, 3, 4, 5 and 6 the instrument is an any-charter offer dummy. Columns 2, 3, 4 and 5 include propensity score controls, while 6 and 7 do not. All 2SLS and OLS estimates include controls for grade tested, gender, origin school charter status, race, gifted status, bilingual status, subsidized school lunch eligibility, special education, limited English proficient status, baseline test scores and year of application. Columns 1, 2 and 3 use the simulated score rounded to 0.01. The semiparametric estimator is described in Section 4.2. The semiparametric model in column 1 only uses score controls for the weighting function. The semiparametric model excludes applicants with a rounded simulated score larger than 0.975 or smaller than 0.025. Standard errors in braces are from a Bayesian bootstrap. Robust standard errors are reported in parentheses. For applicants who applied in both years, we only consider their first time application.

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

Table 7: Enrollment destinies for charter applicants

	All charter applicants		Charter applicants with DA score (frequency) in (0,1)			
			All applicants		Compliers	
	No charter offer	Charter offer	Non-offered mean	First stage + col 3	No charter offer	Charter offer
	(1)	(2)	(3)	(4)	(5)	(6)
A. Decomposing Y_1						
Study charter	0.129	0.884	0.310	0.754	--	1.000
CMO Charter	0.095	0.764	0.248	0.673	--	0.958
Non-CMO Charter	0.034	0.120	0.061	0.080	--	0.042
B. Decomposing Y_0						
Traditional public	0.380	0.066	0.244	0.065	0.405	--
Innovation school	0.283	0.023	0.289	0.109	0.405	--
Magnet school	0.191	0.018	0.126	0.056	0.157	--
Alternative school	0.009	0.005	0.021	0.014	0.015	--
Contract school	0.007	0.002	0.009	0.000	0.022	--
Non-study charter	0.000	0.000	0.001	0.000	0.002	--
N	4,917	3,805	962	2,098	--	--

Notes: This table describes school enrollment outcomes for charter applicants. Columns 1-2 show enrollment by sector for all applicants without and with a charter offer. The remaining columns look only at those with a DA (frequency) score strictly between zero and one. Column 4 adds the non-offered mean in column 3 to the first stage estimate of the effect of charter offers on charter enrollment. School sectors are classified by grade. CMO charters are listed in table 2. Innovation schools design and implement innovative practices to improve applicant outcomes. Magnet schools serve applicants with particular styles of learning. Alternative schools serve applicants 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. There is a single contract school, Escuela Tlatelolco, a private school contracted to serve DPS applicants, and a single non-study charter that closed in May 2013. Complier means in columns 5 and 6 were estimated using the 2SLS procedures described by Abadie (2002), with the same propensity score and controls as were used to construct the estimates in Table 6. For applicants who applied in both years, we only consider their first time application.

Table 8: School sector effects

	DA score (frequency) controls, saturated				
	Single Sector Models			Multi-Sector Models	
	CMOs	Non-CMO	Innovation	Additive score controls	Joint score controls
	(1)	(2)	(3)	(4)	(5)
First Stage	0.488***	0.330***	0.372***	--	--
	(0.024)	(0.057)	(0.033)	--	--
			A. Math		
CMO Charters	0.443***	--	--	0.462***	0.440***
	(0.047)	--	--	(0.057)	(0.063)
Non-CMO Charters	--	-0.083	--	0.008	-0.025
	--	(0.166)	--	(0.165)	(0.157)
Innovation	--	--	-0.131	0.085	0.122
	--	--	(0.092)	(0.097)	(0.104)
N	1,967	401	937	2,679	2,365
			B. Reading		
CMO Charters	0.209***	--	--	0.134**	0.129*
	(0.047)	--	--	(0.064)	(0.070)
Non-CMO Charters	--	-0.267	--	-0.272	-0.198
	--	(0.188)	--	(0.195)	(0.193)
Innovation	--	--	-0.160	-0.114	-0.088
	--	--	(0.101)	(0.113)	(0.120)
N	1,971	402	937	2,685	2,370
			C. Writing		
CMO Charters	0.293***	--	--	0.341***	0.320***
	(0.052)	--	--	(0.068)	(0.075)
Non-CMO Charters	--	0.008	--	0.062	0.067
	--	(0.168)	--	(0.173)	(0.179)
Innovation	--	--	-0.080	0.101	0.059
	--	--	(0.102)	(0.115)	(0.118)
N	1,979	401	942	2,693	2,377

Notes: This table reports 2SLS estimates of CMO, non-CMO charter and innovation attendance effects for applicants to schools in these sectors. CMO charters are described in Table 2. Column 1 reports attendance effects of CMO charters, estimated in models using a CMO offer instrument. Column 2 reports attendance effects of non-CMO charters, estimated in models using a non-CMO offer instrument and non-CMO specific saturated score controls constructed like those used for charter applicants. Column 3 reports attendance effects of innovation schools, estimated in models using an innovation school offer instrument and innovation-specific saturated score controls constructed like those used for charter applicants. Column 4 report coefficients from a three-endogenous-variable/three-instrument 2SLS model for the attendance effects of CMOs, non-CMO charters and innovations, conditioning additively on CMO-specific, non-CMO charter-specific, and innovation-specific saturated score controls. Column 5 shows results from joint-effect models that add interactions between the three scores to the specification that generated column 4. We include the same controls used in Table 6. The first stage reported is for math scores. Robust standard errors are reported in parentheses. For applicants who applied in both years, we only consider their first time application.
*significant at 10%; **significant at 5%; ***significant at 1%

Table 9: 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	0.731***	0.457***		
	--	(0.017)	(0.018)		
First stage for charter enrollment	0.444***	0.347***	0.227***		
	(0.024)	(0.022)	(0.021)		
	B. 2SLS estimates				
Math	0.417***	0.515***	0.379***		
	(0.050)	(0.064)	(0.092)	1.64	3.45
Reading	0.178***	0.258***	0.198**		
	(0.050)	(0.062)	(0.086)	1.56	2.97
Writing	0.302***	0.316***	0.344***		
	(0.056)	(0.071)	(0.094)	1.60	2.81
N	2,092	2,222	3,502		

Notes: This table compares alternative 2SLS estimates of charter attendance effects using the same sample and control variables used to construct the estimates in Table 6. Column 1 repeats the estimates from column 4 in Table 6. The row labeled "First stage for charter offers" reports the coefficient from a regression of 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 2SLS estimates computed using a first-choice charter offer instrument. Column 3 reports charter attendance effects computed using an any-charter qualification instrument. These alternative IV models control for risk sets making the first-choice and qualification instruments conditionally random; see Section 4.5 for details. Columns 4 and 5 report the multiples of the first-choice offer sample size and qualification sample size needed to achieve a precision gain equivalent to the gain from using the any-charter offer instrument. The last row counts the number of schools for which we observe in-sample variation in offer rates conditional on the score controls included in the model. Robust standard errors are reported in parentheses. For applicants who applied in both years, we only consider their first time application.

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

Table 10: DPS charter school attendance effects by choice and CMO status with DA score (frequency) controls, saturated

	Math		Reading		Writing	
	(1)	(2)	(3)	(4)	(5)	(6)
First-choice charters	0.403***		0.147***		0.294***	
	(0.049)		(0.049)		(0.056)	
First-choice charters * CMO		0.427***		0.157***		0.308***
		(0.050)		(0.051)		(0.059)
First-choice charters * non-CMO		0.011		-0.268		0.070
		(0.169)		(0.234)		(0.221)
Other-choice charters	0.249***		-0.018		0.282***	
	(0.075)		(0.078)		(0.085)	
Other-choice charters * CMO		0.297***		0.030		0.291***
		(0.072)		(0.072)		(0.082)
Other-choice charters * non-CMO		0.023		-0.380		0.193
		(0.287)		(0.348)		(0.341)
N	2,525	2,525	2,525	2,525	2,525	2,525

Notes: This table presents 2SLS estimates for a 2 endogenous/2 IV model using saturated frequency score controls. The 2 endogenous variables are first choice charters and other choice charters. Estimates are also presented for a 4 endogenous/ 4 IV model using saturated frequency score controls. Both models use the same controls as Table 6. The 4 endogenous variables are: first-choice CMO, first-choice non-CMO, other-choice CMO and other-choice non-CMO. See Table 2 for notes on CMO and non-CMO charters. The first-choice charter instrument is constructed as a dummy variable for an offer from a first choice charter. The other-choice charter instrument is a dummy variable for an offer from a charter which is ranked below the applicant's first choice. The first-choice CMO instrument is a dummy for an offer from a CMO charter which is the applicant's first choice. The other-choice CMO instrument is a dummy for an offer from a CMO which is ranked below the applicant's first choice. The first-choice non-CMO instrument is a dummy for an offer from a non-CMO charter which is the applicant's first choice. The other-choice non-CMO instrument is a dummy for an offer from a non-CMO charter which is ranked below the applicant's first choice. Robust standard errors are reported in parentheses. For applicants who applied in both years, we only consider their first time application.

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

A Theoretical Appendix

A.1 Equal Treatment of Equals

The text claimed that a number of mechanisms satisfy the ETE property. In this section, we formally demonstrate that other mechanisms using lottery numbers satisfy ETE. Fix the set of applicants and suppose that each applicant is assigned L lottery numbers. For example, L is equal to 1 if every applicant is assigned a single random number as in a DA with single tie breakers, and L is equal to the number of schools if every applicant is assigned a potentially different lottery number at every school. Let $r_i = (r_{i1}, ..., r_{iL})$ be the vector of applicant i's lottery numbers and $r = (r_i : i \in I)$. Assume that, for any $\ell \in \{1, ..., L\}$, $r_{i\ell} = r_{j\ell}$ if and only if $i = j$.

Recall that a stochastic mechanism maps a school choice problem into a distribution of possible assignments. Given lottery numbers r, we can be more explicit about how a stochastic mechanism is constructed by defining a function ϕ which maps the set of applicants and their random numbers to a deterministic assignment. ϕ is the allocation produced for a particular lottery realization. We suppress the fixed set of applicants in the notation below for the sake of expositional simplicity. Let $\phi_i(r)$ denote i's assignment when lottery numbers are given by r and $\phi(r) = (\phi_i(r) : i \in I)$.

Given r and $i, j \in I$, let i and j swap their random number vectors and denote the resulting set of random number vectors as $(r_{(i,j)}, r_j, r_i)$. We say that ϕ is *anonymous* if for any i, j and r such that $i \neq j$ and $\theta_i = \theta_j$, we have

$$\begin{aligned}
&\phi_i(r_{(i,j)}, r_j, r_i) = \phi_j(r), \\
&\phi_j(r_{(i,j)}, r_j, r_i) = \phi_i(r), \text{ and} \\
&\phi_k(r_{(i,j)}, r_j, r_i) = \phi_k(r) \text{ for all } k \in I \setminus \{i, j\}
\end{aligned}$$

We construct a stochastic mechanism by taking a draw over lottery numbers. Let h be a probability distribution over r with support R^h. Then given (ϕ, h), we can construct the corresponding stochastic mechanism, which we denote ϕ^h. Let $\phi_i^h(s)$ is the probability that i is assigned s, that is,

$$\phi_i^h(s) = \sum_{r \in R^h} h(r) \mathbb{1}(\phi_i(r) = s)$$

when the support of h is countable, and

$$\phi_i^h(s) = \int_{R^h} h(r) \mathbb{1}(\phi_i(r) = s) dr$$

otherwise. We say that a random lottery h is *symmetric* if we have $h(r) = h(r_{(i,j)}, r_j, r_i)$ for any i, j and r such that $i \neq j$ and $\theta_i = \theta_j$.

Lemma 1. *If ϕ is anonymous and h is symmetric, then stochastic mechanism ϕ^h satisfies Equal Treatment of Equals, that is,*

$$\phi_i^h(s) = \phi_j^h(s)$$

for all s and i, j such that $\theta_i = \theta_j$.

Proof. Assume that ϕ is anonymous and h is symmetric. Consider any i and j such that $i \neq j$, $\theta_i = \theta_j$.

The set of possible assignments is finite. So, by grouping together the sets of random numbers that yield the same assignment and redefining h, we can assume without of loss of generality that R^h has finite cardinality. Formally, let $M = \{\phi(r) : r \in R^h\}$ be the set of all assignments by (ϕ, h), which is a finite set. Construct a new lottery g as follows: Let R^g denote the support of g. For each assignment $m \in M$, pick some $r \in R^h$ such that $\phi(r) = m$, include it in R^g and set

$$g(r) = \int h(z)\mathbb{1}(\phi(z) = m)dz$$

Then, by construction,

$$\phi_i^g(s) = \phi_i^h(s)$$

for all s and i. So assume without loss of generality that R^h has finite cardinality.

Let $\{R, R^{ij}\}$ be a partition of R^h such that $r \in R$ if and only if $(r_{(i,j)}, r_j, r_i) \in R^{ij}$. Such partition exists trivially by the symmetry of h and finite cardinality of R^h. Then

$$\begin{aligned}
\phi_i^h(s) &= \sum_{r \in R \cup R^{ij}} h(r)\mathbb{1}(\phi_i(r) = s) \\
&= \sum_{r \in R} h(r)\mathbb{1}(\phi_i(r) = s) + h(r_{(i,j)}, r_j, r_i)\mathbb{1}(\phi_i(r_{(i,j)}, r_j, r_i) = s) \\
&= \sum_{r \in R} h(r_{(i,j)}, r_j, r_i)\mathbb{1}(\phi_j(r_{(i,j)}, r_j, r_i) = s) + h(r)\mathbb{1}(\phi_j(r) = s) \\
&= \sum_{r \in R \cup R^{ij}} h(r)\mathbb{1}(\phi_j(r) = s) \\
&= \phi_j^h(s),
\end{aligned}$$

where the first equality is the definition of $\phi_i^h(s)$, the second definition follows from the way the partition is constructed, the third follows from (i) $h(r) = h(r_{(i,j)}, r_j, r_i)$ by symmetry of h, and (ii) $\phi_i(r) = s \Leftrightarrow \phi_j(r_{(i,j)}, r_j, r_i) = s$ by ϕ being anonymous. The fourth equality follows from the way the partition is constructed. Finally the fifth equality is by definition. This completes the proof. □

This result implies that the following mechanisms with a symmetric lottery satisfy ETE: DA, the immediate acceptance ("Boston") mechanism, random serial dictatorship mechanism, and TTC, since each is, by construction, anonymous. To see why, consider any i and j with $\theta_i = \theta_j$. When i and j swap their lottery numbers, they swap their roles in the implementation of each mechanism as well, consequently they swap their assignments. It's worth noting that Lemma 1 allows us to conclude that versions of these mechanisms using school-specific tie-breaking satisfy ETE. The only requirement on lotteries is symmetry. ETE is also satisfied with more elaborate lotteries where certain types are favored in tie-breaking.

A.2 Defining DA: Details

Our general formulation defines the DA match 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

$$Q_s(\mathbf{c}^t) = \{i \in I \mid \pi_{is} \leq c_s^t \text{ and } s \succ_i \tilde{s} \text{ for all } \tilde{s} \in S \text{ such that } \pi_{i\tilde{s}} \leq c_{\tilde{s}}^t\}. \tag{15}$$

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

Lemma 2. *Consider an economy described by a distribution of applicants F and school capacities as defined in Section 2.1. 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,

$$\begin{aligned} & F(\{i \in Q_s(\mathbf{c}^T) \text{ such that } \pi_{is} \leq c_s^{T+1}\}) \\ & \quad = F(\{i \in Q_s(\mathbf{c}^T) \text{ such that } \pi_{is} \leq c_s^T\}) + F(\{i \in Q_s(\mathbf{c}^T) \text{ such that } c_s^T < \pi_{is} \leq c_s^{T+1}\}) \\ & \quad \geq F(\{i \in Q_s(\mathbf{c}^{T-1}) \text{ such that } \pi_{is} \leq c_s^T\}) + F(\{i \in Q_s(\mathbf{c}^T) \text{ such that } c_s^T < \pi_{is} \leq c_s^{T+1}\}) & (16) \\ & \quad \geq q_s + F(\{i \in Q_s(\mathbf{c}^T) \text{ such that } c_s^T < \pi_{is} \leq c_s^{T+1}\}) & (17) \\ & \quad > q_s. & (18) \end{aligned}$$

Expression (16) 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 (17) follows by the inductive assumption and since $c_s^T < K+1$.
Expression (18) 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 (18) 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.3 Proof of Theorem 1

Note first that 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 $\tilde{s}$.

Now, consider the propensity score for school s. Applicants who don't rank s have $p_s(\theta) = 0$. Among those who do rank s, those of type $\theta \in \Theta_s^n$ have $\rho_{\theta s} > \rho_s$. Therefore $p_s(\theta) = 0$ for every $\theta \in \Theta_s^n \cup (\Theta \backslash \Theta_s)$.

Applicants of type $\theta \in \Theta_s^a \cup \Theta_s^c$ may be assigned $\tilde{s} \in B_{\theta s}$, where $\rho_{\theta \tilde{s}} = \rho_{\tilde{s}}$. Since lottery numbers are uniform, the proportion of type θ applicants assigned some $\tilde{s} \in B_{\theta s}$ where $\rho_{\theta \tilde{s}} = \rho_{\tilde{s}}$ is $MID_{\theta \tilde{s}}$. In other words, the probability of not being assigned any $\tilde{s} \in B_{\theta s}$ where $\rho_{\theta \tilde{s}} = \rho_{\tilde{s}}$ for a type θ applicant is $1 - MID_{\theta s}$. Every applicant of type $\theta \in \Theta_s^a$ who is not assigned a higher choice is assigned s because $\rho_{\theta s} < \rho_s$, and so

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

Finally, consider applicants of type $\theta \in \Theta_s^c$ who are not assigned a higher choice. The fraction of applicants $\theta \in \Theta_s^c$ who are not assigned a higher choice is $1 - MID_{\theta s}$. Also, the random numbers of these applicants is larger than $MID_{\theta s}$. If $\tau_s < MID_{\theta s}$, then no such applicant is assigned s. If $\tau_s \geq MID_{\theta s}$, then the ratio of applicants that are assigned s within this set is given by $\frac{\tau_s - MID_{\theta s}}{1 - MID_{\theta s}}$. Hence, conditional on $\theta \in \Theta_s^c$ and not being assigned a choice higher than s, the probability of being assigned s is given by $\max\{0, \frac{\tau_s - MID_{\theta s}}{1-MID_{\theta s}}\}$. Therefore,

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

A.4 Proof of Theorem 2

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

Lemma 3. *(Cutoff almost sure convergence)* $\hat{\mathbf{c}}_n \xrightarrow{a.s.} \mathbf{c}$.

Lemma 4. *(Propensity score almost sure convergence) For all* $\theta \in \Theta$ *and* $s \in S$, $p_{ns}(\theta) \xrightarrow{a.s.} \varphi_s(\theta)$.

A.4.1 Proof of Lemma 3

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 applicant 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 applicants of types in Θ_0 with random numbers worse than r_0 and better than r_1 as

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

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 applicants,

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

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 deterministic sequence of economies described by a sequence of distributions $\{f_n\}$ over applicants, together with associated school capacities, so that for all n, $f_n \in \mathcal{F}$ is a potential realization produced by randomly drawing n applicants 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 applicants in F, which contains the set of applicants in f_n for all n. In other words, I_n^s are the set of applicants ranking school s first who have an applicant 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 applicants 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

$$
\begin{aligned}
&\tilde{f}_n(\{i \in Q_s(\mathbf{c}_n) \text{ such that } \pi_{is} \leq c_s\}) \\
&\quad = \tilde{f}_n(\{i \in Q_s(\mathbf{c}_n) \text{ such that } \pi_{is} \leq \tilde{c}_{ns}\}) + \tilde{f}_n(\{i \in Q_s(\mathbf{c}_n) \text{ such that } \tilde{c}_{ns} < \pi_{is} \leq c_s\}) \\
&\quad = \tilde{f}_n(\{i \in Q_s(\mathbf{c}_n) \text{ such that } \pi_{is} \leq \tilde{c}_{ns}\}) && (19) \\
&\quad \leq q_s. && (20)
\end{aligned}
$$

Expression (19) follows from Assumption 1 by the following reason. (19) 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 (20) follows by our definition of DA, which can never assign more applicants 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 (20) 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 applicants assigned $\tilde{s}_n$ under $\tilde{f}_n$ and F, respectively. All applicants 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 applicants rank $\tilde{s}_n$ first, there must exist a positive measure (with respect to $\tilde{f}_n$) of applicants 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 $\tilde{f}_n(A_{\tilde{s}_n}) \rightarrow F(A_{\tilde{s}_n})$ 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 applicants 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 applicant 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 $\tilde{\mathbf{c}}_n \rightarrow \mathbf{c}$. The following claim uses this to show that $\mathbf{c}_n \rightarrow \mathbf{c}$.

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

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 3 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 applicants from F. Note that F_n is random since it involves randomly drawing n applicants. $F_n \overset{a.s.}{\to} F$ 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 3. □

A.4.2 Proof of Lemma 4

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

For Lemma 4, it is enough to show deterministic convergence of this finite-market score, that is, $p_{ns}(\theta) \to \varphi_s(\theta)$ as $f_n \to F$. To see this, let F_n be the distribution over $I(\Theta_0, r_0, r_1)$'s induced by randomly drawing n applicants from F. Note that F_n is random and that $F_n \overset{a.s.}{\to} F$ by the Glivenko-Cantelli theorem (Theorem 19.1 in van der Vaart (2000)). $F_n \overset{a.s.}{\to} F$ and $p_{ns}(\theta) \to \varphi_s(\theta)$ allow us to apply the Extended Continuous Mapping Theorem (Theorem 18.11 in van der Vaart (2000)) to obtain $\tilde{p}_{ns}(\theta) \xrightarrow{a.s.} \varphi_s(\theta)$.

We prove convergence of $p_{ns}(\theta) \to \varphi_s(\theta)$ 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 $\varphi_s(\theta)$ and $p_{ns}(\theta)$ as follows.

$$\varphi_s(\theta) = \max\{0, \tau_{\theta s} - \tau_{\theta s_-}\}$$

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

where P_n is the probability induced by randomly drawing lottery numbers given f_n, and R is a random (not realized) lottery number for any type θ applicant, where we omit an applicant subscript for simplicity. R's marginal distribution is $U[0,1]$.

By Lemma 3, with probability 1, for all $\epsilon_1 > 0$, there exists N_1 such that for all $n > N_1$,

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

which implies that with probability 1,

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

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

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

where the second-to-last inclusion is because with probability 1, there exists N such that for all $n > N$ such that $|\tilde{\tau}_{n\theta s} - \tau_{\theta s}|, |\tilde{\tau}_{n\theta s_-} - \tau_{\theta s_-}| < \epsilon_1$ and $R \sim U[0,1]$. This means $p_{ns}(\theta) \to \varphi_s(\theta)$, completing the proof of Lemma 4. □

A.5 Example 2 in the Continuum

In the large-market analog of Example 2, 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, so

$$B_{3a} = \{b\} \quad \text{and} \quad B_{2a} = \{b, c\}.$$

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 applicants 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 applicants (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 applicants for seats at b. We therefore have $1.5n$ school-b hopefuls but only n seats at b. All type 3 applicants with lottery numbers below 0.5 get seated at b (the type 2 applicants 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 applicants in the upper half of the lottery distribution, implying that the highest lottery number seated at b is $\tau_b = 0.75$.

Since there are no priorities, type 2 and type 3 are in Θ_s^c and type 2 and 3 applicants 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 (2) gives the DA score for types 2 and 3:

$$\begin{aligned}\varphi_a(\theta) &= (1 - MID_{\theta a}) \times \max\left\{0, \frac{\tau_a - MID_{\theta a}}{1 - MID_{\theta a}}\right\} \\ &= (1 - 0.75) \times \max\left\{0, \frac{5/6 - 0.75}{1 - 0.75}\right\} \\ &= \frac{1}{12}.\end{aligned}$$

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

A.6 Proof of Proposition 2

Proof. By the definition of MID, for any θ and s, there exists $\tilde{s}$ such that $MID_{\theta s} = \tau_{\tilde{s}}$, which is the decimal part of $c_{\tilde{s}}$. Cutoff vector $\mathbf{c}$ also pins down Θ_s^a and Θ_s^c. Thus, the assumption ($r_i \perp\!\!\!\perp \mathbf{c}$) implies that individual lottery numbers r_i are uniformly distributed over [0, 1] (not only unconditionally but also) conditional on any cutoff, MID, Θ_s^a, Θ_s^c, and type. This gives us both unbiasedness and conditional independence. When $\tilde{p}_s(\theta)$ is the formula version of the estimated DA propensity score, the DA propensity score is unbiased for the true propensity score, i.e., $E(\tilde{p}_s(\theta)) = p_s(\theta)$ for every applicant type θ since

$$\begin{aligned}
&E(\tilde{p}_s(\theta)) \qquad \text{(a)}\\
&= E(1\{\theta_i \in \Theta_s^a\}(1 - MID_{\theta_i s}) + 1\{\theta_i \in \Theta_s^c\}(1 - MID_{\theta_i s}) \max\{0, \frac{\tau_s - MID_{\theta_i s}}{1 - MID_{\theta_i s}}\}|\theta_i = \theta) \qquad \text{(b)}\\
&= E(E(1\{\theta_i \in \Theta_s^a\}1\{MID_{\theta_i s} < r_i\} + 1\{\theta_i \in \Theta_s^c\}1\{MID_{\theta_i s} < r_i \le \tau_s\}\\
&\quad |\theta_i = \theta, \tau_s, MID_{\theta_i s}, \Theta_s^a, \Theta_s^c)|\theta_i = \theta)\\
&= E(E(1\{\theta_i \in \Theta_s^a \text{ and } MID_{\theta_i s} < r_i\} + 1\{\theta_i \in \Theta_s^c \text{ and } MID_{\theta_i s} < r_i \le \tau_s\}\\
&\quad |\theta_i = \theta, \tau_s, MID_{\theta_i s}, \Theta_s^a, \Theta_s^c)|\theta_i = \theta)\\
&= E(1\{\theta_i \in \Theta_s^a \text{ and } MID_{\theta_i s} < r_i\} + 1\{\theta_i \in \Theta_s^c \text{ and } MID_{\theta_i s} < r_i \le \tau_s\}|\theta_i = \theta)\\
&= E(1\{(\theta_i \in \Theta_s^a \text{ and } MID_{\theta_i s} < r_i) \text{ or } (\theta_i \in \Theta_s^c \text{ and } MID_{\theta_i s} < r_i \le \tau_s)\}|\theta_i = \theta)\\
&= P(D_i(s) = 1|\theta_i = \theta)
\end{aligned}$$

where the first and fourth equalities are by the law of iterated expectation, and the second equality is by $r_i \sim U[0,1]$ conditional on any cutoff, MID, Θ_s^a, Θ_s^c, and type. To obtain this result for the frequency version of the estimated DA propensity score, insert the following lines between equations (a) and (b).

$$
\begin{aligned}
&(a)\\
=&E(\Sigma_{\delta=a,c,n}1\{\theta_i \in \Theta_s^\delta\}\frac{\Sigma_{j\in I}D_j(s)1\{1\{\theta_j \in \Theta_s^\delta\}=1\{\theta_i \in \Theta_s^\delta\}, MID_{\theta_j s}=MID_{\theta_i s}\}}{\Sigma_{j\in I}1\{1\{\theta_j \in \Theta_s^\delta\}=1\{\theta_i \in \Theta_s^\delta\}, MID_{\theta_j s}=MID_{\theta_i s}\}}|\theta_i=\theta)\\
=&E(E(\Sigma_{\delta=a,c,n}1\{\theta_i \in \Theta_s^\delta\}\frac{\Sigma_{j\in I}D_j(s)1\{1\{\theta_j \in \Theta_s^\delta\}=1\{\theta_i \in \Theta_s^\delta\}, MID_{\theta_j s}=MID_{\theta_i s}\}}{\Sigma_{j\in I}1\{1\{\theta_j \in \Theta_s^\delta\}=1\{\theta_i \in \Theta_s^\delta\}, MID_{\theta_j s}=MID_{\theta_i s}\}}\\
&|\theta_i=\theta, \tau_s, MID_{\theta_i s}, \Theta_s^a, \Theta_s^c)|\theta_i=\theta)\\
=&E(E(1\{\theta_i \in \Theta_s^a\}(1-MID_{\theta_i s})+1\{\theta_i \in \Theta_s^c\}(1-MID_{\theta_i s})\max\{0, \frac{\tau_s-MID_{\theta_i s}}{1-MID_{\theta_i s}}\}\\
&|\theta_i=\theta, \tau_s, MID_{\theta_i s}, \Theta_s^a, \Theta_s^c)|\theta_i=\theta)\\
=&(b)
\end{aligned}
$$

where the first equality is by the definition of the frequency DA score, the second equality is by the law of iterated expectation, and the third equality is by $r_i \sim U[0,1]$ conditional on any cutoff, MID, Θ_s^a, Θ_s^c, and type.

Assignment is independent conditional on the formula version of the estimated DA propensity score, i.e., $P(D_i(s)=1|\tilde{p}_s(\theta_i), \theta_i) = P(D_i(s)=1|\tilde{p}_s(\theta_i))$ by the following reason:

$$
\begin{aligned}
&P(D_i(s)=1|\tilde{p}_s(\theta_i)=p, \theta_i)\\
&=E(1\{(\theta_i \in \Theta_s^a \text{ and } MID_{\theta_i s}<r_i) \text{ or } (\theta_i \in \Theta_s^c \text{ and } MID_{\theta_i s}<r_i\le\tau_s)\}|\tilde{p}_s(\theta_i)=p, \theta_i)\\
&=E(E(1\{(\theta_i \in \Theta_s^a \text{ and } MID_{\theta_i s}<r_i) \text{ or } (\theta_i \in \Theta_s^c \text{ and } MID_{\theta_i s}<r_i\le\tau_s)\}\\
&\quad|\tau_s, MID_{\theta_i s}, \Theta_s^a, \Theta_s^c, \tilde{p}_s(\theta_i)=p, \theta_i)|\tilde{p}_s(\theta_i)=p, \theta_i) \qquad (c)\\
&=E(E(1\{(\theta_i \in \Theta_s^a \text{ and } MID_{\theta_i s}<r_i) \text{ or } (\theta_i \in \Theta_s^c \text{ and } MID_{\theta_i s}<r_i\le\tau_s)\}\\
&\quad|\tau_s, MID_{\theta_i s}, \Theta_s^a, \Theta_s^c, \theta_i)|\tilde{p}_s(\theta_i)=p, \theta_i)\\
&=E(1\{\theta_i \in \Theta_s^a\}(1-MID_{\theta_i s})+1\{\theta_i \in \Theta_s^c\}(1-MID_{\theta_i s})\max\{0, \frac{\tau_s-MID_{\theta_i s}}{1-MID_{\theta_i s}}\}|\tilde{p}_s(\theta_i)=p, \theta_i)\\
&=E(\tilde{p}_s(\theta_i)|\tilde{p}_s(\theta_i)=p, \theta_i) \qquad (d)\\
&=p,
\end{aligned}
$$

which is independent from θ_i conditional on $\tilde{p}_s(\theta_i)=p$. In the above calculations, the first equality is by the definition of $D_i(s)$, the second equality is by the law of iterated expectation, the third equality is by the fact that $\tau_s, MID_{\theta_i s}, \Theta_s^a$, and Θ_s^c pin down $\tilde{p}_s(\theta_i)$, the fourth equality is by $r_i \sim U[0,1]$ conditional on any cutoff, MID, Θ_s^a, Θ_s^c, and type, and the fifth equality is by the definition of $\tilde{p}_s(\theta_i)$. Assignment is also independent conditional on the frequency version of the estimated DA propensity score since for the frequency version, equation (c) directly implies (d). □

A.7 Modes of Inference

Econometric inference typically tries to quantify the uncertainty due to *random sampling*. What then, to make of the fact that the analysis reported here uses data on all DPS applicants from 2012? On one hand, we might imagine that the applicants we happen to be studying constitute a random sample from some larger population of possible applicants. At the same time, the statistical 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 even when the sample of applicants is viewed as fixed.

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. Regression conditioning on the simulated score produces near-perfect balance in Table 4 so this distribution is what we should expect to see under the null hypothesis of no difference by treatment assignment. Randomization p-values are therefore given by quantiles of the t-statistics in the distribution resulting from these repeated draws.

The p-values associated with conventional robust t-statistics for covariate balance turn out to be close to the corresponding randomization p-values. For the number of charter schools an applicant has ranked, for example, the conventional p-value for balance is 0.885 while the corresponding randomization p-value is 0.850. This is consistent with a classic result on the asymptotic equivalence of randomization and sampling tests for differences in means (see, e.g., 15.2 in Lehmann and Romano 2005).

Abadie et al. (2014) generalize results on the large-sample equivalence of randomization and sampling inference to cover regression estimates of treatment effects and tests for covariate balance of the sort reported here. If the regression function is linear and the regression of treatment on controls is linear, the usual robust covariance matrix associated with random sampling is asymptotically valid for the sampling distribution induced by random assignment.[36] The treatment in our case is an offer dummy, while the controls are dummies or a linear model for the propensity score. The second of these requirements holds here when the controls fully saturate the propensity score (ignoring any additional covariates). The first requires constant offer effects given a saturated model for the score. The models estimated here don't quite satisfy these conditions (they're not fully saturated) but do not seem to be so far off that this matters for inference.

A related issue arises from the fact that the empirical strategy used here conditions on estimates of the propensity score (the simulated score is also an estimate since it's based on a finite number of draws). As noted by Hirano et al. (2003) and Abadie and Imbens (2016), conditioning on an estimated as opposed to a non-stochastic known score may affect sampling distributions of the resulting estimated causal effects. We therefore checked conventional large-sample p-values

[36] This is Theorem 3, in Abadie et al. (2014), a result predicated on independent treatment assignments. In practice, DA assignments are correlated. Here too, however, the large market approximation smooths things out. In the continuum, cutoffs are fixed, and treatments are determined by individual independently drawn lottery numbers. We can therefore think of the asymptotic equivalence of randomization and conventional inference as a further consequence of our large market approximation.

against randomization p-values for the reduced-form charter offer effects associated with the 2SLS estimates discussed in the next section. Robust asymptotic sampling formulas again 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 robust standard errors and test statistics for inference about 2SLS estimates of treatment effects.

A.8 First Choice and Qualification Instruments: Details

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

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

Proof. In general,

$$\begin{aligned}
&\Pr(D_i^f = 1|\theta_i = \theta)\\
&= \Pr(\pi_{i\tilde{s}_i} \leq c_{\tilde{s}_i}|\theta_i = \theta)\\
&= \Pr(\rho_{i\tilde{s}_i} + r_i \leq c_{\tilde{s}_i}|\theta_i = \theta)\\
&= \Pr(r_i \leq c_{\tilde{s}_i} - \rho_{i\tilde{s}_i}|\theta_i = \theta)\\
&= c_{\tilde{s}_i} - \rho_{i\tilde{s}_i},
\end{aligned}$$

which depends on θ_i only through $X(\theta_i)$ because cutoffs are fixed in the continuum.. □

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

Proposition 4. *In any continuum economy, D_i^q is independent of θ_i conditional on $X(\theta_i)$.*

Proof. In general, we have

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

which depends on θ_i only through $X(\theta_i)$ because cutoffs are fixed in the continuum. □

A.9 Extension to a General Lottery Structure

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

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

Recall $B_{\theta s}$ is defined as $\{s' \in S \mid s' \succ_\theta s\}$. Partition $B_{\theta s}$ into $\bar{m}$ disjoint sets $B^1_{\theta s}, ..., B^{\bar{m}}_{\theta s}$, so that s' and s'' use the same lottery if and only if $s', s'' \in B^m_{\theta s}$ for some m. Note that this partition is specific to type θ. With single-school lotteries, $\bar{m}$ simplifies to $|B_{\theta s}|$, the number of schools type θ ranks ahead of s.

The *most informative disqualification*, $MID^m_{\theta s}$, is defined for each m as

$$MID^m_{\theta s} \equiv \begin{cases} 0 & \text{if } \rho_{\theta \tilde{s}} > \rho_{\tilde{s}} \text{ for all } \tilde{s} \in B^m_{\theta s}, \\ 1 & \text{if } \rho_{\theta \tilde{s}} < \rho_{\tilde{s}} \text{ for some } \tilde{s} \in B^m_{\theta s}, \\ \max\{\tau_{\tilde{s}} \mid \tilde{s} \in B^m_{\theta s} \text{ and } \rho_{\theta \tilde{s}} = \rho_{\tilde{s}}\} & \text{if } \rho_{\theta \tilde{s}} = \rho_{\tilde{s}} \text{ for } \tilde{s} \in B^m_{\theta s} \text{ and } \rho_{\theta \tilde{s}} > \rho_{\tilde{s}} \text{ otherwise.} \end{cases}$$

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

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

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

where we set $\varphi_s(\theta) = 0$ when $MID^*_{\theta s} = 1$ and $\theta \in \Theta^c_s$.

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

A.10 The Boston (Immediate Acceptance) Mechanism

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

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

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

- Step t: Each applicant 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 applicants up to its remaining capacity and rejects every other applicant.

This algorithm terminates at the first step in which no applicant applies to a school. Boston assignments differ DA in that any offer at any step is fixed; applicants receiving offers cannot be displaced later. This important difference notwithstanding, the Boston mechanism can be represented as a special case of DA by redefining priorities as follows:

Proposition 5. *(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* $k = 1, 2...$, $\{$*applicants who rank s k-th*$\}$ $\succ_s^*$ $\{$*applicants who rank s* $k+1$*-th*$\}$

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

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

B Empirical Appendix

B.1 Data

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 applicants can be matched across years and files. Applicant data are from the 2012-2013 SchoolChoice assignment file and test score data are from the CSAP (Colorado Student Assessment Program) and the TCAP (Transitional Colorado Assessment Program) files. The CSAP was discontinued in 2011, and was replaced by the TCAP beginning with the 2012-2013 school year. Enrollment, demographic, and outcome data are available for applicants enrolled in DPS only; enrollment data are for October.

Applications and assignment: The SchoolChoice file

The 2012-2013 SchoolChoice assignment file contains information on applicants' preferences over schools (school rankings), school priorities over applicants, applicants' school assignments (offers) and lottery numbers, a flag for whether the applicant is subject to the family link policy described in the main text and, if so, to which sibling the applicant is linked. Each observation in the assignment file corresponds to an applicant applying for a seat in programs within schools known as a bucket.[37] Each applicant receives at most one offer across all buckets at a school. Information on applicant preferences, school priorities, lottery numbers, and offers are used to compute the DA propensity score and the simulated propensity score.

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

Enrollment and demographic characteristics

Each observation in the enrollment files describes a student enrolled in a school in a year, and includes information on grade attended, student sex, race, gifted status, bilingual status, special education status, limited English proficiency status, and subsidized lunch eligibility.[38] Demographic and enrollment information are from the first calendar year a student spent in each grade.

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

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

Applicant outcomes: CSAP/TCAP

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

School classification: Parent Guide

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

Table B1: DPS SchoolChoice application records

	All applicants		In DPS at baseline	
	Applicants	Types	Applicants	Types
	(1)	(2)	(3)	(4)
	A. 2013			
All applicants	25,687	15,283	15,487	9,018
Applicants for grades 4 through 10	12,507	6,970	10,898	6,245
Applicants to any charters (for grades 4 through 10)	5,669	4,606	4,964	4,124
	B. 2014			
All applicants	27,364	17,169	16,558	10,102
Applicants for grades 4 through 10	12,997	7,243	11,413	6,535
Applicants to any charters (for grades 4 through 10)	5,920	4,842	5,239	4,342

Notes: Applications are for the 2012-13 and 2013-14 academic years. Columns 1 and 2 count all applicants in the SchoolChoice assignment file. Columns 3 and 4 exclude applicants not enrolled in DPS in the relevant baseline grade (the grade prior to application grade) in the baseline year (2011-12 and 2012-2013). Applicants to grade "EC" (early childhood, or pre-kindergarten) are excluded from columns 3 and 4. Columns 2 and 4 count unique combinations of applicant preferences over school programs and school priorities in those programs.

Table B2: Attrition by offer status

	Non-offered mean	No controls	Simulated score controls (hundredths)	DA score controls: Frequency (saturated)	DA score controls: Formula (saturated)
	(1)	(2)	(3)	(4)	(5)
Enrolled in DPS in follow-up year	0.902	0.034***	0.031**	0.032**	0.033**
		(0.005)	(0.014)	(0.014)	(0.015)
Has scores in follow-up year	0.878	0.035***	0.033**	0.037**	0.035**
		(0.006)	(0.015)	(0.015)	(0.016)
N	5,674	9,879	2,714	2,436	2,335

Notes: This table reports coefficients from regressions of DPS enrollment and test-score availability indicators on charter offers, similar to the tests done in Table 5. Column 1 reports follow-up rates for charter applicants who did not receive a charter offer. The propensity score control schemes used to construct the estimates in columns 3, 4 and 5 parallel those used for Table 5. Robust standard errors are reported in parentheses. For applicants who applied in both years, we only consider their first time application.

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

Table B3: DPS innovation schools

School	Total applicants 2013 (1)	Total applicants 2014 (2)	Applicants offered seats 2013 (3)	Applicants offered seats 2014 (4)	Simulated score in (0,1): Total applicants 2013 (5)	Simulated score in (0,1): Total applicants 2014 (6)	Simulated score in (0,1): Applicants (first choice) 2013 (7)	Simulated score in (0,1): Applicants (first choice) 2014 (8)
Elementary and middle schools								
Centennial ECE-8 School	0	15	0	8	0	0	0	0
Cole Arts and Science Academy	31	46	15	23	10	5	6	3
DCIS at Fairmont	0	27	0	13	0	8	0	6
DCIS at Ford	16	36	0	15	1	8	0	2
DCIS at Montbello MS	412	463	125	157	170	298	68	125
Denver Green School	153	205	62	80	52	73	18	22
Denver Public Montessori	0	95	0	49	0	27	0	10
Godsman Elementary	10	26	8	10	0	0	0	0
Grant Beacon Middle School	0	483	0	203	0	126	0	24
Green Valley Elementary	53	55	15	23	36	24	2	3
Martin Luther King Jr. Early College	427	430	177	144	122	309	0	71
McAuliffe International School	406	584	165	233	113	180	54	104
McGlone	14	44	2	10	3	14	0	5
Montclair Elementary	15	22	11	5	1	1	0	0
Noel Community Arts School	288	385	108	106	106	291	2	54
Swigert International School	0	25	0	0	0	3	0	0
Trevista ECE-8 at Horace Mann	0	90	0	25	0	2	0	0
Valdez Elementary	6	9	3	2	1	1	0	0
West Generations Academy MS	0	192	0	78	0	65	0	10
West Leadership Academy	0	223	0	107	0	64	0	13
Whittier K-8 School	47	83	8	22	4	29	0	5
High schools								
Collegiate Preparatory Academy	433	312	125	53	165	147	0	17
DCIS at Montbello	506	508	125	131	190	233	76	109
High-Tech Early College	481	524	125	199	226	217	74	10
Manual High School	390	412	130	152	197	104	7	16
Martin Luther King Jr. Early College	515	550	144	183	171	270	29	188
Noel Community Arts School	334	406	78	120	110	197	1	57
West Generations Academy	0	111	0	26	0	40	0	0
West Leadership Academy	0	91	0	22	0	28	0	1

Notes: This table describes DPS innovation applications in a format like that used for charters in Table 2.

Table B4: Statistical tests for balance and differential attrition for DPS innovation schools

	Non-offered mean	No controls	Simulated score controls: Rounded (hundredths)	Simulated score controls: Rounded (thousandths)	DA score controls: Frequency (saturated)	DA score controls: Formula (saturated)
	(1)	(2)	(3)	(4)	(5)	(6)
A. Application covariates						
Number of schools ranked	4.573	-0.396***	0.129	0.115	0.036	-0.012
		(0.039)	(0.085)	(0.092)	(0.088)	(0.085)
Number of charter schools ranked	1.251	0.628***	0.117**	0.113*	0.046	0.014
		(0.021)	(0.052)	(0.060)	(0.049)	(0.051)
First school ranked is charter	0.069	0.611***	-0.008	0.005	0.005	-0.012
		(0.009)	(0.019)	(0.020)	(0.016)	(0.013)
B. Baseline applicant characteristics						
Origin school is charter	0.126	0.138***	0.019	0.037	0.034	0.029
		(0.010)	(0.025)	(0.030)	(0.029)	(0.029)
Female	0.510	-0.007	0.035	0.028	-0.008	0.000
		(0.013)	(0.033)	(0.039)	(0.038)	(0.038)
Hispanic	0.537	0.108***	0.033	0.052	-0.026	-0.006
		(0.013)	(0.031)	(0.035)	(0.035)	(0.034)
Black	0.230	-0.049***	-0.008	-0.016	0.014	0.001
		(0.010)	(0.027)	(0.032)	(0.031)	(0.031)
Gifted	0.222	-0.071***	0.026	0.018	-0.027	-0.006
		(0.010)	(0.025)	(0.030)	(0.028)	(0.029)
Bilingual	0.028	0.009**	-0.027**	-0.022	-0.032**	-0.029*
		(0.005)	(0.013)	(0.015)	(0.014)	(0.015)
Subsidized lunch	0.763	0.064***	0.007	0.020	0.012	0.013
		(0.010)	(0.024)	(0.026)	(0.027)	(0.026)
Limited English proficient	0.288	0.029**	0.009	0.020	0.016	0.006
		(0.012)	(0.031)	(0.035)	(0.034)	(0.034)
Special education	0.104	0.012	-0.001	-0.004	0.018	0.012
		(0.008)	(0.019)	(0.022)	(0.022)	(0.021)
N	2,890	6,127	2,070	1,416	1,078	1,124
Baseline scores						
Math	-0.009	-0.221***	0.052	0.051	-0.029	0.017
		(0.026)	(0.060)	(0.071)	(0.066)	(0.067)
Reading	0.019	-0.211***	0.038	0.022	-0.016	0.013
		(0.025)	(0.059)	(0.069)	(0.066)	(0.067)
Writing	0.009	-0.192***	0.064	0.057	-0.014	0.036
		(0.025)	(0.058)	(0.068)	(0.063)	(0.064)
N	2,847	6,011	2,034	1,393	1,060	1,102
C. Differential attrition						
Enrolls in Denver in follow-up year	0.927	-0.026***	-0.025	-0.009	-0.023	-0.024
		(0.007)	(0.018)	(0.023)	(0.022)	(0.021)
Has scores in follow-up year	0.902	-0.036***	-0.016	-0.004	-0.026	-0.026
		(0.008)	(0.020)	(0.025)	(0.023)	(0.023)
N	2,890	6,127	2,070	1,416	1,078	1,124
Risk set points of support			75	114	66	78
Robust F-test for joint significance		331.8	1.02	0.90	0.77	0.49
p-value		0.000	0.428	0.559	0.710	0.946

Notes: Panels A and B report covariate balance tests for innovation offers in a manner analogous to that used for charter offer balance in Table 5. Panel C tests for attrition in a manner analogous to table B2. Robust standard errors are reported in parentheses. For applicants who applied in both years, we only consider their first time application. P-values for joint significance tests are estimated with stata's mvreg command.
*significant at 10%; **significant at 5%; ***significant at 1%

Table B5: DPS charter schools (2014 only)

School	CMO	Total applicants	Capacity	Applicants offered seats	Simulated score in (0,1): Total applicants	Simulated score in (0,1): Applicants (first choice)
	(1)	(2)	(3)	(4)	(5)	(6)
Elementary and middle schools						
Cesar Chavez Academy Denver		77	76	24	18	9
Denver Language School		12	100	0	0	0
DSST: Byers	Yes	280	156	152	128	45
DSST: Cole	Yes	508	215	205	188	74
DSST: College View	Yes	311	168	163	151	19
DSST: Green Valley Ranch	Yes	905	181	176	386	347
DSST: Stapleton	Yes	827	187	183	224	128
Girls Athletic Leadership School		155	87	73	72	38
Highline Academy Charter School		191	74	12	65	43
KIPP Montbello College Prep	Yes	253	72	64	161	14
KIPP Sunshine Peak Academy	Yes	476	84	75	1	0
Odyssey Charter Elementary		198	30	4	18	8
Omar D. Blair Charter School		375	185	53	138	42
Pioneer Charter School		65	76	13	17	4
SIMS Fayola International Academy Denver		94	37	33	68	18
SOAR at Green Valley Ranch		121	88	5	74	62
SOAR Oakland		58	149	14	7	1
STRIVE Prep - Federal	Yes	605	126	124	308	113
STRIVE Prep - GVR	Yes	416	130	127	279	76
STRIVE Prep - Highland	Yes	243	130	126	58	12
STRIVE Prep - Lake	Yes	310	129	129	114	108
STRIVE Prep - Montbello	Yes	222	70	63	167	39
STRIVE Prep - Westwood	Yes	563	135	133	304	175
Venture Prep		27	8	7	0	0
Wyatt Edison Charter Elementary		60	57	12	18	3
High schools						
DSST: Green Valley Ranch	Yes	764	76	76	259	238
DSST: Stapleton	Yes	480	23	23	130	76
KIPP Denver Collegiate High School	Yes	291	126	110	112	23
SIMS Fayola International Academy Denver		80	27	21	39	12
Southwest Early College		217	48	42	86	14
STRIVE Prep - Excel	Yes	203	140	133	54	1
STRIVE Prep - SMART	Yes	318	153	148	157	145
Venture Prep		137	44	31	65	14

Notes: This table describes DPS charter applications for the academic year 2013-2014. Column 1 lists all CMO schools. CMO stands for Charter Management Organization, and these schools are comprised of the DSST, STRIVE and KIPP networks. Column 2 reports the number of applicants ranking each school. Column 3 reports each school's capacity. Column 4 counts the number of applicants who received an offer from the school. Column 5 counts applicants with simulated score values strictly between zero and one. The simulated score is rounded to 0.001. Column 6 shows the subset of applicants from column 5 who rank each school as their first choice.

Table B6: Frequency Distribution of Types

	All Applicants						Applicants with Simulated Score in (0,1)					
Year	# of Types	Mean	25th Perc.	Median	75th Perc.	Max	# of Types	Mean	25th Perc.	Median	75th Perc.	Max
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
A. Applicants for grades 4-10												
i. Types Defined by All Ranked Choices and their Priorities												
2013	6,245	29.5	1	1	1	235	1,544	3.7	1	1	1	45
2014	6,535	26.6	1	1	1	226	1,919	2.9	1	1	1	29
ii. Types Defined by First Choice and its Priority												
2013	751	72.7	17	17	53	401	116	51.9	16	16	37	144
2014	867	63.5	14	14	46	300	163	45.2	13	13	32	121
iii. Types Defined by First and Second Choice and their Priorities												
2013	2,963	18.5	2	2	6	147	532	18.6	2	2	6	108
2014	3,414	15.9	2	2	5	168	769	11.3	2	2	4	70
iv. Types Defined by First, Second and Third Choice and their Priorities												
2013	5,023	11.7	1	1	2	132	972	10.3	1	1	1	100
2014	5,415	10.9	1	1	2	168	1,321	6.0	1	1	1	61
B. Charter Applicants												
i. Types Defined by All Ranked Choices and their Priorities												
2013	4,124	2.6	1	1	1	46	1,544	3.7	1	1	1	45
2014	4,342	2.4	1	1	1	32	1,919	2.9	1	1	1	29
ii. Types Defined by First Choice and its Priority												
2013	441	46.5	14	14	35	175	116	51.9	16	16	37	144
2014	537	46.1	11	11	32	173	163	45.2	13	13	32	121
iii. Types Defined by First and Second Choice and their Priorities												
2013	2,029	10.9	1	1	4	116	532	18.6	2	2	6	108
2014	2,306	9.8	1	1	3	96	769	11.3	2	2	4	70
iv. Types Defined by First, Second and Third Choice and their Priorities												
2013	3,400	5.1	1	1	1	102	972	10.3	1	1	1	100
2014	3,650	4.8	1	1	1	62	1,321	6.0	1	1	1	61

Notes: This table presents summary statistics on the frequency distribution of types. Panel A is for all applicants for grades 4-10. Panel B is for charter applicants. Type in i. is defined by all choices and their corresponding priorities. Type in ii. is defined by the top choice and its corresponding priority. Type in iii. is defined by the first and second choice and their corresponding priorities. Type in iv. is defined by first, second and third choice and their corresponding priorities.